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Evaluating the Long-term Impact of Antipoverty Interventions in Bangladesh

An Overview

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The International Food Policy Research Institute (IFPRI) was established in 1975. IFPRI is one of 15 agricultural research centers that receive principal funding from governments, private foundations, and international and regional organizations, most of which are members of the Consultative Group on International Agricultural Research (CGIAR).

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ABSTRACT

This paper provides an overview of a research project that assessed the long-term impact of three antipoverty interventions in Bangladesh—the introduction of new agricultural technologies, educational transfers, and microfinance—on monetary and nonmonetary measures of well-being. This paper begins by setting out the conceptual framework, methodology, and empirical methods used for the evaluation of long-term impacts. It discusses the context of the evaluations and the longitudinal data used. Key findings from the individual papers are then presented, followed by an indicative analysis of the cost-effectiveness of these interventions. The overview concludes with implications for programs and policy.

Keywords: Bangladesh, antipoverty interventions, impact evaluation, long-term impact

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1. INTRODUCTION

Despite impressive reductions in poverty over the past 10 years, poverty reduction remains the central policy challenge for Bangladesh. The percentage of the population living in poverty fell from 51 percent in 1995 to 40 percent in 2005 and there have been substantial improvements in nonmonetary indicators of the poorest (BBS 2006; Sen and Hulme 2006). Growth in the rural sector played an important role in this process; the pace of rural poverty reduction was slow in the 1980s, but became considerably faster in the 1990s (Sen 2003). Nevertheless, poverty remains a key challenge with more than 50 million people still living in extreme poverty (BBS 2006). Poverty reduction is the primary objective of government policy, and it is noteworthy that despite changes in the ruling party and administration, there has been remarkable continuity in the policy framework in support of poverty reduction. The high degree of civil society involvement in designing and implementing antipoverty interventions is another remarkable feature of Bangladesh's recent history. Government and donors have also realized the importance of tapping the institutional capacity of large nongovernmental organizations (NGOs); many major food and cash transfer programs targeted to vulnerable groups currently use service delivery mechanisms established by NGOs (Ahmed et al. 2010).

Like other countries in South Asia, there is a strong gender dimension to poverty in Bangladesh. In Bangladesh, rural households headed by women are more likely to be among the poorest (Ahmed et al. 2007). Women also lag behind in terms of education—with more than one in three women having no schooling, compared to one in four men. A recent analysis also showed that lack of education in adult women in Bangladesh is a strong correlate of being "ultra-poor": 80 percent of adult women with no education live below half a dollar a day (Ahmed et al. 2007). Despite a long history of gender discrimination, there are signs of improvement, particularly for the younger generation. Between the 1996-1997 and 2007 Demographic and Health Survey (DHS) rounds, the proportion of women (age 15-49) with a Body Mass Index less than 18.5 has dropped noticeably from 52 percent to 30 percent. Mirroring improvements in maternal nutrition, the proportion of underweight children has dropped from 66 percent in 1989/90 to 46 percent (NCHS standard) in 2007, while the proportion of children with severe stunting fell from 28 percent to 16 percent (Sen et al. 2010).

Given the policy focus on poverty reduction, and the link between gender and poverty, it is no surprise that government and civil society organizations have implemented many important interventions designed to help individuals and households move out of poverty. New agricultural technologies have been introduced through the government extension system, specialized agencies of the Ministry of Agriculture, and nongovernmental organizations (NGOs). NGOs, while initially associated with small-scale vegetable production, poultry, fisheries, agroforestry, and social forestry, are now increasingly moving toward advisory activities in such major crops as hybrid rice and maize. NGOs form groups of target farmers—especially landless farmers and those outside the scope of other extension services—often focusing on poor women (Birner, Quisumbing, and Ahmed 2010). Services provided by NGOs usually include credit, input supply, and training; the targeting of such resources to women is a key innovation that is adapted to this cultural context. Unlike the public-sector extension and advisory agencies, NGOs often provide multiple services, addressing such needs as health awareness and skill development as well as agricultural extension. Constrained by the limited land area of their intended beneficiaries (usually landless and marginal farmers), most NGOs operate at a small scale, although the large national NGOs such as BRAC, ASA, Grameen, and PROSHIKA have networks that allow them to operate at scale. In the area of human capital investment, the gender gap in primary and secondary education has closed (Hausmann, Tyson, and Zahidi 2010), in no small part owing to government programs designed to increase school attendance and grade progression among girls. Although microfinance providers are now modifying the group-lending scheme to attract individual borrowers, and financial sustainability and chronic indebtedness remain issues of concern (Morduch 1999), the microfinance movement is well recognized as one of the significant factors that has contributed to poverty reduction in Bangladesh (Khandker 2005), although more recent evaluations (Roodman and Morduch 2009; Duvendack and

Palmer-Jones 2011) question the evidence base for such optimism regarding microfinance.[1] However, while many evaluations have attempted to assess the short-term impacts of these programs, relatively little is known about their long-term impact or their relative cost-effectiveness.

The papers in this research project attempted to address this gap in knowledge by (1) assessing the *long-term* impact of three antipoverty interventions—the introduction of new agricultural technologies, educational transfers, and microfinance—on a range of monetary and nonmonetary measures of well-being; (2) examining institutional, sociocultural, and contextual factors underlying the performance of these interventions; and (3) to a more limited extent, comparing the cost-effectiveness of these interventions in attaining their development objectives.[2] The research project investigated (1) the long-term impacts of the introduction of agricultural technologies and educational transfers on per capita consumption and gender disaggregated measures of monetary and nonmonetary well-being; (2) the impact of each of the agricultural technology and educational transfers interventions on physical and human capital accumulation; (3) the underlying processes, at the household, community, and national levels, that have contributed to the success or failure of these interventions; and (4) the relative cost-effectiveness of the interventions. As noted later in the text, the cost-effectiveness analysis is limited by data availability. Because of the impossibility of maintaining the control group in the microfinance interventions, our quantitative analysis is limited to the agricultural technology and educational transfers interventions. However, qualitative analysis based on life-histories conducted in the study sites of the agricultural technology, educational transfers, and microfinance interventions aims to uncover the extent to which all three interventions contributed to improvements (or declines) in individual and household well-being over the years, and form a valuable complement to the quantitative analysis (Davis 2009). Institutional factors and processes are also discussed in detail in Ahmed, Khondkar, and Quisumbing (2011), which is based on a longer paper by Ahmed and Khondkar (2009). The comparison of development programs using a common quantitative methodology, the integrated and sequential qualitative analysis, the attention to institutions, policies, and processes surrounding the interventions, and the indicative analysis of cost-effectiveness across interventions is an important contribution of this research project.

This overview paper begins by setting out the conceptual framework, methodology, and empirical methods used for the evaluation of long-term impacts. It then discusses the context of the evaluations and the longitudinal data used. Key findings are then presented, followed by an indicative analysis of the cost-effectiveness of these interventions. The paper concludes with implications for programs and policy.

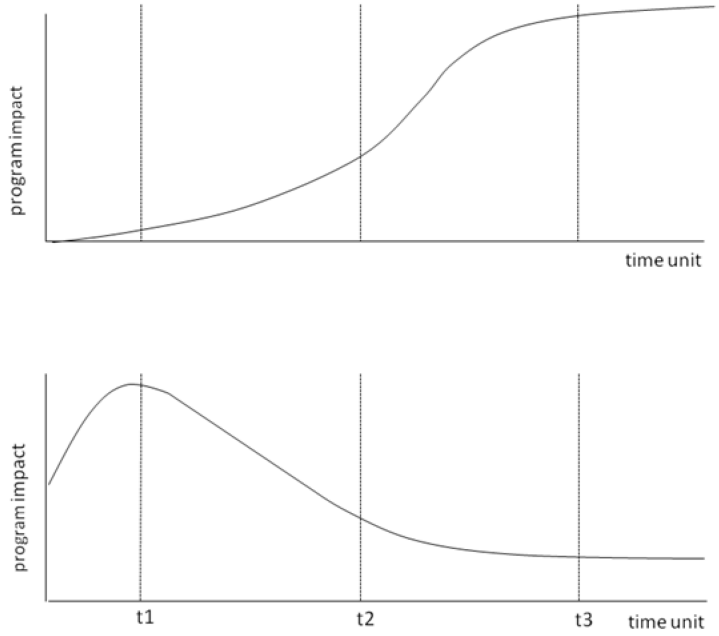
[1] Initial assessments of the impact of microfinance on poverty reduction have probably provided an overly optimistic view of its development impact. Morduch (1999) reviews the cross-country evidence on the microfinance 'revolution' and finds that issues of appropriate mechanism design, savings mobilization, financial sustainability, and scale remain. Roodman and Morduch (2009), using the same data as Pitt and Khandker (1998) and Khandker (2005), find that evidence for impact is weak, mostly because these studies are unable to convincingly remove the effects of endogeneity. Duvendack and Palmer-Jones (2011) apply propensity score matching (PSM) to the Pitt and Khandker (1998) data, differentiate outcomes by the gender of the borrower, and take into account borrowing from several formal and informal sources, and find that the mainly positive impacts of microfinance observed by previous studies are highly vulnerable to selection on unobservables.

[2] The papers that comprise this research project are forthcoming in a special issue of the *Journal of Development Effectiveness*, but most have been released as discussion papers or working papers. References in the text refer to the discussion or working paper version.

2. CONCEPTUAL FRAMEWORK

Many antipoverty interventions are evaluated after three to five years of implementation, often because of project implementation cycles. However, such short-term measures of program impacts may be misleading. King and Behrman (2008) have argued that the timing of evaluations is an important, but relatively understudied, issue in program evaluation. The timing of the evaluation is particularly important for evaluating programs that require changes in the behavior of both service providers and service users. If one evaluates too early, one risks finding only partial or no impact; if one waits too long, one risks losing donor and public support for the program or a scaling up of a badly-designed program (King and Behrman 2008, 3).[3] Because of leads and lags in program implementation, differential adoption rates and the presence (or absence) of fixed costs, the impact of a program is unlikely to be constant over time. Consider a comparison between two hypothetical programs for which the time path of impact differs, as shown in the top and bottom panels of Figure 2.1. An evaluation undertaken at *t*1 would indicate that the case in the bottom panel has greater impact, whereas an evaluation undertaken at *t*3 would suggest the opposite result. King and Behrman argue that the duration of exposure to a treatment might vary across program areas and beneficiaries, leading to different estimates of program impact, because of three broad reasons. First, organizational factors create leads and lags in program implementation—implementing agencies typically have to roll out a program, service providers have to be trained, and the necessary supplies and equipment have to be in place. Second, spillover effects can arise from learning and adoption by beneficiaries and possible contamination of the control groups. Third, responses to the treatment may be heterogeneous (for example, in education- and health-related programs, age and cohort may interact significantly with the duration of the program). Disseminating and adopting agricultural technologies also require changes in the behavior of extension agents and farm households, respectively, and leads, lags, and spillover effects are also a part of the agricultural technology diffusion process.

Figure 2.1—Timing of impact evaluation and impact estimates



Source: King and Behrman (2008).

[3] While the King and Behrman (2008) paper was written in the context of evaluating social programs, it is also relevant to programs disseminating agricultural technologies.

3. QUANTITATIVE AND QUALITATIVE METHODOLOGIES FOR ASSESSING LONG-TERM IMPACT

The core of the evaluation problem is how to assess the counterfactual: what would have happened to beneficiaries in the absence of the intervention or treatment (Baker 2000; Todd 2006; Khandker, Koolwal, and Samad 2010). Random assignment of the treatment to a pool of equally eligible individuals or households followed by a comparison of outcomes for the treatment and control groups before and after treatment is probably the simplest way to solve the evaluation problem (Duflo and Kremer 2003). However, randomized control trials are not appropriate for evaluating all interventions because of sector-wide effects, political or institutional constraints to randomizing the intervention, or behavioral changes that may confound estimates of impact (Barrett and Carter 2010). In the case of the agricultural technology and educational transfers interventions considered here, random allocation of beneficiaries to treatment and control groups was infeasible either because self-selection into the treatment group was involved (the agricultural technology intervention) or because the treatment involved a program with countrywide scope (educational transfers). Moreover, randomized assignment to treatment and control groups was not standard practice in the early 1990s when these programs were being initiated and the baseline established. Even if they had, it would also have been extremely difficult, if not impossible, to avoid spillovers between randomized intervention and control groups over the long period of time spanned by the panel.[4]

The quantitative methodology used in this set of papers therefore rests on an observational approach, which constructs comparable treatment and comparison groups using a type of covariate matching, nearest neighbor matching (NNM) (Abadie et al. 2004; Abadie and Imbens 2009). NNM estimates the counterfactual outcomes for the treatment group by constructing a statistical comparison group of households that did not get the treatment with similar observable characteristics in the baseline survey round (which in our case is after the intervention started). NNM is similar to the popular propensity score matching (PSM) in that it estimates the impact as the average of the difference in the outcome for each beneficiary from a weighted average of outcomes for matched non-beneficiaries using a distance measure to construct the weights. Control households that are closer to the treated households—or those that have similar values for the covariates as the treated households—receive a larger weight than those that are farther away.

The main differences between NNM and PSM lie in the way comparable control households are selected and the weights used to construct the difference in weighted average outcomes. NNM matches treated households to control households directly on the observable characteristics. Each treated household is matched to the control group with the smallest average difference in pre-program characteristics, where this difference is determined using a multidimensional metric across all covariates. The outcome of this procedure is a "like-with-like" comparison of treatment households and controls that eliminates many common sources of bias. A distinct strength of the NNM approach is that it is entirely nonparametric and does not rely on distributional assumptions that underlie probit or logit models used to estimate the propensity scores in PSM. Another benefit of NNM over PSM is that NNM relies on analytical standard errors rather than bootstrapped standard errors.[5]

Having constructed a comparison group using covariate (nearest neighbor) matching, we then estimate average treatment effects on the treated (ATTs) for a range of outcome variables using a difference-in-difference (DID) approach.[6] This involves calculating the change in outcomes that have occurred over time for the matched treatment and comparison groups, and then deducting the changes that

[4] See, for example, Todd (2006) on the difficulty of maintaining treatment and control groups over much shorter periods in the PROGRESA evaluation in Mexico.

[5] We present details on the precise proceeding used for balancing, matching and trimming the study data in each of the quantitative papers in Appendix B.

[6] Steps involved constructing the matched sample (including estimating the propensity score, trimming the sample to ensure common support for the treatment observations in the comparison group, and testing that the balancing property is satisfied) are discussed in Appendix B.

have occurred among the comparison group from those for the treatment group. This double-difference identifies the average impact of an intervention on different outcome variables over time. For succinctness, we refer to this approach as "DID based on matched NN matched samples."

This estimation approach assumes that after controlling for all observable individual, household, and community characteristics that are correlated with program participation, the treatment and comparison groups will have the same average outcome as treatment households/individuals would have had if they did not participate in the intervention. This is known as ignorability of assignment or the unconfoundedness assumption. However, nearest-neighbor matching will provide biased estimates of program impact if, for any chosen outcome, it is not feasible to control for enough observable characteristics to ensure this assumption holds (that is, there is omitted variable bias). To reduce the likelihood of such bias, the authors of the quantitative papers in the research project restrict their estimation sample in different ways. For example, Kumar and Quisumbing (2009a) focus on early and late adopters of the new agricultural technologies, and Kumar and Quisumbing (2009b) focus on NGO and non-NGO members. These comparisons are undertaken not only to reduce the likelihood of omitted variable bias, but also because they are interesting comparisons in their own right. Similarly, Baulch (2009) splits his sample villages into those that were and were not included in the in-kind education transfer program (known as "food-for-education") that preceded the primary education stipend (PES). In addition, the use of difference-in-difference estimation removes the effect of any unobserved time-invariant differences between the treatment and comparison groups, although it does not address the impacts of unobserved or missing time-varying variables nor the possibility that time-invariant variables may have time-varying effects. While these procedures cannot guarantee that selection bias due to time-varying unobservables is not present, they certainly minimize it.[7]

Davis' (2009) takes a quite different qualitative approach to the evaluation counterfactual. He combines life history interviews with process tracing to identify the impact of reported events on the livelihood trajectories of 293 male and female "research participants" (interviewees). Davis and his qualitative research team conducted extremely thorough semi-structured life-history interviews with one male and female adult from 161 households in eight villages drawn from a subsample of the larger panel studies. This subsample was stratified both by intervention and poverty status but the qualitative interviewers were careful not to reveal which intervention(s) they were interested in.[8] The team used historical markers (such as the 1971 war of independence, the 1988 floods, and the deaths of important political leaders) as a background against which the life histories of the respondents could be superimposed, with five common well-being levels being assigned to different periods of their lives (see Davis [2009] and Davis and Baulch [2009] for further details). Counterfactual ideas were then introduced in the life-history interviews by inviting the participants to consider the effect(s) of past events on present circumstances and well-being levels. When participants identified events or episodes that had made a significant difference to their present situation, they were invited to consider the thought-experiment of how different their lives would have been if these events or episodes had not taken place. Such thought-experiments obviously needed to be interpreted carefully but allowed the careful consideration of some linkages and causal mechanisms between events and outcomes, which might not have been possible using observed regularities in the quantitative panel data. Post-coding of these causes was done using NVivo 8 (a qualitative data analysis software package) and the resulting frequency rankings help to identify the main causes of improvement and decline in people's lives (in which only one of the three development interventions we consider features prominently). Davis' qualitative findings therefore provide a useful way to both unpack the causal linkages underlying changes and to contextualize the broader importance of the three development interventions in people's lives.

[7] An alternative estimation approach would have been instrumental variable estimation but this requires the identification of suitable instrument variables that are highly correlated with the treatment but uncorrelated with the outcome variables. Because of the nature of the interventions and program eligibility criteria, it was not possible to identify convincing instruments for the agricultural technology, microfinance and education transfer interventions.

[8] Since the interviews were conducted by a professional survey company (DATA Ltd) supervised by an international expert (Davis), it is also unlikely that many respondents identified them with the interventions under consideration. (Indeed in one village in Nilphamari, the qualitative interview team was initially mistaken for Christian missionaries!).

4. CONTEXT, DATA, AND INSTITUTIONS

Context

The development interventions that we analyze in the research project are the dissemination of improved agricultural technologies using alternative implementation modalities, most of them building on a microfinance platform; and the use of conditional cash transfers to increase school enrolment and improve grade progression. Given their prominence, it is no surprise that these interventions have been the subject of many evaluations; in fact, this study builds on earlier short-term impact evaluations described in Zeller et al. (2001); Bouis et al. (1998); Hallman, Lewis, and Begum (2007); Ahmed (2005); and Ahmed and Arends-Kuenning (2006).

The first intervention, or more appropriately, set of interventions, that we study quantitatively is the introduction of new agricultural technologies using different implementation modalities in 47 villages in three different project sites. These are (1) homestead vegetable production in Saturia; (2) polyculture fish production in household-owned ponds in Mymensingh; and (3) polyculture fish production in group-managed ponds in Jessore. These interventions are among the "food-based" strategies to alleviate micronutrient malnutrition, interventions that can complement supplementation and fortification programs and seek directly to improve dietary quality. For example, it has been argued that promotion of polyculture fish and vegetable production, two foods relatively rich in micronutrients, holds potential for improving micronutrient status in Bangladesh by (1) increasing the supply of micronutrients to the general population; and, (2) directly improving household incomes and intakes of fish and vegetables of producing households (Bouis et al. 1998). Such food-based strategies are exemplified in Helen Keller International's Homestead Food Production program, which, over the past two decades, has reached nearly 4 percent of the population in Bangladesh and covered just over half of all the country's subdistricts (Iannotti, Cunningham, and Ruel 2009). In two of the three interventions considered (the improved vegetables and group fishpond programs), the technology was disseminated through women's groups; in the other (individual fishponds), a donor-funded extension program disseminated the technology. The group-based programs also used a "microfinance plus" platform. Such microfinance programs directed to poor households, often targeting women, are now a familiar feature of the development landscape in Bangladesh. Unlike their formal sector counterparts, these programs have succeeded in delivering financial services (both credit and savings) to the poor, particularly poor women, at very low default rates (Khandker 2005). In the "microfinance plus" model studied in this project, microfinance is provided together with a package of health, agricultural extension/technology, legal counseling, and/or other services. The membership base and service delivery infrastructure of the microfinance program act as the conduits for service delivery.

The second antipoverty intervention we examine in the quantitative analysis is the provision of cash transfers to poor households, conditional on children's school attendance. Bangladesh has some of the longest-running education transfer programs in the world, starting with the Food for Education (FFE) program in 1993, which provided around 5.1 million beneficiary households with a free ration of wheat or rice for sending children of the appropriate age to primary school. The Primary Education Stipend (PES) program, which provides eligible households with children who attend school 85 percent of the time with 100 Bangladeshi taka (BDT) (about US$1.76) per month, replaced the FFE Program in July 2002.[9] During its first phase of implementation from mid-2002 until mid-2007, about 5.5 million children received the PES, with a budget accounting for almost 19 percent of the primary education budget. While initial evaluations (Ahmed and del Ninno 2002) found that FFE increased attendance for both boys and girls, but especially girls, by 18.7 percent and also increased household food consumption by 90 percent, a subsequent evaluation of the short-term impact of the PES (Ahmed 2005) found a more muted effect on primary school enrolments (13.7 percent) and no effect on household food consumption. Because these

[9] Households with more than one primary school age child receive 125 Taka per month.

results were based on a survey conducted the year after the implementation of the PES, its longer-term impact remained unknown (Tietjen 2003).

Data

Our study builds on three studies conducted by the International Food Policy Research Institute (IFPRI) in Bangladesh to evaluate the short-term impacts of microfinance (1994), the new agricultural technologies (1996-1997) and the introduction of educational transfers (2000 and 2003) and a follow-up conducted in 2006-2007. The original evaluations surveyed 1,787 households and 102 villages located in 14 of Bangladesh's 64 districts. These districts and villages were selected to span the range of agroecological conditions found in rural Bangladesh and broadly characterize the variability of livelihoods found in rural Bangladesh. In designing the original evaluation surveys, careful attention was paid to establishing both intervention and comparison/control groups to derive single difference estimates of short-term project impact. In the agricultural technology and educational transfers evaluations, the control groups were drawn from villages (unions, in the case of educational transfers) where the program had not yet been introduced. The agricultural technology and educational transfers cites covered 47 and 48 villages, respectively. For the microfinance evaluation, seven villages were chosen that had NGO microfinance programs that were operating within these villages. Program and control households were then sampled according to landholding strata. Because microfinance programs are now so widespread, it was impossible to maintain the treatment-control design from the initial evaluation in these seven villages, so the quantitative analysis in this research project focuses on the agricultural technology and educational transfers programs. Indeed, the small number of villages in the original microfinance evaluation made it impossible to exploit intervillage variation in the analysis. However, the qualitative analysis will also examine people's perceptions of the long-term impact of microfinance. Table 4.1 summarizes the sample for each of the interventions included in the original evaluation surveys, together with the years in which resurveys occurred.

In 2006, IFPRI, DATA, and the Chronic Poverty Research Centre (CPRC) began a major study to resurvey the households surveyed in all three of the evaluations. While the focus of this study was on understanding of the drivers and maintainers of chronic poverty in rural Bangladesh, the intervention-comparison groups were maintained from the previous study. The resurvey involved both qualitative studies and a follow-up longitudinal survey of households included in the IFPRI studies, and involves three sequenced and integrated phases.

Phase I was a qualitative phase designed to examine perceptions of changes (and why these have come about) through focus group discussions with women and men in a subsample of our survey communities. A total of 116 single-sex focus group discussions, evenly divided between treatment and control villages, were conducted. Phase II was a quantitative survey of the original households and new households that have split off from these households but still lived in the same district. The household survey took place from November 2006-March 2007, the same agricultural season as the original survey (or one of its rounds), and covered 2,152 households, of which 1,787 were core households that took part in the original survey, and 365 are "splits" from the original household. The survey instruments used were designed to be comparable across sites and also to facilitate comparability with the original questionnaire from the evaluation studies. Phase III consisted of a qualitative study based on life histories of 161 households, focusing on the years between the original survey and the most recent survey. Households were stratified by intervention, and then selected based on the four cells of the poverty transition matrix (poor in both waves, moving into poverty, moving out of poverty, and not poor in both waves). Semi-structured interviews were conducted using life-history methods and visualization techniques by a small team of experienced Bengali-speaking researchers to understand the processes and institutional contexts that influenced livelihood trajectories.[10]

[10] The term "process tracing" comes from the political science literature, where it refers to peering "into the box of causation" (Geering 2005, 2007; George and Bennett 2005).

Table 4.1—Sample sizes of treatment and control groups, by intervention

Intervention/Survey years	Districts included	Number of villages	Treatment group		Comparison group	
			Description	Number of households	Description	Number of households
Agricultural technologies						
Improved vegetables (1996/7, 2000, and 2006/7)	Manikganj	10	NGO members in villages where improved technologies were disseminated	110	NGO members in villages where improved technologies had not yet been disseminated	110
Individual fishponds (1996/7 and 2006/7)	Kishoreganj, Mymensingh	22	Individual pond owners members in villages where improved technologies were disseminated	110	Individual pond owners members in villages where improved technologies had not yet been disseminated	110
Group fishponds (1996/7 and 2006/7)	Jessore	9	NGO members in villages where improved technologies were disseminated	110	NGO members in villages where improved technologies were disseminated	110
Educational transfers (2000, 2003, and 2006)	Barisal, Chandpur, Cox's Bazaar, Naogaon, Narail, Nilphamari, Sherpur, Tangail	48	Households in FFE unions	400	Households in non-FFE unions	200
Microfinance (1994/95, 2006/7	Habiganj, Kurigram, Manikganj, Mymensingh	7	Participants in microfinance NGOs in all villages	114	Nonparticipants in microfinance NGOs in all villages (eligible nonparticipants)	118

Source: Author's compilation.

Notes: The agricultural technology sites also include 110 other households that are randomly selected from non-NGO members in each site. The microfinance sites also include 118 households that are not eligible because of owning at least one acre of land. FFE = food-for-education; NGO = nongovernmental organization.

Further information on the sampling and design of this study is provided by Zahidul Hassan et al. (2011). A number of papers analyzing the determinants of chronic poverty and poverty dynamics using the same data can be found on http://www.ifpri.org/dataset/chronic-poverty-and-long-term-impact-study-bangladesh.

Institutions and Policy Processes

Because antipoverty interventions do not occur in a vacuum, it is useful to understand the institutional context of these interventions, the processes by which these interventions began, and the modifications undertaken owing to both changes in the external environment as well as in response to implementation experience and systematic evaluation. Ahmed, Khondkar, and Quisumbing's paper (2011) reviews the background and history of each of the interventions, their targeting mechanisms and performance, and how the interventions developed over time (including, in the case of educational transfers, the food-for-education program being replaced by the cash-based primary education stipend program). Special attention is paid to the degree to which these interventions targeted the poor and attempted to address gender disparities, their linkages with other actors and interest groups, and attention to sustainability.[11] The authors conclude with a plea for systematic impact evaluations to guide the design and modification of antipoverty programs.

[11] For example, the fisheries extension program in Mymensingh originally provided credit directly to farmers, and only used NGOs as a conduit for microfinance services much later. Both the improved vegetables program and the food-for-education program involved the private sector in its activities—using the private sector to set up marketing channels in the former, and relying on private grain dealers to distribute food in the latter.

5. KEY FINDINGS ON THE IMPACT OF THE INDIVIDUAL INTERVENTIONS

Kumar and Quisumbing's (2010a) paper the long-term impact of vegetable and polyculture fish technologies on a range of indicators of household and individual well-being in three sites. The authors take advantage of the roll-out of the technologies to compare early and late adopters of the technologies: early adopters are program members in villages where the disseminating institutions first introduced the technology, and late adopters from villages where technologies were introduced later, mostly owing to staffing constraints in the implementing institution that prevented simultaneous dissemination in all villages, as well as nonprogram members randomly selected from the general population.[12] As these technologies were introduced into these areas some two to four years before the 1996 survey, their estimates of the long-term impact focus on whether early adopters (at the time of the 1996 survey) gained more or less than late adopters (households who had adopted the technologies between 1996 and the 2006 resurvey) of the technologies. The results suggest that the characteristics of the technologies matter considerably to whether the impact of the intervention grows or declines over time. This is demonstrated most clearly by the comparison of the improved vegetable technology (disseminated by a local NGO in the area around Saturia) and the individual polyculture fish technologies introduced in the area around Mymensingh. Despite evidence of positive and significant short-term impacts on household outcomes, difference-in-difference estimates suggest improved vegetables had minimal effects at the household level in the long term, in part because of the diffusion of the technology to other households in the area and because later adopters may have learned from the experience of earlier adopters and become better cultivators of the new technologies.[13] In contrast, after 10 years early adopters of the polyculture fish technology experienced increases in total and food consumption that were significantly larger than those of late adopters in the sites with individual fishponds. This appears to be because the owners of the individual fishponds were able to capture the gains (quasi-rents) from the technology, which requires larger capital investments and had a longer pay-back period than for improved vegetables.[14]

When individual-level outcome variables (such as nutrient intakes and anthropometrics) are examined, a more nuanced picture emerges. Early adoption generally had beneficial effects on intake of calories, protein, iron, and vitamin A for all three interventions. However, stunting and wasting among children and the Body Mass Index (BMI) for adults have gender differentiated impacts. In the improved vegetable villages, among early adopters women's BMI increased while men's BMI decreased, while in both the individual and group fishpond villages no significant changes in BMI were recorded. It is possible that the targeting modality that emphasized women's empowerment, and the dissemination of vitamin A- and iron-rich vegetables that women consume, led to an improvement of women's nutritional status (but not men's) in this area. The proportion of stunted girls and thin (wasted) boys decreased substantially among early adopters relative to late adopters in the improved vegetable villages, but proportion of stunted girls increased among early adopting families compared to late adopting families in the individual and group fishponds villages. Kumar and Quisumbing (2010a) conclude that short- and long-term impacts of agricultural technologies are mediated by a number of factors, some of which are well known (for example, the divisibility and up-front cost of the technology), and others (for example, how NGOs target their members, and the intrahousehold distribution of food), which are less commonly recognized.

[12] By the time of the resurvey in 2006-2007, the technologies had diffused into all survey villages and had been adopted even by households who were not program members at baseline.

[13] Indeed, qualitative assessments reported in Hallman, Lewis, and Begum (2007) showed that women derived social benefits from sharing vegetables with their neighbors, who may have then adopted these varieties. It is quite possible that this sharing also involved the dissemination of technical knowledge, so later adopters would have learned from the experience (and mistakes) of earlier adopters.

[14] The dissemination of the polyculture fish technology through group fishponds in the Jessore area resulted in minimal effects on household expenditures in both the short and the long term. This is due to the costs and benefits of the technology having been diluted over a number of families.

The companion paper by Kumar and Quisumbing (2010b) takes this analysis a step further by examining the long-term impact of the same agricultural technologies on men's and women's asset accumulation. In addition to comparing asset accumulation by early and late adopters using nearest-neighbor matching, they also compare NGO members with non-NGO members, and the accumulation of assets by husbands and wives within the same household. While initial gender disparities in asset ownership are not eliminated, women's assets are found to increase more when technologies are disseminated through women's groups.[15] A comparison of the individual and group fishpond sites, where the same polyculture fish technology was used, finds dissemination through women's groups reduced gender asset inequality in the long term, while asset disparities increased between husbands and wives in the individual fishpond sites. These results suggest that social capital, as embodied in women's groups, not only serves as a substitute for physical assets in the short run, but can help to build up women's asset portfolios in the long run. These results also suggest that implementation modalities, in particular the types of groups that are active in an area, are an important determinant of the impact of new agricultural technologies on men's and women's asset accumulation. One must also recognize the possibility that implementation modalities are endogenous—program implementers choose the modality that is most likely to succeed in the specific location. Mymensingh is a more socially conservative site where NGOs are less common, while Jessore, being closer to the Indian border, is less so.

Baulch (2010) considers a different type of development intervention, the Primary Education Stipend (PES), a conditional cash transfer program introduced in July 2002 to encourage poor parents to send their children to school. Using nearest neighbor matching methods described above, the PES program is shown to have negligible impacts on a range of outcome variables at both the household and individual level between 2000 and 2006. This is consistent with the declining value of the PES (which has remained fixed in nominal terms since 2003) and earlier studies on the short-term impact of the PES on food consumption within the household. At the individual level, matching methods show that the PES had a statistically significant impact on grade progression but that it is paradoxically lower among PES beneficiaries than non-PES beneficiaries. The negative impact on grade progression is strongest among boys from poor households who, unlike girls, are ineligible to receive the stipends administered by a separate education transfer program at the secondary level. However, there are some signs of impact of the PES at the individual level: among children of primary-school age, boys were more likely to experience improvements in their BMIs and girls were more likely to improve their height-for-age Z-scores. Overall, however, the medium-term impacts of the PES are remarkably small for a program of its size. Baulch (2010) identifies poor-targeting of the PES and the declining value of the stipend as the main reasons for this lack of impact.

Finally, Davis (2010) explores the long-term effects of the interventions using a life-history approach that explores the causes of improvement and decline in people's lives. These methods allow Davis to examine the effects of microfinance programs, which are now so widespread in Bangladesh that it is impossible to maintain the treatment and comparison groups from the original 1994 study (Zeller et al. 2001). Using a "medium N" sample of 293 life history interviews with adult men and women (typically husbands and wives) nested within the larger household panel, 55 percent of the life-history interviews contained accounts of microfinance being used for income-generating activities at some point in the past. Microfinance contributed to at least one of the main causes of well-being improvement within the life trajectories of 18 percent of respondents. However, 37 percent of respondents had used microcredit to cope with crises or to maintain consumption, drawing attention to the need to provide alternative policies to supporting the vulnerable that do not result in long-term debt. The life history method was found to be less effective in evaluating the impact of the education transfer and agricultural technology interventions. While respondents often mentioned that they were receiving stipends, the

[15] The analysis in Quisumbing and Kumar (2011) compared changes in husbands' and wives' asset holdings across households as well as for husband-wife pairs within the same households. The second comparison also finds that women's assets increase when technologies are disseminated through women's groups, and are therefore robust to time invariant unobservables at the household level, which have time-invariant effects.

monetary value of these was small compared with the other causes of improvement or decline in people's lives. So while educational transfers were viewed positively in the life histories of 29 percent of respondents, these transfers were listed as a *main* cause of life improvement by only 8 percent of them. The life-history interviews also detected few long-term benefits from the agricultural technology interventions, which were referenced relatively sparsely in the life-history narratives. This sparseness highlights the difficulty in using non-focused life histories to evaluate an intervention with small, yet beneficial, impacts for participants. In addition, because the improved vegetable and fish polyculture technologies were "bundled" with microfinance as part of the overall package, it was often difficult for respondents to attribute an impact to them that was separate from microfinance.

6. COST-EFFECTIVENESS ANALYSIS

Because antipoverty programs involve the use of public resources, the cost-effectiveness of programs in reaching their development objectives is an enduring topic of interest of policymakers. By ranking programs according to their cost-effectiveness, policymakers should, in principle, be able to prioritize programs and choose those that deliver the biggest development impact for each unit of resources. However, this is not so clear-cut in practice. First, cost data may not be collected systematically and are often not available to researchers. Second, if programs have multiple objectives, it is possible that one program may do better at meeting one set of objectives, while another program meets a different set. The summary of our long-term impact results in Bangladesh already hints at possible trade-offs between household-level and intrahousehold-level impacts. We illustrate the dilemmas involved in using cost-effectiveness analysis to compare antipoverty programs below; details on the cost data used for this analysis are in Appendix A.

For each intervention site, we compute the cumulative cost per beneficiary household for the impact period in real terms.[16] The impact period is from 1996-2007 for the agriculture technology interventions and from 2000-2006 for the educational transfer intervention. An alternative way of constructing comparable cost-benefit figures would have been to annualize the cost-benefit stream, say, by dividing the aggregate numbers by the duration of program implementation. However, we were unable to do this because we do not have true baseline estimates for the agricultural technology sites. As mentioned in Appendix A, we only have cost data for the individual fishpond sites, and used data from a large national NGO as a proxy for the costs of the group-based agricultural technology programs. We also assumed that the costs of developing the technology were shouldered by the public agricultural research system, not the disseminating institutions, an assumption that we believe is justified because the vegetable technologies were developed by the World Vegetable Center and the fish technologies by the World Fish Center (see Kumar and Quisumbing [2010] and Hallman, Lewis, and Begum [2007] for details). Moreover, the implementing NGOs had been operating in the area (implementing other programs) before they started introducing the new technologies, so set-up costs for the group-based NGOs were not related to the agricultural technology interventions. However, we do not have estimates of the set-up costs related to the agricultural technologies. For example, while initial extension advice regarding the technologies was given by government and international agricultural research center extension agents, the NGOs (implementing agencies) did train additional extension agents at its own expense. While these costs should be included in our cost-effectiveness analysis, we did not have access to this data.

Rather, we use cost data (see Appendix A) that correspond to the impact periods of the evaluations. The per beneficiary household cost for each intervention during the impact period is presented in Table 6.1. To examine the benefit cost ratios, we extract the average treatment effect on the treated (ATT) on a number of outcomes from the impact papers (see Baulch [2010] and Kumar and Quisumbing [2010]). Since the impact estimates measure percentage impact over the impact period (1996-2007 for the agricultural technology interventions and 2000-2006 for educational transfers), we use these to generate the percentage increase per dollar spent over the entire impact period. These are shown in Table 6.2.

Table 6.1—Cost per beneficiary household for the impact period

	Education transfers	Individual fishponds	Improved vegetables	Group fishponds
Per beneficiary family cost in USD in real terms (cumulative over impact period)	110	75.85	73.15	73.15

Source: Author's compilation.

[16] We use the rural Consumer Price Index published by the Bangladesh Bureau of Statistics to deflate all nominal values to convert them into real terms reflecting 2007 prices.

Table 6.2—Percent increase per dollar spent

	Percent increase per dollar spent			
	Education transfers	Individual fishponds	Improved vegetables	Group fishponds
Adult equivalent monthly food expenditure	0.172*	0.336*	-0.312*	-0.066
Adult equivalent monthly nonfood expenditure	0.097	-0.063	-0.194	-0.029
Adult equivalent monthly household expenditure	0.146*	0.215*	-0.269*	-0.053
Value of total nonland assets excluding livestock	0.148	0.425	-0.746*	0.220
Value of livestock	-0.022	0.515	-1.789*	-1.671*
Value of total nonland assets	0.305*	0.338	-0.824*	-0.223
Total land area owned	-0.156	0.055	-0.403*	-0.112
Fraction of children with HAZ < -2	0.199*	0.349*	-0.062	-0.183
Fraction of boys with HAZ < -2	n.c.	-0.192	-0.388	-0.562*
Fraction of girls with HAZ < -2	n.c.	0.386*	-0.388*	0.599*
Fraction of children with ZBMI < -2	-0.084	0.116	0.046	-0.037
Fraction of boys with ZBMI < -2	n.c.	-0.290	-0.591*	-0.849*
Fraction of girls with ZBMI < -2	n.c.	-0.742	-0.148	-0.094

Source: Author's estimations.
Notes: * indicates that the average treatment effect is significant at 1, 5, or 10 percent; n.c. means not computed owing to small sample sizes. Percent increase per dollar spent is computed using impact estimates from Table 5 of Baulch (2010) and Tables 8 and 11 of Kumar and Quisumbing (2010a) and the cost per household estimates presented in Table 4.1 of this overview paper.

Note that because these outcome measures correspond to different outcomes (some monetary and others nonmonetary), the measures are not equivalent to one another. However, the cost-effectiveness of the different interventions in attaining a given outcome can be compared.

The estimates in Table 6.2 abstract from baseline means and therefore are difficult to compare. To account for this, we evaluate the impact at the baseline mean and then estimate the benefit cost ratios, presented in Table 6.3. For example, the baseline mean of per adult equivalent household expenditure in the educational transfers sites was 1,162 taka and the ATT was 16.1 percentage points. This translates into an increase in per adult equivalent expenditure of 187.2 taka attributable to the educational transfers intervention, which when converted into US dollars and divided by per beneficiary cost, gives the dollar increase in per adult equivalent food expenditure for every dollar spent as 0.02. For outcomes that are measured in percentages, evaluating at the mean is even more important. For example, if the fraction of children stunted (height-for-age Z-score less than minus 2) at baseline was 20 percent, then a 10-percentage-point drop in this fraction is equivalent to a 50-percent drop in stunting, whereas the drop would be 25 percent if the baseline mean was 40 percent.[17]

Figures 6.1 and 6.2 show the impact per dollar for monetary measures of well-being, measured at the household level, and measures of child nutritional status, respectively, over the 10-year period. Because we are analyzing reductions in malnutrition (stunting and thinness) in Figure 6.2, an improvement is represented by a reduction (negative impact) in the percentage in stunted or thin children, and will appear as a negative number. Only impacts that are statistically different from zero at the 10 percent level are presented in both figures.

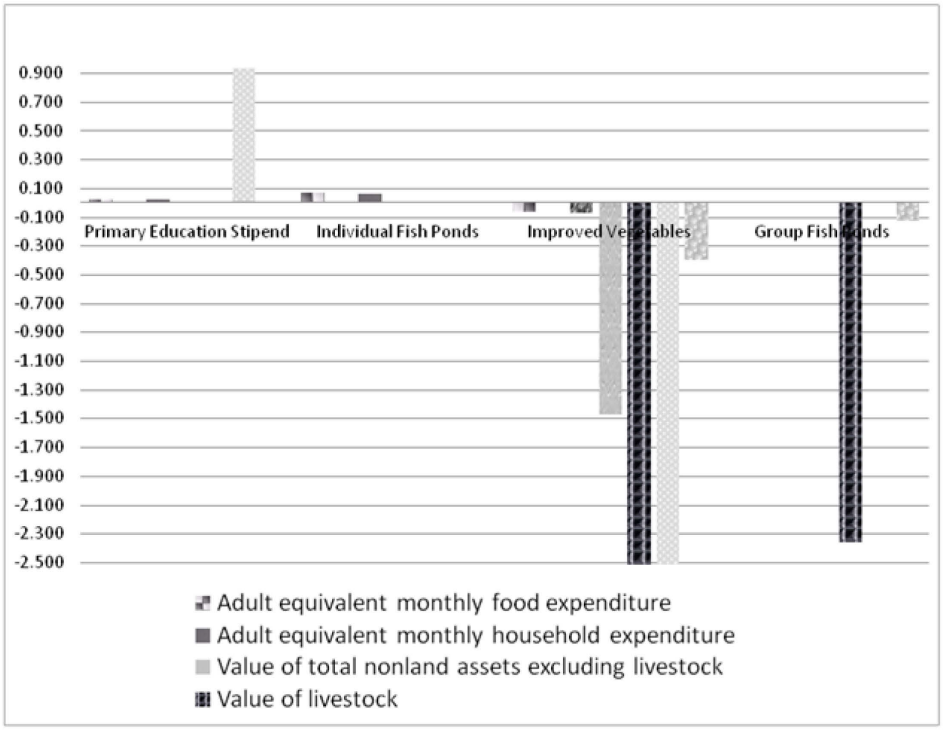
[17] A child is classified as stunted if his or her height-for-age Z-score is less than -2 using the new WHO growth charts.

Table 6.3—Impact relative to baseline mean per dollar spent

	Impact over mean outcomes per dollar spent			
	Education transfers	Individual fishponds	Improved vegetables	Group fishponds
Adult equivalent monthly food expenditure (dollar increase/dollar spent)	0.021*	0.066*	-0.061*	-0.015
Adult equivalent monthly nonfood expenditure (dollar increase/dollar spent)	0.005	-0.005	-0.016	-0.002
Adult equivalent monthly household expenditure(dollar increase/dollar spent)	0.025*	0.060*	-0.075*	-0.016
Value of total nonland assets excluding livestock(dollar increase/dollar spent)	0.287	1.090	-1.472*	0.415
Value of livestock (dollar increase/dollar spent)	-0.024	0.836	-2.562*	-2.361*
Value of total nonland assets (dollar increase/dollar spent)	0.932*	1.414	-2.806*	-0.735
Total land area owned (decimal land increase/dollar spent)	-0.003	0.117	-0.395*	-0.122
Fraction of children with HAZ < -2 (percent increase/dollar spent)	0.007*	0.913*	-0.126	-0.451
Fraction of boys with HAZ < -2 (percent increase/dollar spent)	n.c.	-0.516	-0.824	-1.434*
Fraction of girls with HAZ < -2 (percent increase/dollar spent)	n.c.	0.872*	-0.737*	1.500*
Fraction of children with ZBMI < -2 (percent increase/dollar spent)	-0.004	0.474	0.145	-0.145
Fraction of boys with ZBMI < -2 (percent increase/dollar spent)	n.c.	-1.065	-1.531*	-2.437*
Fraction of girls with ZBMI < -2 (percent increase/dollar spent)	n.c.	-3.630	-0.649	-0.567

Source: Author's estimations.
Notes: * indicates that the average treatment effect is significant at 1, 5 or 10 percent; n.c. means not computed because of small sample sizes.

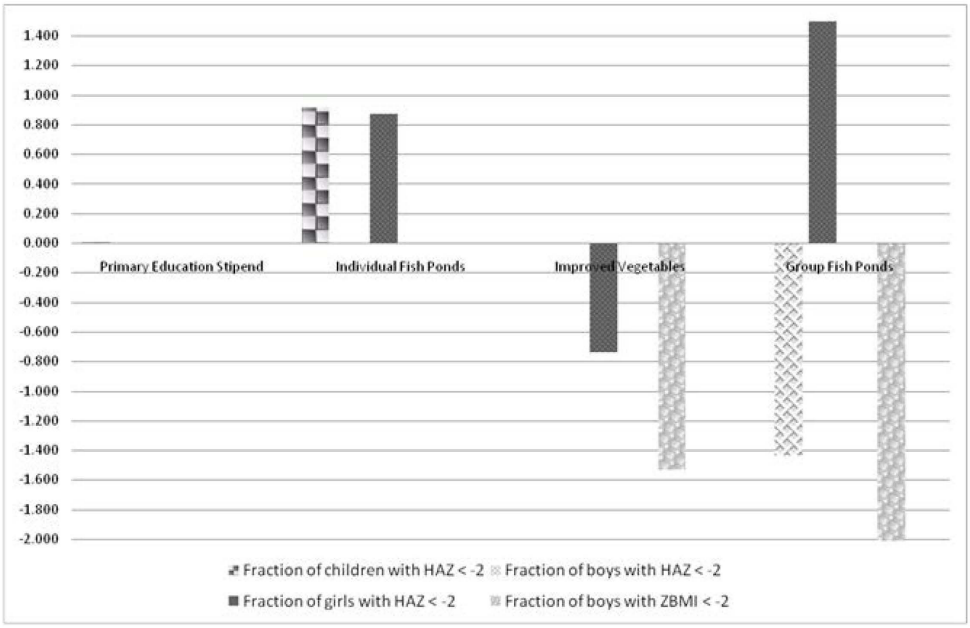
Figure 6.1—Impact on monetary measures of well-being per dollar spent



Source: Author's creation.

Figure 6.1 indicates that only the educational transfers program and the individual fishponds program had significant and positive impacts on monetary measures of well-being, although these were small in magnitude. The individual fishponds program increased monthly expenditures per capita by $0.06 for each dollar over the mean amount spent, and the educational transfers program by $0.025 for each dollar over the mean amount spent. Interestingly, the educational transfers program had an almost one-for-one impact on nonland assets, with nonland assets increasing by $0.90 per dollar transferred. Early adopters in the group fishponds and improved vegetables programs had lower monetary measures of well-being—both expenditures and assets—compared to late adopters of these agricultural technologies. However, the picture changes when we examine individual level impacts (Figure 6.2).The biggest reductions in stunting and thinness rates occur among early adopters of improved vegetables technologies, while stunting, particularly of girls, increases in the individual fishpond sites.

Figure 6.2—Impact on anthropometric measures per dollar spent



Source: Author's creation.

7. IMPLICATIONS FOR PROGRAMS AND POLICY

The results of the studies summarized in this chapter indicate that both government and civil society interventions have the potential to reduce poverty in Bangladesh, but that their household-level and individual impacts differ in the short- and the long-term because of differences in the time path of net benefits from the interventions and spillover effects. Because programs are rarely evaluated in terms of both short- and long-term impact, policymakers may arrive at the wrong conclusions about the effectiveness of these interventions over the long term, which has implications for the choice of programs that will be supported by public resources. Divergence between short-term and long-term impacts may be especially important in interventions that seek to bring about behavioral change, where spillover effects and learning from others may be significant. We hypothesize that as many development interventions have spillover effects that may not be anticipated by program designers or evaluators, much can be gained from looking at both monetary and nonmonetary measures of well-being and employing a mixed methods approach.

Targeting mechanisms and implementation modalities have important implications for the ability of interventions to reach the poor. In the case of the PES, given scarce government resources, better targeting would make it possible to give larger transfers to fewer beneficiaries. In the case of the use of NGOs and groups for service delivery, it is important not to idealize NGOs. There is substantial heterogeneity in the NGO sector, and some NGOs are more effective than others in reaching the poor and very poor. While the present study has found that membership in NGOs and other organizations is pro-poor, Hallman, Lewis, and Begum (2007) find that some very poor households are excluded from NGO membership because they are asset-poor. Lack of social connections contributes to isolation for the very poor, while lack of education can make poor people unconfident about joining an organization. NGOs in general are better at overcoming these barriers (Hallman, Lewis, and Begum 2007, 121-122), but reaching the very poor may require using different approaches than the group-based approach. Indeed, one of Bangladesh's largest NGOs, the Bangladesh Rural Advancement Committee (BRAC), uses an individual rather than a group approach in its Targeting the Ultra Poor Program (Hulme and Moore 2010).

Because of the particular dynamics of gender relations in Bangladesh, group-based approaches that involve women seem to have had favorable impacts on individual nutritional status, particularly of children, even if their impacts on monetary indicators appear low.[18] Evaluating the impact of antipoverty programs must therefore go beyond the household as the unit of analysis, to examine intrahousehold impacts. Impact indicators should also go beyond incomes and expenditures, because of the importance of intergenerational effects of child and maternal nutrition.[19]

Finally, the mixed results of comparing impacts on monetary versus nonmonetary measures of well-being illustrate the dilemmas of using cost-effectiveness to rank antipoverty interventions. Some interventions (such as the Primary Education Stipend and individual fishponds) do well in terms of increasing per capita expenditures or assets, but do poorly in terms of improving nutritional status. While one option would be to assess the extent to which interventions meet their stated development objectives, it is often the case that antipoverty programs have a range of objectives, and spillover effects are present. Moreover, poverty and well-being are multidimensional concepts, and it is usually difficult to adopt a single measure of impact. Assessing the long-term impact and cost-effectiveness of competing development interventions will therefore involve trade-offs between objectives, with all the difficulties of welfare ranking and weighting that such trade-offs imply.

[18] It is impossible to evaluate this against a true counterfactual of targeting nutritional advice to men, because men are rarely (if ever) targeted for nutritional advice in Bangladesh. However, it is probably safe to say that untargeted agricultural programs, such as the individual fishponds program, end up targeting men by default, owing to the perception that women are not involved in the agricultural sector in Bangladesh. Indeed, even among the 30 percent of individual fishpond farmers who were mandated by donors to be women, it was not rare for husbands to do most of the fish cultivation and sale, with the wife only feeding the fish.

[19] Researchers are only beginning to quantify the long-term economic impacts of these investments in early childhood health and nutrition (Hoddinott et al. 2008).

APPENDIX A: COST ESTIMATES FOR COST-EFFECTIVENESS ANALYSIS

Saturia Vegetables and Jessore Group Fishpond Sites

Despite repeated attempts to obtain cost data from the implementing NGOs, we were unable to obtain any cost or administrative data. Instead, we use cost data for 1992-2007 provided by the Association for Social Advancement (ASA), a large national NGO that provides microfinance services and other services (technology package, health education, and so forth) to its members.[20] We use the data on total expenses and total number of clients each year to generate a per client cost for each year. Since the interventions in Saturia and Jessore did not require costs to be incurred in the same proportion each year after the first few set-up years were over, we assume that after five years of set-up, the cost per client can be scaled down by half. We then calculate the cumulative cost per client from 1997-2007 (the period that corresponds with our impact assessment) and use this cost estimate in the benefit-cost analysis. As shown in Table 6.1, the estimated cost per beneficiary household was US$73.15 for improved vegetables and US$73.15 for group fishponds. This is assumed equal because we do not have separate cost data for each of these interventions, and we are only analyzing the cost of service delivery, not the cost of technology development. We assume that these technologies have already been developed or are available from the agricultural research and development system, so we are, in effect, evaluating the cost of alternative approaches of technology dissemination.[21]

Mymensingh Individual Fishpond Sites

This intervention (individual or household fishponds) has by far the best cost data, because the Mymensingh Aquaculture Extension Program was required to keep detailed financial records by its donor, the Danish International Development Agency (DANIDA).[22] We have detailed data on expenditure on various budget categories such as construction, vehicles, manpower, credit, furniture and equipment, at different stages in the project. The largest cost category is labor costs (63 percent), followed by operational costs (19 percent), investment costs (10 percent), and credit (8 percent). In the current analysis, we use the total cost of the project (from inception to consolidation) and divide it by the total number of beneficiaries to estimate per beneficiary cost of the project. The entire project cost was 456,958,000 Bangladeshi taka, which is approximately equal to US$8.2 million. The project reached about 108,000 households, yielding a per household cost estimate of US$75.85 for the entire project.

Education Transfers Program

For this intervention we have the total cost of operating the FFE and PES programs as well as the total number of beneficiary students and households. These data were provided by the Ministry of Primary and Mass Education (MPME) in Bangladesh, but exclude the administrative costs of educational transfers, which are met from the MPME's establishment budget. Using this data, we can generate the cost per beneficiary student and the cost per beneficiary household. Since the impact estimates are at the household level, we use per household cost estimates in the benefit per cost ratios. As shown in Table 6.1, the total cost per beneficiary for the 2000-2006 period was US$110.

For both the agricultural technology and the educational transfers evaluations, unions or villages were selected to include those with and without the intervention. For the agricultural technologies evaluation, roll-out of the program (owing to implementation and staffing constraints of the implementing agency) meant that not all villages in the program area received the technology at the same time. In

[20] The ASA cost structure will not be strictly comparable to the GKT and BS cost structures because the latter are small- to medium-sized NGOs with lower administrative costs. Further information on ASA, which was founded in 1978 and has become a major provider of microcredit, is available at http://www.asabd.org/html/welasa.html.

[21] This is not an unreasonable assumption at this stage of analysis, although both public and private resources were used to develop these technologies at the World Vegetable Center and the World Fish Center.

[22] We thank John Rand for making these cost data available to us.

designing the sample, an equal number of households were interviewed in villages that had and had not benefited from the dissemination of three different technologies (improved vegetables, group fishponds, and individual fishponds) at the time of the initial survey. Both program and nonprogram were surveyed in these villages. For the educational transfers' evaluation, twice as many households were selected from food-for-education (FFE) than from non-FFE unions (subdistricts). In both studies, households were then randomly selected from the relevant sampling frames and strata.

APPENDIX B: STEP-BY-STEP DESCRIPTION OF THE MATCHING METHODS

Because the Stata user written command, *nnmatch*, is not as commonly used as *psmatch*, we have prepared a short technical appendix for this overview paper. This appendix draws heavily from the help.ado file for *nnmatch* in STATA (Abadie et al. 2004).

Step 1: Estimating the Propensity Score and Establish the Balancing Property

In Step 1, we estimate the propensity score of being treated by running the *pscore* command in STATA. This provides the propensity score for each observation and also gives an indication of the overlap between the propensities across the treatment and comparison groups. This command also tests whether the balancing property is satisfied. We proceed to Step 2 if the balancing property is satisfied. If it is not, then we modify the set of covariates used in estimating the propensity score and repeat Step 1 until the balancing property is satisfied.

Step 2: Trim the Sample

Step 2 entails trimming the sample on the basis of the propensity score in order to ensure common support for the treatment observations in the comparison group. In particular, we drop 2 percent of treatment observations with the lowest density of comparison observations with similar propensity scores—where we trim 1 percent each from the bottom and the top. This trimming is done to ensure that the ATT can be estimated with greater accuracy. Once the trimming is done, we save the dataset as a separate file called "testfile" to be used in further analysis.

Step 3: Estimate the Propensity Score Again on the Trimmed Sample and Ensure that the Balancing Property is Satisfied

In Step 3, we make sure that the trimmed sample also satisfies the balancing property. We reestimate the propensity score based on the trimmed sample using the command *pscore* in STATA and make sure that the balancing property is satisfied. If the balancing property is satisfied, we move on to Step 4. In case the balancing property is not satisfied, we go back to Step 1 and iterate until the balancing property is satisfied in Step 3.

Step 4: Estimate Average Treatment Effect on the Treated (ATT) using Nearest Neighbor Matching

In the last step, we use the trimmed sample saved in "testfile" to estimate the average treatment effects of the treated (ATTs) over a range of outcome variables. We estimate the sample ATT for these variables using the *nnmatch* user-written command in STATA on the common support sample allowing for five matches. We use the default weighting matrix, which is the diagonal matrix of inverse sample standard errors for the covariates. We use option *biasadj*, which specifies that the bias-corrected matching estimator be used, as the simple matching estimator might still be biased if matching is not exact. This option runs a regression that adjusts the results, using the covariates used in the matching. We also use the *robust(#)* option, which estimates heteroscedasticity-consistent standard errors (White 1980). This is done by doing a second round of matching in which outcomes among treatment observations are matched to compare variability in outcomes for observations with similar covariates.

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