

# IMPACT EVALUATION OF THE DFID PROGRAMME TO ACCELERATE IMPROVED NUTRITION FOR THE EXTREME POOR IN BANGLADESH

## BASELINE REPORT: EXECUTIVE SUMMARY

### MQSUN REPORT

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Authors:

Shalini Roy, Inka Barnett, Ben Baxter, Jessica Gordon, John Hoddinott,  
Naureen Karachiwalla, Firdousi Naher, Jean-Pierre Tranchant

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## ABOUT MQSUN

MQSUN aims to provide the Department for International Development (DFID) with technical services to improve the quality of nutrition-specific and nutrition-sensitive programmes. The project is resourced by a consortium of six leading non-state organisations working on nutrition. The consortium is led by PATH.

The group is committed to:

- Expanding the evidence base on the causes of undernutrition
- Enhancing skills and capacity to support scaling up of nutrition-specific and nutrition-sensitive programmes
- Providing the best guidance available to support programme design, implementation, monitoring and evaluation
- Increasing innovation in nutrition programmes
- Knowledge-sharing to ensure lessons are learnt across DFID and beyond.

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### MQSUN PARTNERS ARE:

Aga Khan University  
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 ICF International  
 Institute for Development Studies  
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 PATH

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### CONTACT

PATH, 455 Massachusetts Avenue NW, Suite 1000  
 Washington, DC 20001 USA  
 Tel: (202) 822-0033  
 Fax: (202) 457-1466

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### ABOUT THIS PUBLICATION

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# EXECUTIVE SUMMARY

## Introduction

The DFID Programme to Accelerate Improved Nutrition for the Extreme Poor in Bangladesh aims to improve nutrition outcomes for young children, pregnant and lactating mothers, and adolescent girls. Its approach is to integrate direct nutrition interventions into the livelihood support currently provided to extremely poor households in Bangladesh through three existing programmes: the Chars Livelihoods Programme (CLP), the Shiree Economic Empowerment of the Poorest Programme (Shiree or EEP, within which we focus on the Concern subproject), and the Urban Partnership for Poverty Reduction Programme (UPPR).

In order to rigorously and independently assess the impacts of these integrated nutrition and livelihoods programmes, DFID has collaborated with research partners and implementation partners to undertake a mixed methods impact evaluation, entitled “Impact Evaluation of the DFID Programme to Accelerate Improved Nutrition for the Extreme Poor in Bangladesh.” The evaluation team includes IDS (the lead organisation), IFPRI, ITAD, CNRS, and BRAC University. The evaluation uses mixed quantitative and qualitative methods within a strong theory-based design to assess the impacts of the integrated programmes on nutritional status.

As discussed in Section 1.1, the evaluation specifically addresses the following three objectives:

1. To estimate the quantitative impact of the combined direct nutrition and livelihoods interventions in three different DFID programmes on nutritional status of children under two; and to compare this with the impact of the existing livelihoods interventions;
2. To explain this impact, drawing on qualitative and quantitative evidence regarding programme-specific and wider societal/contextual factors that could affect programme outcomes; and
3. To assess the cost effectiveness (benefit received for cost incurred) of integrating direct nutrition components into the livelihoods interventions of the three existing programmes.

The quantitative impact component involves a baseline survey (conducted in September-November 2013) and an endline survey (to be conducted in September-November 2015). The exploratory/explanatory component includes a qualitative subcomponent (for which the first phase of fieldwork has been ongoing since February 2014), as well as a process evaluation subcomponent (ongoing since July 2014, final results not yet available). The cost effectiveness component began in August 2014 and will be completed in early 2016 following the quantitative endline survey completion. Given these timelines, this baseline report will describe the quantitative component and qualitative subcomponent of the exploratory/explanatory component, and will focus on presenting and analysing the quantitative baseline data (the fieldwork that is already completed).

The primary users of the evaluation are DFID, its programme implementing partners at all levels, and the Government of Bangladesh. DFID also expects the findings to be published and disseminated more widely to benefit other stakeholders in the Bangladesh nutrition and development community, as well as global policy makers, practitioners, and researchers concerned with nutrition programming.

## Overview of quantitative component

The quantitative component aims to provide numerical estimates of the programmes' causal impacts on beneficiaries' nutrition outcomes. IFPRI and IDS lead this component, with in-country support from survey firm DATA on conducting the baseline and endline surveys.

The evaluation design of the quantitative component derives from the notion of 'impact.' In a quantitative evaluation, 'impact' refers to the difference in beneficiary households' observed outcomes after receiving a set of interventions, relative to those same households' counterfactual outcomes in the same time period had they not received the interventions. The three key research questions regarding programme impact that will be addressed are:

1. What is the impact on nutrition outcomes of receiving a combination of livelihoods and direct nutrition interventions (denoting this scenario (L+N)), relative to receiving a livelihoods intervention only (denoting this scenario (L))?
2. What is the impact on nutrition outcomes of receiving a combination of livelihoods and direct nutrition interventions (L+N), relative to receiving no intervention (denoting this scenario (C) for control)?
3. What is the impact on nutrition outcomes of receiving a livelihoods intervention only (L), relative to receiving no intervention (C)?

As detailed in Section 1.2.1, in order to construct a proxy for the (L+N) households in the counterfactual (L) scenario, randomisation is used. Among the households that already receive the livelihoods intervention at baseline, half are randomly assigned to additionally receive the nutrition intervention after the baseline (denoted the (L+N) group). The remaining half continue to receive only the livelihoods intervention (denoting the (L) group). Randomisation is conducted at the level of primary sampling units (PSUs) that cover an entire locality, rather than at the level of individual households. The randomisation makes it very likely that characteristics of the (L) and (L+N) groups will on average be similar at baseline. (L) is then a valid proxy for (L+N), and average differences between the groups at endline can be interpreted as impacts caused only by the addition of the nutrition component rather than pre-existing differences.

In order to construct a proxy for the (L+N) households in the counterfactual (C) scenario of no intervention, non-randomised approaches are used. Since none of the original livelihoods interventions was rolled out following a randomised control trial design, there is no obvious set of comparable non-beneficiaries to serve as the counterfactual. Because a control group is nonetheless required to assess the absolute benefits of either (L) or (L + N) interventions, attempts were made to construct the best possible control group out of non-randomly selected non-beneficiaries. It is important to emphasise that a non-random control group is not expected to be on average identical to beneficiary households. In the baseline survey, the objective was simply to sample a group of non-beneficiaries as similar as possible to the beneficiaries except for receipt of the intervention.

The main body of this baseline report focuses on analysing average differences between beneficiary groups and the control group as a diagnostic. Following completion of the main baseline report but prior to the endline survey, additional econometric analysis using propensity score matching was conducted to explore potential for finding subsets of control households that were very similar pre-programme to the beneficiary households and could potentially be used to estimate absolute programme impact. This exploratory analysis for each programme is summarized in Appendix G.

As described in Section 1.2.1.2, impacts will be estimated using both baseline and endline data following the completion of the endline survey. The estimation will use the ‘double-difference’ approach and/or the Analysis of Covariance (ANCOVA) specification. It is worth emphasising that the estimated impacts will be based on the specific programme contexts in this study. In particular, as in all evaluations, the findings will be shaped by the study environment, and although the design will aim to ensure internal validity, it cannot guarantee external validity such that results will necessarily generalise to very different contexts. Nonetheless, attempts will be made to draw broader lessons from the findings by comparing patterns across the three distinct programme settings and triangulating using the exploratory/explanatory analysis.

The quantitative sample design is described in Sections 1.2.2. The sample is designed around the ability to detect impacts in key outcomes of interest – in particular, 0.25 standard deviation changes in height-for-age Z-scores (HAZ scores) of children aged 0-24 months. Sample size calculations indicate that a minimum of 70 localities must be sampled for each intervention arm ((L+N), (L), (C)) for each programme (CLP, Shiree/Concern, UPPR), with about 18 children aged 0-24 months sampled per locality. Therefore the design implies 1,260 children per intervention arm per programme, for an overall total of 11,340 children aged 0-24 months.

As described in Section 1.2.3, the quantitative baseline survey was fielded from September-November 2013. Because all three intervention arms were interviewed at the same time of year, seasonal factors are not expected to bias comparisons across the ((L+N), (L), and (C) groups within a programme. The endline survey, designed as a repeated cross-section, is scheduled to be fielded 24 months after the baseline survey, from September-November 2015. The key instrument of the quantitative component is a questionnaire that collects information on indicators along the causal chain for nutrition outcomes. In particular, it captures indicators that allow tracing out that, without improvement in ‘intermediate outcomes,’ it is highly unlikely that improvement in ‘final outcomes’ will be observed. Accordingly, it collects direct measures of anthropometry, but also elicits information on the following: household demographic characteristics; assets; savings and loans; livelihoods; food consumption; maternal knowledge; attitudes and practice regarding care behaviours; infant and young child feeding practices; measures of health status and recent illness; women’s status; and retrospective information on some of these domains. Logistical details of the fieldwork and resulting datasets are described in Section 1.2.3.

### Overview of explanatory/exploratory component

The exploratory/explanatory component of the evaluation consists of two complementary and interlinked subcomponents: a process evaluation and qualitative evaluation.

The process evaluation aims to identify the critical processes in the implementation of the programme’s strategy and to assess whether these processes were implemented as planned. Furthermore, the process

evaluation will map the quality of programme delivery to more proximate outcomes identified in the quantitative survey and use this to explain the impacts detected or not. ITAD leads this component in partnership with Centre for Natural Resources Studies (CRNS), Bangladesh. The process evaluation will start in July 2014 and will draw on the findings from the quantitative baseline and the first data collection in the qualitative subcomponent of the exploratory/explanatory component.

The qualitative evaluation component aims to explain how and why the combination of livelihood and nutrition interventions may be effective in triggering behaviour change and improving child nutrition within the context of the three programmes. The sub-aims of the qualitative investigation are:

1. To investigate interactions between societal, community, family and programme structures and how these might influence intervention uptake and behaviour change;
2. To identify contextual factors that can enhance or hinder the programme uptake. This will include an in-depth examination and testing of the programme assumptions and causal chain processes (described in the Theory of Change) within the context of the study communities.

IDS leads this component in collaboration with BRAC Institute of Governance and Development (BIGD, formerly BRAC Development Institute) at BRAC University. Two qualitative, longitudinal data collection processes are being carried out as part of this evaluation. The first phase of qualitative data collection began in February 2014 (delayed due to political instability from its originally scheduled start in November 2013) and was completed in May 2014. Follow up field visits in some selected sites is ongoing, with the final second phase of fieldwork expected to be carried out between September-November 2015 to coincide with the quantitative endline survey.

As described in Section 1.3.2.1, sites were selected for the qualitative component using a multi-site case study approach, with purposefully selected community focus clusters. A subsample of the quantitative study communities were selected to include (L) and (L+N) intervention groups in the three programme areas, as well as control sites for each programme. In each of the selected community clusters, study participants were selected purposefully using stratified purposeful sampling to illustrate characteristics of different relevant subgroups and to allow comprehensive understanding of the programme in different settings. First, the research teams met with the local programme implementers and described the purpose and aims of the evaluation. The aim of the meetings was to understand the current status of the programme implementation and in particular the progress of different intervention arms. Second, the research teams conducted informal walks (so called transect walks) through the community to facilitate building of rapport and identification of key informants who could provide background context on the locality. Third, enlisting the help of the key informants and programme implementers, participants for focus group discussions, in-depth interviews, observations and life histories were identified. The aim was to collect information from a variety of interviewees with different experiences, perceptions, and points of view. Interviewing different household members was important for obtaining insight into gender roles and intrahousehold decisionmaking in relation to child nutrition and health.

Section 1.3.2.2 details the data collection tools used to obtain qualitative data from different sources. The development of the topic guides for the in-depth interviews and focus group discussions was informed by preliminary findings of the quantitative survey analysis and review of existing literature.

Major topics areas covered by the qualitative data collection tools include the following:

1. Social, economic, institutional and political context of the community
2. Local practices, resources, customs in regards to health, hygiene, nutrition and care of children, pregnant and lactation mothers, adolescent girls
3. For (L) and (L+N) sites: Perceived impact of the livelihood intervention
4. For (L+N) sites: Perceived synergies and disconnects between the nutrition and livelihood interventions in the communities
5. For (L+N) sites: Micro-dynamics of the nutrition intervention at the community level and how beneficiaries perceive/experience the intervention

Preliminary analysis of the qualitative data will be conducted by the BRAC field teams in collaboration with IDS as soon as the data become available. Following an ‘observe, think, test, revise’ (OTTR)-sequence, emerging results will be discussed in the team during several debriefing meetings in Dhaka and then inform the collection of further data and in some cases the inclusion of new interviewees. In addition to the concurrent analysis, a content analysis approach will be employed for the analysis of the textual data after completion of the qualitative data collection. The content analysis will facilitate the unpacking of the mechanism-based causality being triggered in specific contexts and, in particular, behavioural change among beneficiaries of the nutrition intervention.

### Overview of mixed method approach

Section 1.4 discusses in detail the sequential mixed method approach in which the different quantitative, exploratory/explanatory, and cost effectiveness components are integrated across the stages of the evaluation. As part of this iterative process, findings from the quantitative baseline survey and first round of qualitative data collection were presented and compared at a joint partner workshop in Dhaka in June 2014 in order to establish the context in which the livelihood and nutrition interventions are embedded and against which the effectiveness of the interventions will be assessed, and to help shape the design of remaining quantitative and qualitative fieldwork activities. Key findings from this analysis are presented in Appendix F<sup>1</sup>.

Synergies are highlighted between the quantitative and exploratory/explanatory components (the two components where activities have already begun).

#### *Quantitative baseline analysis*

Analysis of the quantitative baseline data in Sections 2 through 4 serves two key purposes as a foundation for the eventual impact estimation. The first is to explore how well the baseline samples align with the

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<sup>1</sup> Appendix F has been prepared as a separate and standalone product originally planned to synthesise and report after the baseline quantitative reporting. The version annexed here is expected to undergo further revisions before being published as an output of this project and is included here for reference as a work in progress.

specifications of the evaluation design described. Sections 2 through 4 empirically assess the similarity of the groups sampled for (L), (L+N), and (C) groups in the quantitative baseline survey for each of the programmes. If the randomisation was successful, the baseline differences in each outcome indicator between the (L) and (L+N) groups should be small and statistically insignificant. As described above, it is not expected that the full (C) group will be as similar to (L) or (L+N) as the latter two groups are to each other. However, the analysis of average differences in this report is informative in that it provides preliminary assessment on the feasibility of finding a subset of the (C) group that can serve as a valid counterfactual for the (L) and (L+N) groups in each programme.

Following completion of the main body of this report but prior to the endline survey, additional econometric analysis was conducted using propensity score matching to explore the overlap in observable characteristics between the (L) and (L+N) groups and subsets of the (C) group in each programme. This exploratory analysis is summarized in Appendix G.

The second key purpose of the quantitative baseline analysis is to provide context for the impact estimates, by describing the situation of each programme's sample prior to the introduction of direct nutrition interventions. It is important to reiterate that these statistics are representative of the population being studied (beneficiary households with at least one child aged 0-24 months located in each programme area that is included in the study; similar non-beneficiary households with at least one child aged 0-24 months). Therefore they should not be expected to match statistics on national averages or even on averages of all programme beneficiaries irrespective of household composition.

Detailed quantitative baseline findings are presented by programme in Sections 2 through 4. A summary of these findings over all three programmes is given in Section 5, along with a brief synopsis of implications from the exploratory analysis described in Appendix G.

## Summary of quantitative baseline findings regarding the evaluation design

### *Comparability of (L) and (L+N)*

As described in Section 5, the analysis in Sections 2 through 4 shows that the randomisation is successful in statistically balancing nearly all indicators across all three programmes between the (L) and (L+N) arms. In the isolated cases where statistically significant average differences in indicators are found between (L) and (L+N), the magnitudes of differences tend to be quite small. It is worth noting that given the large numbers of indicators over which tests of statistical significance are conducted in Sections 2 through 4, it is expected that some statistically significant differences may appear by chance due to multiple testing. Because this baseline report's purpose in analysing statistically significant differences in baseline indicators is illustrative, to broadly assess patterns of similarity or difference rather than to estimate individual impacts, no formal tests are conducted in this report to adjust for multiple testing. Adjustments for multiple testing (such as use of Bonferroni corrections or tests of joint significance) will be made during the estimation of quantitative impacts to be presented in the final report. Moreover, even the small baseline differences found here between (L) and (L+N) can be controlled for in the eventual impact analysis.



The pattern of similarity across (L) and (L+N) includes receipt of other social programmes, as shown in Sections 2.3.2, 3.3.2, and 4.3.2. As would be expected in Bangladesh, a country with considerable NGO presence and government support, both (L) and (L+N) beneficiaries in all three programmes report receiving a number of other social programmes in addition to the livelihoods interventions that are the focus of this evaluation. However, because coverage of these other programmes tends to be well balanced across (L) and (L+N) groups, any average effects from exposure to their benefits should affect the two groups similarly; the small baseline differences between (L) and (L+N) in coverage of certain social programmes can moreover be controlled for in the eventual impact analysis. Therefore, despite there being several other social programmes operating in the (L) and (L+N) areas, their presence does not complicate the *comparison* of (L) and (L+N), and therefore does not complicate attribution in estimating the relative impact of adding the nutrition interventions to the livelihoods interventions.

Overall, the analysis of the quantitative baseline data therefore gives confidence that any statistically significant differences between the (L+N) and (L) groups later found at endline can be causally attributed to the additional nutrition component, rather than any pre-existing differences between the two groups. In addition to there being good statistical balance on nearly all baseline characteristics, the eventual impact estimation will control for any baseline difference in each outcome of interest through the double-difference or ANCOVA specification, and can further control for any small statistically significant baseline differences that are found in other characteristics.

For CLP, these additional baseline covariates could potentially include the following, in which statistically significant but typically small baseline differences were found between (L) and (L+N): household head's education; any household member's loss of employment in preceding 5 years; value of livestock assets received by CLP; share of adolescent girls who consumed meat in the preceding 7 days; decisionmaker on whether women work, how to spend money on education, how to spend money on clothing; duration of receiving iron supplements conditional on receiving them.

For Shiree Concern, these additional baseline covariates could potentially include the following, in which statistically significant but typically small baseline differences were found between (L) and (L+N): women's occupation; death of a household member in preceding 5 years; receipt of financial training from Shiree; receipt of dairy cows from Shiree; receipt of the external Vulnerable Group Feeding safety net programme; ownership of productive assets such as a hammer and mason's equipment; dimensions of dietary diversity for households, mothers, and adolescent girls; weight for age among children age 0-5 years; decisionmaker on whether women work, how to spend money on clothing, whether to take a loan; reason for women not working; control over money to buy food at the market; reasons for not using birth control; frequency of husbands' threats of taking another wife; women's ability to do simple addition; women's receipt of antenatal care; index child's birth attended by a friend or neighbour; adolescent girls' knowledge of when a baby should start getting breast milk and what seasoning is fortified with iodine.

For UPPR, these additional baseline covariates could potentially include the following, in which statistically significant but typically small baseline differences were found between (L) and (L+N): education of the index child's mother/caregiver; occupation of the household head; household access to a sanitary latrine; access to the Maternity Allowance Programme for Poor Lactating Mothers; ownership of consumer durables such as a trunk/suitcase; ownership of productive assets such as hammers; some dimensions of adolescent girls' dietary diversity; whether a child is stunted; whether a child is severely

underweight; decisionmaker on spending the money women earn, spending money on food, spending money on housing, spending money on education, spending money on clothing, taking a loan, and spending money from the loan; receipt of advice on pre- or post-lacteals; mothers' knowledge of what seasoning is fortified with iodine.

To summarize, although the baseline balance between (L+N) and (L) is likely strongest for UPPR, followed by CLP and then by Shiree, the evaluation design appears strong across all three programmes, with respect to identifying relative impacts of the combined livelihoods and nutrition intervention, over and above the livelihoods intervention only.

#### *Comparability of (C) with (L) and (L+N)*

As also described, although the full (C) group sampled for each programme is not expected to statistically balance with the (L) and (L+N) groups, the smaller the average differences, the more likely it is that a comparable subset can be found. Results in the main report indicate that the average similarity of the control groups to the beneficiary groups differs by programme. In particular, the preliminary analyses of average baseline differences in Sections 2 through 4 suggest that UPPR's control group might give the strongest basis to estimate absolute differences, Shiree/Concern's may fall somewhere in between, and CLP's remaining control group (excluding Cohort 2.6) is likely to be the weakest. This pattern includes exposure to other social programmes, as shown in Sections 2.3.2., 3.3.2, and 4.3.2. In all three programme samples, receipt of other social programmes is well balanced between the (L) and (L+N) groups, but different to varying degrees in the (C) group. In particular, in the UPPR and Shiree/Concern (C) groups, there is higher or lower coverage of certain social programmes than among the (L) and (L+N) groups, although these social programmes are accessed to some degree by all three intervention arms. In these cases, the observation indicates that it will be important to account for differential exposure between the control and beneficiary groups when constructing the counterfactual.

The additional exploratory analysis in Appendix G supports a similar pattern of findings. For CLP – where only riverine chars appear in the treatment group and only mainland chars remain in the control group after exclusion of Cohort 2.6 – the treatment and control groups exhibit almost literally no overlap in propensity scores based on observables. This lack of overlap reflects that observable characteristics are very different for CLP treated and control households, showing quite conclusively that the control group without Cohort 2.6 cannot serve as a proxy for the treatment group. For Shiree – where the programme had nearly blanket coverage of areas meeting its eligibility criteria – the distributions of propensity scores for the treatment and control groups exhibit a small amount of overlap, but over a very small proportion of the sample. The implication is that the Shiree control group is not an ideal proxy for the treatment group; although it might be possible to find a very small subset of control households to serve as a proxy for a very small subset of the treatment group, in practice these matched samples with common support in observables might be too small for meaningful impact estimation. For UPPR – which covers only a subset of the many urban slums in Bangladesh – the distributions of propensity scores for the treatment and control groups exhibit a considerable amount of overlap, over a small but potentially meaningful proportion of the sample. This overlap indicates that there is reasonable potential for a subset of the UPPR control group to serve as a proxy for a subset of the treatment group; although the matched

samples with common support are likely considerably smaller than the full sample, they might be sufficient for impact estimation on some outcomes.

These findings are largely as expected, given the discussion in Section 1.3 on scope for identifying non-intervention areas similar to intervention areas for each programme. However, it is important to note that factors other than the matching analysis will also play a role in the final determination of which programmes' control groups should be included in the impact evaluation. Although for CLP the analysis makes quite clear that the control group without Cohort 2.6 is not comparable and should not be included, for Shiree Concern and UPPR the value of including the control group depends on the size of impact that could reasonably be expected from the respective programme. The smaller we expect the impact to be, the larger we would need the overlapping control group subset to be in order to statistically detect that impact. Therefore, relevant factors for this decision in Shiree Concern and UPPR would include the evolving quality of implementation in each programme component, as well as the timing of each component's start and end relative to the evaluation's study period. As details of these and other features of the implementation develop between baseline and endline, decisions will be finalized on how best to design the sampling of the endline.

### Summary of quantitative baseline findings regarding beneficiary characteristics

Based on Sections 2 through 4, a general pattern emerges that at baseline, UPPR sample beneficiaries tend to have the “best” indicators related to nutrition, CLP sample beneficiaries tend to fall somewhere in between, and Shiree/Concern sample beneficiaries tend to have the “worst” indicators related to nutrition.

Table E.1 below highlights these differences over a subset of key indicators. The pattern described above holds consistently in terms of key anthropometric indicators, including in the height-for-age Z-score (the primary outcome measure in this evaluation). It also holds in terms of a range of demographic and socioeconomic conditions, asset ownership, dietary diversity, women's status, nutrition practices and services, etc.

These characteristics align with what might be expected given the environments of each of the three programmes. UPPR beneficiaries live in urban areas and have the strongest linkages, while Shiree/Concern beneficiaries live in perhaps the most challenging setting of the three programmes (both remote and flooded for several months of the year), with corresponding implications for access to infrastructure, education, food, services, etc.

As simple descriptives, the baseline differences seen across programmes are not intended to show conclusively which factors are the key determinants of undernutrition in each context. However, they do provide support for the relationships described in the evaluation's theory of change. For example, the analyses show that the context with the lowest ‘intermediate outcomes’ (such as the lowest dietary diversity, the lowest access to services, the lowest knowledge of feeding practices, the lowest perceived empowerment of women) is also the context with the lowest ‘final outcome’ of height-for-age Z-scores, and vice versa. While it cannot be concluded which if any of these low ‘intermediate outcomes’ is the dominant factor underlying low ‘final outcomes,’ if the nutrition intervention is able to meaningfully increase such an ‘intermediate outcome,’ there may be potential for meaningful impact on the ‘final outcome.’

As such, these baseline conditions, along with the intensity of the eventual nutrition interventions, are likely to shape the potential for impact in each programme. For example, if an equally intensive nutrition intervention were to be provided within all programmes and if underlying constraints to nutrition were addressed by the intervention (the exploratory/explanatory components will examine the extent to which these appear to be true), then Shiree/Concern beneficiaries would have the greatest potential scope for improvement given their lowest starting point. As part of the mixed methods approach of this evaluation, analysis drawn from the exploratory/explanatory component will help illuminate how intensively and effectively each of the programmes' nutrition interventions does in fact reach its beneficiaries.

**Table E.1. Baseline means of anthropometric indicators and selected household characteristics among beneficiaries, by programme**

	CLP	Shiree	UPPR
<b>Anthropometric Indicators</b>			
Height-for-age Z-score (HAZ) for children under 5	-1.33 to -1.30	-1.74 to -1.65	-0.97 to -1.08
Proportion stunted (HAZ<-2)	0.32 to 0.35	0.45	0.25 to 0.28
Proportion severely stunted (HAZ<-3)	0.11 to 0.12	0.19 to 0.22	0.08 to 0.10
Weight-for-age Z-score (WAZ) for children under 5	-1.41 to -1.39	-1.71 to -1.62	-1.13 to -1.06
Proportion underweight (WAZ<-2)	0.29 to 0.31	0.38 to 0.41	0.22 to 0.23
Proportion severely underweight (WAZ<-3)	0.07 to 0.08	0.12 to 0.13	0.04 to 0.06
Weight-for-height Z-score (WHZ) for children under 5	-0.91 to -0.92	-1.00 to -0.97	-0.75 to -0.73
Proportion wasted (WHZ<-2)	0.15	0.16 to 0.18	0.13 to 0.15
Proportion severely wasted (WHZ<-3)	0.02 to 0.03	0.04	0.03
Body Mass Index (BMI) for mothers of index children	19.33 to 19.34	19.05 to 19.13	21.47 to 21.57
Proportion underweight (BMI<18.5)	0.40	0.44 to 0.45	0.21 to 0.22
Proportion overweight (BMI>25)	0.02	0.02	0.16 to 0.18
<b>Household Demographic and Socioeconomic Characteristics</b>			
Household size	4.4	5.5	5.3
Age of male household head	34	37 to 38	38 to 39
Female headed household (%)	10 to 11	9 to 11	8 to 10
Age of female household head	32	36 to 37	45 to 46
Number of children < 2 years	1.0	1.0	1.0
Dependency ratio	1.1	1.4 to 1.5	0.8 to 0.9
Head's years of schooling	1.3 to 1.6	0.9 to 1.0	3.4 to 3.5
Child's mother/caregivers's years of schooling	2.1 to 2.3	1.2 to 1.4	4.6 to 5.1
Child's mother/caregiver in a non-earning occupation (%)	19 to 20	41	68 to 69
Dirt floor (%)	100	100	56 to 58
Tin wall (%)	54	55 to 56	40 to 43
Other non-permanent wall (%)	46	43	18 to 19
Tin roof (%)	95	93 to 94	91
Other non-permanent roof (%)	4	5 to 6	3
Access to electricity (%)	4	9 to 16	91
Access to sanitary latrine (%)	35	8 to 10	50 to 57
Access to safe drinking water (%)	99	96	99
Loss of home to river erosion, last 5 years (%/)	11 to 12	1	-

Loss of crops/assets to floods, last 5 years (%)	14	5	0.5
Loss of livestock to illness/ theft, last 5 years (%)	8	6	2
Loss of crops/assets to storms/drought/theft, last 5 years (%)	4	7 to 8	1 to 2
Household Asset Ownership (numbers owned)			
Bicycle	0.1	-	0.2
Metal Cooking Pots	4.8	6.8	12.4
Bed/Khat/Chowki	1.3	1.0	1.8
Armoire/Cabinet/Alna	0.4	0.2	1.3
Table / chair	1.0	0.5	1.9
Electric fan	0.0	0.1	1.4
Wall clock /watch	0.1	0.0	0.4
Television (Color)	-	-	0.5
Solar energy panel	0.1	0.1	-
Sewing machine	-	-	0.5
Rickshaw	-	-	0.1
Boat	0.0	0.2	-
Mobile phone set	0.6	0.5	1.3
Hammer	0.1	0.1	0.2 to 0.3
Fishing net	0.3	0.7	0.1
Spade (Kodal)	0.5	0.3	0.2
Axe(Kural)	0.1	0.1	0.2
Shabol	0.1	0.1	0.2
Hoe	0.7	0.1	0.1
Winnower	0.9	0.7	0.1
Hand tube well	0.4	0.0	0.2
Bulls/oxen	0.2	0.1	0.1
Milk Cow	0.2	0.1	0.1
Goat	0.1	0.1	0.1
Sheep	0.1	0.3	0.0
Chicken	2.8	2.2	0.7 to 1.1
Duck	0.5	2.1 to 2.8	0.3
Total amount of current cash savings (tk)	3,600 to 3,900	1,200	4,800 to 5,100
Household Dietary Diversity			
Household consumed cereal in last 7 days (%)	100	99 to 100	100
Household consumed vitamin A-rich vegetables in last 7 days (%)	41 to 44	7 to 8	47 to 48
Household consumed white tubers/roots in last 7 days (%)	91 to 93	93	97 to 98
Household consumed green leafy vegetables in last 7 days (%)	87 to 88	72 to 73	81 to 83
Household consumed other vegetables in last 7 days (%)	33 to 34	15 to 17	62
Household consumed vitamin A-rich fruits in last 7 days (%)	99 to 100	99 to 100	100
Household consumed other fruits in last 7 days (%)	5 to 7	4	7 to 8
Household consumed meat in last 7 days (%)	48 to 49	48 to 52	62
Household consumed eggs in last 7 days (%)	24 to 26	14 to 15	53 to 54
Household consumed fish in last 7 days (%)	89	99	95 to 96

Household consumed beans peas lentils in last 7 days (%)	29 to 32	17 to 20	39 to 41
Household consumed Dairy in last 7 days (%)	70 to 72	69 to 70	95 to 96
Number of groups (of 12)	7.2	6.4	8.4
Women's Status			
Women's perceived position on a 9-step "power" ladder (people on Step 1 are completely without rights, people on Step 9 have a lot of power)	2.43 to 2.57	2.31 to 2.24	2.69 to 2.73
Women's perceived position on a 9-step "control" ladder (people on Step 1 are totally unable to change their lives, people on Step 9 have full control over their own lives)	2.85 to 2.97	2.54 to 2.69	3.02 to 3.03
Nutrition Practices and Services			
Any antenatal sessions	0.70	0.41 to 0.50	0.89
Number antenatal sessions	2.74 to 2.88	2.38	3.84
Received advice on food to eat	0.81	0.40 to 0.41	0.68 to 0.70
Followed advice	0.86 to 0.88	0.78	0.90 to 0.92
Received iron supplement	0.68 to 0.69	0.35 to 0.40	0.68 to 0.70
Birth attended by: Doctor	0.05	0.05	0.42 to 0.45
Birth attended by: Midwife or nurse	0.05	0.04 to 0.05	0.49 to 0.50
Birth attended by: Any trained person	0.38 to 0.41	0.34 to 0.40	0.73 to 0.75
Exposed to any source of information about nutrition	0.75 to 0.78	0.25 to 0.27	0.67
Did health worker give advice on IYCF during home visit?	0.52 to 0.55	0.10 to 0.11	0.23
Attend group meeting	0.55 to 0.58	0.10 to 0.12	0.08 to 0.09
Any home visit or group meeting	0.77	0.16 to 0.17	0.27
Score on test of nutrition knowledge (of 10), mother	7.40 to 7.42	6.39 to 6.47	7.46 to 7.53
Knows when should a baby start getting breast milk, mother	0.90 to 0.91	0.83 to 0.84	0.90 to 0.92
Knows what should mother do with colostrum, mother	0.85 to 0.86	0.87 to 0.89	0.85 to 0.86
Knows at what age babies should be given other foods, mother	0.63	0.53	0.74 to 0.75
Knows what seasoning is fortified with iodine, mother	0.57	0.26 to 0.27	0.56 to 0.62
Score on test of nutrition knowledge (of 10), adolescent	6.48 to 6.68	5.65 to 6.09	6.74 to 6.94
Knows when should a baby start getting breast milk	0.65 to 0.68	0.54 to 0.67	0.73 to 0.74
Knows what should mother do with colostrum, adolescent	0.60 to 0.73	0.62 to 0.65	0.68 to 0.71
Knows at what age babies should be given other foods, adolescent	0.54 to 0.56	0.43 to 0.46	0.58 to 0.63
Knows what seasoning is fortified with iron, adolescent	0.66 to 0.72	0.37 to 0.54	0.71 to 0.76