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Alternative Sampling Designs for Emergency Settings: A Guide for Survey Planning, Data Collection and Analysis

September 2009

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Contents

Page

Abbreviations and Acronyms

33x6 (design)	33 clusters, 6 observations in each: n=198
67x3 (design)	67 clusters, 3 observations in each: n=201
ACF	Action Against Hunger US
AED	Academy for Educational Development
ARI	Acute respiratory infection
BCG	Bacille Calmette Guerin
CCF	Christian Children's Fund
CI	Confidence interval
CRS	Catholic Relief Services
Deff	Design effect
DR	Decision rule
ESS	Effective sample size
FANTA	Food and Nutrition Technical Assistance Project (1998-2008)
FANTA-2	Food and Nutrition Technical Assistance Project II
FSAU	Food Security Analysis Unit
H_a	Alternative hypothesis
H_0	Null hypothesis
HSPH	Harvard School of Public Health
ICC	Intra-cluster correlation
LQAS	Lot quality assurance sampling
M&E	Monitoring and Evaluation
MOH	Ministry of Health (not country-specific)
MUAC	Middle upper arm circumference
NGO	Nongovernmental organization
NIH	United States National Institute of Health
OFDA	Office of U.S. Foreign Disaster Assistance
OSU	Ohio State University
PPS	Probability proportionate to size
PSU	Primary sampling unit
SC/US	Save the Children US
SD	Standard deviation
Sequential (design)	Up to 67 clusters, 3 observations in each: $n \le 201$
SI	Sampling interval
sqrt	Square root
SRS	Simple random sampling
USAID	United States Agency for International Development
VAC	Vitamin A capsule
WHZ	Weight for height z-score

Acknowledgments

This Guide is the result of six years of work (2002-2008) during which three alternative sampling designs were developed, field tested, and validated. This work was completed under the Food and Nutrition Technical Assistance Project II (FANTA-2)'s predecessor project, FANTA (Food and Nutrition Technical Assistance) which ran from 1998 through 2008.

There are many individuals and organizations that have been involved in one or more stages of the alternative sampling design work completed to date. All have made substantial contributions to the development, refinement, testing, and validation of the designs.

Joseph J. Valadez (previously at NGO Networks and FANTA) and Kari Egge (previously at Catholic Relief Services (CRS)) perceived the need for alternative sampling designs to be available for use in emergency settings and guided the early conceptual work that led to the investigation of the use of lot quality assurance sampling (LQAS) for assessment of the prevalence of acute malnutrition in 2002-2003.

Stacy Hoshaw-Woodard, during her tenure at Ohio State University (OSU), with input from Joseph J. Valadez and Kari Egge, conducted the first set of simulations studies to develop the designs.

Joseph J. Valadez, Megan Deitchler (FANTA-2), and Kari Egge designed the first field study, which compared the alternative sampling designs against the 30x30 design. Cherinet Abuye (Consultant), Megan Deitchler, and Mary Hennigan (CRS) supervised data collection for that 2003 field test in Ethiopia. The CRS and Christian Children's Fund (CCF) country offices provided administrative and logistic support throughout fieldwork and Soledad Fernandez (OSU) provided statistical technical assistance to FANTA for interpretation of the results of that study.

A second field test of the alternative sampling designs was carried out in West Darfur in 2005 as a joint project of Save the Children US (SC/US) and FANTA. Megan Deitchler, Hedwig Deconinck (previously SC/US, now FANTA-2), and Gilles Bergeron (FANTA-2), with input from Caroline Abla (Office of Foreign Disaster Assistance (OFDA)), Eunyong Chung (US Agency for International Development (USAID)), Bruce Cogill (previously FANTA), and Anne Swindale (FANTA-2), designed the Darfur study. Hedwig Deconinck and Megan Deitchler supervised data collection. Ali Nassr (SC/US Khartoum) and Omar Karouri (SC/US Geneina) made substantial contributions to the organization of field work and collection of data, and Henry Lu (previously FANTA) provided assistance in data analysis. The SC/US Khartoum and Geneina offices and the Sudan Ministry of Health, Geneina, provided staff, administrative and logistic support for the study.

A second simulation study of the alternative sampling designs was carried out in 2006. This work was led by Casey Olives and Marcello Pagano in the Department of Biostatistics at the Harvard School of Public Health (HSPH). Megan Deitchler, Bethany Hedt (previously HSPH), Kari Egge, and Joseph J. Valadez also made contributions to that work.

With use of the alternative sampling designs becoming more widespread, feedback from users of the designs in different settings has helped to refine guidance for implementation of the designs. Tom Oguta, Grainne Moloney, and Louise Masese (Food Security Analysis Unit (FSAU) – Somalia) and David Doledec (Action Against Hunger US (ACF)), in particular, have provided valuable input, generously sharing insights from their experiences using the alternative sampling designs in difficult and insecure settings in Somalia and Southern Sudan.

This document, ***Alternative Sampling Designs for Emergency Settings: A Guide for Survey Planning, Data Collection and Analysis***, was prepared by Megan Deitchler. Rachel Elrom and Wendy Putnam edited the Guide. The figures in the Guide were created by Heather Finegan (FANTA-2).

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Peer review journal articles reporting data collected from field applications of the alternative sampling designs are referenced throughout the Guide. In some cases, portions of the text and tables included in those articles are reproduced. We thank the International Journal of Epidemiology at Oxford University Press, Emerging Themes in Epidemiology at BioMed Central Press, and The Journal of the Royal Statistical Society: Series A at Wiley-Blackwell for permission to reproduce those materials, through their licensing policies which enable authors to retain copyright ownership.

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Introduction

The purpose of this Guide is to provide government agencies and humanitarian organizations with instructions to carry out rapid yet statistically reliable population-based surveys in difficult settings where information on the prevalence of acute malnutrition is needed. It is intended for use by program managers, monitoring and evaluation (M&E) specialists, and survey leaders. The materials included in this Guide should not be used for training purposes without further adaptation.

Three different sampling designs are of focus in the Guide, all of which are appropriate for emergency settings, where the time spent collecting data should be limited but must be sufficient to obtain the necessary information about the population.

In emergency settings, population-based surveys are conducted to fulfill two main objectives: 1) to assess the severity and magnitude of the situation; and 2) to obtain data for problem analysis and response planning. The first objective is usually accomplished by assessing the prevalence of acute malnutrition among children less than five years (6-59 months).[1] The second objective can be fulfilled by collecting information on indicators related to morbidity, coverage of vaccination services, household food security, and water and sanitation.

Government and humanitarian agencies need population-based data on these indicators to understand the aggravating and underlying causal factors of undernutrition and to select the most appropriate actions to improve the health, nutrition and survival of the population. In many cases, information on the severity of the situation and the causal factors of undernutrition is needed before there can be any allocation of resources or planning of interventions.

Carrying out a population-based survey can, however, be time- and resource-consuming, particularly in an emergency setting. The nature of an emergency exacerbates all the usual challenges associated with data collection while intensifying the urgency with which reliable data on the population are needed. Emergencies can affect vast populations over dispersed geographic areas; they may occur in areas of relative insecurity and can have a rapid onset. In an emergency situation, certain areas may need to be assessed recurrently over a relatively short time period in order to determine what type of assistance may be required and for how long. Government decision makers and humanitarian agencies need tools for rapid and effective prioritization of vulnerable areas and practical and reliable methods for on-going monitoring of the situation. Survey methods for use in emergency settings therefore need to be both time- and resource-efficient.

The survey method used frequently in emergencies is a two-stage 30x30 cluster survey. This method provides reliable population-level estimates, but is time- and resource-intensive.

To respond to the need for efficient sampling designs for use in emergency settings, the Food and Nutrition Technical Assistance Project (FANTA) together with Catholic Relief Services (CRS), Ohio State University (OSU), Save the Children US (SC/US), and a team of statistical experts[2] at Harvard School of Public Health (HSPH) began an investigation to explore alternative sampling designs appropriate for use in emergency settings. The work led to the development, testing and validation of three alternative sampling designs: 1) 33x6 (33 clusters, 6 observations in each: n=198); 2) 67x3 (67 clusters, 3 observations in each: n=201); and 3) a sequential design (up to 67

[1] Mortality estimation can also provide valuable information to assess the severity of an emergency; however, there are currently few rapid, yet reliable methods available for assessing the extent of recent mortality. See Section 1 of the Guide for further discussion on this topic.

[2] A full list of individuals who have made substantial contributions to the development, testing, and/or validation of the designs is provided in the Acknowledgments.

clusters, 3 observations in each: n ≤ 201). All three designs are hybrid designs, combining aspects of cluster sampling and analysis, with lot quality assurance sampling (LQAS) analysis.

The three designs were developed to provide reliable methods for rapid assessment of the prevalence of acute malnutrition[3] and useful measures of secondary indicators relevant to needs assessment and response planning, including child- and household-level indicators such as morbidity prevalence, vaccination coverage, household food security, and access to water and sanitation.

Field applications have shown that the designs provide meaningful and valid results, and for data collection to require substantially less time and cost than is required for carrying out a 30x30 cluster design. As of 2008, the designs are sufficiently validated for wide-scale adoption by government and humanitarian agencies, so that real data collection needs can be met in a time- and cost-efficient manner.

This Guide provides instruction for the planning, implementation, and analysis of data collected by the 33x6, 67x3, and sequential design. The Guide is organized into five sections with several appendices included for technical reference.

In Section 1 of the Guide, an overview of the sampling and analytic characteristics of the 33x6, 67x3, and sequential design is provided by comparing each design with the sampling and analytic characteristics of a 30x30 cluster design.

In Sections 2 and 3 of the Guide, instructions are provided for carrying out the designs. Section 2 provides instruction for questionnaire development, sampling, and data analysis for the 33x6 and 67x3 designs while Section 3 provides instruction specific to the sequential design.

Section 4 of the Guide provides guidance to identify which sampling design may be most appropriate to use given the objectives of the survey and the geographic characteristics of the area to be assessed.

Finally, Section 5 of the Guide describes promising applications and innovative adaptations of the 33x6 and 67x3 designs. Some of the adaptations presented in this Section are evolving areas of work that require further development before they could be considered fully validated.

The Appendices at the end of the Guide provide tools for LQAS analysis and additional technical information related to the development and validation of the alternative sampling designs.

3 Unless otherwise specified, for the purpose of this Guide, acute malnutrition is defined as a child who is wasted (determined by weight for height z-score [WHZ] < -2 standard deviations [SDs] the reference median) or with bilateral pitting edema.

Child-level indicators

Child-level indicators are outcomes that are measured and reported using children as the unit for analysis. Examples of child-level indicators include morbidity, vaccination, and anthropometric status. These indicators are measured at the child level because the outcomes vary by individual – including among children living in the same household.

Household-level indicators

Household-level indicators are outcomes that are measured and reported using households as the unit for analysis. Access to a latrine and access to potable water are examples of household-level indicators. These indicators are measured at a household level (rather than separately, by individual household members) because the outcomes do not vary among household members.

SECTION 1.

Introducing the 33x6, 67x3 and Sequential Designs

The 30x30 cluster survey is one of the main sampling approaches used to carry out a population-based survey in an emergency setting. The method focuses on obtaining precise estimates for child-level indicators; however this is achieved with substantial drawbacks in terms of the time and cost it takes to collect the data. Sampling alternatives are therefore needed for situations where the objective is to rapidly establish the level of need in an area or to plan interventions for crisis situations.[4] This Guide introduces alternative cluster design options—a 33x6, a 67x3, and a sequential design—that provide ***sufficiently*** precise results for situations like these and do so at substantially reduced time and cost.

[4] If the main objective of a survey is to obtain estimates of finely discriminating precision for child-level indicators, or to assess small but statistically significant changes in child-level indicators over time (particularly anthropometric indicators), the 30x30 cluster survey (or some variant two-stage cluster design with a large sample size) may be the most appropriate design to use. Refer to Section 2.6 for a discussion of these issues.

1.1 LQAS Analysis of Acute Malnutrition

LQAS is a quality assurance analysis method that originated in the manufacturing industry and is now frequently applied in international health. LQAS analysis provides a method to classify whether a binary (yes/no) outcome is at or above a critical threshold level. The analysis approach is relevant for health programs since it is often useful to know whether a certain condition (e.g., the prevalence of a particular disease) in a given population exceeds a critical threshold level or if a program (e.g., immunization mop-up operation) has reached a certain target.

Binary outcome

An outcome for which there are only two possible values for any observation in the sample. For a binary indicator, these two outcomes are usually:
1) yes, the outcome of interest pertains to the observation sampled; or
2) no, the outcome of interest does not pertain to the observation sampled.

LQAS analysis is also useful in emergency settings, where government and humanitarian agencies often need to know whether the prevalence of acute malnutrition has exceeded a certain threshold level or not. The threshold levels of 10%, 15%, and 20% acute malnutrition prevalence are often used to determine the severity of a situation, the scale of the response warranted, and the most appropriate type of nutrition intervention to implement (e.g., targeted supplementary feeding, blanket supplementary feeding) (WHO 2000; Ethiopia DPPC 2002). In this context, LQAS analysis has great utility. Results from LQAS analysis provide decision-makers with the information they need to determine the public health significance of the situation and the type of response justified. In addition, when representative data are available for multiple geographic areas, LQAS classifications for geographically specific areas can be compared to identify which areas are most in need of humanitarian assistance. Areas classified as having a prevalence of acute malnutrition above the threshold level analyzed would be identified as being most in need of humanitarian assistance, while areas classified as having a prevalence of acute malnutrition below the threshold level would be of a lesser priority for humanitarian assistance. Refer to Section 5.1 for more information regarding this latter analysis option.

LQAS would perhaps be an easy analysis method to understand if it were appropriate to use on all types of population-based data collected. However, this is not the case. In order for LQAS to be an appropriate analysis method to use, certain criteria must be met by the sample of data that has been collected and by the indicator(s) of interest on which LQAS analysis is applied. These analytic criteria (described in Appendix 1) are generally met by acute malnutrition data collected with the 33x6, 67x3, and sequential designs, which permits the use of LQAS analysis to classify the prevalence of acute malnutrition against key threshold prevalence levels (10%, 15%, 20%).

The method to analyze acute malnutrition data with LQAS is straight forward. Once data are collected, the number of observations in the sample with acute malnutrition is counted. This number is then compared against a pre-established decision rule (DR) for the obtained sample size in order to make an assessment as to whether the prevalence of acute malnutrition in the population is at or above; or below the threshold of interest. If the number of children with acute malnutrition in the sample is equal to or less than the DR, the prevalence of acute malnutrition in the population is classified as below the threshold level analyzed. If the number of children with acute malnutrition in the sample is greater than the DR, the prevalence of acute malnutrition in the population is classified as at or above the threshold level analyzed (Deitchler et al. 2008). For the 33x6 and 67x3 designs, decision rules are available for the respective total sample sizes of 198 and 201 (refer to Table 7 on page 31). For the sequential design, decision rules are available throughout the data collection process, after each cluster of data is collected (refer to Appendix 2).

Decision Rule

A decision rule (DR) in LQAS analysis is a pre-established number that the data collected are compared against to determine what classification should be made for the threshold analyzed. The decision rule is defined by the sample size available for analysis and the statistical parameters defined for the LQAS classification procedure.

1.2 Comparing the 33x6 and 67x3 Designs to the 30x30 Design

The 33x6 and 67x3 designs differ from a conventional 30x30 design with respect to: 1) sample size; 2) precision of estimates; and 3) time and cost of data collection. These design features are described below for the 33x6 and 67x3 designs, in comparison to the 30x30 approach.

1.2.1 Sample Size

The 30x30 design is a two-stage cluster design in which data are collected from 30 clusters (e.g., villages), with 30 observations (e.g., children, households) sampled per cluster. The total sample size is 900 (30 clusters x 30 observations per cluster = 900 observations). The design allows for tabulation of point estimates for child- and household-level indicators and provides results that are representative of the entire assessment area.

The 33x6 and 67x3 designs are also two-stage cluster designs. Data for the 33x6 design are collected from 33 clusters with 6 observations sampled per cluster, for a total sample size of 198; data for the 67x3 design are collected from 67 clusters with 3 observations sampled per cluster, for a total sample size of 201. Like the 30x30 design, the 33x6 and 67x3 designs allow for tabulation of child- and household-level indicators that are representative of the entire assessment area.

1.2.2 Precision of Estimates

When population-based surveys are used to obtain point estimates for indicators, it is important to consider the precision and accuracy of the estimates derived from the data. These two concepts are described below.

The precision of an estimate is a statistical quantification of the reproducability of the measurement. It is usually reported as a 95% confidence interval (CI) and interpreted as follows: If one was to draw repeated samples of the same size from the population, the true population value would fall within the CI calculated in 95% of those samples. The smaller the width of the 95% CI, the more precise the measurement of that indicator; conversely, the larger the width of the 95% CI, the less precise the measurement is (refer to Figure 1 on the next page). Results from a population-based survey should always include the level of precision (95% CI) associated with the point estimate for each indicator reported, no matter the sampling design used.

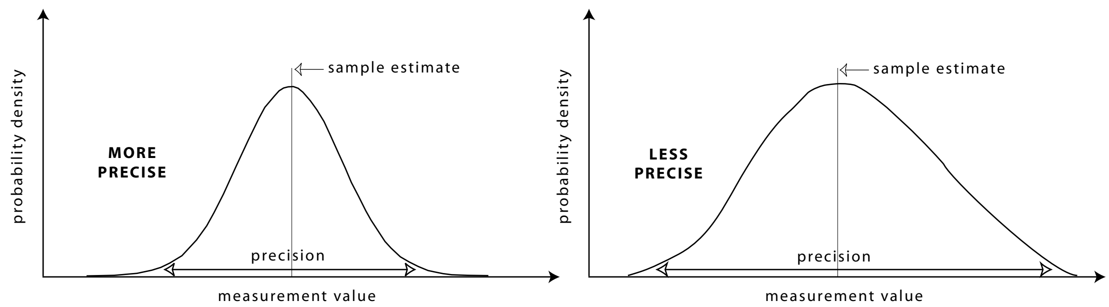
Two-stage cluster design

In a two-stage cluster design, the sample is selected in two stages. First the geographic areas (clusters) to be sampled are selected randomly. In the second stage of sampling, the households, or individuals, to be sampled in each cluster are selected. This method is in contrast to a simple random sample (SRS) survey, which identifies the households, or individuals, to be sampled in only one stage.

Point estimate

The value estimated for an indicator from a population-based sample. For a binary indicator, the point estimate is the proportion of the sample that exhibits the outcome of interest. For an indicator calculated from continuous data, the point estimate is usually the mean of all sample values.

Figure 1. An Illustration of the Concept of Precision



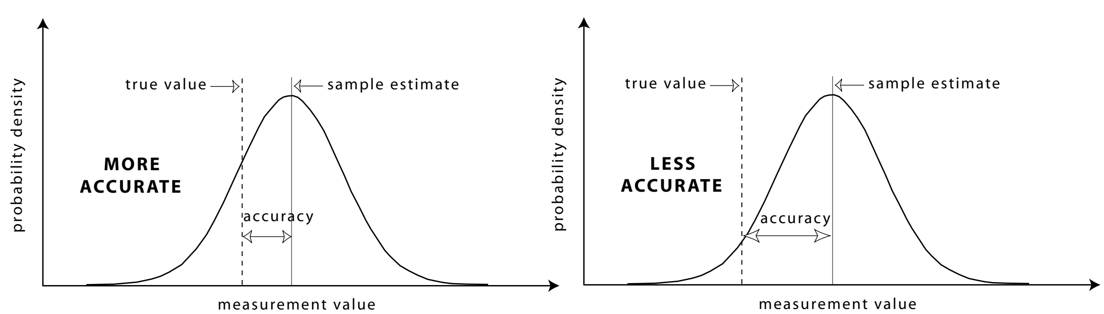
Precision is different from accuracy. Whereas precision is a statistical quantification of the certainty of the measurement, accuracy is the veracity of a measurement. The greater the extent to which the survey provides an estimate close to the true population value, the more accurate the measurement is (refer to Figure 2 below). Unfortunately, one can never know if the estimate obtained by a population-based survey is the true value of the indicator, or how close the estimate is to the true value, unless data are collected from every individual or household in the assessment area.

Several comparative field studies of the 33x6, 67x3, and 30x30 cluster designs have been conducted (Deitchler et al. 2007; Deitchler et al. 2008; Oguta et al. 2007; Oguta et al. 2008). Available results show that the 33x6 and 67x3 designs provide reasonably precise estimates for programmatic decision making. The 67x3 design provides estimates nearly as precise as those provided by a 30x30 design for almost all child-level indicators and estimates that are more precise than the 30x30 design for most household-level indicators. Although the 33x6 design generally provides less precise estimates for child-level indicators, results for household-level indicators are nearly as precise as the 30x30 design (Deitchler et al. 2008). For a more in-depth discussion about the relative precision of each design for estimating child- and household-level indicators, refer to Section 4.1.

An exception to the above findings is indicators of mortality. The sample sizes of the 33x6 and 67x3 designs are too small to provide a useful epidemiologic measure of a rare event such as mortality. This limitation, however, may not be unique to the 33x6 and 67x3 designs. The utility of a 30x30 cluster survey for deriving estimates of the crude and under-five mortality rates is currently the subject of debate[5] (Deitchler et al. 2008). Work to develop and validate new methods to assess mortality—some of which may be appropriate for use in complement with the 33x6 and 67x3 designs—is ongoing (Rose et al. 2006; Checchi 2009).

[5] Although mortality estimation is often a priority in emergency settings, the survey methods currently available for estimation of mortality in emergency settings have substantial limitations. Due to concerns about the use of the 30x30 cluster survey for estimation of mortality in emergency situations, and the inability of the 33x6, 67x3, and sequential designs to provide useful estimates of mortality, measurement of morality is not addressed in this Guide.

Figure 2. An Illustration of the Concept of Accuracy



1.3 Exploring the Sequential Design

The sequential design is a special form of the 67x3 design. It is based on a sampling approach involving data collection from up to 67 clusters with 3 observations sampled per cluster. The design provides a potentially very-rapid method for assessing if the prevalence of acute malnutrition has reached or exceeded a predetermined threshold level. This is accomplished by analyzing the data with LQAS while data collection is in progress.

With the sequential design, a "***look***" at the data can be made after collection of each cluster of data. The "***look***" will indicate one of three outcomes: 1) the threshold level of acute malnutrition has been reached or exceeded in the assessment area; 2) the threshold level of acute malnutrition has not been reached or exceeded in the assessment area; or 3) a clear decision about the threshold level of acute malnutrition in the assessment area cannot be made.

The outcome will depend on the number of children who are acutely malnourished in the sample relative to the total number of clusters of data collected at the time of analysis. If the data indicate a clear decision about the threshold level of interest (outcomes 1 or 2, above), data collection can stop. If a clear decision about the threshold level of acute malnutrition cannot be made, data collection continues.

The sequential design thus allows for sampling to stop early – provided that the empirical data give a clear indication as to whether the prevalence of acute malnutrition is above or below the threshold level before data from the full 67 clusters have been collected. As such, the sequential design has a maximum total sample size of 201 (67x3), but, the final sample size could be substantially smaller.

The obvious advantage of the sequential design is the potential for a smaller sample size. The drawback of the design is that it does not allow for point estimates of child- and household-level indicators unless the full sample size of 67 clusters, 3 observations per cluster (n=201) is collected.[6]

[6] Technically, it may be possible to calculate point estimates and 95% CIs before the full sample of 201 is collected however, no simulation work has been conducted to assess how to best account for the increased variance that would be associated with the estimates derived from using the sequential design in this way (Phatak and Bhatt 1967; Cohen 1970). We therefore do not advise the use of the sequential design for calculation of point estimates unless the full sample size (n=201) is collected.

1.4 Time and Cost of Data Collection

Given the different sample size requirements for the 33x6, 67x3, and sequential designs in comparison to that required for a 30x30 design, it should be no surprise that there are both time and cost implications for data collected by the different designs. While it is not possible to make a general statement about the time and cost required for each sampling design, all field studies to date have shown the alternative sampling designs to offer substantial logistic savings over the 30x30 design: the 33x6 design requiring one-third to one-quarter the time; the 67x3 design requiring one-half to one-third the time; and the sequential design requiring one-half to one-ninth the time required for data collection with the 30x30 design (Deitchler et al. 2007; Deitchler et al. 2008). Data collection for the alternative sampling designs have also been shown to offer substantial cost savings over the 30x30 design (Deitchler et al. 2008). These time and cost comparisons should be considered only as approximate guidelines, however, as the extent of the time and cost savings offered by the 33x6, 67x3, and sequential designs will vary according to the geographic setting where the assessment is conducted. For a more in-depth discussion about how the geographic context may affect the time required for data collection, refer to Section 4.3.

1.5 Summary of Section One

This Section of the Guide provided a brief overview of the 33x6, 67x3, and sequential design. Each design was compared and contrasted against the sample size, analysis features, and time and cost required for implementing a 30x30 cluster design. Table 1 provides a summary of the sampling requirements and analysis features by design. Further details about each sampling method, including specific instruction for carrying out data collection and analysis for each design, are provided in Sections 2 and 3 of the Guide.

Table 1. Sampling Requirements and Analysis Features by Design

SAMPLING DESIGN	SAMPLING REQUIREMENT			ANALYSIS FEATURES		
	# Clusters	# Observations per Cluster	Total Sample Size (n)	Child-level Indicators Point Estimates with 95% CI	HH-level Indicators Point Estimates with 95% CI	LQAS Analysis of Acute Malnutrition Prevalence
30x30 Design	30	30	900	Yes	Yes	No*
33x6 Design	33	6	198	Yes	Yes	Yes (once full sample n=198 is collected)
67x3 Design	67	3	201	Yes	Yes	Yes (once full sample n=201 is collected)
Sequential Design	Varies (Up to 67)	3	Varies (up to max n=201)	No (unless full sample n=201 is collected for child sample)	No (unless full sample n=201 is collected for household sample)	Yes (a "***look***" at the data can be made after each cluster of data is collected)

*Simulation studies would be needed to evaluate statistical feasibility. A classic hypothesis test adjusted for the cluster sample design would be an alternative option for classifying the prevalence of acute malnutrition.

Instructions for Implementing the 33x6 and 67x3 Designs

SECTION 2.

This Section of the Guide provides instruction for implementing the 33x6 and 67x3 designs. The following aspects of survey implementation are addressed: questionnaire development, sampling, questionnaire administration, and data analysis and reporting.

Section One highlighted several similarities among the 33x6, 67x3 and 30x30 designs. Given the commonalities, in many cases, the conventional guidance for collecting and analyzing data for a 30x30 design needs only slight adaptation to accommodate the 33x6 and 67x3 designs. This Section aims to provide the specific instruction necessary to implement the 33x6 and 67x3 designs correctly, but does not address all of the considerations that would apply equally to the implementation of a 30x30 design.

2.1 Questionnaire Development

Questionnaire development for the 33x6 and 67x3 designs is no different than for a 30x30 cluster design. Just as the questionnaire for a 30x30 design should capture information about the indicators required for reporting, intervention planning, and decision-making purposes, so too should questionnaires for the 33x6 and 67x3 designs. In an emergency setting, the content of a questionnaire should be balanced against the need for the information given the urgency of the situation, and the time and resource constraints associated with data collection. The Office of Foreign Disaster Assistance (OFDA) at the US Agency for International Development (USAID) has developed a list of standard indicators for use in emergency settings (Appendix 3). This list provides a useful reference to help identify priority indicators often needed in an emergency.

Tips for good questionnaire development include: using the standard set of questions and case definitions when collecting data for internationally recognized indicators; clearly defining key words used in the survey, such as what constitutes a household and who are household members; adapting coding responses to the context of the survey environment, as appropriate; and ensuring that all skip patterns are clearly and correctly documented on the questionnaire. Forward and backward translation and pilot testing of the questionnaire are critical to ensure appropriate terms are used, that respondents understand the questions as they are intended, and to prepare interviewers to administer the questionnaire correctly. Perhaps the most important point to emphasize is that the formulation of the questions should not be changed simply because a 33x6 or a 67x3 design is being carried out in lieu of a 30x30 design. The same questionnaire could be used for any of the sampling designs.

In emergencies, data on both child- and household-level indicators are usually needed to inform the most appropriate set of interventions for the affected population. When this is the case, the questionnaire should include both a child and a household section, and the data collected should fulfill the sample size required for both a child- and a household-level sample. An example of a typical questionnaire used in emergency settings is provided in Appendix 4. This questionnaire includes both a child and household section, and could be administered when collecting data using a 33x6 design, 67x3 design, or 30x30 design.

2.2 Sampling Approach for the 33x6 and 67x3 Designs

Simple random sampling (SRS) is considered the gold standard method for carrying out population-based sampling; however, SRS is neither time- nor cost-efficient in difficult field circumstances such as those common to data collection in developing countries, and especially in emergency settings. To implement a SRS design, the sample to be surveyed is selected randomly from a complete list of individuals or households residing in the assessment area. Using this sort of method for sample selection can result in interview teams needing to travel to as many different villages as there are individuals or households sampled – a situation that is rarely practical. Cluster sampling provides a feasible alternative to SRS and is therefore most commonly used in the developing country context.

With cluster sampling the sample to be surveyed is selected in multiple stages rather than just one stage. The first stage of sample selection is the selection of clusters, or geographic areas (also called Primary Sampling Units, or PSUs), to be sampled; the second stage of sample selection is the selection of observations (e.g., children or households) to be sampled within each cluster. The cluster sampling approach guarantees a certain amount of logistic convenience for data collection. When cluster sampling is used, data on multiple observations are collected in any one geographic area (cluster) selected for sampling.

The 33x6 and 67x3 designs, like the 30x30 design, are both cluster designs. Sampling therefore occurs in two distinct stages: 1) the selection of clusters (e.g., villages), or primary sampling units (PSUs) to be sampled; and 2) the selection of observations to be sampled within the selected clusters. Instructions on how to carry out the first stage of cluster sampling are provided below. Section 2.4 provides instruction for carrying out the second stage of sample selection.

2.3 First Stage of Sampling: Selection of Clusters

The recommended method for selection of the clusters to be sampled for the 33x6 and 67x3 designs is Probability Proportionate to Size (PPS). With PPS, the probability of selecting a PSU for sampling is proportionate to the population size of the PSU. A more populous PSU therefore has a greater chance of being selected for sampling. Since data are collected on a fixed number of observations per cluster at the second stage of sampling, using PPS at the first stage of sample selection helps to ensure that each observation in the survey area has approximately[7] the same probability to be selected for inclusion in the sample.

Detailed instructions for carrying out PPS are provided in Box 1.

[7] The word "approximately" is used here since the population numbers used for the PSUs listed in the sampling frame are estimates, and may not reflect the exact proportion of the total population in the assessment area currently living in that PSU.

BOX 1. INSTRUCTIONS FOR CARRYING OUT THE PPS PROCEDURE

STEP 1

Prepare the sampling frame. This entails making a complete list of the PSUs in the survey area along with the respective population size of each PSU (refer to "Village" and "Total Population" columns in Tables 2 and 3). When compiling the sampling frame, the smallest geographic unit for which there is accurate population information should be used to represent the PSUs. In cases where there is limited population data, or there has been substantial population movement, it may be necessary to update the last documented population numbers in consultation with various local agencies working in the area. The order of the PSUs in the sampling frame is not important so long as the PSUs are not listed by ascending or descending order of population size. Generally, a random ordering of PSUs or ordering of the PSUs according to region is preferred. (The PSUs in the sampling frame in Tables 2 and 3 are organized by district).

STEP 2

Starting at the top of the list, calculate the cumulative population size and continue this process for the entire PSU list ("Cumulative Population" column).

STEP 3

List the cumulative range of the population in each PSU, based on the cumulative sum tabulated above ("Range" column).

STEP 4

Compute the Sampling Interval (SI) by dividing the total cumulative population by the total number of clusters to be sampled. For the 33x6 design, the total number of clusters to be sampled is 33. For the 67x3 design, the total number of cluster to be sampled is 67.

STEP 5

Use a computer random number generator or a random number table (refer to Appendix 5) to blindly select a random number between 1 and the SI. The PSU corresponding to where the random number falls is the first cluster selected for sampling.

STEP 6

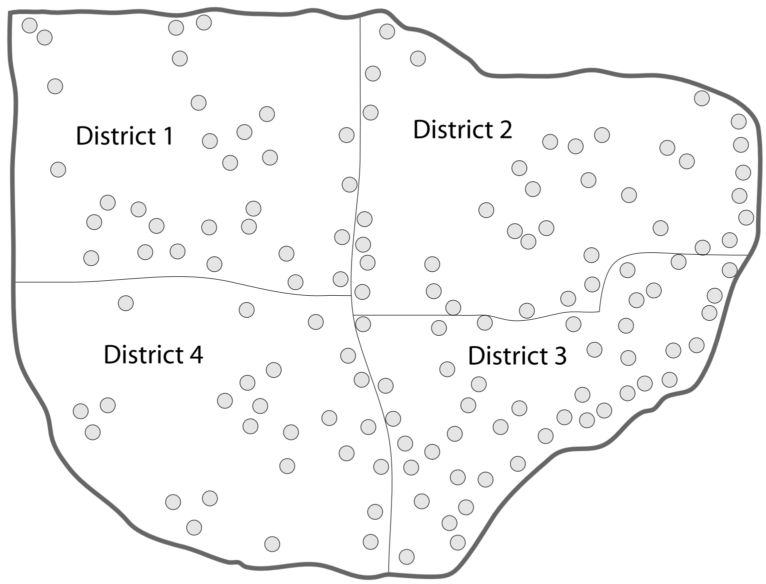
Subsequent clusters are selected by adding the SI to the number identified in the previous step ("Cluster Allocation" column). This last step continues until the complete list of PSUs has been exhausted. After completing this process, the targeted number of clusters should have been selected.

NOTE

A verification that can be used to catch if a mistake was made in the PPS calculation for cluster selection is to add the SI to the final sum calculated in **Step 6**. If there is sufficient population in the sampling frame to select more clusters than designated by the design, a mistake has been made in the PPS procedure.

Examples for how to carry out PPS to select the clusters to be sampled for the 33x6 (Box 2, Table 2) and 67x3 design (Box 3, Table 3) are provided below. In each case, assume the area to be assessed is the fictitious Wobelleno Province. The province has a total population of 123,498 and is comprised of 4 districts and 135 villages.

Figure 3. Map of the Fictitious Wobelleno Province: Districts and Villages



BOX 2. EXAMPLE OF PPS CLUSTER SELECTION FOR A 33X6 DESIGN, WOBELLENO PROVINCE

NOTE

The first three steps of PPS will result in the same output for a given area no matter what type of cluster design is implemented, a 33x6, 67x3, or a 30x30 design.

STEP 1

The first step of the PPS procedure is shown in columns titled "Village" and "Total Population" of Table 2.

STEPS 2 - 3

The second and third steps of the PPS procedure are shown in columns titled "Cumulative Population" and "Range" of Table 2.

STEP 4

The fourth step in carrying out PPS is to compute the Sampling Interval (SI) by dividing the total cumulative population by the total number of clusters to be sampled. For the 33x6 design, we divide the total population of Wobelleno, 123,498, by the 33 clusters that need to be selected for sampling: 123,498 / 33 = 3,742.36.

STEP 5

A random number is then selected between 1 and 3,742. For this, a computer random number generator or a random number table (refer to Appendix 5), should be used. Assume the random number 1,820 is selected. In this case, Dabi is the first of the 33 clusters selected for sampling. This is because the number 1,820 falls between the corresponding cumulative population range of 1,409 and 2,758 in the "Range" column.

STEP 6

The procedure for selecting the remaining 32 clusters to be sampled is shown below. Notice the decimals are kept for cumulative addition, but not for cluster selection.

Cluster 1 = 1,820.00
Cluster 2 = 1,820.00 + 3,742.46 = 5,562.46
Cluster 3 = 5,562.46 + 3,742.46 = 9,304.92
Cluster 4 = 9,304.92 + 3,742.46 = 13,047.38
Cluster 5 = 13,047.38 + 3,742.46 = 16,789.84
Cluster 6 = 16,789.84 + 3,742.46 = 20,532.30
Cluster 7 = 20,532.30 + 3,742.46 = 24,274.76
Cluster 8 = 24,274.76 + 3,742.46 = 28,017.22
Cluster 9 = 28,017.22 + 3,742.46 = 31,759.68
Cluster 10 = 31,759.68 + 3,742.46 = 35,502.14
Cluster 11 = 35,502.14 + 3,742.46 = 39,244.60
Cluster 12 = 39,244.60 + 3,742.46 = 42,987.06
Cluster 13 = 42,987.06 + 3,742.46 = 46,729.52
Cluster 14 = 46,729.52 + 3,742.46 = 50,471.98
Cluster 15 = 50,471.98 + 3,742.46 = 54,214.44
Cluster 16 = 54,214.44 + 3,742.46 = 57,956.90
Cluster 17 = 57,956.90 + 3,742.46 = 61,699.36
Cluster 18 = 61,699.36 + 3,742.46 = 65,441.82
Cluster 19 = 65,441.82 + 3,742.46 = 69,184.28
Cluster 20 = 69,184.28 + 3,742.46 = 72,926.74
Cluster 21 = 72,926.74 + 3,742.46 = 76,669.20
Cluster 22 = 76,669.20 + 3,742.46 = 80,411.66
Cluster 23 = 80,411.66 + 3,742.46 = 84,154.12
Cluster 24 = 84,154.12 + 3,742.46 = 87,896.58
Cluster 25 = 87,896.58 + 3,742.46 = 91,639.04
Cluster 26 = 91,639.04 + 3,742.46 = 95,381.50
Cluster 27 = 95,381.50 + 3,742.46 = 99,123.96
Cluster 28 = 99123.96 + 3,742.46 = 102,596.42
Cluster 29 = 102,596.42 + 3,742.46 = 106,338.88
Cluster 30 = 106,338.88 + 3,742.46 = 110,081.34
Cluster 31 = 110,081.34 + 3,742.46 = 113,823.80
Cluster 32 = 113,823.80 + 3,742.46 = 117,566.26
Cluster 33 = 117,566.26 + 3,742.46 = 121,308.70

Table 2. PPS Cluster Selection for a 33x6 Design, Wobelleno Province

	VILLAGE	TOTAL POPULATION	CUMULATIVE POPULATION	RANGE		CLUSTER ALLOCATION
DISTRICT 1	Shola	1200	1200	0	1200	
	Elmiol	208	1408	1201	1408	
	Dabi	**1350**	**2758**	**1409**	**2758**	**1**
	Doabol	450	3208	2759	3208	
	Elmajoha	148	3356	3209	3356	
	Madradam	**5002**	**8358**	**3357**	**8358**	**2**
	Esalaman	**1250**	**9608**	**8359**	**9608**	**3**
	Elmajor	205	9813	9609	9813	
	Salala A	2000	11813	9814	11813	
	Salala B	**2505**	**14318**	**11814**	**14318**	**4**
	Andarou	1,080	15398	14319	15398	
	Ardaba	425	15823	15399	15823	
	Elzama	755	16578	15824	16578	
	Eltaraj	**880**	**17458**	**16579**	**17458**	**5**
	Elwasa East	1120	18578	17459	18578	
	Elwasa West	1,250	19828	18579	19828	
	Emara A	**1815**	**21643**	**19829**	**21643**	**6**
	Emara B	1,438	23081	21644	23081	
	Sabi	**1320**	**24401**	**23082**	**24401**	**7**
	Arbaba	1810	26211	24402	26211	
	Elthoram	879	27090	26212	27090	
	Giman East	682	27772	27091	27772	
	Giman West	**940**	**28712**	**27773**	**28712**	**8**
	Ramakal	725	29437	28713	29437	
	Sunaba	584	30021	29438	30021	
	Adar East	118	30139	30022	30139	
	Adar West	1110	31249	30140	31249	
	Borala A	**1487**	**32736**	**31249**	**32736**	**9**
	Borala B	423	33159	32737	33159	
	Kabanba	408	33567	33160	33567	
DISTRICT 2	**Koborga**	**3812**	**37379**	**33568**	**37379**	**10**
	Mandarlam	**2205**	**39584**	**37380**	**39584**	**11**
	Naban	1408	40992	39585	40992	
	Rafol	1210	42202	40993	42202	
	Umdo A	**2350**	**44552**	**42203**	**44552**	**12**
	Umdo B	1250	45802	44553	45802	
	Tamorala	**4182**	**49984**	**45803**	**49984**	**13**
	Estab A	**3242**	**53226**	**49985**	**53226**	**14**
	Estab B	**2512**	**55738**	**53227**	**55738**	**15**
	Lionar	2123	57861	55739	57861	
	Doba	**1254**	**59115**	**57862**	**59115**	**16**
	Satun	1226	60314	59116	60314	
	Galda	**1723**	**62064**	**60315**	**62064**	**17**
	Sabar	824	62888	62065	62888	
	Bagar	737	63625	62889	63625	
	Shakam	104	63729	63626	63729	
	Faiga East	1100	64829	63730	64829	
	Faiga West	**2090**	**66919**	**64830**	**66919**	**18**
	Shaloko	198	67117	66920	67117	
	Egaz	284	67401	67118	67401	
	Elrassa	**2587**	**69988**	**67402**	**69988**	**19**
	Elfah	2235	72223	69989	72223	
	Efa A	242	72465	72224	72465	
	Efa B	**737**	**73202**	**72466**	**73202**	**20**
	Korogoa	436	73638	73203	73638	
	Emaja	842	74480	73639	74480	
	Elhaba A	324	74804	74481	74804	
	Elhaba B	234	75038	74805	75038	
	Eltamon	151	75189	75039	75189	
	Elwa East	89	75278	75190	75278	
	Elwa West	23	75301	75279	75301	
	Elhoor	151	75452	75302	75452	
	Emdad	352	75804	75453	75804	
	Sambara	262	76066	75805	76066	
	Abaolo	111	76177	76067	76177	
	Eshowara	**2515**	**78692**	**76178**	**78692**	**21**
	Nesa A	262	78954	78693	78954	
DISTRICT 3	Nesa B	363	79317	78955	79317	
	Ramak	842	80159	79318	80159	
	Sunata	**1254**	**81413**	**80160**	**81413**	**22**
	Darbu	2352	83765	81414	83765	
	Bukabal	124	83889	83766	83889	
	Bordiro	**737**	**84626**	**83890**	**84626**	**23**
	Bagunda	236	84862	84627	84862	
	Golonaj	484	85346	84863	85346	
	Rongaba	685	86031	85347	86031	
	Garsa	346	86377	86032	86377	
	Bagaia	457	86834	86378	86834	
	Fota	346	87180	86835	87180	
	Ribo A	234	87414	87181	87414	
	Ribo B	236	87650	87415	87650	
	Moraldab	**848**	**88498**	**87651**	**88498**	**24**
	Toesa A	373	88871	88499	88871	
	Toesa B	548	89419	88872	89419	
	Lomolab	362	89781	89420	89781	
	Salan	262	90043	89782	90043	
	Bidojol	473	90516	90044	90516	
	Nalda	578	91094	90517	91094	
	Tunaj	**734**	**91828**	**91095**	**91828**	**25**
	Ramoro	845	92673	91829	92673	
	Kamshak	573	93246	92674	93426	
	Galei A	235	93481	93247	93481	
	Galei B	135	93616	93482	93616	
	Loki	262	93878	93617	93878	
	Gazrat	123	94001	93879	94001	
	Rasgooz	151	94152	94002	94152	
	Falah	515	94667	94153	94667	
	Fasaja A	262	94929	94668	94929	
	Fasaja B	287	95216	94930	95216	
	Lamako A	**222**	**95438**	**95217**	**95438**	**26**
	Lamako B	188	95626	95439	95626	
	Shati East	337	95963	95627	95963	
	Shati West	126	96089	95964	96089	
	Damonor	190	96279	96090	96279	
	Wahda	67	96346	96280	96346	
	Esdab	378	96724	96347	96724	
	Elorzaj	327	97051	96725	97051	
	Tedadal	96	97147	97052	97147	
	Saj	48	97195	97148	97195	
	Arbal	283	97478	97196	97478	
DISTRICT 4	Warat	89	97567	97479	97567	
	Sagima A	151	97718	97568	97718	
	Sagima B	738	98456	97719	98456	
	Khaya	**955**	**99411**	**98457**	**99411**	**27**
	Tajab	1545	100956	99412	100956	
	Darbu Adar	825	101781	100957	101781	
	Andar Agal	279	102060	101782	102060	
	Boto bol	379	102439	102061	102439	
	Hagal	**954**	**103393**	**102440**	**103393**	**28**
	Bagooj	236	103629	103394	103629	
	Robagol	437	104066	103630	104066	
	Mansa	480	104546	104067	104546	
	Nabag	594	105140	104547	105140	
	Rota	265	105405	105141	105405	
	Umbo A	216	105621	105406	105621	
	Umbo B	**3219**	**108840**	**105622**	**108840**	**29**
	Tamab	2235	110075	108841	110075	
	Eresta A	**2984**	**114059**	**110076**	**114059**	**30, 31**
	Eresta B	344	114403	114060	114403	
	Lilbono	1262	115665	114404	115665	
	Masanaj	151	115816	115666	115816	
	Dobesab	1621	117437	115817	117437	
	Gobada	**162**	**117599**	**117438**	**117599**	**32**
	Sasunlaj	**3738**	**121337**	**117600**	**121337**	**33**
	Batamoro	2161	123498	121338	123498	

BOX 3. EXAMPLE OF PPS CLUSTER SELECTION FOR A 67X3 DESIGN, WOBELLENO PROVINCE

STEP 1

The first step of the PPS procedure is shown in columns titled "Village" and "Total Population" of Table 3.

STEPS 2 - 3

The second and third steps of the PPS procedure are shown in columns titled "Cumulative Population" and "Range" of Table 3.

STEP 4

To compute the SI for the 67x3 design, we divide the total population of Wobelleno, by the 67 clusters that need to be selected for sampling: 123,498 / 67 = 1,843.25.

STEP 5

A random number is then selected between 1 and 1,843 using the same procedure as recommended for the 33x6 design (Box 2). If we assume the random number of 628 is selected, Shola is identified as the first of the 67 clusters selected for sampling.

STEP 6

The procedure for selecting the remaining 66 clusters to be sampled is shown below. Notice the decimals are kept for cumulative addition, but not for cluster selection.

Cluster 1 = 628.00	Cluster 35 = 61,455.25 + 1,843.25 = 63,298.50
Cluster 2 = 628.00 + 1,843.25 = 2,471.25	Cluster 36 = 63,298.50 + 1,843.25 = 65,141.75
Cluster 3 = 2,471.25 + 1,843.25 = 4,314.50	Cluster 37 = 65,141.75 + 1,843.25 = 66,985.00
Cluster 4 = 4,314.50 + 1,843.25 = 6,157.75	Cluster 38 = 66,985.00 + 1,843.25 = 68,828.25
Cluster 5 = 6,157.75 + 1,843.25 = 8,001.00	Cluster 39 = 68,828.25 + 1,843.25 = 70,671.50
Cluster 6 = 8,001.00 + 1,843.25 = 9,844.25	Cluster 40 = 70,671.50 + 1,843.25 = 72,514.75
Cluster 7 = 9,844.25 + 1,843.25 = 11,687.50	Cluster 41 = 72,514.75 + 1,843.25 = 74,358.00
Cluster 8 = 11,687.50 + 1,843.25 = 13,530.75	Cluster 42 = 74,358.00 + 1,843.25 = 76,201.25
Cluster 9 = 13530.75 + 1,843.25 = 15374.00	Cluster 43 = 76,201.25 + 1,843.25 = 78,044.50
Cluster 10 = 15374.00 + 1,843.25 = 17217.25	Cluster 44 = 78,044.50 + 1,843.25 = 79,887.75
Cluster 11 = 17217.25 + 1,843.25 = 19060.50	Cluster 45 = 79,887.75 + 1,843.25 = 81,731.00
Cluster 12 = 19060.50 + 1,843.25 = 20903.75	Cluster 46 = 81,731.00 + 1,843.25 = 83,574.25
Cluster 13 = 20903.75 + 1,843.25 = 22747.00	Cluster 47 = 83,574.25 + 1,843.25 = 85,417.50
Cluster 14 = 22,747.00 + 1,843.25 = 24,590.25	Cluster 48 = 85,417.50 + 1,843.25 = 87,260.75
Cluster 15 = 24,590.25 + 1,843.25 = 26,433.50	Cluster 49 = 87,260.75 + 1,843.25 = 89,104.00
Cluster 16 = 26,433.50 + 1,843.25 = 28,276.75	Cluster 50 = 89,104.00 + 1,843.25 = 90,947.25
Cluster 17 = 28,276.75 + 1,843.25 = 30,120.00	Cluster 51 = 90,947.25 + 1,843.25 = 92,790.50
Cluster 18 = 30,120.00 + 1,843.25 = 31,963.25	Cluster 52 = 92,790.50 + 1,843.25 = 94,633.75
Cluster 19 = 31,963.25 + 1,843.25 = 33,806.50	Cluster 53 = 94,633.75 + 1,843.25 = 96,477.00
Cluster 20 = 33,806.00 + 1,843.25 = 35,649.75	Cluster 54 = 96,477.00 + 1,843.25 = 98,320.25
Cluster 21 = 35,649.75 + 1,843.25 = 37,493.00	Cluster 55 = 98,320.25 + 1,843.25 = 100,163.50
Cluster 22 = 37,493.00 + 1,843.25 = 39,336.25	Cluster 56 = 100,163.50 + 1,843.25 = 102,006.75
Cluster 23 = 39,336.25 + 1,843.25 = 41,179.50	Cluster 57 = 102,006.75 + 1,843.25 = 103,850.00
Cluster 24 = 41,179.50 + 1,843.25 = 43,022.75	Cluster 58 = 103,850.00 + 1,843.25 = 105,693.25
Cluster 25 = 43,022.75 + 1,843.25 = 44,866.00	Cluster 59 = 105,693.25 + 1,843.25 = 107,536.50
Cluster 26 = 44,866.00 + 1,843.25 = 46,709.25	Cluster 60 = 107,536.50 + 1,843.25 = 109,379.75
Cluster 27 = 46,709.25 + 1,843.25 = 48,552.50	Cluster 61 = 109,379.75 + 1,843.25 = 111,223.00
Cluster 28 = 48,552.50 + 1,843.25 = 50,395.75	Cluster 62 = 111,223.00 + 1,843.25 = 113,066.25
Cluster 29 = 50,395.75 + 1,843.25 = 52,239.00	Cluster 63 = 113,066.25 + 1,843.25 = 114,909.50
Cluster 30 = 52,239.00 + 1,843.25 = 54,082.25	Cluster 64 = 114,909.50 + 1,843.25 = 116,752.75
Cluster 31 = 54,082.25 + 1,843.25 = 55,925.50	Cluster 65 = 116,752.75 + 1,843.25 = 118,596.00
Cluster 32 = 55,925.50 + 1,843.25 = 57,768.75	Cluster 66 = 118,596.00 + 1,843.25 = 120,439.25
Cluster 33 = 57,768.75 + 1,843.25 = 59,612.00	Cluster 67 = 120,439.25 + 1,843.25 = 122,282.50
Cluster 34 = 59,612.00 + 1,843.25 = 61,455.25	

Table 3. PPS Cluster Selection for a 67x3 Design, Wobelleno Province

District	Village	Total Population	Cumulative Population	Range		Cluster Allocation
DISTRICT 1	**Shola**	**1200**	**1200**	**0**	**1200**	**1**
	Elmiol	208	1408	1201	1408	
	Dabi	**1350**	**2758**	**1409**	**2758**	**2**
	Doabol	450	3208	2759	3208	
	Elmajoha	148	3356	3209	3356	
	Madradam	**5002**	**8358**	**3357**	**8358**	**3,4,5**
	Esalaman	1250	9608	8359	9608	
	Elmajor	205	9813	9609	9813	
	Salala A	**2000**	**11813**	**9814**	**11813**	**6,7**
	Salala B	**2505**	**14318**	**11814**	**14318**	**8**
	Andarou	**1,080**	**15398**	**14319**	**15398**	**9**
	Ardaba	425	15823	15399	15823	
	Elzama	755	16578	15824	16578	
	Eltaraj	**880**	**17458**	**16579**	**17458**	**10**
	Elwasa East	1120	18578	17459	18578	
	Elwasa West	**1,250**	**19828**	**18579**	**19828**	**11**
	Emara A	**1815**	**21643**	**19829**	**21643**	**12**
	Emara B	**1,438**	**23081**	**21644**	**23081**	**13**
	Sabi	1320	24401	23082	24401	
	Arbaba	**1810**	**26211**	**24402**	**26211**	**14**
	Elthoram	**879**	**27090**	**26212**	**27090**	**15**
	Giman East	682	27772	27091	27772	
	Giman West	**940**	**28712**	**27773**	**28712**	**16**
	Ramakal	725	29437	28713	29437	
	Sunaba	584	30021	29438	30021	
	Adar East	**118**	**30139**	**30022**	**30139**	**17**
	Adar West	1110	31249	30140	31249	
	Borala A	**1487**	**32736**	**31249**	**32736**	**18**
	Borala B	423	33159	32737	33159	
	Kabanba	408	33567	33160	33567	
DISTRICT 2	**Koborga**	**3812**	**37379**	**33568**	**37379**	**19.20**
	Mandarlam	**2205**	**39584**	**37380**	**39584**	**21,22**
	Naban	1408	40992	39585	40992	
	Rafol	**1210**	**42202**	**40993**	**42202**	**23**
	Umdo A	**2350**	**44552**	**42203**	**44552**	**24**
	Umdo B	**1250**	**45802**	**44553**	**45802**	**25**
	Tamorala	**4182**	**49984**	**45803**	**49984**	**26,27**
	Estab A	**3242**	**53226**	**49985**	**53226**	**28,29**
	Estab B	**2512**	**55738**	**53227**	**55738**	**30**
	Lionar	**2123**	**57861**	**55739**	**57861**	**31,32**
	Doba	1254	59115	57862	59115	
	Satun	**1226**	**60314**	**59116**	**60314**	**33**
	Galda	**1723**	**62064**	**60315**	**62064**	**34**
	Sabar	824	62888	62065	62888	
	Bagar	**737**	**63625**	**62889**	**63625**	**35**
	Shakam	104	63729	63626	63729	
	Faiga East	1100	64829	63730	64829	
	Faiga West	**2090**	**66919**	**64830**	**66919**	**36**
	Shaloko	**198**	**67117**	**66920**	**67117**	**37**
	Egaz	284	67401	67118	67401	
	Elrassa	**2587**	**69988**	**67402**	**69988**	**38**
	Elfah	**2235**	**72223**	**69989**	**72223**	**39**
	Efa A	242	72465	72224	72465	
	Efa B	**737**	**73202**	**72466**	**73202**	**40**
	Korogoa	436	73638	73203	73638	
	Emaja	**842**	**74480**	**73639**	**74480**	**41**
	Elhaba A	324	74804	74481	74804	
	Elhaba B	234	75038	74805	75038	
	Eltamon	151	75189	75039	75189	
	Elwa East	89	75278	75190	75278	
	Elwa West	23	75301	75279	75301	
	Elhoor	151	75452	75302	75452	
	Emdad	352	75804	75453	75804	
	Sambara	262	76066	75805	76066	
	Abaolo	111	76177	76067	76177	
	Eshowara	**2515**	**78692**	**76178**	**78692**	**42,43**
	Nesa A	262	78954	78693	78954	
DISTRICT 3	Nesa B	363	79317	78955	79317	
	Ramak	**842**	**80159**	**79318**	**80159**	**44**
	Sunata	1254	81413	80160	81413	
	Darbu	**2352**	**83765**	**81414**	**83765**	**45,46**
	Bukabal	124	83889	83766	83889	
	Bordiro	737	84626	83890	84626	
	Bagunda	236	84862	84627	84862	
	Golonaj	484	85346	84863	85346	
	Rongaba	**685**	**86031**	**85347**	**86031**	**47**
	Garsa	346	86377	86032	86377	
	Bagaia	457	86834	86378	86834	
	Fota	346	87180	86835	87180	
	Ribo A	**234**	**87414**	**87181**	**87414**	**48**
	Ribo B	236	87650	87415	87650	
	Moraldab	848	88498	87651	88498	
	Toesa A	373	88871	88499	88871	
	Toesa B	**548**	**89419**	**88872**	**89419**	**49**
	Lomolab	362	89781	89420	89781	
	Salan	262	90043	89782	90043	
	Bidojol	473	90516	90044	90516	
	Nalda	**578**	**91094**	**90517**	**91094**	**50**
	Tunaj	734	91828	91095	91828	
	Ramoro	845	92673	91829	92673	
	Kamshak	**573**	**93246**	**92674**	**93426**	**51**
	Galei A	235	93481	93247	93481	
	Galei B	135	93616	93482	93616	
	Loki	262	93878	93617	93878	
	Gazrat	123	94001	93879	94001	
	Rasgooz	151	94152	94002	94152	
	Falah	**515**	**94667**	**94153**	**94667**	**52**
	Fasaja A	262	94929	94668	94929	
	Fasaja B	287	95216	94930	95216	
	Lamako A	222	95438	95217	95438	
	Lamako B	188	95626	95439	95626	
	Shati East	337	95963	95627	95963	
	Shati West	126	96089	95964	96089	
	Damonor	190	96279	96090	96279	
	Wahda	67	96346	96280	96346	
	Esdab	**378**	**96724**	**96347**	**96724**	**53**
	Elorzaj	327	97051	96725	97051	
	Tedadal	96	97147	97052	97147	
	Saj	48	97195	97148	97195	
	Arbal	283	97478	97196	97478	
DISTRICT 4	Warat	89	97567	97479	97567	
	Sagima A	151	97718	97568	97718	
	Sagima B	**738**	**98456**	**97719**	**98456**	**54**
	Khaya	955	99411	98457	99411	
	Tajab	**1545**	**100956**	**99412**	**100956**	**55**
	Darbu Adar	825	101781	100957	101781	
	Andar Agal	**279**	**102060**	**101782**	**102060**	**56**
	Boto bol	379	102439	102061	102439	
	Hagal	954	103393	102440	103393	
	Bagooj	236	103629	103394	103629	
	Robagol	**437**	**104066**	**103630**	**104066**	**57**
	Mansa	480	104546	104067	104546	
	Nabag	594	105140	104547	105140	
	Rota	265	105405	105141	105405	
	Umbo A	216	105621	105406	105621	
	Umbo B	**3219**	**108840**	**105622**	**108840**	**58,59**
	Tamab	**2235**	**110075**	**108841**	**110075**	**60**
	Eresta A	**2984**	**114059**	**110076**	**114059**	**61,62**
	Eresta B	344	114403	114060	114403	
	Lilbono	**1262**	**115665**	**114404**	**115665**	**63**
	Masanaj	151	115816	115666	115816	
	Dobesab	**1621**	**117437**	**115817**	**117437**	**64**
	Gobada	162	117599	117438	117599	
	Sasunlaj	**3738**	**121337**	**117600**	**121337**	**65.66**
	Batamoro	**2161**	**123498**	**121338**	**123498**	**67**

The 33 clusters selected for sampling by the PPS procedure are highlighted on the map of Wobelleno in Figure 4. The 67 clusters selected for sampling are highlighted on the map of Wobelleno below in Figure 5.

Figure 4. 33 Clusters Selected for Sampling by PPS, Wobelleno Province

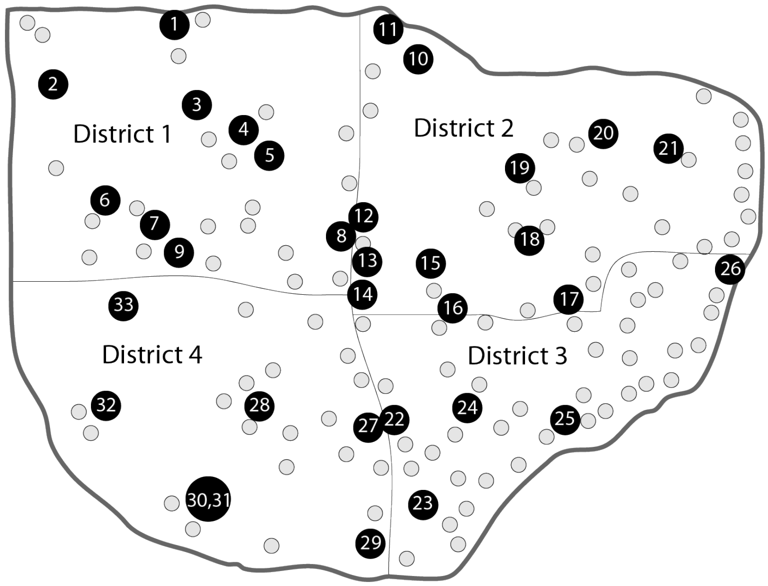
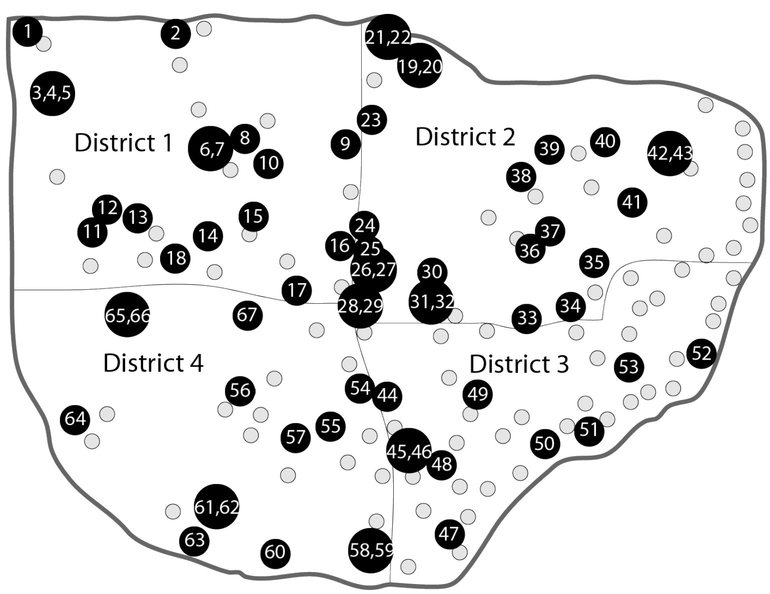


Figure 5. 67 Clusters Selected for Sampling by PPS, Wobelleno Province



It may be the case that some of the clusters selected for sampling are later found to be inaccessible due to travel difficulties or security concerns. This situation should be avoided as much as possible as the inability to collect data from any of the original clusters selected for sampling can bias the results obtained. If it is known in advance that there are PSUs in the assessment area that will not be able to be sampled due to the security situation or travel difficulties, then those PSUs should not be included in the sampling frame used for the PPS cluster selection. PSUs contained within the assessment area but not included in the sampling frame should be explicitly mentioned in the results report as the data collected in the survey are not representative of those PSUs. Data collected in population-based surveys are only representative of those areas that were ever included in the sampling frame, and individuals and/or households that ever had a chance for inclusion in the sample.

If it is not known in advance that certain PSUs are inaccessible and these PSUs are selected for sampling, replacement clusters should be selected for sampling. There is no perfect solution to replacing clusters that have been selected randomly for sampling but cannot be accessed. One reasonable approach is to select the same number of replacement clusters as could not be accessed by PPS from the sampling frame of PSUs. This would be done following steps 1 thru 6 in Box 1 – with the inaccessible clusters removed from the sampling frame. Any time such a replacement is made of an original cluster selected for data collection, the situation and all related decisions must be clearly documented and reported along with the results of the survey. The situation is undesirable as it can raise questions about the validity of the results and should therefore be avoided as much as possible.

2.4 Second Stage of Sampling:[8] Selection of Observations

There are many appropriate methods for selecting the observations to be sampled within a cluster. Three of the most commonly used and well-accepted methods for selection of observations for a 30x30 cluster survey are: 1. random walk; 2. compact segment sampling; and 3. simple random sampling (refer to the Box 4 for advantages and disadvantages of each). These same methods are also appropriate to use with the 33x6 and 67x3 designs. Of the above methods, the random walk method is most frequently used in emergency settings. Detailed instructions for carrying out the random walk method are provided in Box 5.

[8] In certain contexts, it may not be possible to obtain estimates of the population size of small geographic units, such as villages, comprising the assessment area. When this occurs the geographic unit selected for sampling may be too big to implement the second stage of sampling using the instructions provided here. In these cases, an intermediate stage of sampling can be added, between stages 1 and stage 2. For guidance on how to carry out an intermediate stage of sampling refer to Appendix 6.

BOX 4. ADVANTAGES AND DISADVANTAGES OF COMMONLY USED METHODS FOR SELECTION OF OBSERVATIONS WITHIN A CLUSTER

Random Walk. This method is most frequently used in emergency settings because it is easiest to implement. With this method, the principle of maintaining a strictly equal probability of selection is not maintained since the observations (children and/or households) sampled in the cluster are selected not by random selection but by geographic proximity to the first household randomly selected. The method also does not maintain an approximate self-weighting sample if population growth has been uneven across clusters in the time since the population estimates of the clusters were made. The difference in results due to this possibility is usually of less concern in emergencies, where accurate population estimates are especially difficult to obtain and complete mapping and enumeration of clusters is often not possible due to the security situation and rapidity with which information about the population are needed. Despite the shortcomings of the method, the random walk method is still widely adopted and accepted as an appropriate method to select observations within a cluster in emergency settings.

Compact Segment Sampling. This method can be difficult to carry out if both child- and household-level data will be collected. Achieving an approximately self-weighting sample can be particularly challenging when this method is used if the segments for sampling are rigidly defined. Sample weights may need to be applied to the data at the analysis stage. Like the random walk method, a strictly equal probability of selection is not maintained because the observations (children and/or households) included in the sample are selected by proximity, namely, their membership in the compact segment selected for sampling. The method also does not maintain an approximate self-weighting sample if population growth has been uneven across clusters in the time since the population estimates for the clusters were made.

Simple Random Sampling. This method is the preferred method for selecting observations within a cluster as it ensures that each observation has the same probability for selection so long as population growth has been even across clusters in the time since the population estimates of the clusters were made. The method is rarely used in an emergency setting, however, as it requires a complete listing (or enumeration) of all the observations (children or households) residing in the clusters selected for sampling.

BOX 5. INSTRUCTIONS FOR CARRYING OUT THE RANDOM WALK METHOD

STEP 1

Greet Community Leader and Seek Permission to Conduct Survey

When an interview team arrives at a cluster site (e.g., village) selected for sampling, a community leader is located and informed about the arrival of the interview team. The community leader should already be aware that the survey is being conducted as official notification should have been made earlier to sensitize all communities in the assessment area about the survey activity. The interview team reminds the community leader about the purpose of the survey and how the data collected will be used, and proceeds by seeking permission from the community leader to collect data from randomly selected households in the community.

STEP 2

Explain the Random Selection Process

It is recommended that the interview team requests to be accompanied by the community leader or another respected member of the community during data collection at the cluster site. The community leader/member is a useful guide; S/he knows the village environment, and can introduce the team to the households selected for sampling. The team should describe to the community leader/member the importance that a random procedure be used to select the households to be sampled. The method that will be used to select households should be explained so that the process for household selection is transparent and well understood before the selection procedure has begun.

STEP 3

Identify the Center of the Cluster Site

Once the community leader/member has been informed about the how the households will be selected for sampling, the interview team requests the community leader/member to lead them to the center of the cluster site.

STEP 4

Spin the Pen

At the center of the cluster site, a ball point pen is thrown upward into the air and allowed to fall to the ground without any interference. This step can be carried out by the community leader/member, if s/he is interested in participating in the selection process. After the pen has dropped, the interview team, along with the community leader/member, should take careful note of the direction the ball of the pen is pointing. This is the direction that has been randomly selected for the interview team to walk in order to identify the first random household to be sampled in the cluster.

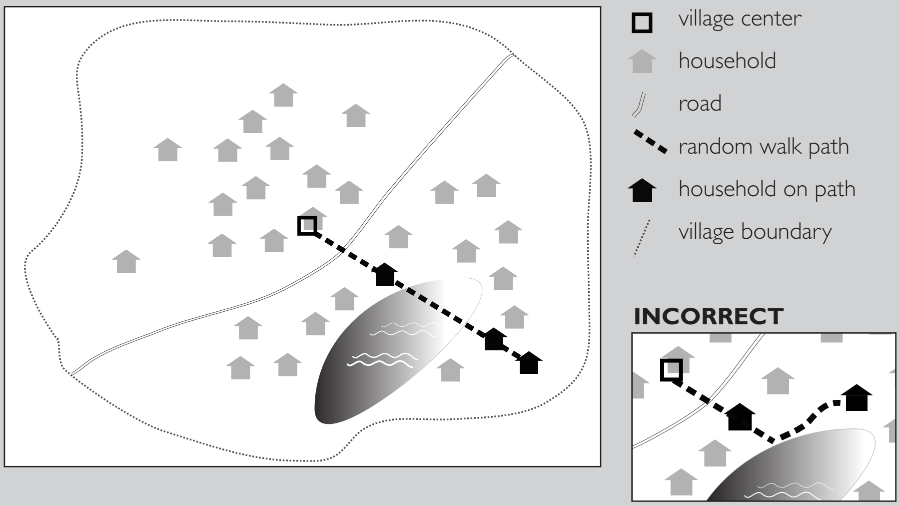
STEP 5

Map and Enumerate Households in Randomly Selected Direction

The interviewer team, preferably with the community leader/member, walks from the center of the cluster site to the perimeter of the cluster site, in the direction indicated by the ball of the pen. Households that lie approximately along the line extending from the center of the cluster site to the perimeter of the cluster site in the direction of the ball of the pen are mapped and enumerated. If the community leader/ member knows the name of the family residing in the households being mapped, this information is useful to write down as it can facilitate identification of the first random household to be sampled in **Step 7.**

STEP 5
continued

In carrying out this step, interview teams must be careful not to take the easiest path of walking that lies near the direction indicated by the ball of the pen. Even if walking in the indicated direction is difficult, this is the direction along which the households in the cluster need to be mapped and enumerated. It is important that this detail be well addressed during interviewer training and pilot testing of the questionnaire.



STEP 6

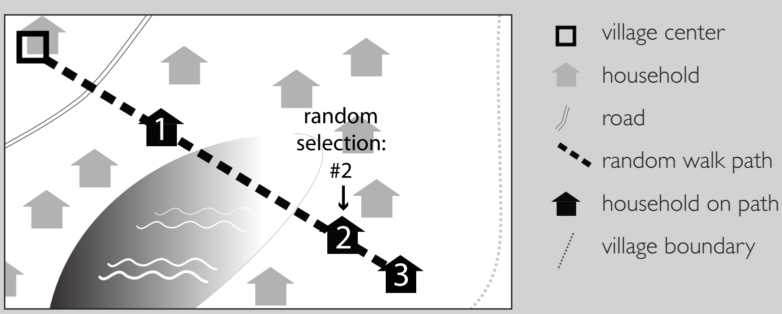
Select a Random Number

Once all of the households along the indicated path are mapped and enumerated, a random number is blindly selected from a random number table. The community leader/member can also carry out this step. The random number selected should fall between 1 and the total number of households enumerated during the walk to the perimeter of the village. If the random number selected is greater than the total number of households enumerated a new random number should be selected.

STEP 7

Identify the First Random Household to Sample in the Cluster

The enumerated household **(Step 5)** that had been assigned the random number is the first random household selected for sampling in the cluster. Depending on the dispersion of the households, it can sometimes be difficult to locate the correct household by the assigned enumeration number alone. It is for this reason that the family name of the household can be useful to write down during the enumeration/ mapping process in **Step 5**. If a family name is recorded, the community leader/ member can usually lead the interview teams to the random household selected without difficulty.



STEP 8

Select Subsequent Households to Sample in the Cluster

Once the first random household has been selected, the remaining households to be sampled in the cluster can be selected by proximity. The method for selection of the subsequent households should be defined clearly for interview teams from the outset so that the same method is used in all clusters. The more detailed the specifics of the sampling protocol, the more standardized the household selection process is for all interview teams, and the less chance there is for sampling bias to affect the survey results.

Interview teams should be given specific instruction as to whether it is the next nearest household to the right or to the left that should be sampled next. A protocol should also be outlined to address how interview teams should proceed if there is ever an instance where there are no more houses in the direction in which sampling is meant to proceed– or, in the case that the interview team has reached the perimeter of the cluster during the household sampling process.

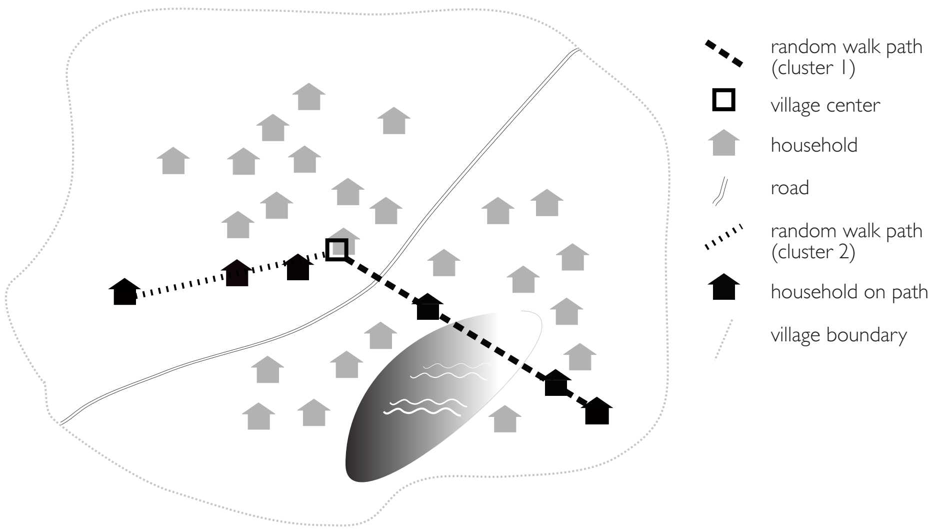
The sampling protocol must also outline how to handle situations when the selected individual, or household, is not at home at the time of the interview team's visit. It is important that at least one follow up attempt be made to reach any individual and/or household selected for sampling but not available during the initial visit by the interview team.

An example of the type of detailed instructions that should be outlined for selecting households within each cluster is provided in Appendix 7.

2.4.1 Special Cases: Selection of a PSU for Multiple Clusters of the Survey

It can happen that the same PSU is selected for more than one cluster of data collection. In principle, there is no problem with the same PSU being selected for multiple clusters of the same survey. However, when this situation occurs, it is essential that the second stage of sampling be carried out independently for each of the clusters. If the random walk method is being used, this means that interview teams must conduct the random walk procedure once for each cluster of data to be collected in the PSU (refer to Figure 6). At the same time, it is important to ensure that no individual or household be included in the sample more than once. Therefore, in cases where one PSU is selected for multiple clusters, interview teams should be made aware that other clusters of data will be collected in the same PSU and find methods to ensure that the same individuals and households are not sampled more than once.

Figure 6. Example of Conducting the Random Walk Procedure for Two Clusters of Data to be Collected in One PSU



2.5 Questionnaire Administration

Questionnaire administration for the 33x6 and 67x3 designs should follow the same principles as would be followed for the 30x30 design. In emergencies, the standard practice is to collect data on ***all*** children of the target age range (usually 6-59 months)[9] who live in a household selected for sampling. This protocol helps to ensure that children are not excluded from sampling due to sickness, and that a child currently living in a household but not present at the time of sampling will be followed up for inclusion in the child sample. The protocol also makes data analysis more straight-forward, since collecting data on all eligible children living in a household maintains an approximately equal probability of selection for all children included in the sample.

For a 33x6 survey collecting data on both child- and household-level indicators, the required number of children to be sampled is 198 and the required number of households is, likewise, 198. For the 67x3 design, the required number of children and households to be sampled is 201. A cluster for a 33x6 survey, in which data are collected on both child- and household-level indicators, should therefore only be considered complete when data have been collected on a minimum of 6 children in a cluster and a total of 6 households. Likewise, a cluster for a 67x3 design should only be considered complete when data have been collected on a minimum of 3 children in a cluster and a total of 3 households.

In emergencies, the child section of the questionnaire is administered as many times as there are children of the target age range living in the household, while the household section of the questionnaire is administered once at each household selected for sampling. Whereas 6 households per cluster will always be required to fulfill the household sample required for the 33x6 design (and, similarly, 3 households per cluster will be required to fulfill the household sample for the 67x3 design), the number of households that need to be sampled to obtain data on the minimum required number of children per cluster cannot be defined in advance. This is because not every household sampled for household-level indicators will have children of the target age range and, certain households sampled will have multiple children of the target age range.

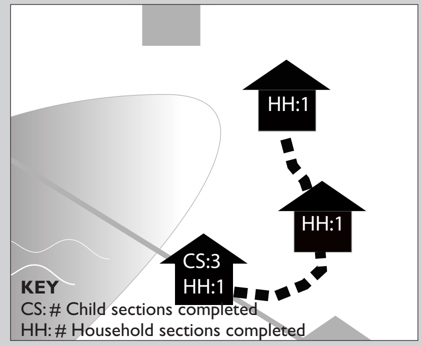
9 In emergencies, indicators are often reported for only a broad age range of children, such as 6-59 months, or sometimes 6-23 and 24-59 months. If information is needed about more narrow age ranges (e.g., 0-5 months, 12-23 months), a modified sampling approach may be needed to ensure adequate sample sizes for the indicators will be available at analysis.

The instructions in Box 6 describe how to administer the child and household sections of the questionnaire when a different number of households need to be visited in a cluster to fulfill the respective child and household cluster sample size. The corresponding figures provide an illustration of how each different scenario should be addressed. For each scenario, the 67x3 design is used in way of example.

BOX 6. INSTRUCTIONS FOR ADMINISTERING THE CHILD AND HOUSEHOLD SECTIONS OF THE QUESTIONNAIRE

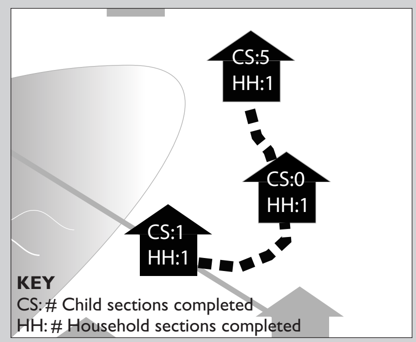
Scenario One: The necessary sample of children is fulfilled before the necessary sample of households.

Subsequent households in that cluster should only be administered the household section of the questionnaire since the required child sample has already been obtained for that cluster.



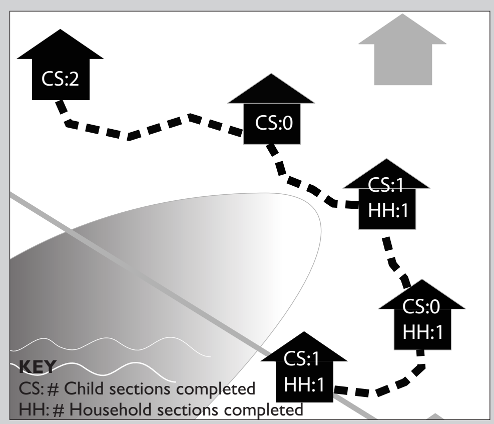
Scenario Two: A household has more children of the targeted age range than are necessary to fulfill the child sample required for the cluster.

Data should be collected on all children in the household, not just the number of children required to fulfill the child sample for the cluster. As a result, it is expected that certain clusters will have data on more than the necessary number of children.



Scenario Three: The required sample of children in a cluster is not yet fulfilled though the number of households required for the household sample has already been obtained in the cluster.

Sampling needs to continue, but only the child section of the questionnaire should be administered at the remaining households sampled in the cluster. By doing so, the approximate self-weighting principle of the household sample is maintained since all clusters will have data collected from the same numbers of households.



2.6 Data Analysis and Reporting

Once the data for the 33x6 or 67x3 design have been collected, the data need to be entered, cleaned, and analyzed. The process for entering and cleaning data collected with the 33x6 or 67x3 design is no different than it would be for data collected using a 30x30 design. As always, proper quality assurance procedures should be taken, including verification of a sub-sample of data so far as time, logistic arrangements, and field circumstances allow, and double data entry, whenever possible. Before undertaking any analysis for reporting, the data should be cleaned of implausible values, including setting flagged anthropometric data[10] to missing. Once the data set is clean, the data are ready for analysis.

Two types of analyses can be performed with data collected using the 33x6 or 67x3 design: 1) tabulation of point estimates and 95% CIs; and 2) LQAS analysis to assess threshold levels of acute malnutrition.

2.6.1 Tabulation of Point Estimates and 95% CIs

Tabulation of point estimates for child- and household-level indicators is straightforward. For binary indicators, tabulation of an indicator involves calculating the proportion of the sample that exhibit the outcome of interest. The resulting proportion is referred to as a point estimate for the indicator. For indicators based on a continuous value, the tabulation process usually involves calculating the mean value for the sample. The resulting mean value for the indicator is also referred to as a point estimate. For all indicators tabulated, whether they are binary indicators or indicators based on a continuous value, a 95% CI should also be calculated. Only by calculating the 95% CI for a point estimate is it possible to know the precision of the estimate obtained (refer to Section 1.2.2).

To properly calculate a 95% CI for an indicator collected with a cluster survey, the sampling design used to collect the data must be accounted for. The concept is referred to as the ***design effect***, and is fundamental to proper analysis of data collected by a cluster survey.

The design effect is a measure of the extent to which a cluster design is compromised in its ability to capture the true heterogeneity of the population in the assessment area. Formally, a design effect (Deff) is defined as the ratio of the actual variance of a cluster sample to the variance of a SRS of the same sample size (Kish 1995). The greater the design effect, the greater the difference between the data collected by a cluster design and a SRS of the same sample size. A smaller design effect is always desirable.

The design effect will vary by indicator and the type of sampling design used to collect the data. Whereas the design effect for a SRS is generally assumed to be 1.0 for all indicators, the design effect for a cluster design—no matter the sample size—will almost always be larger than 1.0.

The formula to estimate the design effect for data collected with a cluster design is shown below (Kish 1995).

Formula 1.

$$\text{Deff} = 1 + roh\,(b-1)$$

where roh = the intra-cluster correlation and b = the cluster size (number of observations sampled per cluster)

From Formula 1, it can be seen that there are two factors that determine the design effect for an indicator: 1) the intra-cluster correlation for the indicator; and 2) the sampling design used to collect the data. The first element is a measure of the homogeneity among observations within a cluster with respect to a certain outcome. It is measured by a statistic called ***roh***, which in population-based surveys is also referred to as the intra-cluster correlation coefficient (ICC).

Like the design effect, the ICC varies by indicator. For population-based surveys conducted in emergency settings, the ICC for most indicators will be greater than 0.0

[10] Software programs that calculate anthropometric z-scores against the NCHS 1977 child growth references or the WHO 2005 child growth standards (e.g., Epi-Nut 6.0, WHO Anthro 2005) automatically generate flags to highlight z-scores that should be checked for data entry or recording error.

and less than 0.70. The ICC will remain relatively constant across different sampling designs measuring the same indicator among a population, which explains why the design effect for an indicator varies most substantially according to the sampling design used to collect the data. To illustrate, a hypothetical example is provided in Box 7.

When a constant ICC is assumed for an indicator, the design effect will always be highest for the 30x30 design and lowest for the 67x3 design. This is due to the larger number of observations sampled per cluster when the 30x30 design is used, as compared to the 33x6 and 67x3 designs. When observations (children or households) are sampled within geographically defined clusters, they are often more similar to one another than they would be if the observations had instead been randomly selected one by one from a complete sampling frame, as is the case with SRS. Thus, the more observations sampled per cluster, the less the sampling design will capture the same level of heterogeneity of the assessment area as a SRS of the same sample size.

The ICC used in the example below is relatively low, at roh = 0.10. With roh = 0.10, the design effect for the 33x6, 67x3, and 30x30 designs is calculated as 1.5, 1.2 and 3.9 respectively. Using an example with roh = 0.40 illustrates how a higher ICC impacts the design effect for each sampling design. Applying Formula 1, the design effect for the 33x6, 67x3, and 30x30 designs is now calculated as 3.0, 1.8, and 12.6, respectively.

BOX 7. EXAMPLES OF ESTIMATING THE DESIGN EFFECT

Assume it is known that the intra-cluster correlation of an indicator is usually around 0.10. The design effect for the indicator could then be estimated by applying information about the sampling design used for data collection to Formula 1.

Example 1: Estimating the Design Effect for Data Collected with a 33x6 Design

To estimate the design effect for data collected with a 33x6 design, b = 6 and roh = 0.10. The design effect for the indicator is estimated as 1.5.

Deff = 1 + 0.10 (6-1) = 1 + 0.10 (5) = 1 + 0.50 = 1.5

Example 2: Estimating the Design Effect for Data Collected with a 67x3 Design

The design effect for the same indicator collected with a 67x3 design is 1.2. Here, b = 3 and roh = 0.10.

Deff = 1 + 0.10 (3-1) = 1 + 0.10 (2) = 1 + 0.20 = 1.2

Example 3: Estimating the Design Effect for Data Collected with a 30x30 Design

For the 30x30 design, b = 30 and roh = 0.10. The design effect is 3.9

Deff = 1 + 0.10 (30-1) = 1 + 0.10 (29) = 1 + 2.9 = 3.9.

The 30x30 design again has the highest design effect and the 67x3 design again has the lowest design effect. Now, however, the difference in the design effect between sampling designs is more substantial. This is because the larger cluster size of the 30x30 design interacts with the high ICC to further diminish the ability of the 30x30 data to capture the true heterogeneity of the assessment area. The loss is much less severe for the 33x6 and 67x3 designs since these sampling designs each have smaller cluster sizes. In Table 4, the design effect for a range of ICCs is shown by sampling design.

The design effect impacts the calculation of the 95% CI for an indicator by effectively reducing the sample size available for analysis. The concept is referred to as the ***effective sample size*** (ESS) of a cluster design, and is closely related to the design effect. The ESS is the ratio of the sample size available for analysis of the indicator (n) to the design effect for that indicator (Kish, 1995):

Formula 2.

$$ESS = n / Deff$$

For the first example presented (Box 7) where roh = 0.10, the ESS for the 33x6, 67x3 and 30x30 design is 132 (198/1.5), 167 (201/1.2), and 230 (900/3.9), respectively. The ESS for indicators with higher ICCs is shown in Table 5 for each sampling design.

The ESS is important because it represents the number of observations that would be required in a SRS design in order to provide the same level of precision (i.e., width of CI) as obtained with the cluster design used. A higher ESS is always preferred.

Table 4. Design Effect by Intra-cluster Correlation of Indicator and Sampling Design

Sampling Design	Intra-cluster Correlation (ICC) of Indicator						
	roh=0.10	roh=0.20	roh=0.30	roh=0.40	roh=0.50	roh=0.60	roh=0.70
33x6 Design	1.5	2.0	2.5	3.0	3.5	4.0	4.5
67x3 Design	1.2	1.4	1.6	1.8	2.0	2.2	2.4
30x30 Design	3.9	6.8	9.7	12.6	15.5	18.4	21.3

Table 5. Design Effect and Effective Sample Size by Intra-cluster Correlation of Indicator and Sampling Design

Sampling Design (sample size)	Intra-cluster Correlation (ICC) of Indicator						
	roh=0.10	roh=0.20	roh=0.30	roh=0.40	roh=0.50	roh=0.60	roh=0.70
33x6 Design (n=198)	Deff=1.5 ESS=132	Deff=2.0 ESS=99	Deff=2.5 ESS=79	Deff=3.0 ESS=66	Deff=3.5 ESS=56	Deff=4.0 ESS=49	Deff=4.5 ESS=44
67x3 Design (n=201)	Deff=1.2 ESS=167	Deff=1.4 ESS=143	Deff=1.6 ESS=125	Deff=1.8 ESS=111	Deff=2.0 ESS=100	Deff=2.2 ESS=91	Deff=2.4 ESS=83
30x30 Design (n=900)	Deff=3.9 ESS=230	Deff=6.8 ESS=132	Deff=9.7 ESS=92	Deff=12.6 ESS=71	Deff=15.5 ESS=58	Deff=18.4 ESS=48	Deff=21.3 ESS=42

Since there is an effective loss of sample size due to the use of clusters, correct tabulation of CIs for an indicator collected with a cluster design requires adapting the 95% CI formula that would be used for a SRS. The design effect (or ESS) must be accounted for in the CI calculation. Otherwise, the precision of the survey results will likely be overestimated, and misrepresented – which can consequently affect the extent to which decisions can be made reliably from the results of the survey. Thus, whereas the formula to calculate a 95% CI for a binary indicator collected with a SRS is:

Formula 3.

p +/- [[sqrt [[(p) (1-p)] /n]] * [1.96]]

where p is the point estimate, and n is the sample size available for analysis of the indicator;

the formula to calculate a 95% CI for a binary indicator collected with a cluster sample is:

Formula 4.

p +/- [[sqrt [[(p) (1-p)] / (n/deff)]] * [1.96]]

which is equivalent to:

Formula 5.

p +/- [[sqrt [[(p) (1-p)] / ESS]] * [1.96]]

Formulas 4 and 5 illustrate how the design effect and total sample size available for analysis together determine the precision of an estimate produced by data collected with a cluster design. With a cluster design, it is the ESS rather than the actual number of observations collected that is used in the calculation of the 95% CI.

Figures 7 and 8 on the next page show point estimate results and 95% CIs for one child-level and one household-level indicator collected from a field application of the 33x6, 67x3, and 30x30 designs. The first result (labeled w/Deff) for each sampling design shows the CI for the indicator when the design effect has been accounted for. This is the correct calculation. The second result (labeled w/o Deff) for each design shows the CI for the indicator when the design effect is not taken into account. This is statistically incorrect and, as highlighted by the figures, would suggest a more precise estimate than was actually the case.[11]

Calculation of CIs that properly account for the design effect of an indicator require the use of special software such as the CSample sub-routine of EpiInfo 6.0, the Complex Samples module in SPSS, or the survey data analysis function in STATA. If unsure about how to use these software applications to estimate and account for the design effect of indicators collected with a cluster design, seek technical assistance. It is important not to overlook this important step in data analysis and reporting. Accounting for the design effect in the tabulation of CIs is necessary to gain a proper understanding of the extent to which the estimates of the survey are reliable and can help to define the parameters by which the change in an indicator can be statistically detected over time.

2.6.2 LQAS Analysis and Reporting

In addition to point estimate and CI tabulation, the 33x6 and 67x3 designs also allow for LQAS analysis of the prevalence of acute malnutrition. As described earlier (refer to Section 1.1), LQAS analysis provides a method to classify whether an outcome is at or above a critical threshold level. Using data collected by the 33x6 or 67x3 design, the number of children with acute malnutrition can be counted and compared against a decision rule to make a classification as to whether a threshold prevalence level has been reached or exceeded in the population.

The type of LQAS analysis we describe for the 33x6 and 67x3 designs is relevant only for binary data. Therefore, a cumulative binomial distribution is used for LQAS analysis. The binomial distribution is based on the probability of an outcome occurring given there are two distinct possibilities that could occur for each observation sampled. In the LQAS classification procedure defined for use with the 33x6 and 67x3 designs, the binary outcome of interest is acute malnutrition. The two distinct possibilities that could occur are: 1) the child is acutely malnourished; 2) the child is not acutely malnourished.

[11] The point estimate for an indicator remains the same whether the design effect is accounted for or not.

Figure 7. Example of Correct and Incorrect Confidence Interval Calculation: 33x6, 67x3 and 30x30 Results for Measles Vaccination Coverage

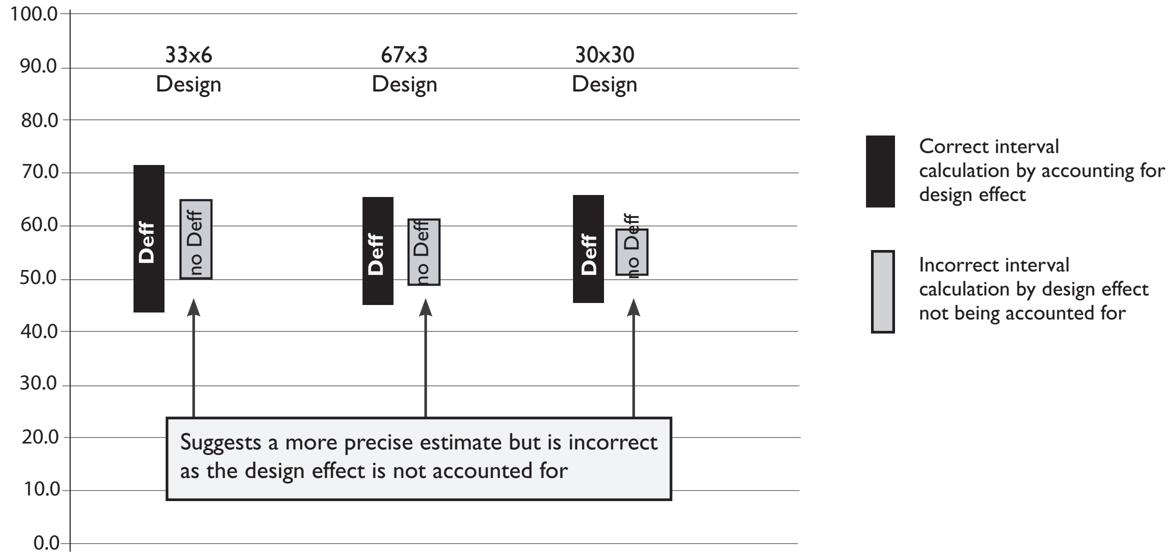
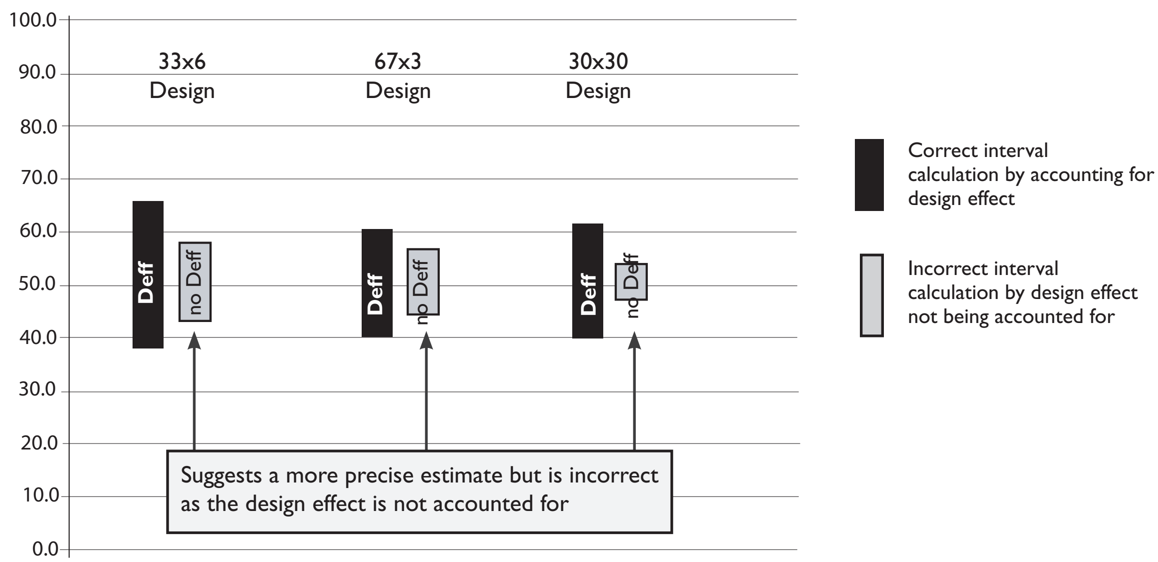


Figure 8. Example of Correct and Incorrect Confidence Interval Calculation: 33x6, 67x3 and 30x30 Results for Access to Latrine



Certain elements of hypothesis testing are relevant to LQAS analysis. The LQAS classification procedure, for example, can be expressed in a form analagous to that of a hypothesis test.

Formula 6.

$$H_0: p \geq p_0 \text{ vs. } H_a: p < p_0$$

where p is the true prevalence and p_0 is the prevalence level the data are classified against.

There are, however, several characteristics that distinguish LQAS analysis from a classic hypothesis test. LQAS analysis is unique from classic tests of hypothesis in its use of upper and lower thresholds. The upper threshold in LQAS is the threshold level against which the data will be classified, p_0 (in Formula 6). The 33x6 and 67x3 designs allow for classifying the prevalence of acute malnutrition against the upper thresholds of 10%, 15%, or 20%. In contrast to a classic hypothesis test, LQAS analysis requires that a corresponding lower threshold also be defined. Whereas the upper threshold for acute malnutrition is a critical prevalence level which it is imperative to know if the population has reached and/or exceeded, the lower threshold for acute malnutrition is a prevalence level that is sufficiently low so as not to be of as imminent a concern (Deitchler et al. 2007). In the context of the 33x6 and 67x3 designs, the lower thresholds are set at 5%, 10%, and 15% for the respective 10%, 15% and 20% upper thresholds.[12]

As with any other type of analysis of population-based data, there is statistical error with LQAS analysis. When describing the statistical error associated with classic hypothesis tests, the concepts of type I and type II errors are often used. These concepts are also relevant to LQAS analysis. A type I error occurs when the "null hypothesis" (H_0) is incorrectly rejected. A type II error occurs when the "null hypothesis" is incorrectly not rejected (refer to Table 6).

When using LQAS analysis to assess if the prevalence of acute malnutrition has reached or exceeded the threshold of 15%, the "null hypothesis" is that the prevalence of acute malnutrition is ≥15%. The "alternative hypothesis" is that the prevalence of acute malnutrition is < 15% (refer to Formula 6). A type I error therefore means the area is classified as having an acute malnutrition prevalence < 15% when the true prevalence is ≥15%. A type II error means the area is classified as having an acute malnutrition prevalence ≥15% when the true prevalence is < 15%. These type I and type II errors are also commonly referred to as alpha and beta errors.[13]

[12] A five percentage point spread between the upper and lower thresholds was selected as the delimitation between thresholds for acute malnutrition prevalence to maintain consistency with the acute malnutrition threshold levels most commonly used to classify the severity of a situation (WHO 1995).

[13] In LQAS, these errors are also commonly known as the consumer and producer risk.

Table 6. Type I and Type II Errors Associated with LQAS Analysis

Decision of LQAS Analysis	True Prevalence of Acute Malnutrition	
	Acute malnutrition prevalence is ≥ the threshold level analyzed	Acute malnutrition prevalence is < the threshold level analyzed
Conclude the acute malnutrition prevalence is < the threshold level analyzed	Type I error (alpha error)	Correct decision
Conclude the acute malnutrition prevalence is ≥ the threshold level analyzed	Correct decision	Type II error (beta error)

As with classic hypothesis tests, the desired limits of the alpha and beta error for the LQAS classification procedure should be determined before data collection. This is necessary to ensure that the sample size selected will be sufficient to maintain the tolerable level of error. The 33x6 and 67x3 designs provide the minimal sample size necessary to maintain alpha errors approximately ≤ 0.10 and beta errors approximately ≤ 0.20 for LQAS analysis of the prevalence of acute malnutrition against the 10% and 15% threshold levels.[14] As is the case with hypothesis testing, the smaller the desired error, the larger the sample size needed. Sample sizes smaller than the n=198 and n=201 required by the 33x6 and 67x3 would therefore cause an inflation of the errors associated with LQAS analysis. Also, as is usually the case with hypothesis testing, only one threshold level should be analyzed with data collected using the 33x6 and 67x3 designs. Carrying out LQAS analysis for multiple thresholds will inflate the alpha and beta errors.

The importance of the lower threshold with respect to the precision of the LQAS analysis warrants explanation. If the desired sample size for the design is met, one can be reasonably confident that the desired limits of the alpha and beta errors will be approximately maintained for an assessment area with a true prevalence at or above the upper threshold level; and for an area with a true prevalence at or below the lower threshold level. For an assessment area where the true prevalence lies between the upper and lower threshold, the desired limit for the beta error will be exceeded. This concept is described more fully below.

In LQAS applications, it is the populations most in need (i.e., where acute malnutrition prevalence is ≥ upper threshold) and the populations least in need (i.e., where acute malnutrition prevalence is ≤ lower threshold) that are correctly classified within the stated alpha and beta error limits. Populations with a true prevalence falling in-between those upper and lower thresholds are classified with higher error (Deitchler et al. 2008). Such areas are defined de-facto by LQAS as a second tier priority – they are neither the first priority for humanitarian assistance (because the true prevalence of acute malnutrition does not exceed the upper threshold) nor are they a last priority for humanitarian assistance (because the true prevalence of acute malnutrition is not less than the lower threshold). The prevalence falls in a middle '***grey area***,' but LQAS must still produce a classification – and it does so, but with a higher probability for misclassification (Valadez 1991).

The precision of the LQAS classification procedure is defined, in part, by this grey area between the upper and lower threshold (i.e., the difference in percentage points between the upper and lower threshold). The smaller the spread between the upper and lower threshold, the more precise the analysis. The larger the spread between the upper and lower thresholds, the less precise the analysis.

As might be expected, the sample size required for a sampling design will vary depending on the precision desired for the analysis: the smaller the difference between the upper and lower thresholds, the bigger the sample size needed. If the difference between the upper and lower thresholds were smaller than the five percentage point difference used for the 33x6 and 67x3 designs, a bigger sample size would be needed to maintain the desired error limits.

Let us return to the example of Wobelleno. If the true prevalence of acute malnutrition in Wobelleno was known to be between the lower threshold of 10% and the upper threshold of 15%, we would then expect an increased likelihood (beyond the defined tolerable beta error) for misclassification at or above the 15% threshold (type II error). The probablility that the area would be classified as <15% would decrease from approximately 80% to 10%, depending on how close the true prevalence is to 15%. This is of minor consequence, however, because, with LQAS, areas with a true prevalence closer to the upper threshold are more likely to be classified as at or above that threshold than areas with a true prevalence closer to the lower threshold. It is probable therefore that an area wrongly classified as having an acute malnutrition prevalence exceeding the upper threshold

14 Alpha and beta errors are slightly higher for assessment of the prevalence of acute malnutrition at the 20% upper threshold (refer to Table 7).

level does, in fact, have a prevalence of acute malnutrition that is closer to that threshold than not.

From a humanitarian standpoint, this type of classification system makes sense: Those areas near the upper threshold level are likely also in need of assistance, and, if we were able to know the true prevalence level among those populations, those areas would almost certainly be identified as a secondary priority for humanitarian response.

When assessing the prevalence of acute malnutrition, it is a less critical error to incorrectly identify an area that is below the upper threshold than to fail to identify an area that is above it. Such classification systems are particularly relevant in emergency settings, when lives can depend on the timely identification of areas requiring humanitarian assistance (Deitchler et al. 2008).

To use LQAS to analyze acute malnutrition data, minor preparation of the data set is required. Because data on all children in the household are collected, it can happen that some clusters have more than the minimum required number of children per cluster (i.e., more than the 6 children per cluster for the 33x6 design; more than the 3 children per cluster for the 67x3 design). For any other sort of analysis, the excess number of children sampled per cluster is of minor consequence. For LQAS analysis, however, the minimum number of children per cluster should not be exceeded (refer to Appendix 1).

To prepare the data set for LQAS analysis, first, the clusters with acute malnutrition data collected on more than the minimum required number of children should be identified. Those clusters with excess data on acute malnutrition need to be reduced to the appropriate sample number by using a random selection procedure.[15] The process should be carried out cluster by cluster, so that children are randomly selected for exclusion from each oversized cluster in the data set. After data preparation, the sample size available for analysis should be 198 for the 33x6 design and 201 for the 67x3 design. If there are flagged anthropometric data in any clusters which did not have excess children sampled, the sample size could be less.

The above data preparation steps do not apply to other types of analyses.[16] To tabulate the point estimates and 95% CIs described earlier in this Section, the full data set can be used without modification. The sample size for point estimate tabulation of child-level indicators (including acute malnutrition) will therefore usually be more than 198 and 201, respectively. Because the exclusionary selection process is only necessary for the LQAS analysis, it is strongly recommended that a copy of the original data set be made to carry out the this analysis,[17] and that this is the only data set from which the random exclusions of extra children in oversized clusters are made.

Once the dataset has been prepared for LQAS analysis, carrying out the classification procedure is simple. Only two steps are required as shown in Box 8 on the next page.

The pre-established decision rules, along with the approximate alpha and beta errors, are shown for the 33x6 and 67x3 designs in Table 7. If, after data cleaning, the sample size is less than the full 198 or 201, an alternative

Table 7. Decision Rules (DR), Alpha (α) and Beta Errors (β)[18] for the 33x6 and 67x3 Designs: Upper thresholds of 10%, 15%, and 20%

Sampling Design	Sample Size	Upper Threshold Level for Assessing the Prevalence of Acute Malnutrition		
		10% Acute Malnutrition Decision Rule (DR) to classify the prevalence < 10%	15% Acute Malnutrition Decision Rule (DR) to classify the prevalence < 15%	20% Acute Malnutrition Decision Rule (DR) to classify the prevalence < 20%
33x6 Design	198	≤ 13 (α≤ 0.08 , α≤ 0.15)	≤ 23 (α≤ 0.13 , α≤ 0.21)	≤ 33 (α≤ 0.17 , α≤ 0.24)
67x3 Design	201	≤ 13 (α≤ 0.06 , α≤ 0.14)	≤ 23 (α≤ 0.10 , α≤ 0.21)	≤ 33 (α≤ 0.13 , α≤ 0.26)

15 It is recommended that a random number generator be used for this step.

16 Different sample sizes for each cluster can cause a sample to no longer be approximately self-weighting, however, this is usually not of concern in emergency settings, or a developing country context, where the probability for selection can only be approximately equal, at best, given the likely inaccuracy of the population estimates for the PSUs included in the sampling frame. If, however, there is large variation in the sample size from each cluster available for analysis, it might be advisable to account for the different cluster sample sizes in analysis, or to randomly reduce the cluster size as is necessary for the LQAS analysis.

17 Alternatively, the observations that are selected for random exclusion can be set to missing for the purpose of the LQAS analysis.

18 These alpha and beta errors are approximate and assume an ICC ≤ 0.05. The exact alpha and beta errors will depend on the intra-cluster correlation of the acute malnutrition data collected. For more information on the range of alpha and beta errors that could be expected given varying levels of intra-cluster correlation, refer to Olives et al. 2009.

BOX 8. INSTRUCTIONS FOR CARRYING OUT LQAS ANALYSIS WITH THE 33X6 OR 67X3 DESIGNS

STEP 1 Count the number of observations in the sample with acute malnutrition.

STEPS 2a - 2b Compare the number of observations in the sample with acute malnutrition against the pre-established decision rule for the threshold level being tested:

2a. If the number of observations with the outcome of interest is equal to or less than the pre established decision rule, classify the prevalence of acute malnutrition in the population as less than the threshold prevalence analyzed.

2b. If the number of observations with the outcome of interest is greater than the pre established decision rule, classify the prevalence of acute malnutrition in the population as equal to or greater than the threshold prevalence analyzed.

decision rule table should be used. Appendix 8 provides decision rules along with the approximate alpha and beta errors to assess the 10% and 15% upper thresholds of acute malnutrition prevalence using smaller than the ideal sample sizes of 198 and 201 for the 33x6 and 67x3 designs.

Examples demonstrating how to use LQAS to analyze acute malnutrition data collected with a 33x6 and 67x3 design are provided in Boxes 9 and 10. In each case, we refer back to the Wobelleno example.

BOX 9. EXAMPLE OF LQAS ANALYSIS WITH THE 33X6 DESIGN

A 33x6 survey was conducted in Wobelleno Province. Data on a total of 198 households, and 200 children were collected. In cluster 6 (Emara A) and cluster 22 (Sunata), data on 7 children were collected. To prepare the data for LQAS analysis, a random number generator is used to select a number between 1 and 7. Assume the number 4 is generated. The fourth child sampled in cluster 6 is excluded from LQAS analysis. The random number generator is also used to generate a number between 1 and 7 to select the child in cluster 22 to be excluded from analysis. Assume the number 1 is generated. The first child sampled in cluster 22 is also excluded from LQAS analysis. There are now a total of 198 children in the sample, with no more than 6 children in each cluster. The data are ready for LQAS analysis.

It is of interest to know if the prevalence of acute malnutrition in Wobelleno has reached or exceeded 15%. Among the 198 children in the sample, there are 21 children who are acutely malnourished. The decision rule for the 15% threshold with a sample size of 198 for the 33x6 design is 23 (refer to Table 7). We compare 21 with the decision rule of 23. Since 21 ≤ 23, we classify the prevalence of acute malnutrition in Wobelleno as less than the threshold level of 15%.

BOX 10. EXAMPLE OF LQAS ANALYSIS WITH THE 67X3 DESIGN

A 67x3 survey was conducted in Wobelleno Province. Data on a total of 201 households, and 204 children were collected. Data were collected on 3 children in all but one cluster. In cluster 45 (Darbu), data were collected on 6 children. To prepare the data for LQAS analysis, a random number generator is used to select three numbers (without replacement) between 1 and 6. Assume the numbers 2, 5, and 6 are generated. The second, fifth, and sixth child sampled in cluster 45 are excluded from LQAS analysis. There are now a total of 201 children in the sample, with no more than 3 children in each cluster.

It is of interest to know if the prevalence of acute malnutrition has reached or exceeded 10%. Among the 201 children in the sample, there are 15 children who are acutely malnourished. The decision rule for the 10% threshold is 13 (refer to Table 7). We compare 15 with the decision rule of 13. Since 15 > 13, we classify the prevalence of acute malnutrition in Wobelleno as equal to or greater than the threshold level of 10%.

2.7 Summary of Section Two

This Section of the Guide provided implementation instructions specific to the 33x6 and 67x3 designs. Questionnaire development, sampling, questionnaire administration, and data analysis and reporting were addressed. Detailed instruction for tabulation of point estimates and 95% CIs was provided, and the need to account for the design effect when calculating the 95% CI for an estimate was explained. The discussion highlighted the two main design factors that affect the precision of an estimate: the total sample size of the design and the cluster size (i.e., number of observations sampled per cluster). It was shown that a larger sample size positively influences the precision of an estimate while a larger cluster size negatively influences the precision of an estimate. Along with this, the role of the ICC of an indicator was highlighted. These factors should always be considered when choosing the most appropriate sampling design to use. The topic will be explored further in Section 4 of the Guide.

Also of focus in this Section was to carry out an LQAS analysis with data collected using the 33x6 and 67x3 design. Instruction for preparing data for LQAS analysis was provided, along with a decision rule table to assess the thresholds of 10%, 15%, and 20% acute malnutrition prevalence with data collected by each design. Additional considerations around the use of the 33x6 and 67x3 designs for LQAS analysis will be addressed in Section 4 of the Guide.

Instruction for Implementing the Sequential Design

SECTION 3.

The sequential design uses a sampling approach involving data collection from ***up to*** 67 clusters with three observations sampled per cluster. The purpose of the design is to use LQAS analysis to assess if the prevalence of acute malnutrition has reached or exceeded a pre-determined threshold level, and to do so more rapidly than with the 33x6 or 67x3 design. In contrast to the 33x6 and 67x3 designs, the sequential design does not allow for tabulation of the point estimate and 95% CI of any indicator, including acute malnutrition.

Because the sequential design is based on the same sampling framework as the 67x3 design, many of the same implementation steps required for the 67x3 design also apply to the sequential design. In this Section of the Guide, new information will be provided when specific instruction is required for carrying out the sequential design. Otherwise, reference will be made to previous Sections of the Guide where instructions for carrying out the 67x3 design apply equally to the sequential design.

3.1 Questionnaire Development

Since the objective of the sequential design is to use LQAS analysis to detect if a critical threshold level of acute malnutrition has been reached or exceeded, the only items required for inclusion in the questionnaire are the standard data needed to assess the outcome of acute malnutrition, namely: date of data collection, name of child, birth date of child, age of child, sex of child, weight of child, length/height of child (standing or supine), and if bilateral pitting edema is present. As always, standard identification codes for data collection are also required, including, cluster name, cluster number, household number, and child number.

3.2 Sampling Approach for the Sequential Design

The primary difference between the 67x3 and the sequential design is data analysis and reporting. Unlike the 67x3 design, the sequential design allows the assessment of threshold levels of acute malnutrition to be carried out ***before*** the full 67 clusters of data are collected. When the empirical data give a clear indication as to how to classify the prevalence of acute malnutrition in the area, data collection for the sequential design can stop. The sequential design therefore has great utility if a decision about a threshold level of acute malnutrition prevalence is needed quickly. However, the implementation of the design precludes the ability to obtain any other information from the sample, unless the full sample plan—which is equivalent to the 67x3 design discussed in Section 2—is implemented.

3.3 First Stage of Sampling: Selection of Clusters

The procedure to select clusters to be sampled for the sequential design is the same as recommended for the 67x3 design: PPS (refer to Section 2.3). Once the 67 clusters for the sequential design have been selected by PPS, the clusters can be sampled in any order.

3.4 Second Stage of Sampling: Selection of Observations

The same methods used to select the observations to be sampled within each cluster of the 67x3 design are also appropriate to use with the sequential design. Section 2.4 provides detailed instruction on how to carry out the random walk method within each cluster selected for data collection.

3.5 Questionnaire Administration

Since the sequential design can only be used for LQAS analysis of the prevalence of acute malnutrition, only child-level data are collected when using this design. As described earlier (refer to Section 2.5), data should be collected on ***all*** children of the target age range who live in a household selected for sampling.

Because of the protocol to sample all children in a household, the number of households that need to be visited to obtain data on the minimum number of children required per cluster cannot be defined in advance. In some cases, the necessary sample of children may be fulfilled in the first household selected for sampling in the cluster. In other cases, it may be necessary to visit as many as three or more households before the minimum required number of children is obtained for the cluster.

The same guidance as described in Section 2.5 applies here, namely, that all children in every household should be sampled, even if the necessary number of children to be sampled in the cluster can be obtained by sampling only some (rather than all) of the children in the last household of the cluster (refer to Scenario 2 in Box 6). As a result of this protocol, it can be expected that certain clusters will have data on more than the necessary number of children (i.e., more than 3 children). This feature of the sampling protocol is addressed in the analysis phase.

3.6 Data Analysis and Reporting

LQAS analysis is at the core of the sequential design. The type of LQAS analysis that can be conducted with the sequential design is similar to that described earlier for the 33x6 and 67x3 designs. Like the 33x6 and 67x3 designs, the sequential design allows for analysis of the 10%, 15%, and 20% threshold levels of acute malnutrition with the respective lower thresholds of 5%, 10%, and 15%. The distinguishing feature of the sequential design is the time at which the analysis is conducted. Whereas the 33x6 and 67x3 designs require LQAS analysis be carried out only after the full sample of data (n=198, n=201, respectively) is collected, the sequential design allows for "***looks***" at the data to be made over the course of data collection.

There are two restrictions about when in the course of data collection LQAS analysis can be carried out with the sequential design: 1) a ***"look"*** at the data can only be made after completing data collection in any one cluster; and 2) only one threshold should be analyzed over the course of data collection to maintain the stated error limits. If more than one threshold is analyzed, the statistical error associated with the classification procedure will exceed the tolerable error limits established.

The table in Appendix 2 provides the appropriate decision rules for each threshold level by number of clusters of data collected and available for analysis.

As long as only one threshold prevalence level is tested, up to 67 "***looks***" at the data can be made over the course of data collection. As with LQAS analysis for the 33x6 and 67x3 designs, a certain amount of statistical error is associated with LQAS analysis carried out with the sequential design. The error limits remain constant over the course of data collection for the sequential design, with alpha $\leq$ 0.10 and beta $\leq$ 0.18 for assessment of the 10% upper threshold, and alpha $\leq$ 0.10 and beta $\leq$ 0.26 for assessment of the 15% upper threshold.[19] These alpha and beta errors are slightly higher

[19] Alpha and beta limits are slightly higher for acute malnutrition assessment at the 20% upper threshold. These alpha and beta errors, as well as those for the 10% and 15% upper thresholds, assume the ICC for acute malnutrition is $\leq$ 0.05 (Olives et al. 2009).

than the errors associated with the 67x3 design at an ICC ≤ 0.05. This is one of the trade offs of using a design with the ability to analyze data with LQAS before the data for the full sample design is collected.

Another distinctive characteristic of the sequential design is that the LQAS analysis may not indicate a clear answer about the threshold level of acute malnutrition. Whereas the 33x6 and 67x3 designs allow for a clear conclusion (with a certain amount of statistical error) as to whether the prevalence of acute malnutrition has reached or exceeded the threshold level or not, a "***look***" that is made with the sequential design will indicate one of three outcomes: 1) the threshold level of acute malnutrition has been exceeded in the assessment area; 2) the threshold level of acute malnutrition has not been exceeded in the assessment area; or 3) a clear decision about the threshold level of acute malnutrition in the assessment area cannot be made within the stated levels of alpha and beta above.

The result of the "***look***" will depend on the number of children who are acutely malnourished relative to the total number of clusters of data collected at the time of analysis. If the data indicate a clear decision about the threshold level of interest, data collection can stop. If a clear decision about the threshold level of acute malnutrition cannot be made, data collection continues. While unlikely, the worst case scenario is that the full sample of n=201 must be fulfilled in order to make a decision.[20] At n=201, the sequential design is equivalent to the 67x3 design, and, therefore, a decision about the threshold level can always be made.

The procedure to carry out LQAS analysis with the sequential design is similar to that described for the 33x6 and 67x3 designs. First, the data must be prepared appropriately (refer to Section 2.6.2). Since data have been collected on all children in sampled households, it is necessary to randomly select children to exclude from LQAS analysis in oversized

20 Simulation studies exploring the efficiency offered by the sequential design have shown the average sample size for assessing the 10% threshold is n=69 (23 clusters) under the hypothesis the true prevalence is ≥ 10% and n=102 (34 clusters) under the hypothesis the true prevalence is < 10%. For the 15% threshold, the average sample size is 105 (35 clusters) under the hypothesis the true prevalence is ≥ 15% and 150 (50 clusters) under the hypothesis the true prevalence is <15%. For the 20% threshold, the average sample size is 120 (40 clusters) under the hypothesis the true prevalence is ≥ 20% and 141 (47 clusters) under the hypothesis the true prevalence is < 20% (Olives et al. 2009).

BOX 11. INSTRUCTIONS FOR CARRYING OUT LQAS ANALYSIS WITH THE SEQUENTIAL DESIGN

STEP 1

Count the number of observations in the sample with acute malnutrition.

STEPS 2a - 2c

Compare the number of observations with acute malnutrition against the pre-established decision rule for the corresponding threshold level and number of clusters of data collected (refer to Appendix 2).

2a. If the number of observations with acute malnutrition is equal to or less than the pre-established decision rule in column i of the table. Classify the prevalence of acute malnutrition in the population as less than the threshold prevalence analyzed.

2b. If the number of observations with acute malnutrition is equal to or greater than the pre-established decision rule in column ii of the table. Classify the prevalence of acute malnutrition in the population as equal to or greater than the threshold prevalence analyzed.

2c. If the number of observations with acute malnutrition is greater than the pre-established decision rule in column i of the table and less than the pre-established decision rule in column ii of the table, a decision about the threshold level cannot be made. Continue sampling, and repeat steps 1 and 2 when it is convenient to look at the data next, but never before data collection for a cluster is completed.

clusters with data on more than three children. The LQAS classification procedure can then be carried out by following the steps outlined in Box 11.

The examples in Boxes 12 and 13 illustrate two different logistic approaches for analyzing acute malnutrition data with the sequential design.

BOX 12. EXAMPLE OF "END OF DAY" LQAS ANALYSIS WITH THE SEQUENTIAL DESIGN

A sequential design is carried out in Wobelleno with the aim of detecting if the prevalence of acute malnutrition in the province has reached or exceeded 15%. Assume the 67 clusters selected for data collection are those that were selected for the 67x3 design in Section 2.3. After data collection is completed each day, teams will return to the survey base. Data will be entered, cleaned, and analyzed at the end of each day of data collection.

After the first day of data collection, a total of 17 clusters of data were collected. Data were collected on all children 6–59 months in the household, which, in this case, meant data on 53 children were collected. The data are entered and cleaned to eliminate any flagged (biologically implausible) anthropometric values. One of the oversized clusters has an observation with flagged anthropometric data. The flagged observation is excluded from the LQAS analysis, making the usable sample size 52. In the other oversized cluster, one child is selected randomly for exclusion from the LQAS analysis. The usable sample size after day one of data collection is therefore 17 clusters, with anthropometric data on 51 children.

Out of the sample size of 51, 7 children are acutely malnourished. The sequential decision rule for assessment of the 15% threshold with 17 clusters of data collected is ≤ 0 to classify the prevalence of acute malnutrition as <15% and ≥ 10 to classify the area as having a prevalence ≥ 15% (Appendix 2). A decision about the 15% threshold cannot be made. Therefore, the teams must continue data collection the next day.

At the end of day two, data were collected from 20 more clusters (on 65 children). The data are entered and cleaned. No biologically implausible anthropometric data are identified, but since four clusters have data on more than the required 3 children, the procedure to exclude the extra observations is carried out in each cluster to eventually yield data on 60 children for the second day of data collection.

The day one + day two cumulative data collected is 37 clusters (111 children). A total of 19 children in the sample of 111 are acutely malnourished. The decision rule for the 15% threshold and 37 clusters is ≤ 7 to conclude the prevalence of acute malnutrition as <15% and ≥ 18 to conclude the population has an acute malnutrition prevalence ≥ 15% threshold (Appendix 2). We classify the prevalence as ≥ 15%. We can stop data collection after sampling only 37 of the 67 clusters selected for sampling as we have obtained a result for the question we were seeking to answer.

BOX 13. EXAMPLE OF "MID-DAY" LQAS ANALYSIS WITH THE SEQUENTIAL DESIGN

A sequential design is carried out in Wobelleno with the aim of detecting if the prevalence of acute malnutrition has reached or exceeded 15%. Seven interview teams will collect data for the survey. It is anticipated that each team will be able to complete data collection for 5 clusters in one day. At mid-day, each team will communicate with the survey coordinator by satellite phone to report the data collected. Depending on the number of clusters completed, data analysis may take place at that time.

At mid-day, a total of 21 clusters of data are collected. The data are reported back to the survey coordinator, entered on a computer and cleaned. Acute malnutrition data for 63 children are available for analysis (3 children in each of the 21 clusters sampled).

The data are analyzed with LQAS to assess if the prevalence of acute malnutrition has reached or exceed 15%. Out of the sample size of 63, 1 child is acutely malnourished. The data indicate the prevalence of acute malnutrition in Wobelleno is < 15%. Data collection can stop.

3.7 Summary of Section Three

This Section of the Guide provided implementation instructions specific to the sequential design. With respect to questionnaire development, sampling, and questionnaire administration, the sequential design is very similar to the 67x3 design. The distinctive feature of the sequential design is when in the course of sampling LQAS analysis can be conducted. The main advantage of the sequential design is the potential to stop sampling before data are collected from all 67 clusters. The design should not be used, however, if point estimates for any indicator are needed.

The examples in this Section highlighted how to conduct LQAS analysis with the sequential design given two different logistic approaches guiding the timing of data entry and analysis. The sequential design also provides a promising approach for rapid, routine monitoring of threshold levels of acute malnutrition over a designated geographic area. As such, the design offers potential and relevance as a low cost tool for acute malnutrition surveillance.

SECTION 4.

Choosing the Most Appropriate Sampling Design

Once it has been determined that a population-based survey needs to be carried out in a given geographic area, a decision must be made as to the sampling design to use. Among the primary factors that should be considered in an emergency setting are: 1) the precision of estimates required (for both child- and household-level data); 2) the information needed from the survey (e.g., if analysis of acute malnutrition by LQAS is all that is desired or whether child- and household-level point estimates and CI results are needed); and 3) the time and resources available for data collection. The relative importance of each of these factors should be determined in accordance with the specific context, objectives, and priorities of the survey to be implemented.

No sampling design will be able to perform best on all three of the factors mentioned above. While each design may offer certain advantages, no one sampling design is perfect.

Selection of the most appropriate sampling design to use requires careful consideration of the trade-offs that must be made. The question of which design to implement therefore becomes a strategic decision to ensure that the primary objectives of the survey will be met and that the trade-offs are tolerable with respect to the context and purpose of the survey being implemented.

To facilitate an informed decision about the most appropriate sampling design to implement, this Section of the Guide provides information about the strengths and weaknesses of the 33x6, 67x3, and sequential design with respect to the three factors listed above (precision, data needs, time/resources). To put the strengths and weaknesses of these designs in proper context, we also present information on the same three factors for the 30x30 design.

4.1 Precision of Estimates

Section 2.6 provides a discussion of the factors that influence the precision of estimates for data collected with a cluster design. In that discussion, we concluded that the two most influential design factors that affect the precision of a point estimate for an indicator are: 1) the sample size of the design; and 2) the cluster size of the design. We determined that a larger total sample size and a smaller cluster size will provide the most precise estimate for an indicator (refer to Formulas 1, 2, 3 and 4).

With respect to sample size, the 30x30 design has the advantage of a larger sample size (n=900) than either the 33x6 (n=198) or 67x3 design (n=201). With respect to cluster size, the 30x30 design has the disadvantage of having the largest cluster size (30 observations per cluster), in comparison to the 33x6 (6 observations per cluster) and 67x3 designs (3 observations per cluster).

To highlight the relative performance of each design, we cite actual data from a field application in which the 33x6, 67x3, and 30x30 designs were implemented concurrently in the same geographic area. We present a comparative discussion of the precision of estimates obtained for child-level indicators first, and then discuss household-level indicators.[21] We do not discuss the sequential design here since it allows only for LQAS analysis of acute malnutrition prevalence.

[21] Refer to Deitchler et al 2008 for a full discussion.

4.1.1 Child-Level Indicators

Table 8 shows the child-level results obtained from a field application of the designs in West Darfur, Sudan (Deitchler et al. 2008). From the results, it is clear that the 30x30 design produces the most precise estimates (i.e., narrowest CIs) for most indicators.

However, in the context of emergencies, the ***most*** precise design may not be the most appropriate sampling design, as high precision is likely to come at the cost of time and resources. In emergencies, a design that allows for ***sufficiently*** precise results may be preferred. It is in this context that it is useful to compare the extent of loss in precision that comes with use of the 33x6 and 67x3 designs.

The difference in the width of the CIs for the 33x6 design compared to the 30x30 design ranges from +/- -0.1 percentage points (diarrhea) to +/- 3.6 percentage points (measles vaccination), with a median difference of +/- 2.6 percentage points. For indicators such as diarrhea prevalence and coverage of vitamin A capsule (VAC) supplementation, the 33x6 design provides results nearly as precise as the 30x30 design. This is because many infectious diseases and health coverage indicators tend to cluster (Deitchler et al. 2008). When there is substantial clustering of an indicator (i.e., high ICC), the most detrimental impact will be on the precision of estimates collected with a 30x30 design. As described earlier, this is because the high ICC interacts with the large cluster size to cause a high design effect and substantial reduction in the effective sample size of the design (refer to Section 2.6.1).

Although the 67x3 design produces less precise results than the 30x30 design for most of the child-level indicators shown here (refer to Table 8), the difference in the width of the CIs between the 67x3 and 30x30 designs is smaller than those between the 33x6 and 30x30 designs. The difference in precision for the 67x3 design compared to the 30x30 design ranges from +/- -0.7 percentage points (VAC supplementation) to +/- 3.0 percentage points (underweight) with a median difference of +/- 1.4 percentage points. As would be expected, indicators with a high ICC, such as VAC supplementation, measles vaccination, and diarrhea prevalence are estimated more precisely with the 67x3 design than the 30x30 design (Deitchler et al. 2008).

Table 8. Point Estimate and 95% CIs (Accounting for Design Effect) for Child-Level Indicators by Sampling Design

Indicator	Sampling Design		
	33x6 Design	67x3 Design	30x30 Design
Acute malnutrition	4.7 (1.9, 7.4) +/- 2.8	8.0 (4.5, 11.5) +/- 3.5	6.9 (4.8, 9.0) +/- 2.1
Low MUAC (<12.5 cm)	4.0 (0.7, 7.4) +/- 3.4	3.0 (0.7, 5.3) +/- 2.3	3.3 (2.0, 4.7) +/- 1.4
Stunting	26.0 (19.1, 32.8) +/- 6.9	25.4 (19.4, 31.4) +/- 6.0	27.3 (23.8, 30.9) +/- 3.6
Underweight	22.9 (16.8, 29.1) +/- 6.2	27.6 (21.1, 34.1) +/- 6.5	27.4 (24.0, 30.9) +/- 3.5
BCG vaccination	26.8 (16.4, 37.1) +/- 10.4	40.3 (31.6, 49.0) +/- 8.7	35.2 (28.0, 42.5) +/- 7.3
Measles vaccination	57.4 (43.7, 71.1) +/- 13.7	55.0 (45.0, 65.0) +/- 10.0	55.4 (45.3, 65.5) +/- 10.1
VAC supplementation (5.5 month recall)	74.1 (61.7, 86.6) +/- 12.5	69.7 (59.7, 79.6) +/- 10.0	77.7 (67.1, 88.4) +/- 10.7
Diarrhea (2 week recall)	6.6 (2.2, 11.0) +/- 4.4	6.0 (2.3, 9.7) +/- 3.7	10.9 (6.6, 15.2) +/- 4.3
ARI (2 week recall)	6.1 (1.9, 10.2) +/- 4.2	6.0 (2.0, 10.0) +/- 4.0	3.4 (1.9, 5.0) +/- 1.6
Fever (2 week recall)	16.2 (9.8, 22.5) +/- 6.4	21.6 (15.3, 28.0) +/- 6.4	18.4 (14.5, 22.2) +/- 3.9

Source: Deitchler et al. 2008

4.1.2 Household-Level Indicators

Table 9 shows results for household-level indicators obtained from the same field application of the designs. Here, the 67x3 design provides the most precise results for all indicators. The difference in precision between the 67x3 and 30x30 designs ranges from +/- -5.7 percentage points (access to potable water) to +/- -1.3 percentage points (ownership of bed nets) with a median difference of +/- -2.2 percentage points. The difference in precision between the 33x6 design and 30x30 designs ranges from +/- -0.4 (access to potable water) to +/- 1.6 (access to latrine) with a median difference of +/- 0.5 (Deitchler et al. 2008). These results indicate that for nearly all household-level indicators, the 33x6 and 67x3 designs provide estimates as precise or more precise than the 30x30 design. In other words, the 700 extra observations required for the 30x30 sample size offer little advantage to the estimation of household-level indicators such as those shown here (Deitchler et al. 2008). Household-level indicators such as those below tend to have a high ICC, which causes a substantial reduction of the effective sample size of the 30x30 design, and ultimately impacts the precision of results than can be obtained.

Table 9. Point Estimate and 95% CIs (Accounting for Design Effect) for Household-Level Indicators by Sampling Design

Indicator	Sampling Design		
	33x6 Design	67x3 Design	30x30 Design
Access to potable water	52.0 (34.6, 69.4) +/- 17.4	53.2 (41.2, 65.3) +/- 12.1	57.4 (30.7, 75.2) +/- 17.8
Access to latrine	50.5 (37.4, 63.6) +/-13.1	50.3 (40.4, 60.1) +/- 9.9	50.8 (39.3, 62.3) +/- 11.5
Ownership of bednet	29.3 (17.1, 41.5) +/- 12.2	36.6 (26.5, 46.7) +/- 10.1	35.1 (23.7, 46.5) +/- 11.4
Food shortage (5.5 month recall)	66.7 (53.7, 79.6) +/- 13.0	62.4 (52.4, 72.5) +/- 10.1	61.7 (48.8, 74.6) +/- 12.9

Source: Deitchler et al. 2008

4.2 LQAS Analysis of Acute Malnutrition

If the only piece of information that is necessary is to classify the prevalence of acute malnutrition against a threshold level, the sequential design may be appropriate to meet the purpose of the survey. The design offers the advantage of a potentially reduced sample size and provides a promising approach for rapid, routine monitoring of threshold levels of acute malnutrition over a designated geographic area. Before adopting the sequential design, the slightly higher alpha and beta errors associated with the LQAS classification procedure should be considered (refer to Section 3.6).

Two statistical factors need to be considered when choosing whether to use a 33x6 or 67x3 design for LQAS analysis, namely: 1) the tolerable error limits for the LQAS classification procedure and; 2) the ICC assumption that must be met by the acute malnutrition data collected. On both of these issues, the 67x3 design is more robust than the 33x6 design.

Simulation studies to validate the 33x6 and 67x3 designs have shown that LQAS analysis can be conducted on acute malnutrition data (without error inflation) so long as the ICC for acute malnutrition is below a certain level. For the 33x6 design, that level is 0.15 for the 10% threshold and 0.00 for the 15% threshold. For the 67x3 design, the level is 0.25 for the 10% threshold and 0.10 for the 15% threshold. The underlying assumptions that must be met by acute malnutrition data collected with a 67x3 design are thus less restrictive than those that must be met by data collected with a 33x6 design.

When a good estimate of the ICC for acute malnutrition is available for the area to be assessed, this information can be useful to help decide which of the two designs to use.

Unfortunately, it is rarely possible to know in advance the exact ICC that exists (for any indicator) in a field setting where a survey will be conducted. Information available to date would suggest, however, that an ICC above 0.05–0.10 would be very unlikely for acute malnutrition.[22,23] What is difficult, if not impossible, is to know in advance if the area to be assessed is one of those few where the ICC for acute malnutrition will be above 0.05–0.10.

Until there is more clarity about the conditions in which the upper levels of ICC would be expected, or possibly exceeded, investigators desiring strict adherence to the stated LQAS error limits of alpha ≤ 0.10 and beta ≤ 0.20 may prefer to err on the side of caution by using the more robust 67x3 design. Investigators who require data rapidly or have fewer resources may prefer to use the sequential design given its potential for a much reduced sample size. Finally, those investigators seeking a balance between limited classification error and potential expediency of data collection may find the 33x6 design best meets their data requirements (Olives et al. 2009).

Since point estimates and 95% CIs for acute malnutrition can also be tabulated with data collected by the 33x6 and 67x3 designs, one may ask what the added benefit of LQAS analysis is in these applications. LQAS analysis is most useful when the 95% CI for acute malnutrition overlaps with a critical threshold prevalence used for decision making. In cases where the CI does not overlap with the threshold level of interest, LQAS analysis is not necessary.

Suppose the 67x3 design was implemented and produced an estimate of 8.0% acute malnutrition prevalence with a 95% CI of 4.5%–11.5%. Given these results, it is not possible to determine from the CI alone whether the 10% acute malnutrition threshold had been exceeded or not. This is because the CI includes the 10% threshold. Recall the correct interpretation of a 95% confidence interval: if one was to draw repeated samples of the same size from the population, the true population value would fall within the CI calculated in 95% of those samples.

A population-based survey provides a point estimate and distribution curve for the ***one sample*** collected. It is not possible to know if the point estimate and accompanying CI for that one sample collected is actually the correct one for the population. If the CI overlaps with the threshold level of interest, it is therefore statistically incorrect to use the point estimate or 95% CI from that one sample to make a statement as to whether the threshold level is exceeded in the population.

LQAS analysis allows for a probability-based classification to be made about whether the threshold prevalence of acute malnutrition in the population has been reached or not. It is in this way that LQAS analysis adds important value to the point estimate and CI, providing useful information for triangulating and interpreting population-based data for decision making about acute malnutrition thresholds.

22 A recent study exploring the plausible range of ICC for acute malnutrition prevalence showed nearly all acute malnutrition data (>90%) from Demographic and Health Surveys conducted in 46 developing countries to have an ICC < 0.10 (Fenn et al. 2004). (Acute malnutrition in this study was defined as weight for height z-score < -2 standard deviations of the 1977 NCHS reference median).

23 Deitchler et al's field validation of the 33x6 and 67x3 designs in West Darfur showed ICCs < 0.05 for acute malnutrition in two assessment areas (Deitchler et al. 2008).

4.3 Time and Cost of Data Collection

In all field tests to date the 33x6, 67x3, and sequential design have required less time for data collection than a 30x30 design (Deitchler et al. 2007; Deitchler et al. 2008). Comparisons of the time required by design are shown below for three different field sites where the 33x6, 67x3, and 30x30 designs were implemented concurrently (refer to Table 10).

Table 10. Estimated Number of Person Days Required for Data Collection by Sampling Design and Assessment Area

Assessment Area	Sampling Design			Assumption of Work Day Length[24]
	33x6 design (in person days)	67x3 design (in person days)	30x30 design (in person days)	
Fur Baranga, West Darfur, Sudan	24.75	33.51	90.00	8.5 hr work day
Habila, West Darfur, Sudan	12.39	16.74	45.00	12.0 hr work day
Siraro, Oromiya, Ethiopia	16.95	25.68	45.00	12.0 hr work day

Source: Deitchler et al. 2007; Deitchler et al. 2008

To explore how the time savings offered by the alternative sampling designs in comparison to the 30x30 design would be influenced by the size of assessment area and geographic size of the clusters, a sensitivity analysis was conducted to assess the difference in the time required for data collection when greater distances of travel to and among clusters were assumed. This was done once assuming the distance of travel to and among clusters was increased by three times, and once assuming the distance of travel was increased by five times the actual average distances in the two field sites with the most comprehensive travel data available. In all scenarios tested, the 33x6 and 67x3 designs were still estimated to require less time for data collection than the 30x30 design (Deitchler et al. 2008). Thus, in terms of time expenditure, the 33x6 and 67x3 designs offer a clear benefit over the 30x30 design.

Besides the smaller sample size required for the 33x6 and 67x3 designs, experience has shown that travel for these designs can be planned strategically so that one team might be able to complete all of the clusters located in a faraway region in one day. This is not as feasible with the 30x30 design because a team cannot usually complete more than one cluster per day if travel to the region is of substantial distance. Also, in cases where the sampling frame for an assessment area is relatively small (e.g., <50 PSUs listed), many of the areas listed in the sampling frame will be selected by PPS for multiple clusters of the 67x3 design, which can also reduce the amount of total travel necessary. These potential advantages are expected to be common to most applications of the 33x6 and 67x3 designs, however, road infrastructure, the size of the cluster, and dispersion of households within the cluster would also affect the time required for data collection with each design (Deitchler et al. 2008).

24 Length of work day varies for each assessment area because of the method used to estimate time expenditure. The method involved using a time estimation formula that took into account the average time to complete each component of data collection by design. Within each assessment site, the length of the work day for the time estimation calculation was determined by the average time required to complete one (or two) 30x30 clusters in one work day. In other words, it was assumed that no 30x30 cluster would need to be revisited to complete data collection from a previous work day. Those assumptions were made for ease of calculation, though this has probably resulted in the time required for the 30x30 design to be underestimated (Deitchler et al. 2008).

4.4 Summary of Section Four

This Section of the Guide reviewed factors to consider when choosing the most appropriate sampling design to use. Three primary factors were considered: 1) precision of estimates (for child-level and household-level indicators); 2) LQAS analysis of acute malnutrition; and 3) time and cost of data collection.

As discussed earlier, no sampling design will perform best on all criteria. Trade-offs must always be made. The sampling design to use should therefore always be selected in accordance with the context, purpose, and objectives of the survey, and within the parameters of any financial and logistic constraints.

The benefit of the 33x6 and 67x3 designs over the 30x30 design is clear so long as a reduction in the precision of child-level indicators is acceptable. With regard to the reduced precision of child-level anthropometric indicators, this is at least partly compensated for by the fact that the 33x6 and 67x3 designs allow for LQAS analysis to be carried out on the acute malnutrition data.

For survey objectives limited to monitoring the prevalence of acute malnutrition, any of the above designs could be used, but the sequential design provides perhaps the most promising and time-effective approach for making a rapid decision about threshold levels of acute malnutrition prevalence. When using this design, it is important to keep in mind, however, that point estimates cannot be tabulated for any indicator unless data for the full sample are collected.

SECTION

5. Promising Applications and Adaptations

In this Section of the Guide we focus on innovations related to the use and implementation of the designs. The supplementary information provided here describes both promising practices and newly evolving areas of work to address issues such as: assessing areas where pockets of high prevalence of acute malnutrition is expected; and using LQAS to assess the prevalence of other measures of acute malnutrition.

The Section is divided into three parts. Part 1 discusses the use of a stratified approach to implementing multiple 33x6 or 67x3 sampling designs. Parts 2 and 3 explore potential adaptations and extensions of LQAS analysis for use with the 33x6 and 67x3 designs.

5.1 A Stratified Approach to Implementing the 33x6 and 67x3 Designs

In emergencies, representative data are often required for small geographic pockets where it is known that the population is especially vulnerable, or the local impact of the situation is particularly severe. If a survey is implemented to collect data that are representative of a large geographic area, results for smaller geographic "pocket" areas within the area cannot be tabulated. Instead, data for the most severely affected areas are averaged together with data for the less severely affected areas to obtain an average result reflective of the whole geographic area. Carrying out only one survey for a large geographic area can, in this way, mask the level of need among the more vulnerable populations within the broader area.

In this part of the Guide we describe how to implement the 33x6 and 67x3 designs so that representative data are available both for vulnerable pocket areas and the broader geographic area. To do so, a stratified approach for carrying out multiple 33x6 or 67x3 designs is recommended. To use this approach, one full survey is carried out in each stratum to yield geographically disaggregated estimates for all indicators. The approach capitalizes on the time savings offered by the 33x6 and 67x3 designs. Since the 33x6 and 67x3 designs require respectively one-quarter and one-third of the time it takes to carry out a 30x30 design, approximately four 33x6 designs and three 67x3 designs could be implemented for the same time expenditure as one 30x30 design in the same area.

To illustrate how such a stratified approach could be usefully implemented, we return to the Wobelleno example. Suppose a persistent drought affects the entire province, rainfall is erratic across districts, and a measles epidemic affects the eastern region. The situation leads to an expectation that pronounced differences exist in the level of acute malnutrition and other key indicators across local areas in Wobelleno. It is determined, therefore, that it would be most useful to implement multiple 33x6 surveys,[25] one representing each precipitation zone, or district, within Wobelleno rather than any one survey for the whole province.

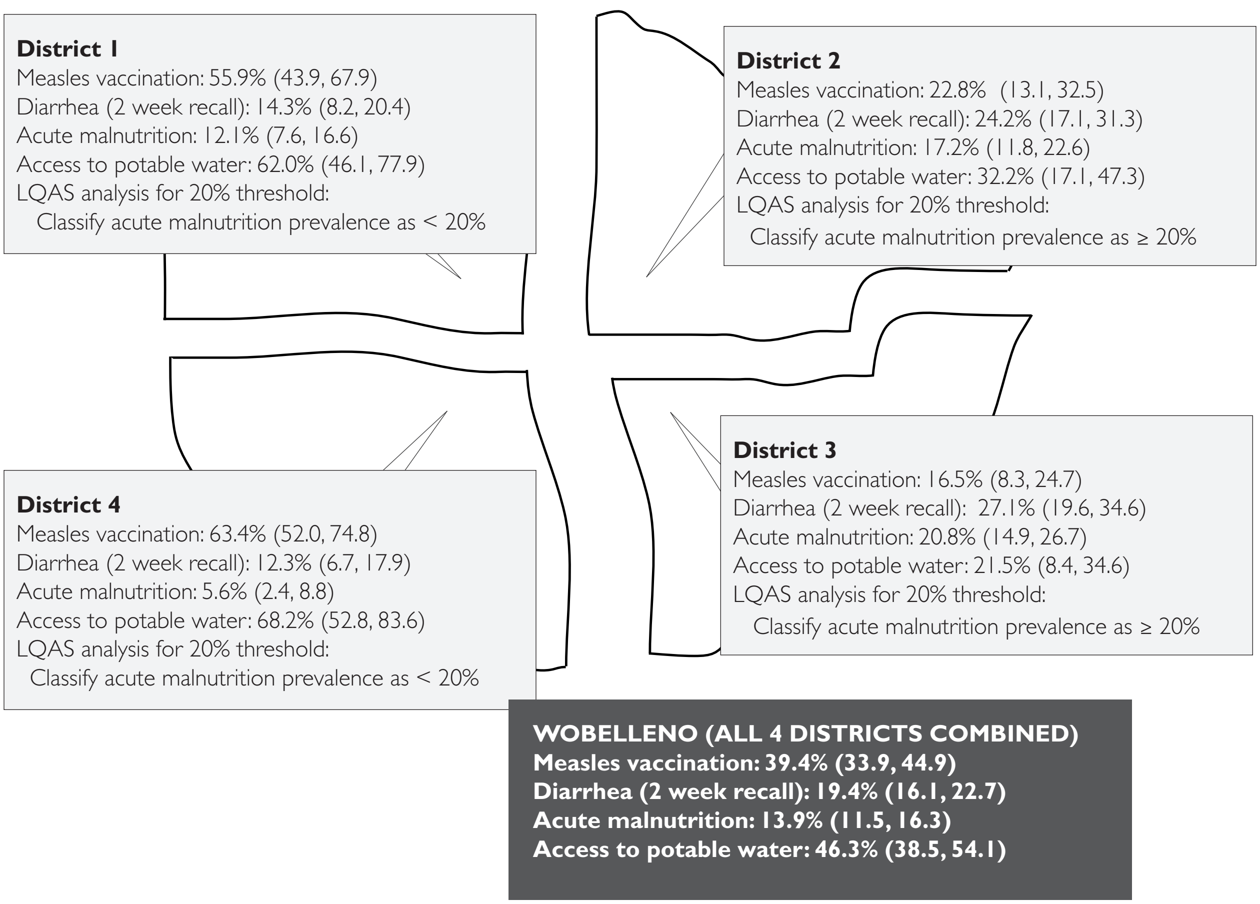
25 The same stratification approach described here could also be applied to the 67x3 design, although it is likely that only three 67x3 designs (as opposed to four 33x6 designs), could be completed in the same time as required for one 30x30 design.

The procedure to collect data for this stratified approach is the same as the data collection procedure described for the 33x6 design in Section 2. The only difference is that here four different surveys are being implemented whereas in Section 2, only one survey was implemented.

When using the stratified approach, one sampling frame is used for each survey to be conducted, and the selection of clusters to be sampled is carried out separately on each sampling frame using the PPS method (Appendix 9). Together, the sampling frames should comprise the whole area that is Wobelleno Province. In other words, all villages in Wobelleno should be listed in one of the four sampling frames and no village should be included in more than one sampling frame.

Once the data for all four surveys are collected, entered, and cleaned, the data are ready to be analyzed. Assuming both child- and household-level data are collected, the sample size for each district assessed should be (at least) 198 children and 198 households. To analyze the data, point estimates and 95% CIs are tabulated separately for each 33x6 survey completed. LQAS analysis to assess the prevalence of acute malnutrition against the 20% threshold is also conducted separately for each 33x6 survey completed. The procedure for conducting these analyses is the same as the analysis instructions outlined in Section 2.6. Hypothetical results are shown below in Figure 9.

Figure 9. Results from Stratefied Approach Used to Survey Four Districts



The stratified approach makes representative data available for each of the pocket areas that were of concern. The disaggregated information provides useful information for targeting and prioritizing areas to receive assistance. The difference in acute malnutrition prevalence between districts is clear. Because the four surveys were implemented in contiguous, non-overlapping geographic areas comprising all of Wobelleno, the data from each survey can also be aggregated to provide results representative of the whole province (refer to the bottom of Figure 9). The aggregated point estimate results can be calculated in Excel by weighting the estimates provided for each district (p_i) by the proportion of the total population in the province that resides in that district (N_i / $\sum N_i = w_i$),[26] and aggregating the data across all districts together, by adding the resulting proportions ($\sum(p_i w_i \times 100\%)$). In the example in Table 11 below, the aggregated measles vaccination coverage for Wobelleno Province is 39.4% (15.19% + 8.38% + 2.47% + 13.36% = 39.40%)

To calculate the aggregate point estimate results with CIs, a statistical software program should be used to account for the design effect. To do so, a new variable (the weighting factor) needs to be added to the data set. In this example, each observation in the same district (stratum) would be given the same value for the weighting factor variable, w_i. When analyzing the aggregate results, this weighting factor needs to be specified and applied. The strata and clusters must also be specified to allow for correct CI tabulation. The estimates provided by the software program will then account for the different population sizes across districts (strata), and weight the aggregate results accordingly.

There is no drawback to implementing a stratified approach such as this—only benefits. Results can be generated about disaggregated areas and the broader geographic area as well; yet, the total time required for collecting data for four 33x6 cluster surveys[27] is likely equivalent to one 30x30 design that would only have provided representative data about the province as a whole. Further, the aggregated results obtained using such a stratified approach will be much more precise for both child- and household-level indicators than those obtained by one 30x30 design. The stratified approach described above was four 33x6 surveys, which is effectively a 132x6 (n=792) design when data from the four surveys are aggregated together. As discussed earlier, a larger sample size and smaller cluster size positively influence the precision of estimates.

Table 11. Example of Aggregated Measles Vaccination Coverage for Wobelleno Province

District	Indicator	District Coverage (p_i)	District Population Size (N_i)	Weighting Factor (w_i)	$p_i * w_i$	$p_i w_i * 100$
1	Measles Vaccination	0.559	33,567	0.2718	0.1519	15.19%
2	Measles Vaccination	0.228	45,387	0.3675	0.0838	8.38%
3	Measles Vaccination	0.165	18,524	0.1500	0.0247	2.47%
4	Measles Vaccination	0.634	26,020	0.2107	0.1336	13.36%
					Aggregated Results Across All 4 Districts:	39.40%

26 For example, 33,567 (the population in District 1) / 123,498 (the total population in Wobelleno Province) = 0.2718, the weighting factor (w_i) for District 1.

27 To further optimize the time required for data collection using the stratified approach, logistics should be planned according to the strata to be assessed. Recommended logistic strategies include dispersion of interviewer teams so that interviewer teams are assigned to collect data in one particular stratum and data collection across strata occurs concurrently; or movement of all interview teams across the strata comprising the assessment area together so that data collection across strata occurs sequentially.

5.2 Using the 33x6 or 67x3 Design for LQAS Analysis of Severe Acute Malnutrition

In situations of famine, conflict, or extreme emergency, the prevalence of acute malnutrition can be ubiquitously high, with the prevalence exceeding 20% in many areas.[28] In this context, the LQAS classification procedure described earlier in the Guide may not provide the necessary information for prioritization of the affected areas. Thresholds higher than 20% would need to be analyzed to prioritize the level of assistance among areas with a ubiquitously high prevalence of acute malnutrition. However, as described earlier, analyzng threshold levels above 20% is not recommended with the 33x6 and 67x3 designs due to the expected inflation of the alpha and beta errors.

An alternative to this limitation is to establish thresholds and decision rules for assessing the prevalence of severe acute malnutrition.[29] The idea is that in a context with a ubiquitous, high prevalence of acute malnutrition, using LQAS analysis to assess a low prevalence of severe acute malnutrition may be more appropriate than LQAS analysis to assess a high prevalence of acute malnutrition. Statistically, there would be benefits to doing so, namely, the potential for greater precision of the LQAS classification procedure and less statistical error.[30]

While the possibility for expanding LQAS analysis to assess the prevalence of severe acute malnutrition is indeed promising, no simulation work on this topic has yet been conducted. Before use of the 33x6 and 67x3 designs for LQAS analysis of severe acute malnutrition prevalence could be recommended, simulations need to be undertaken to explore the range of ICC necessary, as well as the optimal precision for the classification procedure, associated decision rules, and approximate alpha and beta errors. FANTA-2 intends to explore the possibility of extending the 33x6 and 67x3 designs for this purpose in the future.

5.3 Using the 33x6 or 67x3 Design for LQAS Analysis of Low Middle Upper Arm Circumference

Collecting data on children's MUAC is logistically easier and quicker than collecting data on the weight and height of children, and is therefore often the preferred method for assessing acute malnutrition. A point estimate and 95% CI for the proportion of children moderately and severely malnourished according to MUAC can, like any other binary indicator, be tabulated with data collected using the 33x6 and 67x3 designs.

To use the 33x6 and 67x3 designs for LQAS assessment of a threshold prevalence of moderate or severe malnutrition (as measured by MUAC), the MUAC data collected would probably have to meet the same criteria required by acute malnutrition data analyzed by LQAS (refer to Appendix 1).

Little work has been undertaken to explore the use of LQAS for assessment of the prevalence of acute malnutrition as measured by MUAC, but the results available to date suggest that the 33x6 design may not be appropriate for this purpose, as the ICC for MUAC will generally be too high to reliably carry out the LQAS classification procedure. Data for the 67x3 design are more promising, as the ICC for MUAC data collected with the 67x3 design is consistently lower.

If work in this area was to advance, it is likely that the thresholds that MUAC data could be analyzed against would be the same as those used for acute malnutrition (as measured by weight for height z-score < -2 standard deviations the reference median, or with bilateral pitting edema) and would need to meet the same criteria described in Appendix 1: an upper threshold above 20% should not be assessed; a five percentage point difference should always be used between the upper and lower threshold; and, as is always the case, only one threshold level should be analyzed for the indicator of interest.

28 Such situations have been reported recently in Ethiopia and Southern Sudan, using the NCHS 1977 child growth references. Upon adoption of the WHO 2005 growth standards, these situations may become more common, as the median reference weight of the WHO growth standards is higher than that of NCHS for the heights of most children 6-59 months (de Onis et al. 2006).

29 Severe acute malnutrition is defined as a child who is severely wasted (determined by weight for height z-score [WHZ] < -3 standard deviations [SDs] the reference median) or with bilateral pitting edema.

30 Because the variability of a sample with respect to an outcome is highest at 50% prevalence, LQAS analysis of a threshold prevalence further away from 50% will have less error associated with it, all other things being equal.

5.4 Summary of Section Five

This Section of the Guide explored promising applications for implementing the 33x6 and 67x3 designs—focusing in particular on how to capitalize on the time benefit of the 33x6 and 67x3 designs to implement a stratified survey approach and obtain representative data on pocket areas as well as the larger geographic area. In addition, the potential for further work to extend the 33x6 and 67x3 designs to allow for LQAS analysis of alternative measures of acute malnutrition (severe acute malnutrition and low MUAC) was discussed. These latter areas of work have only begun to be explored. Further simulation studies are needed to validate the use of the 33x6 and 67x3 designs for these purposes before the use of these measures of acute malnutrition could be recommended for LQAS analysis.

Conclusion
SECTION 6.

This Guide has described three sampling design alternatives to the 30x30 cluster design. The designs were developed in response to an expressed need for alternative sampling designs to be conducted in resource-limited, time-sensitive, insecure, and emergency settings. Six years later, the 33x6, 67x3, and sequential designs have been extensively tested and validated, both in the field and by computer simulation.

The alternative sampling designs provide a statistically valid approach for collecting reliable data on key child- and household-level indicators, and do so with limited time and resource expenditure. In settings where data on the prevalence of acute malnutrition is of foremost importance, the alternative sampling designs are especially useful.

When the 95% CI for the prevalence of acute malnutrition overlaps with a key threshold prevalence used for decision making, it is statistically not possible to determine from the CI alone if the threshold prevalence of acute malnutrition has been reached in the population or not. LQAS analysis addresses this limitation by providing users with additional statistical information to help classify whether the threshold prevalence level has been exceeded or not.

Using a stratified approach for implementation capitalizes on the time–and cost–savings of the 33x6 and 67x3 designs, allowing for statistically representative results to be available for multiple small ("pocket") geographic areas as well as the broader aggregated area–potentially for the same time and cost as required for one 30x30 survey.

With this Guide, we hope the alternative sampling designs will become familiar and accessible to more users, and that program managers, M&E specialists, and survey leaders will have the information they need to decide if the alternative sampling designs are appropriate for their purposes.

As experience using the alternative sampling designs continues to grow, and field applications of the designs becomes more varied, we would look forward to receiving feedback, both on your experience in using the designs as well as on the information contained in this Guide.

We welcome your comments and questions, which can be sent to fanta2@aed.org.

SECTION 7.

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APPENDIX I.

Sampling and Data Requirements for LQAS Analysis

To use LQAS (using the binomial distribution) to analyze data collected with a population-based survey, a number of criteria must be met by the sampling design used to collect the data, and the outcome on which the LQAS classification procedure will be carried out.

Of foremost importance is that the assumptions of the binomial distribution be met, namely:

1. *Each trial must result in one of two possible, mutually exclusive, outcomes. One of the possible outcomes is denoted (arbitrarily) as a success, and the other is denoted a failure.*

2. *The probability of a success, denoted by p, must remain constant from trial to trial. The probability of a failure, 1-p, is denoted by q.*

3. *The trials are independent; that is, the outcome of any particular trial is not affected by the outcome of any other trial.*

   (Daniel 1999)

Assumption #3 requires that all observations in the sample be selected independently. This assumption is violated with cluster sampling, as the nature of cluster sampling gives rise to the possibility of a positive ICC, which can cause an inflation of the alpha and beta errors associated with the binomial distribution. When using the binomial distribution for LQAS analysis, a simple random sample—as opposed to a cluster sample—is therefore generally required.

The LQAS analysis carried out with the 33x6, 67x3 and sequential designs is a special case. Two simulation studies and multiple field tests of the 33x6, 67x3, and sequential designs have shown that the alpha and beta errors associated with the binomial distribution remain accurate when using LQAS to access the acute malnutrition thresholds of 10%, 15%, and 20% with the 33x6, 67x3, and sequential designs (Hoshaw-Woodard 2003; Olives et al. 2009; Deitchler et al. 2007; Deitchler et al. 2008). The simulation studies have shown that LQAS analysis can be carried out on acute malnutrition data (without error inflation) so long as the ICC for acute malnutrition is below a certain level. For the 33x6 design, that level is 0.15 for the 10% threshold and 0.00 for the 15% threshold. For the 67x3 design, the level is 0.25 for the 10% threshold and 0.10 for the 15% threshold. For the sequential design, the level is 0.25 for the 10%, 15% and 20% threshold levels (Olives et al. 2009). The assumptions that must be met by acute malnutrition data collected by the 67x3 and sequential design are thus less restrictive than those that must be met for a 33x6 design. As a result, the errors for the LQAS classification procesure with the 67x3 and sequential designs are less affected by a higher ICC than the 33x6 design.

All simulation studies conducted to date have assumed clusters of size 6 for the 33x6 design and clusters of size 3 for the 67x3 and sequential designs. Acute malnutrition data collected with these designs and analyzed by LQAS should therefore also have at most 6 or 3 observations per cluster, respectively. Because of the sampling protocol for children in emergency settings (i.e., sampling all children in a selected household), specific data preparation steps should be taken before carrying out the LQAS classification procedure on the acute malnutrition data (refer to Section 2.6.2).

Decision Rules (DR) for the Sequential Design: Upper Thresholds of 10%, 15% and 20%[1]

APPENDIX 2.

ND signifies that no decision is made and sampling continues.

UPPER THRESHOLD LEVEL FOR ASSESSING THE PREVALENCE OF ACUTE MALNUTRITION

Cluster	10%		15%		20%	
	i. DR to classify the prevalence <10%	*ii. DR classify the prevalence ≥ 10%*	*i. DR to classify the prevalence <15%*	*ii. DR classify the prevalence ≥ 15%*	*i. DR to classify the prevalence <20%*	*ii. DR classify the prevalence ≥ 20%*
1	ND	≥ 3	ND	≥ 4	ND	≥ 5
2	ND	≥ 3	ND	≥ 5	ND	≥ 6
3	ND	≥ 3	ND	≥ 5	ND	≥ 7
4	ND	≥ 3	ND	≥ 5	ND	≥ 7
5	ND	≥ 4	ND	≥ 6	ND	≥ 8
6	ND	≥ 4	ND	≥ 6	ND	≥ 8
7	ND	≥ 4	ND	≥ 6	ND	≥ 9
8	ND	≥ 4	ND	≥ 7	ND	≥ 9
9	ND	≥ 4	ND	≥ 7	ND	≥ 10
10	ND	≥ 5	ND	≥ 8	ND	≥ 10
11	ND	≥ 5	ND	≥ 8	ND	≥11
12	ND	≥ 5	ND	≥ 8	≤ 0	≥ 11
13	ND	≥ 5	ND	≥ 9	≤ 0	≥ 12
14	ND	≥ 6	ND	≥ 9	≤ 1	≥ 12
15	≤ 0	≥ 6	ND	≥ 9	≤ 1	≥ 13
16	≤ 0	≥ 6	ND	≥ 10	≤ 2	≥ 13
17	≤ 0	≥ 6	≤ 0	≥ 10	≤ 2	≥ 14
18	≤ 0	≥ 6	≤ 0	≥ 11	≤ 3	≥ 14
19	≤ 1	≥ 7	≤ 1	≥ 11	≤ 3	≥ 15
20	≤ 1	≥ 7	≤ 1	≥ 11	≤ 4	≥ 15
21	≤ 1	≥ 7	≤ 1	≥ 12	≤ 4	≥ 16
22	≤ 1	≥ 7	≤ 2	≥ 12	≤ 5	≥ 16
23	≤ 1	≥ 8	≤ 2	≥ 12	≤ 6	≥ 17
24	≤ 2	≥ 8	≤ 2	≥ 13	≤ 6	≥ 17
25	≤ 2	≥ 8	≤ 3	≥ 13	≤ 7	≥ 18
26	≤ 2	≥ 8	≤ 3	≥ 14	≤ 7	≥ 19
27	≤ 2	≥ 8	≤ 4	≥ 14	≤ 8	≥ 19
28	≤ 2	≥ 9	≤ 4	≥ 14	≤ 8	≥ 20
29	≤ 3	≥ 9	≤ 4	≥ 15	≤ 9	≥ 20
30	≤ 3	≥ 9	≤ 5	≥ 15	≤ 9	≥ 21
31	≤ 3	≥ 9	≤ 5	≥ 15	≤ 10	≥ 21
32	≤ 3	≥ 9	≤ 5	≥ 16	≤ 10	≥ 22
33	≤ 4	≥ 10	≤ 6	≥ 16	≤ 11	≥ 22
34	≤ 4	≥ 10	≤ 6	≥ 16	≤ 11	≥ 23
35	≤ 4	≥ 10	≤ 6	≥ 17	≤ 12	≥ 23
36	≤ 4	≥ 10	≤ 7	≥ 17	≤ 12	≥ 24
37	≤ 4	≥ 11	≤ 7	≥ 18	≤ 13	≥ 24
38	≤ 5	≥ 11	≤ 8	≥ 18	≤ 13	≥ 25
39	≤ 5	≥ 11	≤ 8	≥ 18	≤ 14	≥ 25
40	≤ 5	≥ 11	≤ 8	≥ 19	≤ 14	≥ 26
41	≤ 5	≥ 11	≤ 9	≥ 19	≤ 15	≥ 26
42	≤ 6	≥ 12	≤ 9	≥ 19	≤ 15	≥ 27
43	≤ 6	≥ 12	≤ 9	≥ 20	≤ 16	≥ 27
44	≤ 6	≥ 12	≤ 10	≥ 20	≤ 16	≥ 28
45	≤ 6	≥ 12	≤ 10	≥ 21	≤ 17	≥ 28
46	≤ 6	≥ 13	≤ 11	≥ 21	≤ 18	≥ 29
47	≤ 7	≥ 13	≤ 11	≥ 21	≤ 18	≥ 30
48	≤ 7	≥ 13	≤ 11	≥ 22	≤ 19	≥ 30
49	≤ 7	≥ 13	≤ 12	≥ 22	≤ 19	≥ 31
50	≤ 7	≥ 13	≤ 12	≥ 22	≤ 20	≥ 31
51	≤ 7	≥ 14	≤ 12	≥ 23	≤ 20	≥ 32
52	≤ 8	≥ 14	≤ 13	≥ 23	≤ 21	≥ 32
53	≤ 8	≥ 14	≤ 13	≥ 24	≤ 21	≥ 33
54	≤ 8	≥ 14	≤ 14	≥ 24	≤ 22	≥ 33
55	≤ 8	≥ 14	≤ 14	≥ 24	≤ 22	≥ 34
56	≤ 9	≥ 15	≤ 14	≥ 25	≤ 23	≥ 34
57	≤ 9	≥ 15	≤ 15	≥ 25	≤ 23	≥ 35
58	≤ 9	≥ 15	≤ 15	≥ 25	≤ 24	≥ 35
59	≤ 9	≥ 15	≤ 15	≥ 26	≤ 24	≥ 36
60	≤ 9	≥ 16	≤ 16	≥ 26	≤ 25	≥ 36
61	≤ 10	≥ 16	≤ 16	≥ 26	≤ 25	≥ 37
62	≤ 10	≥ 16	≤ 16	≥ 27	≤ 26	≥ 37
63	≤ 10	≥ 16	≤ 17	≥ 27	≤ 26	≥ 38
64	≤ 10	≥ 16	≤ 17	≥ 28	≤ 27	≥ 38
65	≤ 11	≥ 17	≤ 18	≥ 28	≤ 27	≥ 39
66	≤ 11	≥ 17	≤ 18	≥ 28	≤ 28	≥ 39
67	≤ 14	≥ 15	≤ 23	≥ 24	≤ 34	≥ 35

[1] This table is adapted from Olives et al. 2009.

APPENDIX 3.

OFDA Standard Outcome and Impact Indicators for Use in Emergency Settings[1]

Summary Table: USAID/OFDA Sectors, Sub-Sectors, Indicators, and Cross-Cutting Themes

SECTOR	SUB-SECTOR	INDICATORS
AGRICULTURE & FOOD SECURITY	Fisheries	Capture Fisheries: -Average number of kilograms of fish consumed/week/fisher household -Average monthly income from captured fish/fisher or household Aquaculture: - Average number of kilograms of fish consumed/week/fish farmer household -Average income (in USD) from fish sales/fish farmer/week
	Livestock	-Number and percentage of targeted animals vaccinated
	Seed Systems and Agricultural Inputs	- Number and percent of seed recipient farmers with sufficient seeds to plant fields for next agricultural seasons -Number of months of food self-sufficiency due to distributed seed production for beneficiary families
	Veterinary Medicines and Vaccines	-Prevalence of disease in area of intervention, represented as both the percentage of total herd as well as the total number of affected animals. -Increase in number and percentage of animals vaccinated, by type, from baseline of pre-program numbers vaccinated
HEALTH	(General Indicators)	-Utilization rates of USAID/OFDA supported health facilities -Reduction in CMR or CDR with a target of less than 2x baseline or ≤1/10,000/day if baseline unknown) -Reduction in U5MR or 0-5DR maintained or reduced (target less than 2x baseline or U5MR ≤2/10,000/day if baseline unknown.)
	Child Health	Malaria (in malaria endemic countries) -Number and percent of total population, pregnant women, and < 5 effectively treated for malaria with appropriate anti-malarial -Percent of coverage with ITNs of target population (total, <5 and pregnant women) -Percent utilization of ITNs in target population (total, <5 and pregnant women) -Incidence rate of malaria (total, <5 and pregnant women) -Proportional morbidity from malaria (total, <5 and pregnant women) -Proportional mortality from malaria (total, <5 and pregnant women) Acute Respiratory Tract Infections (ARI) -Number and percent of population < 5 effectively treated for ARI with appropriate antibiotic -Proportional morbidity of children < 5 years of age from ARI -Proportional mortality of children < 5 years of age from ARI Diarrhea -Number and percent of population < 5 effectively treated for diarrhea with ORT and zinc (antibiotic for dysentery) at the health facility and/or community level -Percentage of children exclusively breastfed for at least 6 months or continued feeding -Incidence rate of diarrhea -Proportional morbidity of children < 5 years of age from diarrhea -Proportional mortality of children < 5 years of age from diarrhea

1 The indicators shown in Appendix 2 reflect only the outcome and impact indicators included in the OFDA Program Guidelines as it is expected output indicators would not be collected using a population-based survey. For a complete list of indicators refer to the USAID/OFDA Guidelines for Unsolicited Proposal and Reporting, December 15 2006 at http://www.usaid.gov/our_work/humanitarian_assistance/disaster_assistance/resources/pdf/OFDA_Guidelines_Unsolicited_Proposals_Reporting.pdf. Accessed December 20, 2007.

HEALTH	Maternal Health	-Percentage of pregnant women who have attended at least 2 ANC visits -Percentage of pregnant women receiving iron and folic acid at the ANC -Percentage of pregnant women who received a clean delivery kit -Percentage of women vaccinated with tetanus toxoid (TT2) -Percentage of pregnant women in their 2nd and 3rd trimester attending an ANC who receive intermittent presumptive therapy for malaria -Percentage of pregnant women utilizing ITNs -Percentage of pregnant women who deliver assisted by a trained provider (indicate type of provider such as midwife, TBA) -Percentage of women who attend at least one postpartum visit with trained provider (including provision of Vitamin A where appropriate) -Maternal mortality ratio (MMR) -Neonatal mortality rate
	Expanded Program of Immunization (EPI)	-Immunization coverage <1 year age (DPT3, Polio3). -Measles immunization coverage, ages 6 month to 15 years old. -Vitamin A coverage, 6 to 59 months.
	Health Education	-Percentage of target population properly washing hands at appropriate times (see hygiene section of WASH for more details) -Percentage of target population with knowledge of and practicing two methods to prevent diarrhea -Percentage of population properly managing diarrhea at home -Percentage of women exclusively breastfeeding for six months -Percentage of target population able to identify the transmission and prevention of malaria -Percentage of utilization rate of ITNs in total population, children under 5, and pregnant women -Percentage of target population with knowledge of when to seek care for children with respiratory difficulty, fever and diarrhea (dehydration), complications of pregnancy
	HIV/AIDS	-Percentage of target population with the knowledge of two types of transmission and prevention of HIV/AIDS
NUTRITION	Community Therapeutic Care	General -GAM and SAM rates decreased to pre-crisis level. (Provide current crisis level and pre-crisis data.) -CTC program as a whole: -Coverage rate: in rural areas >70%; in urban areas >70%; in IDP camps > 90% Particular to Outpatient Therapeutic Programs (OTP) -Default rate: <15% -Death rate: <10% -Average length of stay in OTP: <60 days -Weight gain: >4g/kg/day Particular to Stabilization Centers (SC) -Average length of stay in SC 4-7 days -Referrals to hospital are <10% of exits
	Nutrition Education	-Percentage change in practice pertaining to nutrition education topics.
	Supplementary Feeding Programs	-MAM rates decreased to pre-crisis level. (Provide current crisis level and pre-crisis data, focusing on moderate acute malnutrition rates.) -Stand Alone SFP: Coverage rate: in rural areas >50%; in urban areas >70%; in IDP camps >90% (Sphere Guidelines) -If the SFP is part of a CTC program then the coverage rate should be as follows: in rural areas >70%; in urban areas >70%; in IDP camps >90% -Default rate: <15% -Cure rate: >75% -Death rate: <3% -Number of beneficiaries treated in the SFP (disaggregated by under-fives and adults)
	Therapeutic Feeding Programs	-SAM rates decreased to pre-crisis level (provide current crisis level and pre-crisis data) -Number of beneficiaries treated in the TFP (disaggregated by under- fives and adults) -Coverage rate: in rural areas >50%; in urban areas >70%; in IDP camps > 90% -Default rate: <15% -Death rate: <10% -Cure rate: >75%

SHELTER & SETTLEMENTS	Camp Design and Management	-Percentage of total affected population receiving shelter assistance.
	Emergency Shelter	-Percentage of total affected population receiving shelter assistance
	Transitional Shelter and Settlements	-Percentage share of total affected population receiving shelter assistance
WATER, SANITATION & HYGIENE (WASH)	Hygiene Promotion	-Average increase in good hand washing practices (reported as percentage of people) -Average increase in correct water usage practices (reported as percentage of people)
	Water	-Percent of household water supplies with 0 fecal coliforms per 100 ml
CROSS-CUTTING THEMES	Artisanal Production	-Number and percentage of participants with increased production -Number and percentage of participants with increased sales -Average increase in income of participants in USD over the life of the program
	Capacity Building / Training	Training -Percentage of beneficiaries using their skills training within their livelihood activity
	Cash for Work (CFW)	-Percentage of beneficiaries reporting increased access to food and non-food items as a result of the program.
	Gender Relations	-Percentage of the men and women in the targeted population or who report improvements in gender relations as a result of the activities
	Livelihoods / Income Generation	-Average amount of income in USD generated by participant/ month
	Market Rehabilitation	-Percentage of targeted beneficiaries with increased engagement in economic activities
	Micro-Finance / Micro-Credit	-Percentage of micro-credit recipients who rehabilitate or establish new businesses -Average amount (in USD) of increase in income of participants over the life of the program (based on pre-program baseline)
	Protection Mainstreaming	Agriculture and Food Security Sector: -Percent of vulnerable populations receiving full designated rations -Percentage of women escorted to agricultural fields in insecure areas

APPENDIX 4.

Example of a Questionnaire for Use in an Emergency Setting

Team #_______ Admin. Unit___________________________ Date __________

Cluster #______ Cluster Name ________________________ HH #__________

REQUEST FOR PARTICIPATION

Once participation of respondent is secured, ask the questions on the next page.

(1.) INTERVIEWER: ASK FOR RESPONDENT'S PARTICIPATION IN THE SURVEY	1. YES, ACCEPTED 2. NO, REFUSED 3. TEMPORARILY ABSENT (MAKE A NOTE FOR HOW TO FIND HOUSEHOLD, FOR RETURN VISIT)	☐ IF 1, CONTINUE. ***IF 2 OR 3, END INTERVIEW & GO TO NEXT HOUSEHOLD***
(2.) RESPONDENT'S ROLE IN HOUSEHOLD (HH)	1. MALE HEAD OF HH 2. FEMALE HEAD OF HH 3. FEMALE ADULT 4. MALE ADULT 5. CHILD (BELOW 15 YEARS)	☐
(3a.) HEAD OF HOUSEHOLD	1. MALE 2. FEMALE 3. CHILD (BELOW 15 YEARS)	☐
(3b.) MARITAL STATUS OF HEAD OF HH	1. MARRIED; FAMILY HAS ONE WIFE 2. MARRIED; FAMILY HAS MORE THAN ONE WIFE 3. WIDOWED/WIDOWER 4. NEVER MARRIED/SINGLE 5. DIVORCED/SEPARATED	☐
(4.) ORIGIN OF THE FAMILY	1. RESIDENT (NEVER DISPLACED) 2. NOMAD 3. IDP 4. RETURNEE (RETURNED IDP)	☐

HOUSEHOLD ROSTER

LINE #	First Name of HH member First , ask about HH members currently in HH. Then, ask about HH members who no longer live in HH but were present at beginning or recall period.	Age in Years (If <1 year, code 0 years) CIRCLE AGE IN YEARS, IF <5 YEARS	Sex 1=Male 2=Female
	(5)	(6)	(7)
01			
02			
03			
04			
05			
06			
07			
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20			

HOUSEHOLD SECTION OF QUESTIONNAIRE

8. WHAT IS THE MAIN SOURCE OF YOUR FOOD?	1. OWN PRODUCTION 2. FREE FOOD (GFD) 3. BORROW 4. BOUGHT 5. FOOD FOR WORK 6. EXCHANGE 7. GIFT 8. OTHER	
9. WHAT IS THE MAIN SOURCE OF YOUR HH'S INCOME?	1. PASTORALIST 2. AGRICULTURE 3. TRADE 4. LABOR 5. OTHER EMPLOYMENT 6. SELL RELIEF ITEMS 7. OTHER 8. NONE	
10. HOW MANY OF EACH KIND OF ANIMAL DO YOU HAVE? (READ EACH TYPE OF ANIMAL) (CODE 99 FOR DON'T KNOW)	CAMEL ααααααααααα COW ααααααααααα DONKEY ααααααααααα HORSE ααααααααααα POULTRY ααααααααααα SHEEP ααααααααααα GOAT ααααααααααα	☐☐ ☐☐ ☐☐ ☐☐ ☐☐ ☐☐ ☐☐
11. DID THE HH HAVE A FOOD SHORTAGE IN THE LAST 3 MONTHS?	1. YES 2. NO 3. DON'T KNOW	☐ IF 1, GO TO Q12; IF 2 OR 3, SKIP TO Q13

12. HOW DID THE HH FILL THE GAP? (DO NOT READ RESPONSES; MARK ALL THAT RESPONDENT MENTIONS, MULTIPLE RESPONSES ALLOWED).	01. SOLD LIVESTOCK αααααα 02. SOLD HH ASSETS αααααα 03. UNUSUAL CONSUMPTION OF WILD FOODSαααααα 04. LABOR MIGRATION αααααα 05. SELL RELIEF ITEMS αααααα 06. ATE FEWER MEALS αααααα 07. ATE LESS FOOD PER MEAL αααααα 08. BORROWED CASH OR RAIN αααααα 09. SOLD FIREWOOD αααααα 10. SCHOOL WITHDRAWAL αααααα 11. ATE LESS PREFERRED FOOD αααααα 12. SOLD, ATE SEEDS 13. MATERNAL BUFFERING 14. WORK OUTSIDE THE FARM αααααα 15. SOLD PERSONAL EFFECTS αααααα 16. OTHER 99. DK αααααα	☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
13. DO YOU HAVE A RATION CARD?	1. YES 2. NO 3. DON'T KNOW	☐

14. DID YOU RECEIVE A RATION FROM THE GFD IN THE LAST 3 MONTHS?	1. YES 2. NO 3. DON'T KNOW	IF 1, GO TO Q.15 IF 2 OR 3, SKIP TO Q.17
15. HOW MANY TIMES IN THE LAST 3 MONTHS DID YOU RECEIVE A RATION FROM THE GFD? (Link 3 months with event) (CODE 99 FOR DON'T KNOW)	NUMBER OF TIMES ααααααααααα	
16. WHICH COMMODITIES DID YOU RECEIVE WITH THE LAST GFD? (MARK ALL THAT RESPONDENT MENTIONS, MULTIPLE RESPONSES ALLOWED).	1. CEREALS αααααα 2. LENTILS αααααα 3. CSB αααααα 4. OIL αααααα 5. SALT α 6. SUGAR αααααα 7. OTHER αααααα 8. DON'T KNOW αααααα	
17. WHAT IS THE NUMBER OF MEALS THAT ADULTS IN THIS HH EAT EACH DAY IN NORMAL TIMES, AT THIS TIME OF YEAR? (CODE 99 FOR DON'T KNOW)	NUMBER OF MEALS PER DAY IN NORMAL TIMES ααααααααααα	
18. WHAT IS THE NUMBER OF MEALS THAT ADULTS IN THIS HH ATE YESTERDAY? (CODE 99 FOR DON'T KNOW)	NUMBER OF MEALS YESTERDAY ααααααααααα	
19. WHAT IS YOUR MAIN SOURCE OF DRINKING WATER?	1. STREAM 2. HAND PUMP 3. HAFIER (POND) 4. MASHIESH (SHALLOW WELL) 5. WELL (UNPROTECTED) 6. DONKEY CART 7. OTHER 8. DON'T KNOW	

20. HOW LONG DOES IT TAKE TO WALK (ONE WAY) TO COLLECT WATER?	1. LESS THAN 30 MINUTES 2. FROM 30 MINUTES TO 2 HOURS 3. MORE THAN 2 HOURS 4. DON'T KNOW	
21. HOW MANY JERRY CANS OF WATER DID YOUR HH USE YESTERDAY (TOTAL CONSUMPTION: DRINKING, COOKING, WASHING AND CLEANING)? (CODE 99 FOR DON'T KNOW)	TOTAL NUMBER OF JERRYCANS YESTERDAY	
22. DOES YOUR HH HAVE A LATRINE?	1. YES 2. NO 3. DON'T KNOW	
23. DO THE MEMBERS OF YOUR HH USE A LATRINE?	1. YES, ALL MEMBERS USE 2. YES, SOME MEMBERS USE 3. NONE USE 4. NOT APPLICABLE, HH HAS NO LATRINE	
24. HOW FAR IS IT TO WALK TO THE NEAREST HEALTH FACILITY?	1. LESS THAN 30 MINUTES 2. FROM 30 MINUTES TO 2 HOURS 3. MORE THAN 2 HOURS 4. DON'T KNOW	
25. DO YOU HAVE BEDNETS IN YOUR HOME?	1. YES 2. NO 3. DON'T KNOW	IF NO, SKIP TO Q26a.
26. RETURN TO THE HH ROSTER AND FOR EACH HH MEMBER ASK IF HE/SHE SLEPT UNDER A BEDNET LAST NIGHT.	CIRCLE THE LINE NUMBER OF EACH HH MEMBER REPORTED TO SLEEP UNDER A BEDNET LAST NIGHT. MARK (AT THE HH ROSTER) Y FOR YES OR N FOR NO IF IT WAS IMPREGNATED	
26a. IN THE PAST FOUR WEEKS, WAS THERE EVER NO FOOD TO EAT OF ANY KIND IN YOUR HOUSE?	1. YES 2. NO	IF NO, SKIP TO Q27a.
26b. HOW OFTEN DID THIS HAPPEN?	1. SOMETIMES (1 – 10 TIMES IN THE PAST FOUR WEEKS) 2. OFTEN (MORE THAN 10 TIMES IN THE PAST FOUR WEEKS)	

27a. IN THE PAST FOUR WEEKS, DID YOU OR ANY HOUSEHOLD MEMBER GO TO SLEEP AT NIGHT HUNGRY BECAUSE THERE WAS NOT ENOUGH FOOD?	1. YES 2. NO	☐ IF NO, SKIP TO Q28a.
‘ 27b. HOW OFTEN DID THIS HAPPEN?	1. SOMETIMES (1 – 10 TIMES IN THE PAST FOUR WEEKS) 2. OFTEN (MORE THAN 10 TIMES IN THE PAST FOUR WEEKS)	☐
28a. IN THE PAST FOUR WEEKS, DID YOU OR ANY HOUSEHOLD MEMBER GO A WHOLE DAY AND NIGHT WITHOUT EATING ANYTHING BECAUSE THERE WAS NOT ENOUGH FOOD?	1. YES 2. NO	☐ IF NO, SKIP TO CHILD SECTION OF QUESTION-NAIRE.
28b. HOW OFTEN DID THIS HAPPEN?	1. SOMETIMES (1 – 10 TIMES IN THE PAST FOUR WEEKS) 2. OFTEN (MORE THAN 10 TIMES IN THE PAST FOUR WEEKS)	☐

FOR EACH CHILD 6-59 MONTHS ASK THE QUESTIONS ON THE NEXT PAGE

CHILD SECTION OF QUESTIONNAIRE

For the questions below, ask to speak to the caregiver of the 6-59 month child you are asking about. If the caregiver of the child is not available, speak to the caregiver of the child. Keep in mind that if the household has children with different caregivers, the respondent for the questions may differ by child. One row should be completed for each child 6-59 months living in the household. If not every child 6-59 months living in the household is at the home during time of your visit, follow the protocol established during interviewer training.

Team #	Admin Unit	Date
Cluster #	Cluster Name	HH #

#	NAME	SEX	BIRTH-DATE	AGE IN MTH	RESP.	CHILD AVAIL	VACCINATION AND MICRONUTRIENT SUPPLEMENTATION			SICKNESS IN LAST TWO WEEKS		SF	IN PATIENT CARE		HEIGHT OR LENGTH	WEIGHT	WHM	MUAC	BILATERAL PITTING EDEMA	REFERRAL
Child Number	Name of Child	Sex (Male=1, Female =2)	Record Date, Month and Year in Gregorain Calendar (ex. 29-04-99)	Enter age in months (Use events calendar)	Respondent (1-Mother, 2=Father, 3=Other)	Is child currently available for survey to be measured? (1=Yes; 2=Absent; 3= Refused)	BCG Mark	Measles	Vit. A in last 6 months (fix with event)	1=3 or more watery stools in 24 H/Diarrhea 2=Fever and difficult breathing/ARI 3=Fever and chills/ Malaria; 4=Other 5=Unknown 6=Not Applicable		Is the child enrolled in a selective feeding prorgam?	Is the child currently staying at an in patient care centre?		In cm Record to nearest 0.1 cm Circle if measured supine	In KG Record to nearest 0.1 KG	Calculated with the combined for boys and girls	In cm Record to nearest 0.1 cm		Does the child need forwarding to the CTC?
							Yes=1 No=2 DK=3	Yes, card=1 Yes, no card=2 No=3 DK=4	Yes=1 No=2 DK=3	Yes=1 No=2 DK=3	Type of illness (see codes above) (Multiple responses allowed for different episodes of illness)	1=SFP 2=OTP 3=Day care centre 4=SC 5=No 6=DK	Yes=1 No=2	Where					Yes=1 No=2 DK=3	Yes=1 No=2
31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51
1																				
2																				
3																				
4																				
5																				
6																				
7																				

APPENDIX 5.

Random Number Table

Source: The Rand Corporation, *A Million Random Digits with 100,000 Normal Deviates,* New York: The Free Press, 1955.

13962	70992	65172	28053	02190	83634	66012	70305	66761	88344
43905	46941	72300	11641	43548	30455	07686	31840	03261	89139
00504	48658	38051	59408	16508	82979	92002	63606	41078	86326
61274	57238	47267	35303	29066	02140	60867	39847	50968	96719
43753	21159	16239	50595	62509	61207	86816	29902	23395	72640
83503	51662	21636	68192	84294	38754	84755	34053	94582	29215
36807	71420	35804	44862	23577	79551	42003	58684	09271	68396
19110	55680	18792	41487	16614	83053	00812	16749	45347	88199
82615	86984	93290	87971	60022	35415	20852	02909	99476	45568
05621	26584	36493	63013	68181	57702	49510	75304	38724	15712
06936	37293	55875	71213	83025	46063	74665	12178	10741	58362
84981	60458	16194	92403	80951	80068	47076	23310	74899	87929
66354	88441	96191	04794	14714	64749	43097	83976	83281	72038
49602	94109	36460	62353	00721	66980	82554	90270	12312	56299
78430	72391	96973	70437	97803	78683	04670	70667	58912	21883
33331	51803	15934	75807	46561	80188	78984	29317	27971	16440
62843	84445	56652	91797	45284	25842	96246	73504	21631	81223
19528	15445	77764	33446	41204	70067	33354	70680	66664	75486
16737	01887	50934	43306	75190	86997	56561	79018	34273	25196
99389	06685	45945	62000	76228	60645	87750	46329	46544	95665
36160	38196	77705	28891	12106	56281	86222	66116	39626	06080
05505	45420	44016	79662	92069	27628	50002	32540	19848	27319
85962	19758	92795	00458	71289	05884	37963	23322	73243	98185
28763	04900	54460	22083	89279	43492	00066	40857	86568	49336
42222	40446	82240	79159	44168	38213	46839	26598	29983	67645
43626	40039	51492	36488	70280	24218	14596	04744	89336	35630
97761	43444	95895	24102	07006	71923	04800	32062	41425	66862
49275	44270	52512	03951	21651	53867	73531	70073	45542	22831
15797	75134	39856	73527	78417	36208	59510	76913	22499	68467
04497	24853	43879	07613	26400	17180	18880	66083	02196	10638
95468	87411	30647	88711	01765	57688	60665	57636	36070	37285
01420	74218	71047	14401	74537	14820	45248	78007	65911	38583
74633	40171	97092	79137	30698	97915	36305	42613	87251	75608
46662	99688	59576	04887	02310	35508	69481	30300	94047	57096
10853	10393	03013	90372	89639	65800	88532	71789	59964	50681
68583	01032	67938	29733	71176	35699	10551	15091	52947	20134
75818	78982	24258	93051	02081	83890	66944	99856	87950	13952
16395	16837	00538	57133	89398	78205	72122	99655	25294	20941
53892	15105	40963	69267	85534	00533	27130	90420	72584	84576
66009	26869	91829	65078	89616	49016	14200	97469	88307	92282
45292	93427	92326	70206	15847	14302	60043	30530	57149	08642
34033	45008	41621	79437	98745	84455	66769	94729	17975	50963
13364	09937	00535	88122	47278	90758	23542	35273	67912	97670
03343	62593	93332	09921	25306	57483	98115	33460	55304	43572
46145	24476	62507	19530	41257	97919	02290	40357	38408	50031
37703	51658	17420	30593	39637	64220	45486	03698	80220	12139
12622	98083	17689	59677	56603	93316	79858	52548	67367	72416
56043	00251	70085	28067	78135	53000	18138	40564	77086	49557
43401	35924	28308	55140	07515	53854	23023	70268	80435	24269
18053	53460	32125	81357	26935	67234	78460	47833	20496	35645

APPENDIX 6.

Instructions to Carry Out an Intermediate Stage of Sampling Between Sampling Stages 1 and 2

STEPS TO CARRY OUT AN INTERMEDIATE STAGE OF SAMPLING BETWEEN SAMPLING STAGES 1 AND 2

STEP 1

Upon visiting each cluster selected for sampling, the cluster should be mapped and divided into segments, each comprised of an approximately equal number of households. Keep the number of households per segment constant for all PSUs that are sampled for the survey.

STEP 2

Select one of the segments randomly using a random number table. If more than one cluster of data will be collected in the PSU, allow all segments to be available for selection for each cluster. Do not sample segments without replacement.

STEP 3

Complete the random walk procedure in the selected segment as outlined in Box 5, but use the center of the selected segment as the point from which to spin the pen and the perimeter of the segment as the point from which to enumerate the households.

STEP 4

After selection of the first random household in the segment proceed as previously. For subsequent households selected, it is ok if the household falls outside the boundaries of the selected segment. The segmentation described here is simply used to reduce the random walk burden for the selection of the first random household without altering the probabilities of selection.

Example of Detailed Sampling Guidance for Interviewers

APPENDIX 7.

Detailed instructions for selecting subsequent households for sampling in a cluster

After selection of the first random household in the cluster, the remaining households to be sampled are selected by proximity. For the purpose of this survey, the nearest household to the right of the front door (when facing it) of the first random household should be sampled next, and the next nearest household to the right after that, and so on.

What if there are no more houses to sample in the indicated direction?

If there is ever an instance where there are no more houses in the direction in which sampling is meant to proceed – or, in the case that the interview team has reached the perimeter of the cluster during the household sampling process, turn left 90 degrees and sample the next nearest household to the left, and the next household to the left after that, and so on.

Households should never be sampled more than once. If, after switching directions, there are still no more households within the perimeter of the cluster to sample, the random walk method can be repeated a second time so that sampling continues with a second random start point identified. If the random walk method needs to be implemented a second time, return to the center of the cluster and proceed from steps 1 thru 7 **(Box 5)** to identify where to continue sampling. This should happen very infrequently, if ever, however.

Detailed instructions for handling absentees and refusals

It is essential to know how to handle a situation when an eligible respondent is not at home, or a dwelling is uninhabited. Instructions for how to handle these situations are detailed below. It is just as important that each of these scenarios be addressed correctly as it is that the methods for selection of households and respondents be implemented properly. Both are necessary for unbiased data to be collected for the survey.

What if nobody is home at a household selected for sampling?

Ask the neighbors if the house is inhabited and when the household members will return.

If the occupants are nearby and will be back today, record the household on the Cluster Tracking Form and indicate that a revisit is necessary. Go back to the house later when the members will be home. Do not substitute another household without permission of your supervisor. <u>At least two revisits should be made to collect data from the eligible respondents in that household</u>.

If the occupants are out of town and won't be back before the survey will be completed, record the household on the Cluster Tracking Form and write OUT OF TOWN across the row. You may proceed to the next closest household.

What if no adult is at home at a household selected for sampling?

Arrange to come back at another time, when an adult is at home. Record the household on the Cluster Tracking Form and indicate that a revisit is necessary. Go back to the house later when the members will be home. Do not substitute another household without permission of your supervisor. At least two revisits should be made to collect data from the eligible respondents in that household.

What if a child 6-59 months is not at home?

Administer the appropriate questions with the household members currently at home. If a child 6-59 months is not currently at home or refuses to have anthropometric measurements taken at that time, record the household on the Cluster Tracking Form and indicate that a revisit is necessary. Arrange to come back at another time to complete data collection at the household. If a child is at a therapeutic feeding center, or admitted to the health clinic, you must go to that site to collect anthropometric measurements on the child. Do not substitute another child without permission of your supervisor. At least two revisits should be made to collect data from the children 6-59 months in that household.

What if nobody is at home after two revisits?

Fill in the revisit dates and times for the household on the Cluster Tracking Form. Inform your supervisor who may ask you to revisit this household, or visit another household.

What if a child 6-59 months is not at home after two revisits?

Fill in the revisit dates and times for the household on the Cluster Tracking Form. Inform your supervisor who may ask you to revisit this household, or visit another household to find another eligible child 6-59 months to include in the sample.

What if an entire household or an individual respondent refuses to be interviewed?

It is very important that all selected households and all eligible respondents within households participate in the survey. Every effort should be made to obtain the interviews. If a head of household or a respondent refuses, ask if there is a better time for you to come back and conduct the survey. If they still refuse, inform them that you will ask your supervisor to visit the households. Inform your supervisor of the problem. The supervisor will talk to the reluctant respondent(s). If the household/respondent still refuses, mark "Refusal" for the household/respondent on the Cluster Tracking Form. Your supervisor may ask you to visit another household.

Decision Rules for LQAS Analysis with Smaller than Ideal 33x6 and 67x3 Sample Sizes

APPENDIX 8.

Decision Rules (DR), Alpha (α) and Beta (β) Errors[1] for the 33x6 Design by Sample Size for the 10% and 15% Thresholds

Sample Size	Upper Threshold Level for Assessing the Prevalence of Acute Malnutrition	
	10% Acute Malnutrition Decision Rule (DR) to Classify the Prevalence as < 10%	15% Acute Malnutrition Decision Rule (DR) to Classify the Prevalence as < 15%
198	≤ 13 (α ≤ 0.06, β ≤ 0.12)	≤ 23 (α ≤ 0.10, β ≤ 0.18)
197	≤ 13 (α ≤ 0.06, β ≤ 0.12)	≤ 23 (α ≤ 0.11, β ≤ 0.18)
196	≤ 13 (α ≤ 0.07, β ≤ 0.12)	≤ 23 (α ≤ 0.12, β ≤ 0.18)
195	≤ 13 (α ≤ 0.07, β ≤ 0.11)	≤ 22 (α ≤ 0.08, β ≤ 0.23)
194	≤ 13 (α ≤ 0.07, β ≤ 0.11)	≤ 22 (α ≤≤0.09, β ≤ 0.22)
193	≤ 13 (α ≤ 0.08, β ≤ 0.11)	≤ 22 (α ≤ 0.09, β ≤ 0.22)
192	≤ 13 (α ≤ 0.08, β ≤ 0.10)	≤ 22 (α ≤ 0.10, β ≤ 0.21)

Decision Rules (DR), Alpha and Beta Errors[1] for the 67x3 Design by Sample Size for the 10% and 15% Thresholds

Sample Size	Upper Threshold Level for Assessing the Prevalence of Acute Malnutrition	
	10% Acute Malnutrition Decision Rule (DR) to Classify the Prevalence as < 10%	15% Acute Malnutrition Decision Rule (DR) to Classify the Prevalence as < 15%
201	≤ 13 (α ≤ 0.05, β ≤ 0.13)	≤ 23 (α ≤ 0.09, β ≤ 0.20)
200	≤ 13 (α ≤ 0.06, β ≤ 0.13)	≤ 23 (α ≤ 0.10, β ≤ 0.20)
199	≤ 13 (α ≤ 0.06, β ≤ 0.13)	≤ 23 (α ≤ 0.10, β ≤ 0.19)
198	≤ 13 (α ≤ 0.06, β ≤ 0.12)	≤ 23 (α ≤ 0.10, β ≤ 0.18)
197	≤ 13 (α ≤ 0.06, β ≤ 0.12)	≤ 23 (α ≤ 0.11, β ≤ 0.18)
196	≤ 13 (α ≤ 0.07, β ≤ 0.12)	≤ 23 (α ≤ 0.12, β ≤ 0.18)
195	≤ 13 (α ≤ 0.07, β ≤ 0.11)	≤ 22 (α ≤ 0.08, β ≤ 0.23)

[1] These alpha and beta errors are approximate and assume an ICC of 0.00. The exact alpha and beta errors will depend on the ICC of the acute malnutrition data collected. For more information on the range of alpha and beta errors that could be expected given varying levels of intra-cluster correlation, refer to Olives et al. 2009.

APPENDIX 9.

Example of PPS Cluster Selection for a 33x6 Stratified Design (4 Strata), Wobelleno Province

	VILLAGE	TOTAL POPULATION	CUMULATIVE POPULATION	RANGE		CLUSTER ALLOCATION
DISTRICT 1	**Shola**	**1200**	**1200**	**0**	**1200**	**1**
	Elmiol	208	1408	1201	1408	
	Dabi	**1350**	**2758**	**1409**	**2758**	**2**
	Doabol	**450**	**3208**	**2759**	**3208**	**3**
	Elmajoha	148	3356	3209	3356	
	Madradam	**5002**	**8358**	**3357**	**8358**	**4,5,6,7,8**
	Esalaman	**1250**	**9608**	**8359**	**9608**	**9**
	Elmajor	205	9813	9609	9813	
	Salala A	**2000**	**11813**	**9814**	**11813**	**10, 11**
	Salala B	**2505**	**14318**	**11814**	**14318**	**12,13,14**
	Andarou	**1,080**	**15398**	**14319**	**15398**	**15**
	Ardaba	425	15823	15399	15823	
	Elzama	**755**	**16578**	**15824**	**16578**	**16**
	Eltaraj	**880**	**17458**	**16579**	**17458**	**17**
	Elwasa East	**1120**	**18578**	**17459**	**18578**	**18**
	Elwasa West	**1,250**	**19828**	**18579**	**19828**	**19**
	Emara A	**1815**	**21643**	**19829**	**21643**	**20,21**
	Emara B	**1,438**	**23081**	**21644**	**23081**	**22**
	Sabi	**1320**	**24401**	**23082**	**24401**	**23,24**
	Arbaba	**1810**	**26211**	**24402**	**26211**	**25**
	Elthoram	**879**	**27090**	**26212**	**27090**	**26**
	Giman East	**682**	**27772**	**27091**	**27772**	**27**
	Giman West	**940**	**28712**	**27773**	**28712**	**28**
	Ramakal	**725**	**29437**	**28713**	**29437**	**29**
	Sunaba	584	30021	29438	30021	
	Adar East	118	30139	30022	30139	
	Adar West	**1110**	**31249**	**30140**	**31249**	**30**
	Borala A	**1487**	**32736**	**31249**	**32736**	**31,32**
	Borala B	423	33159	32737	33159	
	Kabanba	**408**	**33567**	**33160**	**33567**	**33**

	VILLAGE	TOTAL POPULATION	CUMULATIVE POPULATION	RANGE		CLUSTER ALLOCATION
DISTRICT 2	**Koborga**	**3812**	**3812**	**0**	**3812**	**1,2,3**
	Mandarlam	**2205**	**6017**	**3813**	**6017**	**4**
	Naban	**1408**	**7425**	**6018**	**7425**	**5,6**
	Rafol	1210	8635	7426	8635	
	Umdo A	**2350**	**10985**	**8636**	**10985**	**7,8**
	Umdo B	**1250**	**12235**	**10986**	**12235**	**9**
	Tamorala	**4182**	**16417**	**12236**	**16417**	**10,11,12**
	Estab A	**3242**	**19659**	**16418**	**19659**	**13,14**
	Estab B	**2512**	**22171**	**19660**	**22171**	**15,16**
	Lionar	**2123**	**24294**	**22172**	**24294**	**17,18**
	Doba	**1254**	**25548**	**24295**	**25548**	**19**
	Satun	**1226**	**26774**	**25549**	**26774**	**20**
	Galda	**1723**	**28497**	**26775**	**28497**	**21**
	Sabar	824	29321	28498	29321	
	Bagar	**737**	**30058**	**29322**	**30058**	**22**
	Shakam	104	30162	30059	30162	
	Faiga East	**1100**	**31262**	**30163**	**31262**	**23**
	Faiga West	**2090**	**33352**	**31263**	**33352**	**24**
	Shaloko	**198**	**33550**	**33353**	**33550**	**25**
	Egaz	284	33834	33551	33834	
	Elrassa	**2587**	**36421**	**33835**	**36421**	**26,27**
	Elfah	**2235**	**38656**	**36422**	**38656**	**28**
	Efa A	242	38898	38657	38898	
	Efa B	**737**	**39635**	**38899**	**39635**	**29**
	Korogoa	436	40071	39636	40071	
	Emaja	**842**	**40913**	**40072**	**40913**	**30**
	Elhaba A	324	41237	40914	41237	
	Elhaba B	234	41471	41238	41471	
	Eltamon	151	41622	41472	41622	
	Elwa East	89	41711	41623	41711	
	Elwa West	23	41734	41712	41734	
	Elhoor	**151**	**41885**	**41735**	**41885**	**31**
	Emdad	352	42237	41886	42237	
	Sambara	262	42499	42238	42499	
	Abaolo	111	42610	42500	42610	
	Eshowara	**2515**	**45125**	**42611**	**45125**	**32,33**
	Nesa A	262	45387	45126	45387	

DISTRICT 3

VILLAGE	TOTAL POPULATION	CUMULATIVE POPULATION	RANGE		CLUSTER ALLOCATION
Nesa B	363	363	0	363	1,2
Ramak	842	1205	364	1205	3,4
Sunata	1254	2459	1206	2459	5,6,7,8
Darbu	2352	4811	2460	4811	9
Bukabal	124	4935	4812	4935	10
Bordiro	737	5672	4936	5672	
Bagunda	236	5908	5673	5908	11
Golonaj	484	6392	5909	6392	12
Rongaba	685	7077	6393	7077	13
Garsa	346	7423	7078	7423	14
Bagaia	457	7880	7424	7880	
Fota	346	8226	7881	8226	15
Ribo A	234	8460	8227	8460	
Ribo B	236	8696	8461	8696	16,17
Moraldab	848	9544	8697	9544	
Toesa A	373	9917	9545	9917	18
Toesa B	548	10465	9918	10465	19
Lomolab	362	10827	10466	10827	20
Salan	262	11089	10828	11089	
Bidojol	473	11562	11090	11562	21
Nalda	578	12140	11563	12140	22,23
Tunaj	734	12874	12141	12874	24
Ramoro	845	13719	12875	13719	25
Kamshak	573	14292	13720	14292	26
Galei A	235	14527	14293	14527	
Galei B	135	14662	14528	14662	
Loki	262	14924	14663	14924	27
Gazrat	123	15047	14925	15047	
Rasgooz	151	15198	15048	15198	28
Falah	515	15713	15199	15713	
Fasaja A	262	15975	15714	15975	29
Fasaja B	287	16262	15976	16262	
Lamako A	222	16484	16263	16484	
Lamako B	188	16672	16485	16672	30
Shati East	337	17009	16674	17009	
Shati West	126	17135	17010	17135	31
Damonor	190	17325	17136	17325	
Wahda	67	17392	17326	17392	
Esdab	378	17770	17393	17770	32
Elorzaj	327	18097	17771	18097	
Tedadal	96	18193	18098	18193	
Saj	48	18241	18194	18241	
Arbal	283	18524	18242	18524	33

DISTRICT 4

VILLAGE	TOTAL POPULATION	CUMULATIVE POPULATION	RANGE		CLUSTER ALLOCATION
Warat	89	89	0	89	1
Sagima A	151	240	90	240	
Sagima B	738	978	241	978	2
Khaya	955	1933	979	1933	3
Tajab	1545	3478	1934	3478	4,5
Darbu Adar	825	4303	3479	4303	6
Andar Agal	279	4582	4304	4582	
Boto bol	379	4961	4583	4961	7
Hagal	954	5915	4962	5915	8
Bagooj	236	6151	5916	6151	
Robagol	437	6588	6152	6588	9
Mansa	480	7068	6589	7068	
Nabag	594	7662	7069	7662	10
Rota	265	7927	7663	7927	11
Umbo A	216	8143	7928	8143	
Umbo B	3219	11362	8144	11362	12,13,14,15
Tamab	2235	13597	11363	13597	16,17,18
Eresta A	2984	16581	13598	16581	19,20,21,22
Eresta B	344	16925	16582	16925	
Lilbono	1262	18187	16926	18187	23,24
Masanaj	151	18338	18188	18338	
Dobesab	1621	19959	18339	19959	25,26
Gobada	162	20121	19960	20121	
Sasunlaj	3738	23859	20122	23859	27,28,29,30,31
Batamoro	2161	26020	23862	26020	32,33