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Introduction

A. Why Evaluate Early Childhood Development (ECD) Intervention Programs?

Three recently published papers in a prominent 2007 series in *Lancet* summarize what is known about early childhood development in developing countries. Estimates are that over 200 million children in developing countries under five years of age fail to reach their developmental potential because of risk factors associated with poverty (Grantham-McGregor *et al.* 2007). These risk factors are characterized in particular as including stunting, inadequate cognitive stimulation, iodine deficiencies, and iron deficiency anemia; but it is claimed that the evidence is also sufficient “to warrant interventions for malaria, intrauterine growth restriction, maternal depression, exposure to violence, and exposure to heavy metals,” (Walker *et al.* 2007, p. 145). Therefore, the third paper in this series concludes that “governments and civil society should consider expanding high quality, cost-effective early child development programmes,” (Engle *et al.* 2007, p. 229). Thus this series suggests that there potentially are considerable gains from expanded EDC interventions in developing countries.

As noted in the third of these papers, the interest in developing countries and in international development-oriented organizations in ECD programs has increased in recent years:

“Awareness of child development is increasing in developing countries. The health sector has advocated for early child development programmes for children with low birthweight, developmental delays, and from low-income disadvantaged environments. Child development information is often incorporated into growth monitoring charts. Government-supported preschool programmes for children are increasing; in the past 15 years, at least 13 developing countries have instituted compulsory preschool or pre-primary programmes. By 2005, the World Bank had financed loans to 52 developing countries for child development programmes, for a total of US\$1680 million, at least 30 developing countries had policies on early child development, and UNICEF was assisting governments in supporting parenting programmes in 60 countries.” (Engle *et al.* 2007, pp. 229-230)

But this summary of increased activity relating to ECD in developing countries concludes with a pessimistic evaluation of what is really known about the impacts of ECD programs in these contexts:

“Despite this interest, there have been few systematic evaluations of early child development programmes in developing countries.” (p. 230)

The third paper reviews 19 evaluations of ECD interventions since 1990 that met six criteria: “(a) randomized controlled trial or matched comparison group; (b) intervention before age 6 years; (c) effectiveness or program evaluations (not efficacy trials); (d) child development assessed; (e) targeted disadvantaged children; and (f)

developing country,” (Engle *et al.* 2007, p. 232). These interventions are summarized in Table 1.⁴ Only one of these ECD intervention evaluations included in this table is based on a national sample, and over a third are based on fewer than 10 communities. That this review found that there had been so few ECD interventions in all the developing world over a decade and a half that had been systematically evaluated, and that many of these cases were based on very few communities, reinforces the point that there are likely to be high rates of return in terms of knowledge and in terms of the foundation for policy formation from expanded evaluation of ECD programs of different types in different developing country contexts.

In a nutshell, thus, there are estimates that ECD problems are widespread in developing countries, increasing evidence that what happens in early childhood affects significantly options and productivities over the life cycles but very little systematic evidence to support that the impacts of these ECD programs are large or, more importantly, that the benefit-to-cost ratios of ECD interventions are high – particularly in light of the heterogeneous market, policy, and cultural contexts across developing countries that may limit the transferability of inferences from one context to another. Therefore the returns are potentially great not only for those who already are persuaded that more resources should be devoted to EDC interventions in developing countries in order that they can make their case more persuasively but also for those who are concerned more broadly about prioritizing resource allocations across what might seem to be a number of strong but difficult-to-compare alternatives ranging from other human resource investments to physical infrastructure investments to policies that affect markets such as for international goods and services and labor and capital flows, to have more systematic evaluations of the impacts of ECD interventions and of their benefit-to-cost ratios.⁵ This chapter is devoted to discussions of how assessments of ECD interventions in developing countries can be improved and extended.

⁴ Appendix Table A.1 summarizes evaluations of a number of ECD programs in the United States.

⁵ The “Copenhagen Consensus” is a recent visible effort to try to establish systematically priorities among about 40 interventions in developing countries in ten broad topic areas – education, climate change, communicable diseases, conflicts, financial instability, governance and corruption, malnutrition and hunger, migration, sanitation and water, and subsidies and trade barriers – using benefit-cost ratios as guides with an expert panel composed of eight prominent economists (half of whom have received the Nobel Prize in Economics) ranking the proposals (Lomborg 2004). ECD programs are not considered explicitly as a major topic area in this effort, though some interventions related to ECD are prominent components of some topic areas, particularly those related to hunger and malnutrition (Behrman, Alderman and Hoddinott 2004).

**Table 1. Summary of 20 Available Systematic Evaluations of ECD Programs in Developing Countries Since 1990
Based on Engle *et al.* (2007, Tables 2 and 3)**

	Country	Intervention	Child Age	Outcome Measures	Effect Size of Cognitive Measure	Sample Size for Evaluation	Scale**
I. Primarily Center-Based Programs							
1	Argentina	Increase in preschool places	3-5y	Third-grade mathematics and Spanish achievement tests	0.23	>125,000	3
2	Bangladesh	Preschool run by NGO, feeding	4.5-6.5 y	(1) Cognitive development from WPPSI-III (2) School readiness (3) Play observation Scale	0.20-0.23 1.4 0.19-0.72	208	1
3	Cape Verde	Formal Preschool	3-6 y	Cognitive development (Simplified Boehm Basic Concept Test) at 5 y	0.29; 0.48*	803	3
4	Colombia	Day care enter-based feeding and stimulation; 5 groups: food alone, and food + different time periods of stimulation, high SES control	42-75 m	Stanford-Binet test initially	N/A	333 children (170 at followup)	1
5	Guinea	Informal community-based early learning centers	2-6 y	Cognitive development (Simplified Boehm Basic Concept Test) at 5 y	0.33; 0.66*	877	2
6	Myanmar	Community-based ECD center and community support	3-5 y	(1) Primary school pass rate (2) Repetition rate for grade 1 (3) Test performance	N/A	(1) 3,484 (2) 1880 (3) 268	2
7	Nepal	Community-based ECD center (education and health)	3-6 y	(1) Primary school pass rate (2) Repetition rate for grade 1 (3) Annual drop out rate after 4 y	N/A	935	2
8	Vietnam	Center and home (education, parenting, nutrition)	0-3 y for nutrition 4-5 y for education	Raven's Colored Progressive Matrices at 6.5 to 8.5 y	0.25	313	1

**Table 1. (continued) Summary of 20 Available Systematic Evaluations of ECD Programs in Developing Countries Since 1990
Based on Engle *et al.* (2007, Tables 2 and 3)**

	Country	Intervention	Child Age	Outcome Measures	Effect Size of cognitive measure	Sample Size for Evaluation	Scale**
II. Parenting and Parent-Child Interaction Training							
9	Bangladesh	Parent groups that meet weekly for one year; mean attendance of 12 sessions (range 0-42; assessment 2 m. after end of programme)	2-3 y	(1) Maternal knowledge (2) Home Scale and subscales (3) Receptive Vocabulary (4) Weight/height (5) Five preventative health behaviors (6) Mother-child picture and puzzle task	(1) .31 mother knowledge (2) .34 on HOME	329	2
10	Bolivia	Adult literacy programs and home visits (parenting, health, nutrition)	24 m; some older	Psycho-social development (rating of 1 to 4); fine motor, gross motor, hearing and language, personal and social assessment)		454	2
11	Colombia	Nutritional supplement and/or a stimulation (Home Visit) program	Prenatal to 3y, follow up at 6y	Griffiths at 4, 6, 12, 18, 24, 36m. Locomotor, personal-social, speech & language, eye-hand coordination, Einstein scale applied through 18m	N/A	433 families	1
12	Jamaica	Home-visits by roving caregivers (health, nutrition, parenting, income-generating)	3-36 m	Griffiths Mental Developmental Scales	0.5*	163	1
13	Jamaica	Home-visits by health aides (parenting)	9-30 m	(1) Griffiths Mental Developmental Scales, (2) Mothers' knowledge and practices of childrearing	0.8*	130	1
14	Turkey	3 (Center) x 2 (mother training) design; Center = educational, custodial, or none; Mother training = MT, NMT	3-5 y	(1) School attainment (2) School achievement (3) WISC-R vocabulary test	(2) 0.45	217 families	1

**Table 1. (continued) Summary of 20 Available Systematic Evaluations of ECD Programs in Developing Countries Since 1990
Based on Engle *et al.* (2007, Tables 2 and 3)**

Country	Intervention	Child Age	Outcome Measures	Effect Size of cognitive measure	Sample Size for Evaluation	Scale**	
III. Comprehensive Programs for ECD							
15	Bolivia	Child care centers in home; Feeding health and nutrition monitoring, education)	6-72 mo	Gross and fine motor skills, language and auditory skills, and psychosocial skills	0.4-1.5	1,198	2
16	India	Integrated childcare center; support for pregnant and lactating mothers, growth monitoring, feeding	3-6 y	(1) Motor and mental development using WHO Milestones assessment (2) Binet-Kamat IQ tests	N/A	3,724	3
17	Peru	Preschool and non-formal preschool	3-5 y	Grades (A-C) in mathematics and language (Spanish) as assessed by the first grade teacher.	N/A	304	3
18	Philippines	Home (family day care programs, home visits)	0-4 y	ECD checklist of gross and fine motor skills, receptive and expressive language, socio-emotional skills, cognitive skills, and self-help skills	0.5~1.8	6,693	2
19	Uganda	Communication on ECD, child health days, village grants on nutrition, ECD centers	0-6 y	(1) Ugandan version of the British Abilities Scale (2) Parenting practices (3) Nutritional status	N/A	2,010	2

*Controlling for SES; ** Scale: 1 = coverage < 10 communities; 2 = coverage >10 communities or district, but not national; 3 = national coverage.

Sources: 1. Berlinski, Galliani, and Gertler (2006); 2. Aboud (2006); 3. Jaramillo and Tietjen (2002); 4. McKay *et al.* (1978) and Pollitt and Escamilla (2007); 5. Jaramillo and Tietjen (2002); 6. Save the Children (2004); 7. Save the Children (2003); 8. Watanabe *et al.* (2005); 9. Aboud (2006); 10. Morenza *et al.* (2005); 11. Weber *et al.* (1981) and Super *et al.* (1990); 12. Powell *et al.* (2004); 13. Powell (2004); 14. Kagitcibasi, Sunar and Bekman (2001); 15. Behrman, Cheng and Todd (2004); 16. Vazir and Kashinath (1999); 17. Cuento and Diaz (1999); 18. Armecin *et al.* (2006) and Ghuman *et al.* (2006b); 19. Alderman and Engle (2007).

B. Steps in the Process of Assessing ECD Interventions

The major steps in the process of assessing ECD interventions (or most other interventions) include:

- ascertaining what are the objectives of the interventions and what are good indicators of the impacts and costs associated with those objectives;
- determining the critical characteristics of datasets needed to evaluate the interventions: baseline with longitudinal data with sufficient periodicity to capture dynamic effects of intervention and sufficient duration to capture longer-run effects of intervention, sample representative of some relevant larger population, measurement of critical impacts and ways of valuing those impact and of resource costs, sufficient power to be able to identify impacts of a desired magnitude, establishment of comparable control as well as treatment group, sufficient information with which to be able to locate entities in subsequent survey rounds, sufficient human subject protection;
- determining and testing relevant survey instruments for households/individuals, for relevant communities and for service providers to be able to include good indicators of attainment of relevant objectives, possible other impacts, private and social resource costs and controls for confounding factors;
- undertaking data collection for households/individuals and for relevant service providers and communities with procedures that permit high response rates, low attrition, quick availability of data for analysis, internal consistency and validity checks;
- analyzing the data in a timely fashion beginning with careful examination of the data and then careful systematic analysis of impacts, the evaluation of those impacts to obtain measures of benefits in a common metric, resource costs – all based on comparisons over time between treatment and comparable control groups within frameworks such as suggested below and using estimation techniques to deal with the possible estimation issues noted below and ideally with the distinction between private and social benefits and costs;
- communicating with relevant stakeholders the preliminary results of such analyses and obtaining feedback in order to improve the analyses; encouraging others to undertake independent analyses of the data;
- making available the revised results of the analyses and their interpretation for policies with sufficient information and data so that others can test to see to what extent the results are replicable and to explore how robust they are to different estimation strategies.

These steps are fairly straightforward and seem to reflect common sense. But for such analyses, the “devil is in the details.” How useful the resulting evaluations of ECD

programs are depends very much on the details of the research strategy and its implementation. The sections below address a number of the critical aspects of the systematic evaluation of ECD programs in greater detail.

C. The Credibility of an Intervention

The credibility of an intervention is likely to depend on a number of factors. But very important among them is the quality of the perceived evaluation of impacts of the intervention and the extent to which these impacts are positive and substantial relative to the resource costs of the intervention. If a ECD intervention is perceived to have been well-evaluated and to have substantial impacts relative to resource costs, then not only is the program likely to have stronger backing for maintained or increased allocations from national and international technocrats but also is more likely to fare well in the larger political economy arenas that ultimately shape the political economy of resource allocations.

An informative example is the experience of the well-known Mexican anti-poverty human resource program *PROGRESA/Oportunidades*, which includes some ECD components as well as components focused on schooling and other human resources later in the life cycle. Prior to the introduction of *PROGRESA* in 1997, evaluation of human-resource-related interventions in Mexico tended to be in-house and not very systematic (e.g., often without baseline data, small and idiosyncratic samples, and without persuasive controls or even without controls at all), so they did not contribute much to assessments of the credibility of the interventions. This meant that the programs often were abandoned or left to wither when every six years a new president assumed office and wanted to introduce his own programs, even though for over six decades the presidents were always selected by their predecessors from leading politicians in the same political party (PRI, *Partido Revolucionario Institucional*). *PROGRESA*, in contrast, undertook evaluation with longitudinal data starting with a pre-program baseline with fairly substantial power (about 25,000 households) with random assignment of initial treatment among the 506 communities in the evaluation sample (with the controls included in the program after two years though that was not public knowledge to attempt to minimize “announcement” or “anticipation” effects) with numerous publicly-available written reports on the evaluation undertaken by a relatively “arms-length” international group of experts that solicited and incorporated responses to critiques by others and with the data available for analysis by others. This resulted in a program that was judged generally to be good in attaining its objectives, though the evaluations suggested some changes to improve the program that were implemented (e.g., increasing the coverage of the program through upper secondary school). The credibility of the program perhaps was most vividly reflected in that the program survived the election in 2000 not only of a new president, but also the defeat of PRI after over six decades. The program survived with minor modifications and under a new name, *Oportunidades*, according to informed sources in no small part because it had credibility due to the relatively systematic and transparent evaluation that had been undertaken.

Good empirical analysis of ECD programs is challenging, as noted above. This chapter clarifies this challenge and possible contributions by considering: (1) what are the dimensions of ECD programs, (2) what estimated relations would be informative for improving understanding within a life-cycle behavioral framework with important unobserved variables (e.g. genetic endowments), (3) possible resolutions to estimation problems, and (4) different types of data. Through careful examination of existing data, keeping in mind considerations in this chapter, much can be learned about the impact of ECD interventions conditional on assumptions that are necessary for causal interpretations. But it is also important to be alert to opportunities for improving data and for encouraging collection of new and better data and undertaking new and better analyses.

I. ECD Program Dimensions

A. Types of Interventions

The types of ECD interventions vary considerably in developing countries. Following Engle *et al.* (2007) and the organization in Table 1, it is useful to consider three broad types and then note variants through health centers – while noting that such categorization is not perfect since some programs overlap across these categories.

Center-based ECD Programs

Eight examples of ECD center-based programs are given in the first panel of Table 1. In these programs young children typically spend considerable periods of time – half days or whole days – in centers that are focused on providing ECD-related services. These center-based ECD programs can be further divided into (1) medium and large ECD integrated centers in educational, religious, and stand-alone public, private, and NGO institutions and (2) small public or private ECD integrated centers in neighborhood homes. All eight of the ECD center-based evaluations that are summarized in Panel 1 of Table 1 report significant effects on children’s cognitive development, either through preschools (Bangladesh, Cape Verde, Colombia, Guinea, Myanmar, Nepal, Vietnam) or treatment centers for malnourished children (Bangladesh). All of these programs primarily have a physical center outside of homes, though the Vietnam program has a home component. There also are a few evaluations of home-based public programs that are not included in the first panel of Table 1: the Bolivian *Proyecto Integral de Desarrollo Infantil (PIDI)* program (Behrman, Cheng, and Todd 2004) that is evaluated using matching methods (included in the third panel) and the Colombian *Hogares Comunitarios* program evaluated using instrumental variable techniques (with distance to the program as the instrument) to attempt to identify program impacts (Attanasio and Vera-Hernández 2004). All of these programs that have the necessary data find significantly positive effects on child cognitive skills, in many cases with fairly substantial effect sizes of the order of magnitude of about 0.20 (though information is not available with which to calculate effect sizes in all cases). The evaluations of many of

these programs also report non-cognitive gains such as social skills, self-confidence, willingness to talk to adults, and motivation. The subset of these evaluations that followed children into school (Colombia, Myanmar, Nepal) report improvements in the proportion of children entering school, age of entry into school, retention in school, and performance in school.

ECD Education for Parents to Enhance Parenting and Child Stimulation

Five examples of ECD programs directed towards improving parenting and other care-giving are given in the second panel of Table 1 (and at least one of the programs in the first panel, the one in Vietnam, and at least one in the third group, the one in the Philippines, also have this characteristic). Five of these programs used home visiting; the evaluations of all of these programs report positive effects on child development. For example, in Jamaica, parenting practices improved when children and parents were actively involved in a home-visiting program (though not if the parental component was limited to information sharing). Two programs used group sessions with mothers. In Turkey, for example, where mothers practiced skills to play with their children, there were short and long-term effects on ECD. However, in Bangladesh, where the sessions included providing information but no activities, mothers' knowledge increased, but there was no impact on ECD, leading Engle *et al.* (2007, p. 233) to suggest that "effective parenting programs should have skill-based activities involving children."

Comprehensive ECD Programs

Six examples of evaluations of what Engle *et al.* (2007) characterize as "comprehensive" programs because of their efforts at broader multi-dimensional interventions are summarized in the third panel of Table 1 (and the Colombian *Hogares Comunitarios* program noted in Section I.D also probably could be included in this group). The more recent of these programs are integrated into existing community-based systems and include families more effectively than earlier models. All but the Ugandan ECD program in this group report beneficial effects. Engle *et al.* (2007) suggest that the Uganda program may illustrate that low-intensity ECD programs that do not direct services toward children may have limited impact on child outcomes.

Health Center Programs with Direct ECD Impacts

There are a number of efforts to improve aspects of ECD through health centers, ranging from vaccination programs for infectious diseases to growth monitoring to nutritional supplementation to informing mothers of good breastfeeding and complementary food practices. A recent example is the component of the Mexican conditional cash transfer program *PROGRESA*, mentioned in Section Intro.C above, directed towards infant nutrition through providing micro nutrient supplements as well as maternal training regarding infant and child nutrition. Studies using experimental data with control for non-random program participation and matching methods report a significant and fairly substantial effect on child growth and then on early school performance (Behrman and Hoddinott 2005; Behrman, Parker, and Todd 2006, 2007).

B. Policy Motives and Beneficiaries

The standard motives for using public resources to support ECD, as for other economic activities, relate to *efficiency* and *distribution*.

The *efficiency* motive pertains to differences between the social and the private rates of return to an activity. There may be such a difference, or a distortion, because of externalities or spillovers from one individual to another that are not transferred through markets – a type of “market failure.” An example would be the cure of an infectious disease for a child, which benefits not only the child directly affected but also others who may have been exposed to infectious disease through that child if s/he had not been cured. Or there may be a difference because of policies that result in markets giving incentives for using resources that are different from the social marginal benefit of those resources, or “policy failures.” Examples include policies that place effective ceilings or floors on prices (e.g., minimum wages for child-care workers), overly subsidize goods and services (i.e., beyond what may be appropriate due to market failures), or preclude or discourage entry into an activity (e.g., limiting the provision of child care services or subsidies provided for the provision of such services to public providers). Removing such inefficiencies or distortions has the attractive potential of making everyone better off or of making some people better off without making others worse off – and thus probably increasing welfare. In the real world there appear to be many inefficiencies or distortions due to both market and policy failures. Reducing any one distortion between private and social marginal incentives is likely to improve the productivity and welfare of an economy.⁶

The *distributional* motive could relate to any aspect of distribution, but usually most emphasis is given in the development literature to the poorest, those in the left tail of distributions of income or wealth. Whatever the distributional motive, the question naturally raises regarding how much policies benefit the desired beneficiaries, or how successfully they are targeted. In a short-run sense, policies that succeed in benefiting more the targeted group (e.g., the poor) are more successful. In the background there may be a political economy question, however, of how sustainable various policies are. For this reason, for example, it may be desirable in certain political systems that policies aimed at the poor also benefit some the middle class to assure ongoing political support (if, for example, the median voter is particularly important in assuring sustainability).

Of course, many policies might be possible to improve attainment of either the efficiency or the distributional goals, but the alternatives are likely to have different economic (resource, not budgetary) costs (that should include private costs and distortionary costs, not just public-sector resource costs). To attain a particular efficiency (or likewise with distributional) goal, therefore, one can conceptualize of a *policy*

⁶ “Likely” is used here because, as demonstrated in the literature on “the second best,” it is possible that one distortion is offset by another so that removing only one of these offsetting distortions reduces overall efficiency and welfare. But in the absence of any explicitly offsetting distortion, moving any one distortion is likely to improve efficiency and welfare.

hierarchy in terms of such costs. Generally policy changes that are more directly addressed to the efficiency (or distributional) situation of concern and price (rather than quantitative) policies tend to be higher in such a policy hierarchy, particularly given information imperfections in a rapidly changing world.

Some policies are “win-win” in that they may promote attainment of both efficiency and distributional goals. If the distributional goal is to improve options for those living in poverty and those living in poverty are most affected by capital, insurance, and informational market imperfections, for example, policies directed towards improving the functioning of these markets may be “win-win.” For other policy choices there may be tradeoffs between the efficiency and distributional goals.

Conceptually, to select among policy alternatives, social benefit-cost ratios or social internal rates of return provide guidance.⁷ Such estimates are difficult to make for many aspects of ECD programs (as well as other policy areas) in part because of the multiple impacts over the life cycle, the question of how to value some of the impacts such as adverted mortality, the difficulties in estimating social effects beyond private effects, and the difficulties in estimating some of the resource costs.⁸ Many of the issues about assessing the policy rationale for public support for ECD programs relate to possible efficiency and distributional impacts of ECD. There are many claims, for instance, that there are strong positive externalities of ECD, though very little systematic evidence to support such claims because such externalities often are challenging to identify empirically.⁹

These considerations about policy motives relate to substantial lacunae in the literature on evaluation of ECD programs in developing countries to date. While that literature pays some attention to the distributional motive, particularly with regard to the

⁷ A third alternative is to maximize the net present value. Criteria based on net present values, cost-benefit ratios, and internal rates of return are not equivalent in all cases. In the case of mutually exclusive alternatives (e.g., which bridge to build across the gorge when there is the possibility of building only one bridge), for example, the advantage of net present value is that it is able to reflect the absolute size of the potential benefits (see Belli *et al.* 1998 for further discussion). But for most (perhaps all) ECD projects, these approaches are equivalent.

⁸ The “Copenhagen Consensus,” as noted above, attempted to rank a number of possible interventions in ten broad areas using benefit-cost ratios to guide the ranking. Some of the studies underlying the benefit-cost ratios used present illustrations of how sensitive such estimates are to critical assumptions, such as the value of adverted mortality and the appropriate discount rate (e.g., Behrman, Alderman, and Hoddinott 2004).

⁹ An exception for primary schooling, though not for ECD programs, is Foster and Rosenzweig (1995), which estimates positive spillovers on technological adoption decisions of neighbors. There also is a substantial literature that interprets the associations between adult schooling (particularly women’s schooling) and human resource outcomes of other family members as reflecting spillovers. But, as noted below in Section III, the few studies that have treated parental schooling as behaviorally determined within such a context report that the causal effects are much different (generally much smaller) than these associations. Also, within models such as in Section II, investments in other household members by parents may be motivated in part by private gains due to altruism or expected reciprocal transfers later in life; to the extent that this is the case, there is not a difference between the private and the social gains from investments in the parents’ education.

poor, it pays no (or virtually no) attention to the efficiency motive or much attention to the notion of a policy hierarchy or benefit-cost ratios even for distributional targets. Future evaluations of ECD interventions in developing countries would be much more useful if they would be sensitive to and incorporate into their analyses these concerns about policy motives.

C. Impact Indicators

Process and Program Implementation Indicators

Process and program implementation indicators include time series of measures such as the extent of participation of children in ECD programs, the inputs used in such programs, and the expenditures made in such programs. Such measures are likely to be useful in monitoring how programs are developing over time in some important dimensions. However, they on their own do not provide much insight into the impact of the program, to say nothing of whether the benefits relative to the costs merit maintaining the program or where the program might stand in the policy hierarchy except for the obvious point that presumably the program needs to be functioning reasonably if it is to have an impact.

Short-Term Program Impact Indicators

Most of the available studies of the impact of ECD programs in developing countries focus on fairly short-term indicators of ECD measured while the children are of pre-school age:

- cognitive skills (e.g., Simplified Boehm Basic Concept Test; Receptive Vocabulary, Stanford-Binet Test, Griffiths Mental Development Scales),
- ability (e.g., Raven's Colored Progressive Matrices, Binet-Kamat IQ tests),
- school readiness,
- language and auditory skills,
- biochemical and clinical indicators of ECD (iron deficiencies, iodine deficiencies),
- anthropometry, particularly related to stunting,
- gross and fine motor skills,
- social skills,
- morbidity,
- mortality,
- play observation scale.

Table 2 provides summary information on a wide variety of impact indicators, along with references for most of them. Further information is also provided in Fernald, Raikes, and Dean (2006). Needless to say, the decision on which indicators to use is a complicated one that depends on country circumstances, budget, and the availability of trained personnel. A detailed discussion of how to make this choice is beyond the scope of this paper (and outside the authors' areas of expertise).

Table 2: Indicators of Early Childhood Development

Indicator Name	Type of Indicator	Age Range	Time to Administer (per child)	Can Be Administered to Groups?	Amount of Training Needed
1. Primarily Mental Development or Cognition					
Boehm Test of Basic Concepts ^g	School readiness	5-7 years		Yes	
Stanford-Binet Test ^{a, d}	Intelligence test	2-23 years	45-90 minutes	Yes(?)	Extensive
Griffith Mental Development Scales	Mental development	0-8 years			
Raven's Progressive Coloured Matrices (book form) ^h	Test of mental development	7 years and older	45-90 minutes	Yes, for up to 8-9 children	1-2 days
Binet-Kamat Test of Intelligence	General mental ability	3 – 22 years			
Wechsler Intelligence Scale for Children (WISC) ^{b, d, g}	Intelligence scales	5-15 years	50-75 minutes	No	
Wechsler Preschool and Primary Scale of Intelligence (WPPSI) ^{c, d, g}	Intelligence scales	2.5-7 years	30-60 minutes	No	Extensive
Kaufman Assessment Battery for Children (KABC) ^b	Intelligence and achievement test	2.5–12.5 years			Extensive
Goodenough-Harris Drawing Test (Draw a Man) ^d	Nonverbal ability	5-17 years	10-15 minutes	Yes	
2. Primarily Physical Development					
Hemoglobin Test ^h	Blood test for iron deficiency	0-18 years	5 minutes	No	1 day
Height-for-Age ^e	Chronic malnutrition	0 – 18 years	5 minutes	Only to a small extent	2-4 days
Weight-for-Age ^e	Current malnutrition	0-18 years	5 minutes	Only to a small extent	2-4 days
Weight-for Height ^e	Chronic and current malnutrition	Boys: 0-138 months Girls: 0-120 months	10 minutes	Only to a small extent	2-4 days
Upper-arm circumference ^e	Current nutritional status	6-60 months	5 minutes	Only to a small extent	1 day
3. Motor Development					
Bayley Scales of Infant and Toddler Development ^e	Cognition, language, motor, social-emotional	1-42 months	30-90 minutes	No	Extensive
4. Social and Emotional Responses and Development and Social Skills					
Brazelton Neonatal Behavioral Assessment Scale ^f	Newborn mental and physical development	First 30 days of life	About 1 hour	No	

^a <http://www.answers.com/topic/stanford-binet-test?cat=health>

^b <http://www.answers.com/topic/intelligence?cat=health>

^c <http://harcourtassessment.com/haiweb/Cultures/en-US/dotCom/WPPSI-III.com.htm>

^d <http://www.unu.edu/Unupress/unupbooks/80473e/80473E0h.htm#Description%20of%20methods%20in%20published%20studies> summarizes a number of these measures

^e http://whqlibdoc.who.int/trs/WHO_TRS_854.pdf

^f <http://jpepsy.oxfordjournals.org/cgi/content/full/25/8/577>

^g <http://www.harcourt-uk.com/OccupationFocus.aspx?n=1316&s=1319>

^h <http://www.hemocue.com/index.php?page=2999>

Because of the perceived importance of the home environment, parenting, and stimulation, measures of these characteristics also are common even through presumably these are measures of possible facilitators of ECD rather than ECD itself:

- home environment (e.g., HOME scale),
- maternal knowledge regarding ECD.

Table 1 gives examples of the uses of many of these indicators in developing countries. While some have been used across countries and other contexts that facilitate comparisons across such contexts (e.g., anthropometric measures, Griffiths Mental Development Scales, Simplified Boehm Basic Concept Test), in many cases, in part due to language differences, tests have been developed for local contexts. There is an obvious tradeoff between using standard measures that are used elsewhere to facilitate comparability and using measures that may measure better aspects of local conditions.

Medium- and Long-Term ECD Program Impact Indicators

While some of the short-term impacts mentioned in Section I.C may be of interest themselves because they are direct indicators of pre-school age child welfare, in many cases they are of interest because they are thought to have medium-term (e.g., during the school years)¹⁰ or long-term (e.g., during post-schooling adult life) impacts. There also have been used a number of direct measures of the medium- and long-term impacts on children as they age such as:

- schooling success and cognitive skills (age of school entry, grade repetition, early drop out, school test performance, school enrollment, school attendance, school progression rates, school attainment conditional on age, or completed schooling attainment, cognitive test performance);
- productivity in economic activities (labor force participation, unemployment, occupation, hours worked, wage rates, entrepreneurial activities);
- productivity in non-economic activities (health and nutritional status of family members, fertility control, marital status, migration);
- health, morbidity, mortality and anthropometry;
- risk taking behaviors, particularly those with externalities (smoking, other drug consumption, early and risky sex, crime).

Table 1 (and Appendix Table A.1) gives some examples from the literature. Of course, longitudinal data, perhaps for many years, may be necessary to represent these indicators. For analysis of ECD in developing countries, there are a number of studies with medium-term school-related indicators but relatively few with longitudinal data from early childhood substantially into adulthood. Therefore, in any particular impact assessment it may be necessary to piece together evidence from various sources to obtain

¹⁰ For example, Glewwe and Miguel (2007) discuss the impact of early childhood nutritional indicators on indicators of schooling success.

estimates of the longer-run effects (e.g., Alderman and Behrman 2006, Behrman, Alderman, and Hoddinott 2004).

D. Valuing the Impacts to Obtain Benefits in the Same Metric at the Same Time

To obtain the overall impact of an ECD program (of the “benefits” for a benefit-cost calculation), somehow the various short-, medium-, and long-term impacts that are discussed in Section I.C must be combined. This poses several challenges.

Valuing the Impacts in a Common Metric

To aggregate across the impacts, it is necessary to translate them into some common metric, such as monetary units. Some of the impacts may naturally translate into resources gained or saved through better ECD that relatively easily can be translated into common monetary values. For example, the resources saved through lowered infant morbidity (e.g., less parental time in care, less direct health curative care) can relatively easily be given a monetary value, as can any increase in wages that might result from better ECD. The market prices and wages that people pay or receive indicate the impact on their income of such changes due to better ECD. However from a social point of view, what is relevant is the true marginal resource values (or shadow prices) of these changes, which may differ from the market prices because, for example, of subsidies to the health system. So the value of the private and the social impacts well may differ.

For some impacts, however, there is no easy direct way to translate into monetary terms. Perhaps the best example is the question of how to value in resource terms any averted mortality due to better ECD. There are some who argue that the value of life time earnings or productivity should be used for this evaluation. But, at least from the point of view of the survivors, this would seem to be an overestimate because a child who does not die because of an ECD intervention not only generates income but also consumes resources over the life time. In fact, some recent estimates for Indonesia by Grimm (2006) suggest that for child mortality the net effects on consumption of other family members are basically zero because the reduction in consumption that the child would have consumed is about the same as the reduction in income that the child would have contributed. An alternative approach is to value averted mortality by the value of resources used for the best (in the sense of the least resource cost) available alternative for advertising mortality. Summers (1992, 1994), for example, uses the resource cost of advertising child mortality through inoculations to value the impact of increased female schooling on child mortality in Pakistan (also see Behrman, Alderman, and Hoddinott 2004; Knowles and Behrman 2005). Some recent studies for the developed countries estimate the value of extending life as being much greater under the assumption that the marginal utility of non-health consumption within a period such as a year is sharply diminishing and the interperiod substitutability of utility is limited so that there are large utility gains from extending the periods of life with their initial within-period relatively high marginal utilities of consumption (e.g., Hall and Jones 2007). The bottom line of this

discussion is that there is controversy about how best to value averted mortality and that different methods alter considerably the estimated value of averted mortality (some examples are given in Behrman, Alderman, and Hoddinott 2004). Therefore, if one impact of an ECD intervention is to alter mortality risks, it is important to assess how sensitive the estimates of the overall impacts (benefits) are to alternative ways of valuing averted mortality.

For most probable impacts of ECD interventions the uncertainties regarding the valuation of the impacts are not as large as for averted mortality. But for the impacts whose effects are a relatively large share of the overall benefits, it generally still is desirable to explore how sensitive the estimates are to alternative ways of valuing the impact(s).

Time: Program Age, Child Age, Duration of Exposure and Lags in Impacts

There are a number of respects in which attention needs to be paid to dimensions of time in evaluating impacts of ECD interventions:

First, program implementation is not always coincident with announced schedules and usually there are start-up costs and learning. If attention is not paid to these dimensions of programs the result is likely to be an underestimate of the impacts of programs once they are established and functioning.¹¹

Second, the impact of an ECD program is likely to depend on the age of the children exposed to the program, with the literature suggesting generally greater impacts on children past weaning ages but younger than 24-36 months (e.g., Engle *et al.* 2007).

Third, the impact on children is likely to depend on duration of exposure to the program, with no discernible impact with very brief program impact and then increasing impact over a range with eventual diminishing marginal effects (e.g., Armecin *et al.* 2006; Behrman, Cheng, and Todd 2004).

Fourth, some of the impacts may occur with considerable lags, such as the adult productivity, risk behaviors, and morbidity impacts referred to in Section I.C. In order to compare these impacts with costs or with impacts from other interventions with different time patterns of impacts, it is necessary to translate these impacts into present discounted values (PDVs). The basic point is that an impact of a certain magnitude is more valuable if it is received sooner rather than later because if it is received sooner the returns from the intervention can be reinvested. But there is not agreement on what discount rate is appropriate. Many social sector programs use discount rates of the order of magnitude of five percent, but Belli *et al.* (1998) in a *Handbook on Economic Analysis of Investment Operations* state that the World Bank generally uses a discount rate of 10 to 12 percent in

¹¹ On the other hand those involved in the early stages of a program may devote extra efforts to trying to get it established and to make it work that are not sustained in the longer run.

its economic analyses.¹² The Asian Development Bank (ADB) also recommends the use of a discount rate of 10 to 12 percent for the economic evaluation of its loan projects (Adhikari, Gertler, and Lagman 1999). Some other World Bank references on economic evaluation claim that in most countries the opportunity cost of capital is correctly reflected by the real rate of interest for low-risk securities, which is generally lower (usually less than five percent) and that the real rate of interest on low-risk securities such as World Bank Bonds and United States' Treasuries has remained at about three percent for many decades (Barnum, 1995; Phillips and Sanghvi 1996). The World Bank and WHO have also used a three percent discount rate in calculating the number of disability-adjusted life years (DALY) gained from alternative health interventions or in Global Burden of Disease estimates (<http://www.who.int/healthinfo/statistics>). The problem is that, for impacts with considerable lags, the choice of discount rate to use can make a substantial difference. Table 3 illustrates the impact of discounting and the sensitivity of the PDVs to the discount rate used for impacts that occur with considerable lags. An impact of \$1000 in 30 years (e.g., when an ECD intervention might affect adult productivity), for example, has a PDV of \$412 with a three percent discount rate, \$231 with a five percent discount rate, \$57 with a 10 percent discount rate, and \$4 with a 20 percent discount rate. Therefore, it is important that estimates of the impact of ECD programs show the sensitivity of estimates of the PDV of impacts to different discount rates.

Table 3. Present Discounted Value (PDV) of \$1000 Gained Different Years in the Future with Different Discount Rates

Years in Future	Annual Discount Rate					
	1%	2%	3%	5%	10%	20%
5	\$951.47	\$905.73	\$862.61	\$783.53	\$620.92	\$401.88
10	\$905.29	\$820.35	\$744.09	\$613.91	\$385.54	\$161.51
20	\$819.54	\$672.97	\$553.68	\$376.89	\$148.64	\$26.08
30	\$741.92	\$552.07	\$411.99	\$231.38	\$57.31	\$4.21
40	\$671.65	\$452.89	\$306.56	\$142.05	\$22.09	\$0.68
50	\$608.04	\$371.53	\$228.11	\$87.20	\$8.52	\$0.11
60	\$550.45	\$304.78	\$169.73	\$53.54	\$3.28	\$0.02

Including All the Major Impacts but Avoiding Double-counting

Section I.C indicates a number of possible short-, medium-, and long-run impacts of ECD programs. Of course, it is important to include all the major impacts in order not to underestimate the total impact (benefit) of an ECD program. It should be noted that a key aspect of measuring project impact is to assess the possible displacement of existing services (including privately-provided services) by project-provided services. As noted above, an important and often challenging aspect of estimating the social impacts is to estimate the “spillover” benefits on others’ labor productivity, health, or learning.

¹² This *Handbook* explains that this rate is used as a “rationing device” and that it does not necessarily reflect the true opportunity cost of capital in borrowing countries. It invites analysts to use another rate if it can be justified. However, this *Handbook* cautions that use of a rate lower than 10% might be difficult to justify in light of research suggesting that this is the low end of estimates of the opportunity cost of capital in developing countries.

But on the other hand, analysts need to be careful not to double-count impacts. For example, one important channel through which an ECD intervention might work to increase economic productivity is through increasing schooling success. To the extent that the interest in schooling is because it increases productivities, it would be misleading and overstate the program impact to add to the gain from increased productivities the gain from increased schooling. Only the part of the gain from increased schooling aside from that related to increased productivities should be added to the increased productivities. For this reason it is important to clarify what are the ultimate impacts of interest and what are the channels through which, at least in part, ECD programs may affect the ultimate impacts of interest.

E. Valuing the Resource Costs

As noted in the discussion above about policy motives (Section I.B), evaluations of ECD programs are most useful if they not only have good estimates of impacts and of the aggregate PDV of those impacts (Section I.E) but also have good estimates of the PDV of public and private resources used by the programs. A key feature of economic analysis – in contrast to financial or budgetary analysis – is that the costs and benefits are based on opportunity costs to society, not financial flows. For example, in evaluating the cost of delivering ECD services to children in public ECD facilities, the opportunity cost of inputs such as labor (including volunteer labor), food, books, toys, drugs and medical supplies, buildings (including land), and equipment should be used, instead of the financial costs that may be reflected in governmental or project accounts. Taxes and subsidies should not be included in the resource costs and benefits (even though their values are likely to affect both the project’s fiscal impact and its distributional impact).¹³

The Identification of Project Inputs

ECD projects typically involve several distinct activities. The first step in estimating project costs is to identify the distinct activities that are included in the project and to identify their inputs, outputs and outcomes. It is critically important to identify all relevant inputs, regardless of whether they are provided by the project and regardless of whether they involve any financial expenditure (inputs not provided by the project will often be missing from the project documents). Table 4 lists some of the inputs that are frequently used in ECD projects. These inputs are grouped into broad categories: (1) capital inputs (i.e., inputs that are not completely consumed during a given year) and (2) recurrent inputs (all other inputs). It is necessary to separate capital inputs from recurrent costs not only because they should be handled differently in calculations but also because the distinction is an important one in analyzing the issue of sustainability that is largely a function of recurrent costs.

Project inputs should be limited to the additional inputs required to perform various project activities. For example, in the case of personnel, project inputs should be

¹³ However, activities that are financed by tax revenue may need further adjustments to reflect the distortionary costs of collecting additional taxes or of governmental expenditures, as discussed below.

limited to the additional time required by personnel to perform project-related tasks; therefore, if an ECD-related task is added to the responsibilities of health clinic workers, the time needed to perform that additional task is what should be considered. If the ECD project uses capital inputs (e.g., existing equipment or buildings), the project's inputs should be limited to the portion of the input's total use that is project-related, for example, the space occupied by the project in an existing building or the time during which an existing vehicle is used by the project. This principle should apply equally to both existing capital inputs and to capital inputs purchased by the project.

Table 4. Frequently Used Inputs in ECD Projects

<p>Capital inputs:</p> <ul style="list-style-type: none"> Buildings Land Vehicles Other equipment Renovations/major repairs Basic training (including training of trainers and the cost of developing training materials) <p>Recurrent inputs:</p> <ul style="list-style-type: none"> Personnel Client time Client transportation and related costs Materials and supplies (e.g., medicine, food, micronutrients, books, toys, training materials) Utilities (water, electricity) Telephone/communications Petrol Maintenance and repair Services (for example, legal, accounting) Refresher training

Care must be taken to look closely at the implications of “piggybacking” additional activities onto already existing activities. If, in order to be involved in the proposed ECD project, it is necessary to divert time or space from some other activity, the inputs so used should be included in the cost calculations. Only if those resources are otherwise idle or unoccupied and expected to remain so for the life of the project and beyond should they not be considered inputs used by the project.

The principles of cost measurement are the same for integrated ECD projects or ECD project components included in broader (non-ECD) projects, although their application may be more difficult the more complex the project. It is important to consider for such joint programs the possibility that the costs are not simply the sum of the costs of the individual components. It is likely that there will be some economies from sharing costs, particularly capital costs and management overheads. In fact, the perception that there are such economies of scope is one of the major reasons for having integrated programs or projects (the other being possible synergies in effectiveness/benefits). For integrated programs, it may be very difficult to estimate the cost in isolation of any one component. In such cases, project evaluation may have to focus on the program as a whole rather than on its separate components.

Special attention should be given to identifying the inputs correctly for ECD project activities that are scaled-up from initial “pilots.” The cost of providing a good or service for ECD is likely to change with the scale of operation. Often the cost per unit of output will initially fall, due to economies of scale or increased rates of utilization, as the level of output increases. On the other hand, costs may rise with considerable expansion because the conditions under which the initial ECD “pilot” operation was conducted may have been more favorable than conditions that more generally prevail and the dedication and enthusiasm of innovators may be hard to replicate on larger scales. It is important to include as inputs the management and other inputs that pilots may have received from the organizations that supported them. There is often a tendency to focus only on the direct costs of services without considering the various management costs that may be involved. Costs also are likely to rise with extensive expansion if, for example, children who are less well situated (e.g., because of location, family background, innate characteristics) increasingly are served. Since one of the important decisions to make concerning project design may be the choice of scale, it is useful to explore how costs are likely to vary as output increases.

The level of inputs required by an ECD project may decline over time, at least initially, because there is important learning about how to procure, organize, distribute and manage the inputs and outcomes and how to produce the desired outcomes more effectively. These learning effects are conceptually distinct from scale effects, and it is useful to keep them distinct in undertaking analysis. Learning can occur, for example, even if there is no change in scale. Since learning may be important for many projects, it is useful to explore how the needed inputs are likely to vary as the result of such learning (especially if sensitivity analysis indicates that alternative assumptions in this regard have a significant effect on the conclusions).

Costing the Inputs for ECD Projects

Once the ECD project’s inputs have been correctly identified, the next step is to assign an economic cost to each input. The economic cost of an input is sometimes referred to as its “opportunity cost,” i.e., the value of the input in its next best use. As previously indicated, the opportunity cost of an input is not necessarily the same as its financial cost (for example, the public expenditure cost of the input, or what the project expects to pay for the input). There are at least two important ways in which opportunity costs differ from financial costs:

First, the prices actually paid for inputs may not correctly reflect their opportunity cost. An important example is inputs that are contributed by individuals or by the community (for example, volunteer labor). Although the project does not have to pay for these inputs (or may pay less than market prices for some of them), they do have an economic cost. In all such cases, the full market value of contributed inputs should be estimated and included in the project costs. Governmental personnel are often paid a salary that is considerably lower than the market wage, even when the market value of all allowances and benefits is included. In such cases, the economic cost of the personnel is not what they are actually paid, but is rather the full market value of the time they spend

performing their jobs. The use of some project inputs may impose external costs, for example, the effect of project vehicles in generating additional air pollution and congestion. Ideally, the cost of project inputs should reflect any such externalities. In the case of imported inputs, the local price paid may not reflect the opportunity cost of the input (for example, if the exchange rate is significantly over- or under-valued). However, this type of distortion is less important now than it was in past decades.¹⁴ Lastly, all taxes and subsidies should be removed from the prices paid when costing project inputs (for example, subsidies for petrol or electricity).

Second, some project expenditures or financial outlays do not represent real resource costs of the project but are instead transfers that represent changes in the command over existing resources. The distinction between economic costs and transfers is one of the least-well understood aspects of economic project evaluation. It is particularly important in the context of ECD projects because many ECD projects include in-kind benefits. The case of cash transfers generally is easiest. If for example the government taxes adult workers 100 pesos and gives those pesos to mothers of small children, the cash transfer generally does not represent a project cost. The cash transfer represents only a change in the command over existing resources (e.g., from tax payers to mothers of small children). The actual costs related to such a cash transfer include the cost of administering the cash transfer program and related distortionary costs. The latter would include any reduction in resources available to the economy from any effect the cash transfer may have on work effort on the part of mothers receiving the cash transfers (for example, by reducing their job search effort or by discouraging them from accepting a relatively low-paying job) and from the additional cost of collecting the governmental revenue needed to finance both the cash transfers and the program's administrative costs (this includes both the administrative costs of collecting the additional tax revenue and the distortionary costs that result from increases in the collection of most types of taxes).

Conditional cash transfers (i.e., transfers given to an individual who fulfills one or more conditions, such as enrolling in and regularly attending a pre-school program) are also generally primarily or exclusively transfers.¹⁵ In this case, project costs include (in addition to those discussed above for any cash grant) the additional cost of the activity that is encouraged by the condition attached to the transfer. For example, in the case of a conditional cash transfer provided to young children who enroll in and attend a pre-

¹⁴ A decade ago, for example, in this regard Devarajan, Squire, and Suthiwart-Narueput (1997) stated: "Reforms of trade policy and exchange rate systems have reduced the distortions of most concern. In these circumstances, paying only modest attention to shadow prices may be a sensible allocation of the time of the economists."

¹⁵ Some qualification is made here because conditional transfers, like in-kind transfers, may have real resource costs if, as summarized at the end of this section, (1) they lead to a significant input into the activity being supported and (2) the project results in a net increase in the utilization of this input. For example, consider scholarships (transfers conditional on attending school) for poor, malnourished young children to attend ECD programs. Empirical studies suggest that the increased monetary income provided by such scholarships will lead to increased nutrient consumption by the children (among other effects) that will increase their learning (e.g., Behrman, Alderman, and Hoddinott 2004, Maluccio *et al.* 2007). To the extent that this occurs, part of the conditional transfer is not a pure transfer but a resource cost.

school program, these costs include the cost of additional pre-school inputs required to accommodate any increased enrollment (including any additional costs incurred to offset any negative impact on children previously attending the program, i.e., the costs of “crowding”),¹⁶ increases in household out-of-pocket expenditure on pre-school-related items (for example, uniforms, books, transportation—but not fees to pre-schools, as that would be double-counting), and the opportunity cost of the additional time that grant recipients and their families spend in pre-school-related activities, including travel and volunteering for ECD-related activities.

The distinction between project costs and in-kind transfers is a bit more subtle. The first relevant question is whether the item or items transferred in connection with the project are a significant (i.e., quantitatively important) input into the project-supported activity. Consider, for example, the case where food is given to a family in lieu of a cash grant in the above example. If the food is not a *significant* input into the educational activity that the project is attempting to encourage (for example, if the children in a pre-school program are already adequately nourished), the value of the food should be regarded as a transfer, and benefits and costs are the same as the case of a conditional cash transfer. If the food is an important input into the educational activity (for example, food provided to malnourished children whose malnourishment precludes them from learning to their potential), some or all of the food provided through the program might be regarded as a cost of the program.

To be considered a cost of the project, the food provided through the program has to increase the total daily nutrient intake of the children participating in the program. If it simply substitutes for food previously provided to the same children by their households, it would not represent an increase in the level of this input into the education of school children and the value of the food provided should in this case be treated as a transfer. Even if the feeding program does not result in any increase in children’s nutrient intake, the project might still yield benefits by encouraging children to attend the ECD program. In this case, however, benefits and costs would be similar to those with a conditional cash grant, as discussed above.

Summarizing, the key questions in determining whether a given project expenditure is a project cost or an in-kind transfer are: (1) is the item a *significant input* into the activity supported by the project? (2) does the project result in a *net increase* in the utilization of this input in the project-supported activity, or does it simply affect how an unchanged level of the input is financed?

¹⁶ Even if additional resources have been used in an effort to offset any negative effects of an intervention on others (non-beneficiaries), the estimation of project impacts should still include the possibility that some negative (or possibly positive) effects have been experienced by others. It is conventional to include the monetary value of any negative external effects as part of a project’s costs, while the monetary value of any positive external effects is included among the project’s benefits.

Other Project Costs

In addition to the costs of direct ECD project inputs, ECD projects often entail costs borne by the private sector. For ECD programs, the most important private cost is likely to be the opportunity cost of time of the mother or other caregiver, often in terms of transporting children to programs and in some cases in participating directly in programs (whether they are paid or provide volunteer labor) or related program activities (e.g., parental oversight committees). Another important private cost is likely to be the distortionary cost of raising the necessary additional tax revenue to finance project-related governmental expenditure (including transfers) (Devarajan, Squire, and Suthiwart-Narueput 1997). In addition to the cost of collecting additional revenue, all taxes other than a lump sum tax lead to efforts to avoid the payment of the tax that involve real resource costs. Society has less product because of such distortions. These distortionary costs may be considerable. For example, it has been estimated that the distortionary cost (often called the "deadweight loss") of raising a dollar of tax revenue in the United States ranges from \$0.17 to \$0.56, depending on the type of tax used (e.g., Ballard, Shoven, and Whalley 1985; Feldstein 1995). Estimates for some other countries range from \$0.18 to \$0.85, depending on the tax (van der Gaag and Tan 1997). Harberger (1997) suggests using a shadow price of \$1.20-1.25 for all fiscal flows on a project. The existence of substantial distortionary costs related to the collection of taxes provides one important justification for careful consideration of cost recovery possibilities in projects (Hammer 1996).¹⁷

In addition to distortionary costs related to the collection of additional tax revenue, some project activities may be responsible for modifying other types of private behavior that result in real resource costs. For example, conditional cash grants provided to a family to encourage enrollment in pre-school programs may affect the work effort of other family members.

Treatment of Some Specific Cost Items

To clarify the treatment of some key components of project costs, this section briefly discusses some specific cost items:

Start-up Costs (including costs of technical assistance): Start-up costs for new ECD interventions involve real resource costs, so they should be included in cost calculations. They also by definition occur early in the project life, so they are not discounted to the same extent as recurrent costs and capital service costs that are distributed over time.

Monitoring, Evaluation, and Analysis Costs: These are important activities for any ECD project, particularly new projects. Good projects should include such activities.

¹⁷ Another important reason for using user fees that will result in some cost recovery is to encourage more efficient use of the goods and services provided by making the private incentives for use closer to the social marginal resource costs of the program (see Section 2.2).

Of course, these activities have resource costs in terms of personnel and other recurrent expenses (e.g., materials and supplies) and capital services. But within the range of resource uses for monitoring, evaluation, and analysis that usually are considered, it would appear that the expected gains in terms of program modification or possible abandonment exceed the real resource costs.

Capital Costs: Many capital costs will typically involve investments made by the project. In this case, it is important to consider whether the capital input is consumed completely during the period in which costs and benefits are analyzed. For example, a building financed by a project will probably have a useful life beyond, say, a 10-year period used to evaluate the project's benefits and costs, whereas project-purchased vehicles will probably not. The easiest way to handle the former case is to subtract the present value of the estimated residual value of the capital item at the end of the project from its initial cost.

Some projects may use all or part of an existing capital item as a project input (for example, space in an existing religious institution or NGO or household for center-based ECD programs such as those in the first panel of Table 1). In such cases, a capital cost should be imputed for the input. Capital costs include changes in the market value of the item during the year, such as may occur from the item's depreciation due to use, and the opportunity cost of the capital invested in the item during the year.¹⁸ Maintenance costs may reduce actual depreciation costs, so it is necessary to avoid double counting these two items. Although it is conventional to regard capital costs as a "fixed" cost, some capital costs increase with output levels (for example, the depreciation of vehicles used to provide outreach services). If estimates of building costs are included, it is important if possible to treat building costs differently from land costs. Buildings depreciate (unless maintained impeccably), but land does not. The appropriate capital cost for land is an estimate of its market rental value (with the estimated cost of the building removed). If such an estimate is unavailable, an estimate of rental cost can be obtained by multiplying the approximate market value of the land by the real interest rate (i.e., the opportunity cost of the capital tied up in the land).

Training Costs: Training of trainers, such as those for child-care workers, is a capital cost, as is the cost of developing training materials. The cost of periodic re-training is a recurrent cost. This distinction is most important in the context of sustainability analysis.

Time Matters: As for benefits, the timing of cost matters. Typically the costs tend to be more concentrated early in an ECD project than the benefits due to initial capital and start-up costs. But typically there also are significant ongoing recurrent costs, so the PDV of costs should be used – which again raises the question of how sensitive the estimates are to the discount rate used.

¹⁸ Capital cost estimates often include an estimate of depreciation but not of the opportunity cost of capital.

II. Framework for Estimating Impacts of ECD Programs

To illustrate more concretely the general issues involved, consider the following more formal stylized model of the possible impact of ECD programs on children over their life cycles. Part of what is of interest regarding the impact of ECD programs is whether they increase the resources that children will have as adults – will they make them more productive or healthier or less likely to engage in risky behaviors. A major pathway through which ECD programs might have such an effect is through affecting the children’s intellectual functioning when they become adults. While there are other pathways that may be important as well, the basic points can be illustrated more simply by focusing on this particular pathway and on the impact of adult intellectual functioning on the resources that the child will have when s/he becomes an adult.

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.¹⁹ 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 labor 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:

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

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 ECD programs on the resources for use by this individual are through (a) affecting the human and physical assets that the individual has as an adult by altering the nature of his/her early life experiences and thereby investments in this individual in previous life-cycle stages and (b) changing the options that the individual has as an adult for investments in his/her children. To estimate the impacts of type (a),

¹⁹ It may be desirable for some purposes to utilize a more-disaggregated representation of these resource sources, such as a labor income earnings function, a return to assets function, and a sharing rule for household resources.

however, is a challenge. Identifying the causal effects of early life ECD interventions on the capabilities of adult children (as well as on other variables in relation (1) 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).

To illustrate, consider adult intellectual functioning (K_a^i). The standard assumption is that adult intellectual functioning depends importantly on schooling and the interest in this chapter is in evaluating ECD programs prior to schooling, so consider three life-cycle stages:²⁰

Life-Cycle Stage 1: pre-schooling (from conception through to about age five or six) during which children may be exposed directly to ECD programs and those programs may have important relatively short-run impacts.

Life-Cycle Stage 2: schooling ages during which children no longer are directly exposed to ECD programs but there may be important medium-term impacts of exposure in the pre-school stage.

Life-Cycle Stage 3: adulthood during which the individuals are no longer are directly exposed to ECD programs (though their children may be) but there may be important long-term impacts of exposure in the pre-school stage, perhaps with effects through pathways in the previous life-cycle stage – in particular, related to education.

Adult intellectual functioning (K_a^i), 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:

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

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 right-side 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

²⁰ 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/IOMn2005). For any particular study, moreover, it may be desirable to consider other life-cycle stages. The use of these three life-cycle stages here, nevertheless, serves to make the basic points relevant for this chapter.

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.* 2007), 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 ECD programs experienced during the pre-school life-cycle stage through the school age life-cycle stage on the resources available for use by this individual as an adult and thus the extent of this aspect of the impact of ECD interventions 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 (2)) are quite imperfect and because the experiences for the three life-cycle stages on the right side of these relations all reflect previous behavioral choices. For the latter reason, for example, ordinary least squares (OLS) estimates of relation (2) are likely to be inconsistent due to the endogeneity of the life-cycle stage experiences.

To motivate the assumptions underlying the exploration of how ECD programs may affect the three life-cycle stage experiences and to elucidate some of the estimation issues (e.g., the possible impact of the endowments on estimates that do not control for them), assume a very stylized model in which the “dynasty” (first the parents through intrahousehold bargaining between themselves 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) makes decisions so as to maximize a welfare function W that includes Y_a for each individual. This welfare function is maximized 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 (among which are ECD-related options) that affect household decisions, and stochastic factors:

Life-Cycle Stage 1 (pre-schooling): 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 ECD programs), expected future community characteristics (e.g. expected schooling options in life-cycle stage 2, expected labor 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:

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

where N is a vector of family-determined inputs into the production of E_I (e.g. family-provided nutrients), C_{Ip} is a vector of community inputs into the production of E_I (e.g. community-provided ECD programs, community disease environment, community learning environment), E_0 is the child endowment that directly enters into the production of E_I (e.g. innate robustness), E_0^f is parental endowments that directly affect early childhood development (e.g. innate ability in raising children), and U_{IE} is a stochastic disturbance term that directly affects the production of E_I (e.g. random fluctuations in the infectious disease environment). The parents choose the inputs into this production function N_I (and perhaps some components of C_{Ip} that reflect parental choice that may include, for example, the child's participation in ECD programs) and therefore the expected value of E_I in order to maximize 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_I); all relevant community characteristics for this life-cycle stage C_I (which includes the community characteristics such as the availability of ECD programs that directly affect the production of E_I through C_{Ip} 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_I (which includes U_{IE} 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_I to maximize W depends in part on expectations regarding these variables over the next two life-cycle stages:

$$N_I = N_I^d (F_I, C_I, E_0, E_0^f, U_I, F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, U_{12}^e, U_{13}^e) \quad (4a)$$

and

$$E_I = E_I^d (F_I, C_I, E_0, E_0^f, U_I, F_{12}^e, F_{13}^e, C_{12}^e, C_{13}^e, U_{12}^e, U_{13}^e), \quad (4b)$$

where the superscript d refers to reduced-form demand relations. As noted, E_I is a vector with a number of different components that are measured by indicators such as are given in Section I.C (short-term). Good estimates of relation (4b), with ECD interventions among the components of the vector for C_I , would be informative about the impact of the ECD interventions on these relatively short-run (at least from a life-cycle perspective) outcomes of interest. Though the availability of ECD options from the point of view of parents may be exogenous (though there still may be some estimation issues depending on what determines the placement of ECD programs, as emphasized in the more general discussion of endogenous program placement in Rosenzweig and Wolpin 1986), if child participation in an ECD program is a parental choice then that participation is not on the right side of relations (4a) and (4b), but there is another relation parallel to (4a) that gives the reduced-form demand relation for the child's participation in ECD programs.

Life-Cycle Stage 2 (school-age): The dynasty (initially the parents but increasingly the child) decides on the components of E_2 (such as schooling attainment²¹) 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 (including ECD programs in life-cycle stage 1),²² (b) life-cycle stage 2 family, community and stochastic factors, and (c) the expected values of those factors for life-cycle stage 3:

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

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 :

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

As noted, E_2 is a vector with a number of different components that are measured by medium-term indicators such as are given in Section I.C. Good estimates of relation (6), with ECD interventions among the components of the vector for C_1 , would be informative about the impact of the ECD interventions (but not the parental responses to those interventions) on these medium-term (at least from a life-cycle perspective) outcomes of interest.

Life-Cycle Stage 3 (adulthood): The dynasty (primarily the post-school youth/young adult but perhaps with some input from the parents and in part in interaction with a spouse and the spouse's family) 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 (including ECD programs) 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 (including indirect effects of ECD programs through E_1) that determine schooling and other elements of E_2 ,²³ and (c) life-cycle stage 3 family, community (including ECD options for his/her children that may affect his/her welfare as an adult) and stochastic factors:

$$E_3 = E_3^c (E_1, E_2, E_0, E_0^f, F_3, C_3, U_3). \quad (7)$$

²¹ There are 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.

²² 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.

²³ 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.

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 :

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

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 chapter) adult access to resources (or other indicators of possible long-run impacts of ECD such as indicated in Section I.C) 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:

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

Good estimates of the coefficients of the ECD program variables (either affecting the individual in life-cycle stage 1 and therefore in C_1 or affecting the options for the individual's children and therefore in C_3) in relation (9) permit ascertaining the direct causal impacts of ECD program variables on the child's adult resource access and other relevant adult outcomes.²⁴

Good estimates of relation (9) permit answering a number of important questions about the impact of ECD interventions on adult outcomes. Most directly, how important are the effects on adult outcomes of interest? But also, to what extent do parental family characteristics or other community services substitute for, or complement, the impact of ECD in the pre-school life-cycle stage? And are there important differences in all of these relations by ethnicity or other demographic characteristics? By income or poverty level?

While good estimates of relation (9) are valuable in assessing the impact of ECD interventions, they are not the only estimates that would be illuminating regarding the impact of ECD interventions. 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 impact of ECD interventions. For example: Just how important are various components of ECD programs 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? Does the importance of such factors depend on individual characteristics such as gender or on community characteristics such as the nature of labor or capital markets?

²⁴ Note again that by the ECD program variables are meant the exogenous-to-the-parents ECD program options, not variables that reflect earlier parental decisions such as child participation in ECD programs.

Summary of Implications of this Section for Estimation of the Impact of ECD Programs: The set of relations discussed in this section have a number of implications for the estimation of the impact of ECD programs that relate to the discussion above.

First, they imply that to obtain the total impact of ECD interventions over the life cycle directly requires following individuals exposed to ECD programs when they are young for many decades into mature adulthood. Such data are rare, though do exist in some cases (e.g., Behrman *et al.* 2006 and Maluccio *et al.* 2007 use data on adults 25-42 years of age as well as data on the same individuals when they were 0-7 years of age).

Second, they point to a strategy for obtaining estimates of the long-run impacts even in the absence of longitudinal data over many decades if estimates can be made of the various linkages across life cycle stages. For example, if an ECD intervention affects preschool cognitive skills and stunting and these cognitive skills and stunting, through affecting schooling attainment and adult cognitive skills, affect adult outcomes of interest such as earnings, then it might be possible to link estimates of the short-run impact of ECD program availability on early life cognitive skills and stunting with separate estimates of the impacts of schooling attainment and adult cognitive skills on the outcomes of interest. An illustration of such a strategy is provided in the study of the Bolivian PIDI ECD program by Behrman, Cheng, and Todd (2004) in which study estimates of ECD program impact on short-run child outcomes based on pre-school data are linked to estimates from other studies about the medium-term impacts on schooling and the long-run impacts on adult earnings to obtain the PDV of long-run benefits (albeit with a number of strong assumptions).

Third, the set of relations point to the problem of possible double counting (Section I.D). For example, relation (6) may indicate that an ECD program has impact on schooling attainment, and relations (8) and (9) may indicate that ECD has impact on adult earnings at least in part through affecting schooling attainment. To get the total impact of ECD on adult outcomes, therefore, it would be wrong to sum the effect on schooling attainment plus the effect on earnings (plus other possible effects). What should be included is the effect on earnings plus any effects on schooling attainment after netting out the part of schooling attainment that relates to the effect of schooling attainment on earnings.

Fourth, though the emphasis typically is on the effect of ECD programs on the children who are exposed directly to the ECD program, there also are likely to be effects on their parents (basically through the C_3 component that refers to ECD programs when the individual is an adult with children in relations [9] and [10]). These impacts and costs associated with them have to be incorporated into the analysis to obtain the full impacts of ECD programs, though many studies do not incorporate such effects.

Fifth, these relations point to the fact that impact evaluations of ECD programs are context specific because the nature of community services, markets, culture, and other factors enter into each life-cycle stage and may affect interactively the impact of ECD programs. In different contexts, other (non-ECD) programs and family investments may

compensate differentially for limitations in pre-school child development (e.g., the special education programs that are common in the developed world are not common in poor areas of developing countries). Of course, the extent to which the currently available estimates are generalizable is an important part an empirical question. But there are very few available systematic studies of ECD programs in developing countries (Table 1). Only by undertaking more empirical studies can we learn how generalizable to other locations are the estimates that currently are available.

Sixth, to make inferences about the impact of ECD programs requires good estimates of relations such as are discussed in this section. But obtaining good estimates may be a challenge for reasons to which the next section turns.

III. Estimation Issues and Possible Resolutions

Data limitations, no matter how good the data, lead to possible estimation problems. In all of the right-side relations in Section II there are vectors of variables, and a number of the components of those vectors are likely to be unobserved or poorly measured.²⁵ 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.²⁶ Indeed, if the empirical measure of exposure to a ECD intervention used in a study reflects behavioral choices of parents, caregivers, or program personnel rather than just the exogenous-to-the-parents ECD program availability, what are referred to as “reduced-form” demand relations parallel to those in Section II really are

²⁵ Even if the life-cycle experiences are treated in the estimation as behaviorally-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 are likely to be correlated, and thus the right-side variables in relation (1) also 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. Behrman *et al.* (2006) provide estimates of adult knowledge production functions for rural Guatemala of the form of relation (1) and report that pre-schooling experiences are quite important but that schooling appears much more important than it is if pre- (and post-) schooling experiences are dropped from the specification.

²⁶ Direct estimates of relations such as (1) and (2) without controlling for the behavioral 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 schooling literature has focused is consistent with E_2 (schooling) being correlated positively with both E_0 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 (2004) and Behrman *et al.* (2006), then OLS estimates of relations (1) and/or (2) may lead to biases towards zero in the coefficient estimate for this variable.

conditional reduced-form relations with the conditionality being on the behavioral choice that determined the exposure to the ECD program. In such a case the ECD program experience is endogenous. As a result 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, parental innate health, 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, if the ECD measure used reflects parental choices (e.g., enrollment in ECD programs), it is likely to be correlated with parental and child innate abilities, preferences, and family connections. Indeed, there may be such correlations even if the measure for ECD programs used is just the availability and the quality of local ECD services given endogenous program placement decisions. 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, early-life ECD program experiences or ECD program placement proxies in part for correlated unobserved child and parental abilities, preferences, or family connections.

Better data always help deal with such problems. Section IV addresses different types of data that may be used for the investigation of the ECD interventions, and the better the data, the less likely there will be such problems. But for given data, there exist standard methodologies for dealing at least in part with these problems. Some examples of these problems and how estimation methodologies may help follow:

Sample Selection

Selection may take many forms: only having data on test scores and other developmental measures in early childhood for those participating in ECD programs, only having information on health status or on health impacts of an intervention for those children who attend health clinics, only having data on the impact of early childhood program for those who survived infancy and earlier childhood, only having data on those who do not attrit in longitudinal data, only having data on those who stay in the respective control and treatment groups in experimental data. The general problem is that those who are selected are not likely to be a random subsample of the relevant population. 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 longitudinal data (a major type of data that is discussed in Section IV), some elaboration on this type of selection is provided here (similar points hold for other types of selection). 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 behavior being studied. Consider the following canonical selection model:

$$L_t^* = b_2 + b_3X_t + b_4Z_t + U_t^* \quad (10)$$

and

$$Y_t = b_0 + b_1X_t + U_t^{**} (Y_t \text{ observed only if } L_t^* < 0). \quad (11)$$

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 or such mean differences but whether, and to what extent, attrition (or other forms of selection) invalidates the inferences that can be made for the broader population using the data from the subsample. 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.* 2001 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, b); (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); (4) Explore what are the bounds on the estimates given extreme assumptions on the key variables among those who attrited (e.g. Lee 2002). 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.* 2007). Given the potential importance of attrition in confounding the results, nevertheless, it is desirable for studies of the impact of ECD interventions 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).

Instrumental Variable (IV) or Two-Stage-Least Squares (2SLS) Estimates

One basic problem noted above in relations in Section II in which ECD program interventions are among the right-side variables is that the exposure of a particular child to the ECD intervention may reflect behavioral choices of parents, other caregivers, program administrators, or policy implementers that decide on program placement and program quality. As a result, the empirical representation of the ECD intervention is likely to be correlated with various unobserved variables – and the coefficient estimate for the ECD measure is likely to include not only the effect of the ECD program but also the correlated impact of these unobservables. To break the correlation between the observed right-side variables and the compound disturbance terms that include unobserved determinants in addition to stochastic terms in estimates of the impact of ECD programs over the life cycle, 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.²⁷ Note that potential good instruments include experiments²⁸ and so-called “natural experiments” in the form of natural events and policy changes.²⁹ The IV (or 2SLS) procedure basically consists of making first-stage estimates in which

²⁷ 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). 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 programs or nutrition on E_1 , school characteristics on E_2 , labor 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 labor 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 realized values.

²⁸ For example, 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.* (2007) and Martorell *et al.* (2005); the random assignment of treatment of worms among Kenyan school children as examined in Miguel and Kremer (2004); the random assignment of flip charts among Kenyan schools as examined in Glewwe *et al.* (2004).

²⁹ For example, natural events (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]), policy changes (e.g. the Indonesian school-building program investigated by Duflo [2001]), or other behaviors that changed exposure to the programs of interest but that were not motivated by such programs (e.g., changing school quality due to migration of Jews from Ethiopia to Israel as used by Gould, Lavy, and Paserman [2004]) – though there is some debate about to what extent some of these events are independent of the first-stage compound disturbance term.

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.³⁰ 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 characteristic 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.³¹

Fixed Effects (FE) Estimates

Some of the unobserved variables that are likely to cause problems if they are not controlled in the estimates may be 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 pre-school environment shared by children in the same pre-school program and the community environment shared by residents of the same community. Such factors that are fixed across observations can be

³⁰ Fixed effects estimates to control for fixed unobserved factors, such as are discussed below, are sometimes used together with IV estimates.

³¹ The econometric literature has been evolving recently in the development of diagnostic tests for good instruments (e.g. Stock and Yogo 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).

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 II (e.g. adult sister sibling estimates to control for shared childhood background among adult sisters in the estimation of the impact of mother's schooling attainment on child health, nutrition, and schooling in Nicaragua in Behrman and Wolfe [1984], [1987a, b]; individual fixed effects to control for unobserved malnutrition that determined which children received nutritional supplements in the Mexican *PROGRESA* program in Behrman and Hoddinott [2005] or which children were admitted to pre-school programs in the Bolivian pre-school PIDI program 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.³²

FE estimates, of course, have limitations. *First*, they do not control for unobserved varying characteristics (e.g. time-varying prices in longitudinal estimates that may affect endogenous behaviors), for which reason in some studies they are combined with IV estimates (e.g., the investigation of the impact of nutrition on labor 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, family FE do not permit estimating the impact of parental schooling on child ECD program exposure unless parental schooling varies over the time period in which women are having young children (as in the Rosenzweig and Wolpin [1995] study of the impact of young mothers' schooling on early childhood development in the United States).

³² 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 labor 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. Other recent studies for European countries also report that OLS estimates of intergenerational schooling effects may be quite misleading. Plug (2004) uses data on adoptees to lessen problems of intergenerationally-correlated endowments and Black, Devereux, and Salvanes (2005) use instruments based on changes in mandatory schooling.

Propensity Score Matching (PSM) Estimates

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 program evaluation literature. They are used to try to find the best comparison for someone exposed to the program (“treatment”) among those not treated in terms of pre-program observed variables. 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 *observed* characteristics used to predict the propensity to be treated.³³ An increasing number of studies have been undertaken to estimate in particular program impacts in developing countries that are consistent with the general life-cycle framework presented in Section I (e.g. the impact of early childhood development programs 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). PSM estimates have limitations in that it is not clear what should be the set of variables on which the matching is done (beyond that these variables should not be affected by the treatment or expectations of treatment and that they should lead to overlapping propensities for treatment (“common support”) for the treated and the control groups) and the results at times seem sensitive to the exact choices that are made and in that they do not control for unobserved variables (except when combined with FE, in which case they still do not control for time-varying unobserved variables).

Construction of Standard Errors

Most household sample surveys with information relevant for assessing ECD programs collect data from clusters (e.g. census tracts, villages, neighborhoods) - 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 utilize within-family estimates may be further subject to this problem. Moulton (1990), for example,

³³ Unobserved fixed factors, such as those discussed above, 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]).

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 communities (as in many of the studies in Table 1, though most of these studies do not correct the standard errors for such effects). In such cases 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 to estimate the impact of early-life nutritional interventions on education over the life cycle suggest that at least in this case these methods do not change substantially the inferences from the estimates (Maluccio *et al.* 2007).

IV. Strengths and Limitations of Analysis of Various Types of Data

The previous two sections point to considerable challenges in undertaking empirical estimates of causal relations pertaining to the impact of ECD interventions. 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 impact of ECD interventions 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 related to ECD programs in developing countries, with some

references to studies using data sets of various types in case the readers want to obtain further information about the details of the data or how they have been used.

A. Some Major Characteristics Pertaining to Data Quality

Before turning to different major types of data, it is useful to note five critical aspects of data quality that are common at a general level across different data options:

Representativeness

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), pre-school- or clinic-based data 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.

Power, Sample Size, and Sample Design

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 spending the third year of life in a particular comprehensive ECD program 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 ECD programs and cognitive skills among three-year old girls than to investigate the possibility of the same percentage impact with the same significance between ECD programs 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 on standard errors in Section III). 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 in the research project. Data that in other respects might appear very promising for the analysis of ECD interventions may not warrant analysis if the power is too low.

Coverage of Relevant Variables

To state the obvious, data are of value for the analysis of ECD interventions only if they include some information on variables for both the ECD intervention and the possible impacts on children (and their households) that are the targets of the intervention that capture at least some critical elements of the links across the life-cycle stages that are discussed in Section II. Many data sets, for example, have information on individuals' income and schooling and their co-resident children's schooling (and less frequently, pre-schooling) to date (e.g. most labor force surveys designed to capture the current conditions in the labor market). 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 (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 pre-school or 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 as noted above, and thus make use of estimates of such links to develop the overall picture of the impact of ECD interventions over children's life cycles. But such data do not permit direct estimation of relation (9) nor of many of the links between adult children's income and their early life ECD program experience.

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 ratio³⁴ 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 (FE) 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 and Behrman, Rosenzweig and Taubman 1994). Instrumental variable (IV) estimates, as noted in Section III, may also eliminate this bias towards zero due to random measurement error. However, measurement error is not only random, it might also have systematic components. For example, parental reports on child morbidity and on aspects of child development may depend very much on what their expectations are for “normal” child development, which may be systematically related to the parents' socioeconomic

³⁴ 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 the measured variable 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.

status and education. Such systematic errors may make accurate inferences about relations between ECD programs and such measures of child development difficult.

Human Subject Protection

Somewhat different in flavor than the four general aspects of data noted above, but also of importance, is the question of human subject protection. For existing data, the primary concern generally is protecting confidentiality of sample members. For the collection of new data, in addition to the question of confidentiality, the major questions seem to be what is the burden on the respondents, what benefits the respondents are expected to receive, what compensation is provided for the time that the respondents devote to the process, and what are the risks and protections for more invasive procedures such as the collection of biomedical samples. Considerations regarding the possibility of collecting new data that will inform us better of regarding ECD programs should weigh the expected gains in terms of scientific knowledge against the expected costs, including such costs for the participants in the study.

B. Some Major Types of Data for Evaluating ECD Interventions

Cross-sectional Data

Cross-sectional surveys and censuses are the most common type of available data. Cross-sectional household surveys tend to have some information on some of the links that are discussed in Section II, though not necessarily the ones from early-life ECD program exposure to intermediate outcomes or pathways of interest, and generally not on both early-life ECD experiences and adult outcomes as would be necessary to estimate directly relations such as (9). There are many cross-sectional surveys that are representative, often with a stratified cluster sample design, of populations of interest for this chapter. Also 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 behaviors such as attending pre-school programs 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.

Cross-sectional data sources vary considerably in their sample sizes and statistical power, and, as noted above in Section III, 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, as noted, cross-sectional data do not include information on the variables necessary to estimate directly reduced-form relations between early-life ECD and adult resource access as in relation (9) though some cross-sectional data sets may

have some key information for examining the impact of ECD program options for small children on the behaviors such as time use of other household members (mothers, older sisters, other caregivers). 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 ECD exposure and indicators of child development during preschool (based on recall data) and perhaps the school years for children. There are studies in the literature, for example, of *associations* between ECD program exposure and indicators of child development (e.g., Ghuman *et al.* [2005] for the Philippines).

The typical cross-sectional data permit some, but limited, control for the estimation problems that are discussed in Sections II and III. 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 and ECD program characteristics if not controlled.³⁵ That information is available on a number of children also permits the investigation of time-varying changes that affect siblings differentially; this may be useful for evaluating ECD program effects if the programs change over time (e.g. see Parker, Todd, and Wolpin [2006] on the impact of the Mexican *Oportunidades* program on schooling of children too old to have been affected by the program versus those children of age to have been affected by the program).

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, possibly including ECD program characteristics), 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; (2) such data may make possible within-sibling estimates if different siblings faced different community services related to ECD during critical periods such as early childhood; (3) such data may provide instruments that arguably are independent of the unobserved factors on the right-side of the relations in Section II but that predict sufficiently well the right-side ECD options so that good IV estimates can be obtained (e.g. 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 chapter in the study of health, schooling, and socioeconomic consequences in Indonesia by Maccini and Yang (2006).

Longitudinal or Panel Data

These data follow individuals and/or households over time. They generally provide a more satisfactory means of identifying the impact of ECD interventions than do cross-sectional data because: (1) the prospective data gathered in earlier rounds is likely to be less contaminated with measurement error and more complete than recall data from

³⁵ This would work for ECD program characteristics only if there is variation in such characteristics among the programs in each sample cluster.

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 the dynamics of effects such as 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 migration³⁶ (see discussion in Section III), 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 conditions related to ECD 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, 1992; Glewwe, Jacoby and King 2000; Glewwe and King 2001; 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 (Foster and Rosenzweig 1995, 2004); the Bangladeshi nutritional data with follow-up after over two decades (Pitt, Rosenzweig, and Hassan 1990, 2006); the ICRISAT village-level study (VLS) data starting in 1975 with follow-up ongoing to the present (Behrman 1988a,b; Behrman and Deolalikar 1987).

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 (Behrman, Parker, and Todd 2006, 2007); a number of the Demographic Health Survey (DHS) data sets; the Vietnam Living Standard Measurement Survey (LSMS, Agarwal, Dollar, and Glewwe 2004); 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; Kohler, Behrman and, Watkins 2007); the Kenyan school-based sample (Glewwe *et al.* 2004, Miguel and Kremer 2004);

³⁶ Though not because of mortality, which implies a selection problem for a time series of cross sections following the same birth cohort just as for longitudinal data following that cohort.

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 be, 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.³⁷ Some prominent examples include: The Mexican rural *PROGRESA* program with random assignment of initial treatment versus controls for 506 communities including provision of micro nutrients for infants and young children (Behrman and Hoddinott 2005; Behrman, Parker, and Todd 2006, 2007; the Kenyan random assignment of various treatments (including deworming, flip charts) among 75 schools (Glewwe *et al.* 2004, Miguel and Kremer 2004); the Guatemalan INCAP data with random assignment of nutritional supplements among the four participant communities (Martorell *et al.* 2005, Behrman *et al.* 2006, Maluccio *et al.* 2007); experimental assignment of fees and distances to VCT clinics in the Malawian Ideation and Diffusion Change Project (MDICP, Watkins *et al.* 2003; Kohler, Behrman, and Watkins 2007); random assignment of iron supplements in Indonesia (Thomas *et al.* 2003). Such experiments provide (a) capacity for identifying the causal effect of treatment and (b) the possibility of identifying the impact of one behavioral choice affected by the treatment on another by using the experimental assignment as an instrument for IV estimates. These are considerable strengths. Good experiments rightly are thought to be the “gold standard” for obtaining reduced-form impact estimates, including for ECD programs. 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 the impacts in different market, policy and cultural contexts and even the longer-run impacts in the location in which the experiment was conducted.³⁸ Therefore, while good experiments to improve the evaluation of ECD programs should be strongly encouraged, non-experimental data also

³⁷ 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).

³⁸ At the cost of the assumptions necessary to estimate structural models of the behaviors such as are outlined in Section III, evaluations of counterfactual policies can be made (e.g. different treatments, impacts for longer time periods than observed in the data). Todd and Wolpin (2007) provide an example using the Mexican *PROGRESA* data. They estimate a structural model using baseline data, test the model’s predictions against the experimental results (and find that the model predicts fairly well), and then use the model to conduct counterfactual experiments (e.g. with different scholarship schedules for different grades, with the program running many years even through the experimental data were only for the baseline plus two years of follow-up).

can provide useful insights into key relations relating to the impacts of ECD programs over the life cycle if they are analyzed with sensitivity to the estimation problems raised in Sections II and III and the estimates interpreted with sensitivity to the limitations of the data and the approaches in light of the life-cycle framework outlined in Section II.

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.³⁹ 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 Impact of ECD interventions seem limited, but perhaps underexplored.

Qualitative Data Sources

Most other possible data sources for investigating ECD interventions 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 III) apply for such data sources. That is, the questions of representativeness, power, variable coverage, measurement errors and human subject protection hold for qualitative as well as for quantitative data. Extensive family or individual histories or focus groups may provide useful insights regarding hypotheses regarding ECD interventions 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 ECD interventions 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 possible problems with measurement errors. Likewise, it is necessary to recognize 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 quantitative 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. 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] and Kohler, Behrman and Watkins [2007]).

³⁹ But as noted above, such data are not representative of birth cohorts if there is selective mortality, including in early life.

Conclusions

There are a small number of reasonably good evaluations of ECD programs in developing countries that suggest that the impacts of these programs may be substantial, in some cases over the medium- and long-runs in addition to the fairly short-run effects. In a few cases analysis also suggests that the benefit-cost ratios or rates of return to resources used in these programs are high. But most of the existing systematic analyses of ECD programs consider only a subset of the possible impacts over a fairly short time horizon with little or no attention to issues related to the timing of these impacts, the resource costs of the programs, the sensitivity of the programs to market, policy or cultural contexts, or the implications of the program for the efficiency motive for policies.

Given the widespread perception that ECD programs in developing countries are important and the widespread advocacy for expanding them, therefore, the potential gains from expanding systematic evaluations of ECD programs is considerable. Undertaking good empirical analysis of many aspects of ECD interventions, however, is challenging given data limitations. These challenges are clarified in this chapter by: (1) considering the types of relations that might be estimated to be informative for understanding better aspects of the links that connect ECD programs and adult resource access and other relevant outcomes 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; (3) considering different types of data that are available that might permit advances in knowledge of the impact of ECD interventions in developing countries in light of such a framework of analysis and the related estimation alternatives; and (4) discussing the importance of investigating not only the impacts but how to value those impacts to obtain total benefits and comparing those benefits with total resource costs with sensitivity analysis for the critical parameters such as discount rates and the value of averting early mortality. 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 ECD interventions and for exploring the robustness of estimates related to ECD interventions to alternative strategies for dealing with the estimation problems. In some cases, moreover, researchers can and should provide their understanding of the probable directions and, if possible, magnitudes of biases due to the estimation problems that they can not deal with directly in a particular study of ECD intervention. 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 ECD programs and outcomes over the life cycle that are described in Section II 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?⁴⁰

- What is the nature of data quality with regard to representativeness, power, coverage of important concepts in the linkage between ECD programs and outcomes over the life cycle and therefore the impact of ECD interventions, and measurement error?
- What special features of the data might permit better exploration of the impact of ECD interventions? 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?
- How best can good estimates of the resource costs for ECD programs be obtained?
- How can better estimates of the social versus the private rates of return to ECD programs be obtained so that we would be better informed about the efficiency motive for policies?
- Might incentives or other mechanisms be used to make ECD programs be more efficient and therefore have higher benefit-cost ratios?

Through careful examination of existing data, keeping in mind the considerations that are discussed in this chapter, much can be learned about ECD interventions 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 encouraging the collection of new and better data with better indicators of costs and benefits of ECD programs, more randomization to facilitate more confident identification of causal effects of ECD programs, randomization with regard not only to program availability but also information about programs and key characteristics that may influence the quality of programs, longer time horizons for data collection so that the medium- and long-run effects can be explored directly, and greater variance of market, policy and cultural context so that the robustness of the benefits/costs of ECD programs to the great contextual variation in developing countries can be ascertained. Careful systematic analyses of such data will permit enhancing importantly our knowledge of ECD programs in developing countries.

⁴⁰ 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 economics in particular and in the social sciences more broadly on health and development has emphasized HIV/AIDS and injuries relative to non-communicable diseases much more than current or project future distributions of these health/disease conditions would suggest was warranted (Behrman, Behrman, and Perez 2006a,b).

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Appendix Table A.1

Comparison of Characteristics and Evaluation Results of Some Major U.S. Child Care Intervention Programs

Program Characteristics	Perry Preschool Program ^(a)	Head Start ^(b)	Abecedarian Project ^(c)	Milwaukee Project ^(d)	HIPPY program ^(e)
target group	3-4 year olds	3-5 year olds	Children aged 6 weeks-5 years, staggered ages of entry	Children aged 6 weeks-5 years; staggered ages of entry	4-5 year olds
nature of intervention	Half-day center based preschool program with family visits. Highly trained and well-paid staff; pupil-teacher ratio=5.7	Center-based program; children attend head-start preschool centers, receive preventative medical care, nutritional supplements, and cognitive stimulation.	Full-day, center-based program providing developmental and pediatric surveillance, nutritional supplements, and educational services.	Full-day, center-based day-care/preschool program	Home-based early childhood enrichment program; parents receive training in how to educate their children from paraprofessional trainers.
Program cost	\$7252 per child per year ^(f)	\$4491 per child per year ^(f)	\$11,000 per child per year (1999 dollars)	N/A	N/A
eligibility criteria	Children from families with low SES scores and with 3 year IQ scores between 70-85 point	90% of families in program below poverty line	Eligibility index based on parents education, income, presence of father, welfare, parental IQ.	Children from mothers with low IQ test scores (<75)	Children from predominantly poor and immigrant families who also were already enrolled in a city preschool program
type of evaluation	Two comparison groups first matched on observables, then one randomly assigned as treatment group and other as control group	Nonexperimental; within sibling comparisons for the children of the NSLY data.	Randomized assignment to two experimental conditions: preschool education or not and Home Resource Intervention or not	Randomized assignment to treatment	Randomized assignment to treatment
sample size	123 children; 58 in treatment group, 65 in control group	5000 observations (children with siblings who have mothers in the NLSY; 1/5 participated in Head Start)	111 children; 55 in treated group, 54 in control group	N/A	219; 98 in treatment group and 84 in control group (two cohorts separately analysed)
short term assessments	Effects on educational performance found at age 4-7, but effects faded-out and were not detected at older ages	Effects on PPVT test scores, on the probability of being immunized and on height for age found mainly for white (incl. Hispanic) children.	Immediate effects on IQ test scores of early preschool intervention programs, but no effect found of home resource program	Persistent effects on IQ test scores through age 14	Significant effects of program found on eleven outcomes, incl. cognitive ability and classroom adaptation (but only for one of two cohorts.)
Long term assessments	At age 27, found reduced crime rates, increased educational attainment (for females only), decreased welfare and out of wedlock births	At age 9, lower probability of having repeated a grade for white children	Increased IQ scores to age 21, increased college att., decreased adolescent child-bearing, decreased special ed	N/A	N/A

(a) Schweinhart and Weikart (1998); (b) Currie and Thomas (1995, 1996); (c) Ramey, Campbell and Blair (1998); (d) Ramey, Campbell and Blair (1998), Garber (1988); (e) Baker; Piotrkowski, and Brooks-Gunn (1998).