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A New Method to Estimate Mortality in Crisis-Affected Populations: Validation and Feasibility Study

VERSION 2

Principal Investigators: Francesco Checchi, Bayard Roberts, and Oliver Morgan

March 2009

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Acknowledgments

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Acronyms and Abbreviations

AIDS	Acquired immune deficiency syndrome
AMI	Aide Médicale Internationale
BIC	Bayesian Information Criterion
BMA	Bayesian Model Averaging
CDC	United States Centers for Disease Control and Prevention
CHW	Community health worker
CI	Confidence interval
CMR	Crude mortality rate
CSAS	Centric Systematic Area Sample
d	Day
Deff	Design effect
DHO	District Health Office
EM	Exhaustive measurement
EPI	Expanded Program of Immunization
FANTA	Food and Nutrition Technical Assistance Project
FGD	Focus group discussion
GPS	Global positioning system
h	Hour
HIV	Human immunodeficiency virus
HSA	Health Surveillance Assistant
HTML	HyperText Markup Language
ICD-10	International Classification of Diseases, version 10
IDP	Internally displaced persons
IRC	International Rescue Committee
LSHTM	London School of Hygiene and Tropical Medicine
MSF-F	Médecins Sans Frontières – France
NGO	Nongovernmental organization
OC	Operating characteristic
OLS	Ordinary least squares
PPS	Probability proportional to size
PSU	Primary sampling unit
SMART	Standardised Monitoring and Assessment in Relief and Transition
SMRI	Shoklo Medical Research Institute
SMRU	Shoklo Malarial Research Unit
STI	Sexually transmitted infection
TBA	Traditional birth attendant
TRCS	Tanzanian Red Cross Society
U5MR	Under 5 mortality rate
UNHCR	United Nations High Commissioner for Refugees
USD	United States Dollars
WHO	World Health Organization

Executive Summary

BACKGROUND

Data on mortality rates and the causes and circumstances of death are crucial to guide health interventions in crisis-affected populations, and monitor their effectiveness. The two methods currently available to collect mortality data are prospective surveillance and retrospective surveys. However, these methods require substantial resources and feature important methodological limitations.

RATIONALE AND OBJECTIVES

We evaluated an alternative method for rapidly quantifying mortality through **exhaustive measurement** (henceforth referred to as the **EM method**). The EM method captures deaths through an exhaustive search for all deaths occurring in the community over a defined and very short recall period. Unlike retrospective surveys, it provides nearly real-time mortality estimates, which are most useful for operational purposes in relief settings. The search process mainly depends on key community informants, selected after rapid qualitative work, who lead data collectors to households whom they recall as having experienced a recent death; next-of-kin of decedents can also act as informants leading the data collectors to other recently bereaved households. The process continues until informants can no longer identify households with recent deaths.

The main study objectives were to evaluate the validity and feasibility of the EM method. In terms of validity, we sought to investigate whether the EM method would capture recent deaths in the selected communities with sufficient sensitivity. In terms of feasibility, we sought to record the time and cost required for data collection and analysis, explore the ethical implications of using the EM method, and assess the inclusion of verbal autopsy interviews within the EM method to increase the reliability of reported cause of death. An additional study objective was to determine study design requirements and guide inference decisions for routine implementation of the EM method, with particular attention to the effect of stochastic fluctuation of mortality in small populations and/or over short recall periods.

VALIDATION AND FEASIBILITY STUDY METHODS

Implementation of the EM Method

Between July and October 2008, we evaluated the EM method using a common protocol in four sites worldwide, reflecting different types of human settlement. The sites were (i) District 1 of Kabul city, Afghanistan (population 76,500); (ii) Mae La camp for Karen refugees on the Thai-Burma border (43,800); (iii) rural Chiradzulu District, Malawi (54,400); (iv) Lugufu and Mtabila refugee camps, Tanzania (80,100). Data collection instruments were developed for the study and translated into local languages. In each site we hired and trained local study team members. We used focus group discussions with community members to inform the quantitative data collection, particularly to identify the key community informants on which the EM method relies. With the assistance of the selected community informants, we implemented the EM method to capture deaths over a two month recall period (with the most recent 30 days being the focus of analysis). We needed to estimate total population in two sites. In Chiradzulu district, we also used the most recent standardized World Health Organization verbal autopsy questionnaires.

Validation and Feasibility

We used capture-recapture analysis as the gold standard measurement method to establish the sensitivity of the EM method. Capture-recapture analysis uses several lists of recent deaths from different sources and applies log-linear statistical regression to model the total number of estimated deaths.

A modeling approach was used to compare the observed time and cost inputs for the EM method and a retrospective mortality survey. Ethical implications of the EM method were assessed through data review and discussions with data collectors. The feasibility of using verbal autopsy questionnaires with the EM method was assessed by recording the time required for completing verbal autopsy interviews and analysis.

Validation and Feasibility Results

The EM method showed moderate sensitivity, ranging from 55% to 72.5% over a 30 day period. Sensitivity was consistently lower among children under 5 years:

	Recall Period	District 1, Kabul	Mae La Camp	Chiradzulu District	Tanzania Camps
Total deaths (<5 years) captured by the EM method	60 days	67 (20)	27 (2)	93 (26)	44 (22)
	30 days	11 (1)	16 (0)	37 (12)	21 (8)
Total deaths (<5 years) estimated by gold-standard method	60 days	107 (38)	60 (≥16)	143 (39)	83 (41)
	30 days	20 (≥5)	25 (≥4)	53 (≥15)	31 (17)
Estimated % sensitivity of the EM method among all age groups (95%CI)	60 days	62.6 (39.9-72.8)	45.0 (37.0-48.2)	65.0 (47.9-75.6)	53.0 (36.4-62.9)
	30 days	55.0 (37.9-61.1)	64.0 (50.0-69.6)	72.5 (46.8-82.2)	67.7 (51.2-72.4)
Crude mortality rate * (95%CI) estimated by the EM method	60 days	0.15 (0.12-0.19)	0.10 (0.09-0.11)	0.30 (0.23-0.39)	0.09 (0.09-0.10)
	30 days	0.05 (0.04-0.06)	0.12 (0.11-0.13)	0.26 (0.17-0.38)	0.09 (0.08-0.09)
Crude mortality rate * (95%CI) estimated by the gold-standard method	60 days	0.24 (0.19-0.34)	0.23 (0.20-0.28)	0.51 (0.38-0.67)	0.18 (0.15-0.24)
	30 days	0.09 (0.08-0.12)	0.19 (0.17-0.23)	0.38 (0.25-0.59)	0.13 (0.12-0.15)
* Deaths per 10 000 person-days.					

The EM method required fewer time and monetary inputs than a retrospective survey method:

Inputs	District 1, Kabul		Mae La Camp		Chiradzulu District		Tanzania Camps	
	EM	Survey	EM	Survey	EM	Survey	EM	Survey
Total estimated person-hours	1,482	2,138	168	600	2,295	1,734	444	674
Total estimated costs (USD)	11,933	13,052	1,066	4,390	15,646	15,804	2,610	4,428

The EM method required an average of 163 (90%) fewer respondent-hours than retrospective surveys. In the one site where verbal autopsy questionnaires were administered, this constituted only 3% of total person-time for the method's implementation.

MATHEMATICAL SIMULATION

After verifying the classical Poisson assumption about the probability distribution of mortality on six time series datasets from past surveillance systems, we implemented probabilistic simulations of the EM method assuming two modes of implementation: (i) an exhaustive scenario in which the entire population is investigated, appropriate for small and/or concentrated communities; (ii) a sampling scenario in which the EM method is implemented in a representative sample of population sampling units (PSUs).

We evaluated the performance of the EM method in the above two scenarios, under a range of assumptions about person-time investigated, underlying mortality rate, and threshold of interest, and applying three alternative classification tests: (i) a Poisson test (found to be most conservative), (ii) a sequential test, and (iii) an exact confidence interval (CI) test. Freely available HTML calculators performing the above tests were also developed. We found that the EM method reached acceptable sensitivity and specificity (i.e. correctly classified mortality as either above or below a threshold of interest) when at least 1,000,000 person-days were investigated in the exhaustive scenario, and at least 30 PSUs and 20,000 person-days in the sampling scenario.

DISCUSSION

This study suggests that a new method to estimate mortality in crisis-affected populations based on information provided by community informants, detects a disappointingly low proportion of all deaths in a range of settings. Sensitivity is particularly disappointing among children under 5 years. However, the method's performance is comparable to that of existing surveillance systems. In terms of feasibility, the EM method appears superior to the main alternative, namely retrospective surveys, in terms of time and financial inputs, as well as ethical provisions. Addition of verbal autopsy questionnaires is also feasible and efficient.

We believe the method shows sufficient promise to warrant further development, and that achieving acceptable sensitivity (e.g., >85%) is an achievable aim. A strong existing knowledge of the study community combined with effective implementation of the initial qualitative work is essential if the EM method is to be applied routinely. Further development should thus be focused on improving sensitivity through selection of appropriate key informants, and should be primarily be grounded in social science and qualitative research.

1. Background

Crude and under 5 years mortality rates are key indicators for assessing the health status of a population, and monitoring its evolution.[1] Data on mortality rates and the causes and circumstances of death are crucial to guide the deployment of relief interventions, and to monitor their effectiveness.[2] While mortality data can be used for advocacy and quantification of international humanitarian law violations, most relief agencies on the ground are interested in mortality data to address more operational, immediately relevant questions, namely:

- What is the magnitude of mortality *at the present moment*?
- In which sub-sections or sub-groups of the community is mortality highest?
- What are the main causes and circumstances of death?

The two methods currently available to answer these questions are prospective surveillance and retrospective surveys.

Prospective surveillance (also known as vital events registration) involves exhaustive collection of birth and death events through household visits on a daily or weekly basis. The method furnishes real-time estimates of mortality, which help to guide a quick and appropriate public health response to any observed fluctuations. Data are not subject to sampling errors and can often be broken down by sub-area within a target community. Investigating causes and circumstances of death is theoretically possible within surveillance systems, but requires considerable skill in administration of verbal autopsy questionnaires (see below).

Despite these benefits, prospective surveillance is seldom used in the emergency phase of a crisis, and indeed in most developing country settings, mainly because it requires ongoing human resource inputs and close expert supervision. It is also difficult to set up and monitor in scattered populations.[2] Surveillance systems often decay rapidly, resulting in underreporting of vital events and increasingly unreliable data.

Because of the feasibility constraints associated with the surveillance method, relief agencies often resort carrying out **retrospective mortality surveys**, whereby a representative sample of households is interviewed using a standardized questionnaire about demographic changes (births, deaths, in- and out-migrations) in the household over a specified period in the past (recall period). However, retrospective mortality surveys have serious limitations:

- They are subject to bias (non-sampling error) and imprecision (sampling error) around the point estimate of mortality. Potential forms of bias include household selection, household size reporting, event recall, event reporting, and survival. Imprecision in sample-based surveys can result from inadequate sample sizes, insufficient length of the recall period, an unexpected magnitude of mortality, non self-weighting sampling, and the design effect, a phenomenon inherent in cluster sampling designs. To reduce bias and imprecision, expert input is required in the design, implementation and analysis of these surveys.[3-5]

- They require significant time and resources to carry out because of the large sample sizes required. A typical 30 cluster x 30 households survey covering a small territory may cost about 10-30,000 United States dollars (USD), and require *at least* one month for obtaining authorizations, preparation, data collection, analysis and reporting. These delays further reduce the timeliness and thus the relevance of the findings for public health decision making and programming.

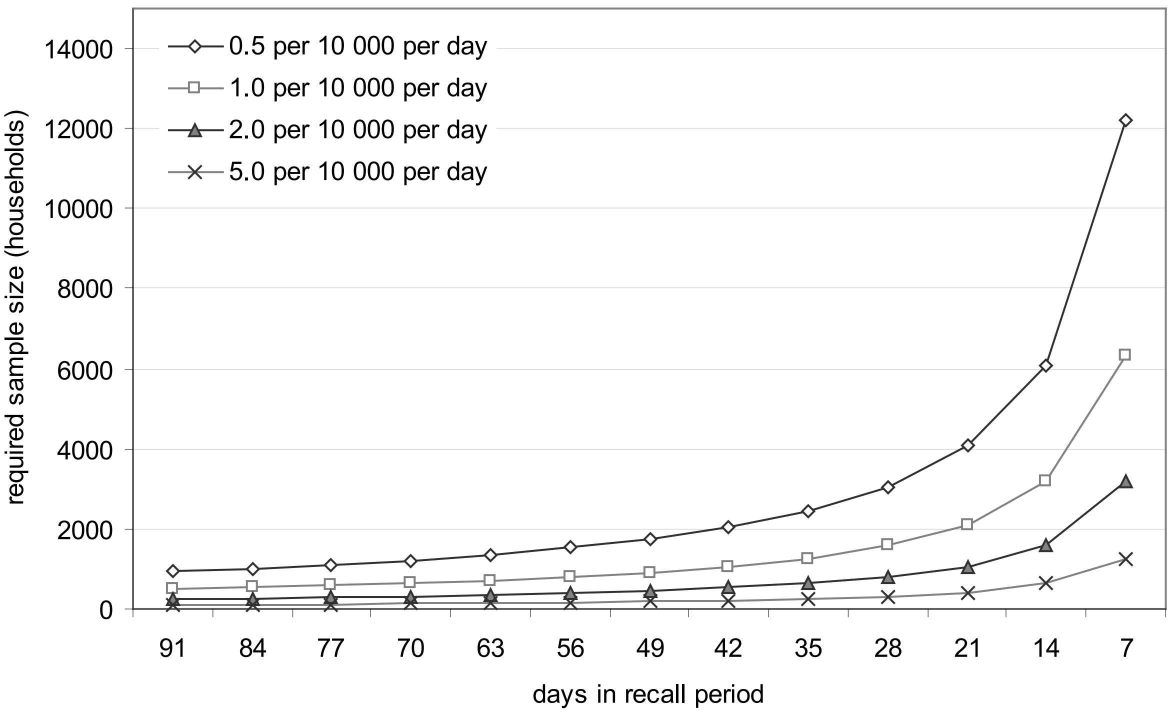
- Critically, they cannot provide sufficiently precise estimates for very recent periods (i.e. data about current mortality), as this would require extremely large sample sizes. Figure 1 demonstrates the sample size (number of households to be interviewed: y axis) required to estimate various magnitudes of mortality rate with precision no worse than ± 30% (assuming a design effect of 2 and mean household size of 5), according to the desired recall period.

- Most data collection time during retrospective surveys is spent collecting data on living people (death is a comparatively rare event), which leaves little time for in-depth investigation of causes and circumstances of death. Most often, next of kin respondents are asked to provide a cause of death, or choose one from among a pre-defined list. Such data may have very limited validity.

- Because of the above, surveys also entail considerable data management requirements (see below).
- Retrospective surveys usually rely on a cluster sampling design, which means the results cannot be broken down by sub-area within the surveyed community, i.e. no estimates for individual clusters can be obtained, unless the sampling design is stratified.
- Cluster sampling in very unstable settings may sometimes result in very high design effects, which results in exceedingly large CIs, hampering interpretation of the estimates; in crises, there is usually no way to accurately predict the observed design effect *a priori*, and adjust sample size accordingly.

In summary, retrospective surveys, while often the only currently available method to measure mortality in crises, do not provide very satisfying answers to the key operational questions faced by agencies wishing to assess a population's health status, and implement and monitor appropriate public health interventions accordingly.

Figure 1. Sample Size Requirements for a Retrospective Mortality Survey Targeting Different Recall Periods



2. Rationale and Objectives

2.1. OVERVIEW OF THE PROPOSED NEW METHOD

In response to the limitations of prospective surveillance and retrospective surveys for estimating mortality, we evaluated an alternative method, mainly reliant on the knowledge of key community informants, for rapidly quantifying mortality over a very recent period through the **EM method**.

Estimation of a mortality rate whether by prospective surveillance, retrospective survey, or the EM method, requires a numerator (deaths during a given period) and a denominator (person-time at risk during the period). In carrying out the EM method, deaths are captured through an exhaustive search for *all* deaths occurring in the community over a defined recall period. The search process is mainly dependent on key community informants, selected after rapid qualitative work, who lead data collectors to households that they recalled as having experienced a recent death; next-of-kin of decedents can also acted as informants leading the data collectors to other recently bereaved households. The process continues until informants can no longer identify households with recent deaths.

If the period is short (i.e. weeks or months, rather than years) and there has been no major population in- and out-migration, one can safely assume that (i) each individual in the population contributes an equal amount of person-time, and (ii) the population at the end of the period is roughly equal to that at the beginning, i.e. the denominator can be more simply expressed as population at the end of the period x duration of the period. Population figures for calculation of person-time at risk can be obtained either from available registration systems or by rapid estimation (further information on the population estimation methods used in the study are described in **Sections 3.3.6** and **Section 3.3.7**).

2.2. POTENTIAL ADVANTAGES

We considered that the EM method would potentially feature numerous benefits compared to current methods for estimating mortality. The following potential advantages over prospective surveillance were initially identified:

- The EM method would be a one-off effort (which can be repeated regularly) rather than an ongoing activity as is required for surveillance. As a result, financial, human resource and material inputs would be much lower.
- It would entail lower requirements for supervision as it would only require a small team of data collectors, and supervisors could potentially be present in most households from which data are collected.
- It would potentially be more efficient for scattered populations and large geographic areas than prospective surveillance, since data collectors would only visit households with recent deaths, minimizing travel and transport requirements.

Similarly, the following potential advantages over retrospective surveys appeared evident:

- The EM method would provide real-time mortality estimates, since it would be able to investigate considerably more recent person-time than a standard survey.
- Only data on events (deaths) would be required, reducing data collection and entry time and requirements. For example, in a community of 100,000 people with a CMR of 2 deaths per 10,000 person-days over a 14 day recall period, the EM method would yield a database of 280 records, compared to 4,000-6,000 for a standard 30x30 cluster survey.
- Data analysis would be simplified in most scenarios by removing the need for weighting and design effect adjustment inherent in sample surveys, or individual person-time calculation when the recall period is long and the cohort very dynamic. The method could therefore be used by program staff with limited research skills.

- The reduced data collection time for the EM method would allow more time to collect detailed data on individual deaths. This could include using verbal autopsy questionnaires to more reliably classify causes of death.
- In contrast to a retrospective mortality survey, findings from the EM method could be broken down by sub-area, and deaths could be mapped using global positioning system (GPS) software to identify areas with high need mortality or assist outbreak investigations.

2.3. STUDY JUSTIFICATION AND OBJECTIVES

2.3.1. Need for Field Validation and Feasibility Study

The above advantages would only be realized if the method were valid and feasible. In terms of validity, the main unknown was whether key community informants selected through rapid qualitative research would capture recent deaths in their communities with sufficient sensitivity. Feasibility questions, related to the EM method, include the time and cost required for data collection and analysis, ease of use, the possibility of including verbal autopsy data collection within the EM method, and ethical implications.

2.3.2. Need for Mathematical Simulation to Guide Sampling Requirements for EM Method Implementation

As there is as yet no documented field experience with the proposed EM method, its statistical properties and robustness, including sample size and inference issues, need to be explored in addition to its validity and feasibility.

While an approach, such as the EM method, which directly targets households with recent deaths, might lead to more precise estimates of mortality than a standard retrospective survey, inferences based on the method's findings would nonetheless have to take into account random effects due to the stochastic fluctuation of mortality over time. In practice, observed mortality is the reflection both of secular trends due to bona fide changes in health determinants (in engineering terms this could be termed the *signal*) and of one of the many possible chance realizations of a distribution of possible mortality levels (equivalent to *random noise*). Instinctively, it is obvious that random noise becomes more influential as the amount of person-time investigated decreases, warranting greater and greater inferential caution. Guidance for potential users of the EM method should thus include recommendations for investigating an amount of person-time (population x time period) sufficient to reduce the effect of random noise to an acceptable level, and for interpreting results while taking this issue into account. In addition, the EM method is subject to potentially considerable imprecision in the denominator, especially if the population size is unknown and a rapid estimation method is used to measure it. Clearly, this uncertainty needs to be explicitly accounted for when analyzing data. Finally, in a community with very large population and/or very scattered settlements (e.g., a rural district comprised of hundreds of villages), users of the EM method might consider it unfeasible to survey the entire population (long transport times would probably be the main limiting factor), and instead wish to sample a representative fraction of it. Clearly, such sampling of communities would entail a different set of recommendations. These operational-related issues are amenable to mathematical simulation, which can efficiently consider a large range of potential scenarios for the EM method's implementation.

In light of the above validity, feasibility and operational questions, we considered that the EM method was deserving of a rigorous validation and feasibility study, coupled with desk-based mathematical simulation to explore minimum person-time requirements and guide inference based on the method's findings. Recognizing that the validity and feasibility of a data collection method can vary considerably according to the local culture and type of human settlement being investigated (e.g., a camp, an urban slum, a rural district), the evaluation of the EM method was designed to be multi-centric.

2.3.3. Specific Objectives

The study had the following objectives:

1. Establish the validity of the EM method for identifying deaths and causes of death in the community, by comparing it against a gold standard measure (capture-recapture analysis of multiple lists) in up to four field sites:

    1.1. Gain a qualitative understanding of (i) which key community informants may be most effective in sharing and collecting information about recent deaths, (ii) patterns whereby information on recent deaths is shared among community members, (iii) potential bias of collecting information on recent deaths through the EM method or gold standard method, and (iv) local concepts of causes of illness and death.

    1.2. Implement the EM method to measure crude mortality rates (CMR) and under 5 mortality rates (U5MR) during a one month recall period in up to four different populations ranging approximately from 50,000 to 100,000, and encompassing a variety of settings, including at least one camp, one urban neighborhood and one rural district.

    1.3. Compare the above results to the CMR and U5MR in the same population and over the same period as estimated by capture-recapture, to estimate the method's sensitivity.

    1.4. Explore any observed under- or over-reporting of mortality through the EM method by comparing the profile of decedents captured by the EM and gold standard methods, and corollary qualitative information.

2. Document the field feasibility of the EM method:

    2.1. Document the financial costs of the EM method in terms of human resources, material and time inputs.

    2.2. Compare the above costs to typical costs incurred by a standard retrospective survey in a similar setting.

    2.3. Predict the feasibility of the EM method in communities of large population size and/or low population density (i.e. scattered in a wide geographical area).

    2.4. Compare the ethical implications, in terms of risks and burden to the community and individual respondents, of the EM method, standard retrospective surveys and the gold standard death registry method.

    2.5. Explore the feasibility of adding World Health Organization (WHO) standard verbal autopsy questionnaires to the EM method to quantify the contribution of different causes of death to overall mortality.

3. Predict statistical requirements, in terms of minimum person-time to be investigated, for routine implementation of the EM method, subject to the indicator of interest (CMR or U5MR), size of the community targeted, and the underlying mortality rate, and quantify resulting uncertainty to guide inference:

    3.1. Parameterize the most appropriate statistical distribution describing the random fluctuation of mortality over time, at least in resource-constrained, developing country settings;

    3.2. Adopting the above distribution, predict mathematically the degree of uncertainty (as a CI or p-value) to be expected if the EM method is applied in different population sizes, time periods and under varying death rates;

    3.3. Based on the above predictions, determine minimum requirements for population size and period (i.e. person-time) to be investigated via the EM method;

3.4. Incorporate imprecision in population estimates into the mortality rate estimates obtained through the EM method.

For clarity's sake, the field validation and feasibility study is presented first, and the methods and results section of the mathematical simulation work are presented jointly thereafter. We conclude with an overall discussion of the method's applicability and recommendations for further development.

3. Validation and Feasibility Study Methods

3.1. STUDY SITES

The EM method was evaluated using a common protocol in four sites worldwide, including (i) District 1 of Kabul, Afghanistan, a chaotic urban community within a fragile state; (ii) Mae La camp for Karen refugees, on the Thai-Burma border; (iii) Chiradzulu District, Malawi, a rural, remote community with poor health status and a typical pre-transition epidemiological profile heavily impacted by the HIV epidemic; (iv) Lugufu and Mtabila refugee camps, Tanzania, housing persons forcibly displaced by armed conflict in the Great Lakes region. A description of each site follows.

3.1.1. District 1, Kabul, Afghanistan

Work in Kabul was led in July 2008 by investigators from the London School of Hygiene and Tropical Medicine (LSHTM); the collaborating partner was the Afghanistan Ministry of Public Health. District (or *nahia*) 1 lies close to the centre of Kabul and is one of the poorest of the 18 districts of Kabul. Much of the District lies on one of the hills of Kabul so transport, water and electricity are more limited than elsewhere in Kabul. The population are ethnically diverse and there are reportedly quite high rates of migration in and out of the district. The population of District 1 was estimated by this study to be 76,476 in July 2008 (see **Section 3.3.6** for population estimation method). There are a small number of public and private health centers in District 1. It is also served by a number of hospitals, the closest of which is Maiwand hospital.

3.1.2. Mae La Refugee Camp, Thailand

Work in Mae La camp was led in July 2008 by the principal investigator from the United States Centers for Disease Control and Prevention (CDC) in collaboration with the non-governmental organization (NGO) Aide Médicale Internationale (AMI). Mae La camp is in Tak Province, northern Thailand and was established in 1984. The camp residents are persons displaced from Myanmar, and almost all are ethnic Karen. In June 2008, the population of Mae La camp was approximately 44,000. AMI coordinate medical services in the camp, and AMI provides inpatient and outpatient clinical services. Public health and community activities are conducted by AMI's Home Visitors. Maternity care is provided by the Shoklo Medical Research Institute (SMRI) and tuberculosis treatment is provided by Médicines San Frontières (MSF).

3.1.3. Chiradzulu District, Malawi

Work in Chiradzulu was led in August-September 2008 by the principal investigators from LSHTM in collaboration with MSF – France (MSF-France). The District Health Office (DHO) provided written approval and support for the EM validation study. Chiradzulu district is located in the Southern Region of Malawi and was selected because it provided a resource-poor rural setting in which to validate the EM method. The estimated population of Chiradzulu district in mid 2008 was 314,200. MSF-F have been providing health services in Chiradzulu district since 1997, including a very large HIV antiretroviral treatment program now serving more than five thousand patients. The district contains 10 government health centers, a district hospital, and a privately funded hospital.

3.1.4. Lugufu and Mtabila Refugee Camps, Western Tanzania

Work in Lugufu and Mtabila camps in September-October 2008 was led by one of the principal investigators from LSHTM in collaboration with the United Nations High Commissioner for Refugees (UNHCR), with the support of the Tanzanian Red Cross Society (TRCS). Lugufu refugee camp and Mtabila refugee camps in Tanzania were selected as sites to test the EM method in situations of forced displacement. Lugufu and Mtabila camps are located in Kigoma district in the north west of Tanzania. Lugufu camp was established in 1997 and almost all of the camp residents are refugees from the Democratic Republic of Congo. Mtabila camp was established in 1994 and almost all of the camp residents are refugees from Burundi. The populations of Lugufu and Mtabila camp were estimated at 38,363 and 41,773 respectively at the time of the study. There is currently a repatriation process for returning camp residents to their counties of origin and so the camp populations are reducing in size. UNHCR have a long history of coordinating protection and support services in

Lugufu and Kigoma camps. TRCS has been responsible for all medical and public health activities in the camps since the camps were established.

3.2. SAMPLING DESIGN

3.2.1. Recall Period

In each site, **the main recall period of interest was set at one month (30 days)**. This was selected as it would typically be of interest to agencies potentially using the EM method in humanitarian crisis situations. However, in order to minimize underreporting due to recall bias, search criteria were expanded to **all deaths occurring over the previous two months (60 days)** within the sampled communities. Results will be shown for both 30 day and 60 day recall periods, as the latter time points also provides interesting information about the method's performance and about mortality in the populations investigated (see **Discussion**).

3.2.2. Population Investigated

We surveyed the populations of District 1, Kabul, Mae La camp on the Thai-Burma border, and Lugufu and Mtabila camps in Tanzania exhaustively (Lugufu and Mtabila are considered as one site for the purposes of this study). Assuming a minimum CMR of 0.2 deaths per 10,000 person-days within each of the above sites, we calculated that the method's **sensitivity** (i.e. proportion of all deaths that are detected by the method, namely our main outcome of interest for validation purposes) would be estimated among ≥26 deaths in each site. Assuming that sensitivity would have been >80%, this number of deaths would have provided a precision within ±20%.

It was considered operationally unrealistic and statistically inefficient to visit all villages comprising Chiradzulu district (approximately 700). Therefore, in this rural setting we adopted a spatial sampling approach, consisting of a modified Centric Systematic Area Sample (CSAS). We overlaid a 5 Km x 5 Km grid on a map of the district: quadrants falling mostly within the district were retained, whilst those falling mostly outside were excluded, leaving 32 quadrants for spatial sampling. The three villages closest to the geographic centre of each quadrant were then selected with the aid of high-resolution maps provided by the Malawi National Statistical Office and used for a recent (June 2008) census exercise, and an updated administrative list of villages provided by the District Commissioner's office. Thus, 96 villages (32 quadrants x 3 villages) were sampled throughout the district. Within each sampled village the search for recent deaths was exhaustive. Assuming an average population per village of 500 (350,000 people / 700 villages), nearly 50,000 people would have been sampled, yielding a minimum precision comparable to that of the other sites.

3.3. IMPLEMENTATION OF THE EM METHOD

3.3.1. Preparatory Activities

Ahead of data collection, we visited with the administrative authorities in each site and sought their approval for the study.

Within each site, the community was divided into "sectors", corresponding to pre-existing administrative areas: *guzar* in Kabul (n=24), each presided over by and known by the name of a male *wakil*, or community leader); sections in Mae La camp (n=22); villages in Chiradzulu District (n=96); and zones in Lugufu and Mtabila camp in Tanzania (n=?).

All data collection instruments were translated into local languages (Dari in Kabul, Karen in Mae La, Chichewa in Chiradzulu, and Ki-Swahili in Tanzania). The translation process included independent back-translation and extensive group review by the study team to address any discrepancies. English versions used in Chiradzulu are provided in **Section 8**, the Report Annexes; elsewhere, minor adaptations were made to questions not pertaining to the fundamentals of the method (questionnaires for each site are available from the investigators upon request).

3.3.2. Composition and Training of Study Team

In each site we hired and trained local study team members, all highly literate and fluent in English and the local languages. In Kabul six data collectors were recruited (three women and three men, due to cultural requirements of potentially requiring the data collectors to be the same sex as the survey respondents) to form three interview teams. In Mae La only one data collector was employed. In Malawi four data collectors (two women and two men) were employed, as well as one clinical officer who both supervised data collection and conducted verbal autopsy interviews (see **Section 3.3.5**). In Lugufu camp, three data collectors (two men and one woman) were employed. In Mtabila camp, another three data collectors (two men and one woman) were employed. All the data collectors worked with TRCS and were experienced in providing health care in the camps.

Data collectors were trained on: (i) background and justification for the study; (ii) the mortality questionnaire; (iii) the exhaustive search process, including use of primary and secondary community informants; (iv) recording information on the household register; (v) standard household visit procedures; (vi) use of the visual calendar aid; (vi) systematic probing; (vii) ethical issues and consent administration; and (viii) identification and removal of multiple records. The training involved informal presentations/discussions by investigators and role-plays. Details on the length of training are provided in **Section 4.5.1**.

In Mae La, all interviews were overseen by a study investigator. In the other study sites, investigators supervised all interviews during the first two days of data collection, after which supervision was relaxed, though investigators accompanied the team on all sector visits and continued to supervise a portion of the interviews, and generally coordinated data collection.

3.3.3. Focus Group Discussions (FGDs)

One FGD was conducted in each study site before data collection. The objectives of the FGD were to: (i) identify a set of primary and a set of secondary key informants on which the exhaustive search for recent deaths would mainly rely; (ii) explore the sharing of information about recent deaths in the community; (iii) explore the degree to which different kinds of deaths might be reported or hidden; (iv) explore local concepts of the main causes of mortality; (v) identify potential alternative sources of mortality information that would provide additional lists for capture-recapture analysis; and (vi) identify locally salient events to help develop a calendar to aid the exhaustive search.

FGD participants were selected based upon their strong knowledge of the community in which the study took place. Local study team members, collaborators and administrative authorities were asked to help identify participants (**Table 1**). In Kabul, for cultural reasons separate FGDs were held simultaneously for men and women. In Mae La camp, more participants attended the FGD than were invited. In Chiradzulu, the FGD was held in a village, to avoid costs and time lost for transport of participants: although persons from two neighboring villages also attended, it was considered unfeasible to seek representation from the entire District, and the village was instead chosen on the recommendation of data collectors because it had proved cooperative with past surveys. In Tanzania, separate FGDs were held in Lugufu and Mtabila camps. Both FGDs were held in meeting halls in the main health centers of the two camps.

The FGDs were entirely held in the local languages, and moderated by local study team members who received one to two days of training on (i) the background and aims of the overall study; (ii) the key aims and questions of the FGD; (iii) techniques for facilitating effective FGDs; and (iv) consent procedures. In Mae La, due to the absence of the appointed moderator, the investigator led the discussion through a translator. A topic guide (**Annex 8.1**), including possible prompts, was used to guide the FGD.

Table 1. Details of FGD Participants, by Study Site

District 1, Kabul	Mae La Camp	Chiradzulu District	Lugufu and Mtabila Camps
Men's FGD (eight participants): five wakil-e-guzar (sector leaders), one mullah, two education officers Women's FGD (four participants): four school teachers	33 participants: Vice Chairman of the Camp Committee, the Camp Committee Health Leader, Mae La Hospital Director, Section Leaders, Religious Leaders, Section Health Workers, AMI Home Visitors, SMRU Home Visitors	Nine participants: village headmen (two males), headman assistant (female), graveyard chairman (male), church elder (one male, one female), Muslim representative (male), teacher (male), member of village health committee (female)	Lugufu camp: 14 participants: Camp chairman (male), seven zone leaders (six male, one female), five village leaders (four male, one female), one radio reporter. Mtabila camp: 22 participants: Camp chairman (male), eight zone leaders (six male, two female), 10 street leaders (six male, four female), two pastors (male), one community security officer (male), one health information team member (male, also one of above pastors).

During the FGD, data collectors took notes on a structured document (available from the investigators upon request). These notes were consolidated, translated and analyzed by the entire study team during a group debriefing session held immediately after the FGD, to inform the quantitative data collection, particularly the selection of key informants. The FGDs were also recorded using digital recorders, transcribed verbatim and translated (in Mae La, staff took notes on the discussion that were then translated into English by an independent translator). The translators were briefed on the importance of accuracy in the translation and transcription. The English versions were reviewed upon completion by the study authors and clarifications sought for any unclear passages.

After data collection, more in-depth analysis of the FGDs was completed using NVivo (version 7) software. An inductive approach was applied which suited the exploratory nature of the FGDs [6]. The transcripts were reviewed and themes and sub-themes were developed to give coherent categories for the data, based upon the aims of the FGDs [7, 8]. The data were then coded based upon the themes and sub-themes. An iterative process was applied, with the themes and sub-themes revisited, altered and additional themes/sub-themes added during the coding process. The coding was then reviewed and adjusted accordingly. This allowed complete familiarity with the data in its entirety and with individual interviews and helped to ensure a reliable, systematic and representative analysis of the data [8-12]. Observations and interpretations were drawn from a selection of observed themes, including similarities and differences between the study sites. Only abridged findings of the FGDs are presented in this report. A more detailed analysis will be presented in a separate paper.

3.3.4. Exhaustive Search for Recent Deaths

In each study site, data collection proceeded sector by sector. Sector leaders were informed about the study team's upcoming visit by telephone in Kabul, letter in Chiradzulu, and in person in Thailand and Tanzania. Primary informants and secondary informants were identified (**Table 8**; see Results for Details and Justification of the Choice of Informants), and efforts were made to revisit the sector as often as necessary to contact any informants found absent during the first visit. After providing a brief introduction to the study and answering any questions, both sets of informants were asked to recall all deaths occurring within the sector during the previous two months, and refer the study team to the households in which those deaths had occurred. We did not actively seek information from any other respondents; however, key informants' recall process often involved consulting family members or assistants: any referral from these was also accepted, as was casual information provided by eavesdroppers or passers-by.

Any reported death was listed on a register (**Annex 8.2**). Each new reported death was registered, even if already referred by another informant. Households on the register were then approached according to a template of household visit procedures (**Annex 8.3**). After confirming that a death had indeed taken place and identifying one or more close next of kin of the decedent aged 18 years or older, data collectors provided information about the study, answered questions, and asked for verbal consent (**Annex 8.4**), leaving a participant information sheet in the local language (**Annex 8.4**) with the respondents. In Mae La, written consent was sought. Differences in consent procedures reflected local advice and literacy levels.

Consenting respondents were then interviewed according to a short structured questionnaire (**Annex 8.5**), designed mainly to establish the basic facts about the decedent: age (under 5 years, older), sex, date and place of death, name and name of father (or of the household in Chiradzulu and camps in Tanzania), the latter variables were included to provide more unique identifiers for the gold standard capture-recapture analysis (**Section 3.4.2**). Several apparently redundant questions were used to establish whether the person was breathing at birth, in case of death of a neonate (stillbirths are not counted as deaths in standard mortality studies); whether the person was indeed a member of the household (defined as people sleeping and/or eating together) or of a household within the surveyed population; when the death occurred, for which a visual calendar (**Annex 8.6**) was used: the calendar was meant to help both interviewers and respondents place deaths unequivocally within or outside the recall period of interest, and within approximately one week of the true date of death; and to estimate weather the person was aged under or over 5 years to assist with classification for the age-specific mortality (under 5 years, older). At the end of the interview, respondents were asked to recall other deaths in their household or in their community within a two month period: any such referrals were entered onto the register. The community informant or a local guide then took the interview team to the household and interview was conducted as above. If there was no referral from the household respondent, the interview team went to the next household on the register list from the primary and secondary informants. Once no more deaths could be identified, we considered the sector as exhausted and moved on to the next sector. In cases where key informants referred us to households from other sectors, these would be visited when the study team canvassed that sector. Households were not visited if they were outside of the population of interest, for example a household outside of District 1 in Kabul or a village not sampled in Chiradzulu district.

3.3.5. Verbal Autopsy Questionnaires

The verbal autopsy component of the study was only performed in Chiradzulu. The most recent, standardized WHO verbal autopsy questionnaires were adopted [13, 14] (http://www.who.int/whosis/mort/verbalautopsystandards/en/index.html): these consist of three different questionnaires according to the age categories of the decedent (less than four weeks; four weeks to 14 years; older than 14 years) which are required for the cause of death data (note that these categories are separate to the age classification for mortality estimation of under 5 years, older); and contain several sub-modules (e.g., on neonatal conditions, injuries, and maternal mortality) depending on the signs and symptoms reported by the respondent. The questionnaires were translated into Chichewa by a clinical officer, and translations were reviewed by another clinician as well as other members of the study team. During the exhaustive search process, data collectors were instructed to invite the next-of-kin of any decedent who had passed away **in the last month** (based on the EM questionnaire responses) to undertake a verbal autopsy questionnaire.[1] All verbal autopsies were conducted by a clinical officer who systematically administered the list of questions, with conditional skips, in the order specified on the questionnaire. Occasionally, a different respondent was chosen if it became clear that (s)he could provide more reliable information on the signs, symptoms and medical history of the deceased (e.g., an effort was made to systematically interview the mother of a dead child, even if for the mortality questionnaire a different member of the household had been interviewed). A study investigator supervised the first few verbal autopsy questionnaires.

Analysis followed WHO guidelines. The clinical officer and a medical doctor working for MSF-France independently reviewed each completed questionnaire and, where available, additional medical records for the patients, and filled out a WHO standard ‘International form of medical certificate of death’ for each decedent. Disagreements on the causes of death were resolved by a third, medically qualified independent reviewer. Investigators then coded the cause of each death based on the final certificate, using the International Classification of Diseases, version 10 (ICD-10), and based on WHO guidelines.

For brevity’s sake, only findings on the feasibility of verbal autopsies will be presented in this report.

[1] Verbal autopsy data was not collected for a 2 month recall period since this component of the study is to assess feasibility not validity, and 1 month was the defined recall period of interest for all measures.

3.3.6. Data Collection for Population Estimation

In **Mae La and the Tanzanian refugee camps**, we did not perform any population estimation, but rather used existing data, including the number of children under 5 years, from well-established prospective demographic surveillance systems managed by AMI and the UNHCR, respectively. As data were updated on a weekly basis, we used the average population over the weeks covered by the study as a best estimate of the mid-period population.

Elsewhere, no recent, reliable population figures were available (a pre-census exercise was carried out in Afghanistan in 2005, but data by *nahia* or city district were not considered reliable; a national census was performed in Malawi in June 2008 but data will only be released in 2009). Thus, we performed our own population estimation, as follows.

District 1, Kabul

The district was composed of (i) a dense, low-lying area mainly featuring narrow streets and multi-storey buildings each with one main door facing the street, and each occupied by several households; and (ii) a more sparsely populated hillside area featuring smaller, well demarcated structures (practically all residential) built on a steep incline. To reduce statistical heterogeneity and thereby improve precision, we divided the District into a low-lying and a hill stratum, within which we estimated population using different methods:

- In the low-lying stratum, we collected population data in a selection of quadrants and then multiplied the average population density by the total number of quadrants in the area. We created a spatial sampling frame by overlaying a grid of approximately 35 m x 35 m resolution onto a recent map produced by the Aga Khan Cultural Foundation and based on satellite imagery. The map indicated areas containing only non-residential structures (this information appeared very accurate after repeat site visits): we excluded from the sampling frame all quadrants falling within these areas, within streets and/or mostly outside of the district boundaries. This left 894 quadrants, from which 30 were selected using simple random sampling (without replacement). After locating the four corners of each sampled quadrant by using zoomed-in maps, data collectors visited each residential building door falling within the quadrant, and, following a short oral consent procedure, asked a consenting respondent aged 18 years or above about the number of households residing within, and the number of people of all ages and children under 5 years who had slept within each household on the previous night.

- In the hill stratum, we counted structures and estimated the number of people per structure. After dividing the stratum into approximately equal sized and geographically distinct sub-areas, two or three independent tallies of all residential structures within each sub-area were performed from vantage viewpoints, and averaged. A grid was then overlaid on the stratum as above, forming 467 quadrants, out of which 20 were selected randomly for population data collection. The residential structure approximately closest the centre of each sampled quadrant was visited, and the number of people of all ages and under 5 years who had spent the previous night within it were counted as above, yielding an average number of people per residential structure.

Finally, data from both strata were analyzed through bootstrapping methods (**Section 3.3.7**) to give a total estimate for District 1.

Chiradzulu District

We estimated the population of the 96 sampled villages to provide the denominator for the mortality rate estimation (estimating the population of all villages in the District was not necessary as the sample was assumed to be representative, allowing for District-wide projections) as follows:

- Within each village, we asked the village headman to identify two counters (one female, one male); we provided them with a tally sheet (**Annex 8.8**), and instructed them to walk through the entire village, counting all residential structures (roofs under which people spend the night). We then averaged the two counts. Upon presenting their completed forms, the counters were paid a nominal honorarium proportional to the size of the village. In four villages that were geographically close to one another, data collectors were accused by some community members of being bloodsuckers, and structure counts, though complete, were not handed over. In these villages, we imputed the number of structures through linear prediction based on an ordinary least squares (OLS) model applied to the data collected in the other 92 villages, in which structure count was treated as a function of number of households (see **Section 3.3.7**).

- Within one of the three villages in each quadrant, selected at random, we used standard Expanded Program of Immunization (EPI) sampling [15] to select two structures, the first being one of the structures along an imaginary line drawn from the centre to the edge of the village in a direction determined by spinning a pen, and the second being two structures removed from the first based on a rule of proximity: this yielded a spatial cluster sample of 64 structures (32 clusters x 2 structures per cluster). Within each structure, we inquired about occupancy during the previous night, as above. The estimated mean number of people and children under 5 years per structure resulting from this sample was weighted for unequal sampling probabilities within each quadrant (see **Section 3.3.7**).

Finally, we multiplied the mean number of people (or children under 5 years) per structure by the total number of structures in the 96 villages to obtain the estimated population of the 96 villages, with 95%CIs accounting for variance in the number of people per structure provided through bootstrapping (see **Section 3.3.7**). Population estimation was done at the same time as the mortality data collection for efficiency reasons (it would have been impractical to revisit villages).

3.3.7. Estimation of Population Size and Mortality Rates

We excluded from the analysis any deaths that met one or more of the following criteria: (i) stillbirth (the baby died on the same day of birth and was not breathing at birth); (ii) reported date of death more than 60 days before the EM survey date; (iii) resident outside of the community (or sampled villages in Chiradzulu) during most or all of the month before death, as determined through answers to the mortality questionnaire and notes taken by data collectors.

We expressed CMR (and U5MR) as deaths (among children under 5 years) per 10,000 person-days (under 5 years). We estimated mortality rates as ascertained by the EM method alone, by all sources combined, and by all source plus estimates of deaths not captured by any source obtained through capture-recapture analysis (see **Section 3.4.3**). We computed point estimates and 95%CIs through bootstrapping programs written in R language (**Annex 8.10**) and specific to each field site. Bootstrapping is an approach that computes best estimates and CIs for a measure of interest by drawing a large number of samples (with replacement) from distributions of the parameters that make up that measure. In our case, bootstrapping programs were designed to simultaneously account for uncertainty in the numerator (number of ascertained deaths plus estimated number of deaths not captured: see **Section 3.4**) and denominator (population estimate). Because samples were not self-weighting in both Kabul (population estimate) and Chiradzulu (EM survey and population estimate), bootstrapping was used to calculate the point estimate as well as the CIs in these two sites. Overall for each site, 1,000 iterations of the survey were simulated, and the median, 2.5^{th} and 97.5^{th} percentiles of the iteration distribution were then adopted as point estimate and 95%CI respectively.

Site-specific details on the remainder of the estimation process are provided below. We note here that 95%CIs do not reflect random variation in the numerator (deaths) due to stochastic effects occurring in the time dimension (**Section 2.3.2**). A separate calculator for estimating mortality rates based on the EM method was also developed for more routine application (see **Section 5.3**).

District 1, Kabul

As previously stated (**Section 3.3.6**), different sampling designs were used in the two strata of District 1 (low-lying and hill) for population estimation. Due to the low number of quadrants and structures sampled, we considered it imprudent to impose parametric assumptions on the population estimation. Instead, we used the empirical (i.e. observed) distributions of (i) number of people (children under 5) per quadrant (low-lying stratum) and (ii) number of people (children under 5) per sampled structure (hill stratum).

During each iteration, we simulated the population estimation process in the low-lying stratum by drawing 30 random values from distribution (i) above, taking the median and multiplying it by the total number of quadrants in the stratum (894). In the hill stratum, we drew 20 random values of people per structure (distribution (ii)) and multiplied the median by the total number of structures, generated randomly as the average structure count ± a random error factor, normally distributed with SD 3% (SD was approximated based on the duplicate or triplicate counts performed for each sub-area: see **Section 3.3.6**). Mortality rates were then computed by dividing the total number of deaths, as ascertained through the EM method or all sources combined, by the sum of the population estimates for the two strata. When computing the true estimated mortality rate in the District, the number of deaths was increased to account for the estimated uncaptured deaths, as described in Section **3.4.3**.

Mae La Camp

Whilst the demographic surveillance system operated by AMI was well established, we considered it prudent to assume a small error factor, normally distributed around the estimated population with a SD of 5%. During each iteration, a random population size was generated from this normal distribution. Mortality rates were then computed based on this population estimate, as above.

Chiradzulu District

During each iteration, a random mortality rate was generated as follows:

$$\text{Rate} = \frac{(\text{n deaths per residential structure x n of structures) x 10 000}}{(\text{n people per structure x n of structures x recall period})}$$

Random values of the parameters in the above equations were generated for each iteration by sampling from their respective distributions: (i) the number of deaths (or deaths of children under 5 years) per residential structure was modeled as a Poisson distribution, based on data on the number of deaths per quadrant, with standard errors adjusted for design effect (Deff) due to clustering and offset for each quadrant provided by the natural log of the structure count in the quadrant; (ii) the total number of structures was taken as the (average) total of the observed structure counts ± a random error factor, normally distributed with SD 3% (SD was approximated based on the duplicate counts performed in each village); and (iii) the number of people (or children under 5 years) per structure was modeled as a Poisson distribution, based on data from the nested cluster survey of residential structures, with standard errors adjusted for Deff. The same routine was also used to estimate the total population sampled.

The sample of 96 villages was selected to be representative of the entire District, but was not necessarily self-weighting, since (i) different quadrants had varying population density and (ii) the three villages selected within each quadrant had varying population size. We thus applied weights to the above Poisson models, as follows. We obtained a dataset of number of households per village maintained by the District Environmental Health Office, last updated in 2008, and collected by a network Health Surveillance Assistants responsible for various health promotion and surveillance activities within each village. We considered that the number of households would be less subject to reporting bias than the total population per village, and thus that it would be an appropriate weighting variable, assuming that the number of people per household (and any error in household numbers) was comparable across the quadrant. Household data were missing for 5/96 (5.2%) of the sampled villages: in these villages, we used the number of structures (obtained through our own count), excluding the 4 villages for which number of structures was obtained through modeling) to impute the number of households, based on an OLS model predicting household count as a function of structure count in the 87 villages with both household and structure count data available (P-value for goodness of fit <0.0001, R-squared 39.6%). Household data were also missing for 39/661 (5.9%) of the villages not sampled but located within the quadrants: we imputed the number of households in these villages as the median of the household counts in the other villages within the same quadrant.

Based on this dataset and available maps, we computed the total number of households per quadrant i (N_i) and the number of households in the three villages (1, 2 and 3) sampled within each quadrant i, taken together ($x_{1i}+ x_{2i}+ x_{3i}$); we then applied a weight to mortality observations from quadrant i equal to $N_i/(x_{1i}+ x_{2i}+ x_{3i})$, namely the amount of quadrant person-time that each cluster of villages was representative of: this attributed a greater weight to populous quadrants, and vice versa.

When analyzing the 32 cluster x 2 structures nested survey to estimate structure size, we applied a weight inversely proportional to each structure's probability of selection, namely $V_i(s_{ji}/2)$, where V is the total number of villages in quadrant i and s is the number of structures in village j sampled within quadrant i.

Tanzania

During each iteration, we generated a random population size for both Lugufu and Mtabila camps, as done for Mae La (see above), and divided the total number of deaths by the sum of the two random values to compute mortality rates.

3.4. VALIDATION

3.4.1. Choice of Gold Standard Method

We used **capture-recapture analysis** as the gold standard measurement method to establish the sensitivity of the EM method, i.e. the proportion of the total number of deaths that are detected by the new method.

This technique, also known as multiple lists analysis or multiple system estimation, was developed by ecologists originally, but since the 1970s has been applied extensively to various epidemiological problems, as the choice method to estimate the completeness of reporting for a given disease or health event, and thereby derive the true number of disease cases or events [16, 17]. Examples of capture-recapture use in epidemiology include research on birth defects, cancer, drug use, infectious disease, injuries, the size of homeless populations, validation of census data, and mortality (including rigorous studies recently commissioned by international bodies in Guatemala, Kosovo and Timor Leste [18-20]). Epidemiological applications of capture-recapture methods rely on establishing several lists of events being studied (in our case, recent deaths) from different sources. While it is recognized that each list is likely to be incomplete, the method exploits information about the overlap (or lack thereof) of different lists, and, through this, estimates the proportion and absolute number of deaths that do not appear on any lists, which is then added to the "known" or ascertained deaths (i.e. those that are found on one or more lists) to provide the gold standard total. When using capture-recapture analysis to evaluate the sensitivity of a data source, it is common to include the data source as one of the lists analyzed [16, 17].

Capture-recapture analysis is subject to two important assumptions: (i) that the probability of appearing on one list does not affect the probability of appearing on the other(s); and (ii) that the probability of a dead person appearing on any given list is equal. In practice these assumptions are almost always violated. To adjust for this, capture-recapture analysis employs hierarchical log-linear statistical regression to model the number of deaths. Dependencies among lists (violation of assumption (i) above) are modeled as statistical interactions among two lists (e.g., list B with list D) as well as among groups of lists (e.g., lists A, B and C). Different probability of capture (violation of assumption [ii]) is modeled by including heterogeneity terms or by stratifying the analysis according to age, gender, or any other variable that is suspected to be associated with a higher or lower probability of appearing on lists (this generally becomes obvious through descriptive analysis of the lists, e.g., if the age or gender breakdown is significantly different across different lists). Stratification removes most of this heterogeneity, and results for the different strata can then be added together to give the total. However, stratification can only be applied if a sufficient number of deaths are recorded.

If there are many lists, one can include in the model various combinations of interactions between lists, and thus many different models are possible: the choice of the best model (and thus of the best estimate of total deaths) is made using standard goodness of fit criteria, or a selection of models are averaged using Bayesian methods (see **Section 3.4.3**).

In this study, we performed capture-recapture analysis on three different lists of deaths collected within each field site, one of those being the list compiled from implementation of the EM method.

3.4.2. Collection of Additional Lists of Deaths

Within each site, in addition to the exhaustive search, we collected **two additional lists** of recent deaths from two sources separate from the key informants. **Table 2** details the different lists of deaths used for capture-recapture. We used the calendar aid to help with recall of dates, and collected basic details for each death (date of death, sex, approximate age, place of residence including house number in Mae La, name of the deceased and name of his/her household in Chiradzulu or of his/her father elsewhere) on a special register (**Annex 8.7**).

Table 2. Details of Lists Used for Capture-Recapture Analysis, by Site

List	District 1, Kabul	Mae La Camp	Chiradzulu District	Tanzania Camps
1	Exhaustive search led by key informants (*wakils*, mullahs)	Exhaustive search led by key informants (section leaders, Karen Women's Organization members)	Exhaustive search led by key informants (village headmen, *fumukazi*)	Exhaustive search led by key informants (community leaders)
2	Convenience stores (small one room shops selling food, drinks and basic household items) and bakeries (community ovens where households bring their flour to be baked; separate female and male bakeries exist)	All Buddhist, Christian and Muslim religious leaders, in charge of funeral rites	Chairmen of village graveyard committees (*azukuru*), in charge of organizing burials	Deaths recorded in the camp register compiled by the camp management agency (World Vision in Lugufu camp, International Rescue Committee [IRC] in Mtabila camp)
3	Inpatient departments of all hospitals within the catchment area of District 1, including Maiwand Hospital, Ibna Sina Hospital, Indira Gandhi Hospital, Rab-e-Balkhi Maternity Hospital, the French Hospital, and the Tuberculosis Hospital	AMI mortality surveillance system, including data from the Mae La hospital inpatient department; Shoklo Malaria Research Unit [SMRU] maternal clinic	Stabilization and maternity wards of all public health centers in Chiradzulu District (n=10); inpatient departments of Chiradzulu District Hospital (including morgue) and St. Joseph's Nguludi Hospital; Health Surveillance Assistants deputized to each of the 96 villages	Mortality surveillance data collected by TRCS in Lugufu and Mtabila camp

Sources for First Additional List

We selected the sources for the first additional list based on the FGD conducted in each site. The source was selected based on criteria of feasibility (i.e. how easy it would be to contact informants), sensitivity as judged by FGD participants, and independence from the key informants used to collect data for the exhaustive search (i.e. we sought to avoid sources that would themselves obtain their knowledge from the EM method key informants, or vice versa). Data collection within each sector occurred on the same day or the following two days as the exhaustive search to ensure comparability of the recall periods. However, it was done after or in parallel to the exhaustive search, to minimize investigator bias. We provided the same information to these sources as to key informants for the exhaustive search, and used the same search criteria (deaths in the last two months within the sector). The following site-specific procedures were followed:

- In Kabul, after seeking permission from the *wakil*, we approached each open convenience store and/or bakery within each sector, identified by systematically walking through the entire sector. In each store we approached the attendant(s) only, though in practice bystanders were also involved in the conversation.

- In Mae La, we met with religious leaders, including Buddhists, Christians and Muslims, and asked them to draw up a list of persons whose funeral ceremony they had officiated, which we collected on a separate day; we verified any unclear information on each religious leader's list through supplementary household visits.

- In Chiradzulu District, we asked the village headman to refer us to the chairman of the graveyard committee (always a male), though whenever possible we interviewed him separately to minimize any possible reporting bias caused by the headman's presence. In the committee chairman's absence, we interviewed his deputy.

- In Lugufu and Mtabila camps, we consulted the camp register of deaths compiled by the camp management agency (World Vision in Lugufu camp, International Rescue Committee [IRC] in Mtabila camp).

Sources for Second Additional List

The second source was selected *a priori*. It consisted of existing surveillance systems in Mae La and Tanzania, and hospital inpatient records in Kabul (**Table 2**). In Chiradzulu, hospitals and health centers yielded a low number of deaths, and we decided to supplement the list by contacting Health Surveillance Assistants (HSAs; also known as community health workers [CHWs]) seconded to each village: we asked HSAs to spend a day in their village and, relying on their usual information network, report deaths since the June-July 2008 census on a standardized form, including identifier variables as above, whether the person was resident in the village, and who provided the information on the death; we excluded deaths reported by the village headman, *fumukazi* and graveyard chairman so as maximize the independence of the list from the others. HSAs received a nominal payment for this work.

Sources for the second list were accessed at the very end of fieldwork; however, the recall period was extended to the first day of data collection in the exhaustive search, to capture comparable periods. Deaths reported as occurring after the day on which the sector was visited were excluded from analysis.

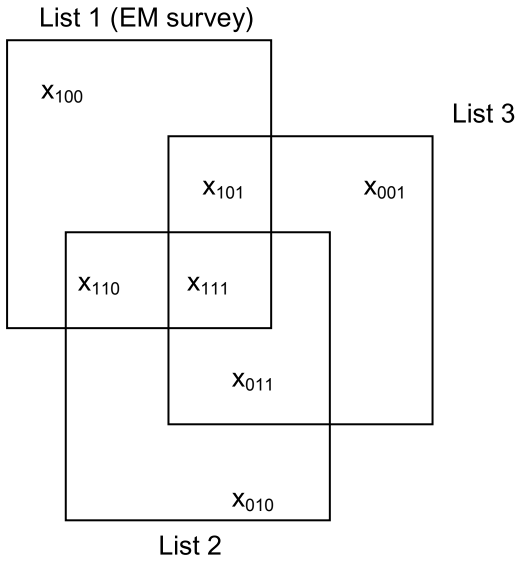
At the end of data collection, we thus had three lists of deaths covering the period and population sampled within each site: one list from the key informants, one from an additional source selected through the FGD, and one from curative and/or health surveillance sources.

3.4.3. Capture-Recapture Analysis

After routine data cleaning based on range and consistency checks, we excluded deaths on any lists based on the same criteria as in **Section 3.3.7** (also see **Section 6.4** for potential limitations). When a death was reported on more than one list but dates of death were discrepant, we adopted the date of death from list 3 (medical sources) if available, and the date from list 1 (EM survey) otherwise.

We matched deaths on different lists manually. Due to the small number of deaths in the three lists and the range of uniquely identifying variables, this process was straightforward and no cases of doubtful identity were encountered. Based on this we determined the number of analyzable deaths that fell within each x_{ijk} category (**Figure 2**), where i, j and k are dummy variables denoting the presence (1) or absence (0) of a death within lists 1, 2 and 3 respectively (thus, x_{110} in Mae La camp is the number of deaths captured by the EM method and religious leaders but not the AMI surveillance system; x_{000} in any site is the unknown number of deaths not captured by any source). For the sake of clarity these are represented as a Venn diagram.

Figure 2. Venn Diagram Representing the Overlap Among the Three Different Lists



Log-Linear Modeling

We fit log-linear Poisson models to the contingency table formed by each x_{ijk} category, as described by Fienberg et al.[21] All eight possible models were fit, including without any interactions, with one interaction among two lists (list 1 x list 2, list 1 x list 3, list 2 x list 3), two interactions (list 1 x list 2 + list 1 x list 3; list 1 x list 2 + list 2 x list 3; list 1 x list 3 + list 2 x list 3) and three interactions (list 1 x list 2 x list 3, known as the saturated model). Note that a fundamental (and unverifiable) assumption of three-list capture-recapture analysis is that there is no three-way interaction (i.e. list 1 x list 2 x list 3). Each model can be used to predict m_{000}, i.e. the estimated x_{000}, as follows:

$$m_{000} = \frac{m_{111} m_{100} m_{010} m_{001}}{m_{110} m_{101} m_{011}}$$

For each model, we computed goodness of fit statistics including the Chi-square p-value based on the residual model deviance (i.e. how well the model fits, compared to the saturated or “full” model), the Bayesian Information Criterion (BIC; low values denote better fit), and the Pearson-adjusted Chi-square (=Chi-square/degrees of freedom; useful to privilege parsimonious models, i.e. with fewer co-variates). We also computed the point estimate of m_{000} for each model, and its 95%CIs as the 2.5th and 97.5th percentile of the likelihood profile.

Instead of selecting one of the models as the most likely, we used Bayesian Model Averaging (BMA), as recommended for capture-recapture problems [21], and as reviewed by Hoeting et al [22].

We first excluded any of the eight models that yielded implausible estimates of m_{000} (>four times the total number of ascertained deaths), or that displayed probable overfitting (P-value >0.60; Ball et al. [20] use a 0.30 cutoff); models that fit known data exceedingly well may in fact predict unknown data points very poorly. In practice, models with severe overfitting often also did not converge to a solution.

Among remaining models, we fit Bayesian log-linear models with uninformative priors, and computed the posterior probability of each model (i.e. how likely it is that the model reflects the truth; note that posterior probabilities among competing models sum to one). We then computed m_{000} (either point estimates or CIs) as $m_{000} = \sum_{i=1}^{i=k} m_{000,i} \Pr_i$, where k is the total number of models averaged over, i is one of these models and Pr is the posterior probability of that model.

Model fitting, selection and averaging were done using a programming code written in R (**Annex 8.12**). To minimize the possibility of mistakes due to faulty programming, results were compared with a freely available Stata code written by Matthias an der Heiden (http://ideas.repec.org/c/boc/bocode/s456859.html), which yielded the same point estimates and only slightly wider 95%CIs: results from this Stata program are not reported for brevity's sake.

Because of the low number of deaths in the lists, we were unable to stratify the analysis to reduce heterogeneity.

Wherever possible, we estimated the number of uncaptured deaths among children under 5 years, as above: however, this was not feasible in many instances due to the very low number of deaths detected.

Alternative Capture-Recapture Analysis Approaches

As an alternative to the log-linear approach, we also used the method of Marks, Seltzer and Krotki [23], which estimates m_{000} by combining information from pairs of lists, as follows:

$$m_{000} = \frac{x_{100}x_{010} + x_{100}x_{001} + x_{010}x_{001}}{x_{110} + x_{101} + x_{011}}$$

This approach is less satisfactory as it does not account well for list interactions, but may be more robust when log-linear models are hindered by the low number of observations in one or more of the lists.

We also applied the simple method of comparing pairs of lists, whereby $m_{00}=(x_{10}x_{01}/x_{11})$ (note that estimates from this method almost always violate capture-recapture assumptions).

Results from these alternative approaches are reported to compare with and corroborate choices of the optimal log-linear model, but are not used for estimating the EM method's sensitivity.

Sensitivity of the EM Method and Other Sources

The estimated total number of deaths D occurring within the recall period and community surveyed was calculated as the total number of ascertained deaths (i.e. captured by any source) plus m_{000} as estimated by BMA. The sensitivity of the EM method or of any other source is thus the total number of deaths captured by the method/source, divided by D.

Adjustment of Mortality Rates to Account for Uncaptured Deaths

The approach to incorporate capture-recapture estimates of undetected deaths into mortality rate estimates was the same for all sites. The estimated number of undetected deaths was based on the selected log-linear model, and its probability distribution was provided by the profile log-likelihood of the model. During each bootstrapping iteration, a random value was sampled from this distribution, and added to the total ascertained deaths to provide the estimated true number of deaths, which was then divided by the population estimate and recall period, as described in **Section 3.3.7**.

3.4.4. Corollary Qualitative Data Collection

An informal FGD, with the data collectors themselves as participants, took place after the quantitative data collection in Kabul, Chiradzulu District and Tanzania. In Mae La, informal discussions were held with key members of the first FGD. The aims of this second FGD were to explore: (i) potential explanations for discrepancies between the EM method and alternative sources on deaths; (ii) observed patterns of chain referral during the data collection; (iii) ethical and confidentiality implications, and burden to respondents and data collectors, of the EM method. No formal analysis of this FGD was done, but notes were used to aid the overall assessment of the method.

3.5. FEASIBILITY OF THE EM METHOD

3.5.1. Economic Feasibility

A modeling approach was used to document the economic feasibility of method. This compared the observed time input into the EM method with that which would be expected for the closest alternative method, namely a retrospective mortality survey, in the four sites. This approach was used because it avoids the challenges associated with using actual monetary costs (mainly obtaining accurate financial data from retrospective mortality surveys conducted in the same/similar location and time-period as the EM method). This approach excluded non-time-based parameters such as materials and supplies as most major inputs were time-based. Estimated monetary costs were then attached to time inputs to also produce a monetary outcome for the model to enable a cost-based comparison between the EM method and retrospective mortality surveys.

We assumed that a retrospective survey would be conducted in each of the four sites broadly in line with guidelines set forth by the Standardized Monitoring and Assessment in Relief and Transition (SMART) initiative (www.smartindicators.org). Taking into account the layout and organization of the four communities, we assumed that cluster sampling with probability proportional to size (PPS) allocation of cluster starting points would be used in Kabul and Chiradzulu, and simple random sampling in the Mae La and Tanzania camps, where household lists are available. We assumed typical sample sizes of 30 clusters of 32 households in Kabul and Chiradzulu (i.e. 960 households in total, based on a design effect of 2.0), and 480 households (i.e. the same effective sample size, but with a design effect of 1.0) in Mae La and Tanzania. Other assumptions are listed in **Annex 8.11**.

The activities included in the feasibility study were those which related to the implementation of the EM method (**Section 3.3**), but not those relating to validation or feasibility evaluation. These included: preparation, training, FGDs (including field-based analysis), population estimation, mortality data collection as part of the exhaustive search, data entry and cleaning, data analysis (estimation of mortality rates) and production of a short field report.

Time inputs (both paid staff time and opportunity costs incurred by the study population) were systematically recorded by the study investigators for the following types of individuals involved in the study: study investigators; other study staff; drivers; collaborators (individuals working with the collaborating agency or others indirectly involved such as community administrative or public health officers); data collectors; and respondents.

Monetary costs were estimated based on discussions with local informants about prices and salary costs in each of the four locations. The calculations for these monetary costs for the EM method and retrospective mortality surveys are provided in **Annex 8.11**.

Two types of analysis were conducted on the economic feasibility data: i) comparison of the time and monetary cost inputs between the EM method and retrospective mortality surveys for each of the four study sites; and ii) time input per death recorded for the EM method.

3.5.2. Assessment of Ethical Implications

This sub-objective sought to compare the ethical implications of the EM method in terms of risks and burden to the community and individual respondents. Findings from discussions with data collectors and personal perspectives of study investigators (based upon their observations and past experience with surveys and prospective surveillance systems) and data review were used to explore issues such as confidentiality arrangements, possible fallout from sharing data with investigators, and burden to respondents (such as the amount of data collected, number of respondents involved in data collection).

3.5.3. Verbal Autopsy Feasibility

Each verbal autopsy interview was timed separately to collect data on the feasibility of adding verbal autopsy to the EM method. The time for verbal autopsy questionnaire analysis and coding was also recorded.

3.6. DATA MANAGEMENT

All mortality data (from the exhaustive search or consultation of additional sources for capture-recapture) were double-entered on EpiData version 3.0 software (EpiData Association, Odense, Denmark). Any discrepancies between the two data entries were verified using the source questionnaires. The dataset was cleaned based on range and consistency checks (e.g., age within plausible range), and multiple reports of deaths were eliminated based on uniquely identifying variables. Population estimation data were single-entered onto Microsoft Excel and hand-verified. All analyses were done using freeshare R software.

3.7. ETHICAL CONSIDERATIONS

Ethical approval for this study was provided by the Ethics Committee of the LSHTM and all the national research ethical review boards where required. These were the Institutional Review Board of the Ministry of Public Health of Afghanistan; the National Health Sciences Research Committee in Malawi; and the National Institute for Medical Research and also the Commission for Science and Technology in Tanzania (approval letters available from the investigators upon request).

Data collectors, translators and key informants were briefed on the principles of confidentiality and informed consent. Investigators were also present during the data collection to help ensure adherence to these principles.

The principle of confidentiality was adhered to as closely as possible in the study. Interviews were conducted as far as possible in a private space chosen by the respondent. The movement of all questionnaires was closely controlled and questionnaires are securely stored. The data from the FGDs was stored electronically. Only the investigators have access to electronic and paper data.

4. EM Validation and Feasibility Results

4.1. FOCUS GROUP DISCUSSION FINDINGS

4.1.1. District 1, Kabul

The separate FGDs for women and men were held in District 1 on 12 July 2008. There were four participants in the women's FGD and four in the men's FGD (see **Table 1)**. There were no marked differences in the findings of the women's FGD and the men's FGD. As a result, they are presented together.

The different sources of mortality information identified by the participants are summarized in **Table 3**.

Table 3. Sources of Information on Deaths Identified During the FGD in District 1, Kabul

Source	Advantages	Disadvantages	Notes
Wakil-e-guzars (sector leaders)	Official role as community leaders means they know about key events, including deaths. Informed either directly by family of deceased or neighborhood informants. Often invited for funeral prayers for deceased.	May not immediately know of deaths because of large population size in their *guzar.* Population change means it is difficult for *Wakil-e-guzars* to be fully informed of all events.	Wakil-e-guzars are all male, and nominated by election. Each controls a population of several thousand There are 24 *guzars* in District 1, each headed by a *wakil*.
Mullahs	Offer prayers for funeral of deceased and so know quickly about a death in the community.	May be new to District 1 and so not familiar with all events.	Mullahs are male, and usually affiliated to Ministry of Islamic Affairs. Each *gazar* contains at least one and typically up to five mosques, both Shi'a and Sunni.
Shopkeepers (*kondora*) and bakers	Meeting places where community members share news about recent/future events such as deaths and funerals	Knowledge mainly limited to few streets surrounding shop/bakery.	The small commodity shops (*kondora*) serve the surrounding streets and so number several hundred in total. There are separate men's and women's bakeries. Each *guzar* has at least one of each.
School teachers	Good knowledge of deaths of their school pupils and also some knowledge of the deaths of families of the pupils.	Knowledge limited to mainly deaths of pupils and their families.	There are a total of 47 public and private schools in District 1 (all ages). The teachers are from inside and outside of District 1.
Hospitals	Critically ill residents from District 1 would attend the main hospitals serving the district. Hospital records of deaths from District 1 should be available.		The main hospitals serving District 1 include Maiwand Hospital, Ibna Sina Hospital, Indira Gandhi Hospital, Rab-e-Balkhi Maternity Hospital, the French Hospital, and the Tuberculosis Hospital

FGD participants were asked about the ways in which information on deaths in the community is shared in District 1. Participants noted that relatives of the deceased would directly inform the wakil-e-guzars and mullahs. They also noted how information is spread about the death and the funeral through household visits and phone calls by family members of the deceased, and loudspeakers at the mosque. The small commodity shops (*kondora*) and bakeries were also noted as meeting places where people exchange news on events in the community. The participants felt that households would also be willing to share information on deaths within the household and also refer data collectors to other households.

FGD participants noted a number of potential challenges to obtaining information on deaths. The population change in District 1 (departures of old residents, and arrivals of many new immigrants, including short-term renters) meant that knowledge about deaths in the community may be weakened which could affect the method, particularly in terms of referrals from other households. In addition, a *wakil* participant noted that it was difficult for them to immediately know of deaths because of the large population size in their *guzars* (up to 4,000 people). The deaths of certain community members may also be less well known about. A cited example was information on the deaths of children, particularly very young children, because knowledge of the community and potential key-informants was less due to their young age and that funerals for children were smaller events more limited to the immediate family. Certain causes of death may also affect knowledge of the deaths. Participants noted that deaths caused by illegal activities such as violence and illicit drugs would not be widely disseminated because of the sense of shame on the family. As a result, the death may not be so well known by community members and potential key-informants and also the true cause of death may be concealed by family members.

4.1.2. Mae La Camp

The FGD was held on 10 July 2008. There were 33 participants (many more attended than were invited; see **Table 1**). The different sources of mortality information identified by the participants are summarized in **Table 4**.

Table 4. Sources of Information on Deaths Identified During the FGD in Mae La Camp

Source	Advantages	Disadvantages	Notes
Section Leaders	Official source of mortality information. Comprehensively cover the whole camp. Are able to report deaths due to infectious diseases directly to health workers, potentially informing prevention or intervention measures.	Not everybody reports to them. Gives an incomplete picture of the population. Often do not know the cause of death (which limits ability to identify outbreaks). Leaders not trained and may not understand causes of death, even if explained to them.	There are 22 sections in Mae La camp. Each one has a leader who is democratically elected. They are key contributors to official statistics about the population in their sector (births, deaths, in- and out-migration).
Section Health Workers	Section Health Workers (as a group) cover the whole camp population.	Have no training and may not understand causes of death.	Are in effect 'public health administrators' who are mostly involved in distribution of some items among the camp population and assist Section Leaders to track population data.
Religious Leaders	They know about deaths not reported to Section Leaders/Section Health Workers. Almost everybody in the camp belongs to a church, temple, or mosque. They are informed about deaths almost as soon as they happen (i.e. very timely data). They know exactly in which household the deaths occur.	They do not report deaths to camp officials. Churches make an annual list, but no written records are kept by the mosques or temples. There are lots of churches/mosques/temples (i.e. fragmented data source). A small number of people practice Animism, for which there is no formal religious authority involved in burials/cremations.	There are no Hindus in Mae La.
Home Visitors	More likely to report deaths to camp officials. Have training in health matters and are more likely to have some knowledge about cause of death.	Seen as quite strict (enforcers of public health practices). Households may not report deaths to them.	Home Visitors are employed by AMI.

SMRU	Has good records from maternity clinic in camp.	Not sure what death data are collected.	Runs a maternity clinic in the camp where the majority of women receive antenatal and postnatal care.
Karen Women's Organization	May be more aware of issues among women and children.	Probably does not have any additional information about deaths.	Members are female. Responsible for certain women and child issues.

Ways in which information on deaths in Mae La camp is shared included religious leaders who conduct wakes and burial ceremonies; via family and friends; people gossiping; and people buying flowers in the morning market for the deceased. Participants felt that households would be willing to agree to participate in the survey, even if the death was very recent.

The potential challenges noted by the FGD participants in trying to collect information on deaths from community members included households being unwilling to respond to a survey about death unless is has the support of the Section Leaders and Camp Committee. The type of death may also affect willingness to share information, particularly if deaths were due to murder or if the deceased was part of an armed group. There may also be embarrassment about deaths due to sexually transmitted infections (STIs)/HIV which could lead to misreporting of the cause of death (although they would probably report that a death had occurred). It was also noted that it may be difficult to identify deaths of new arrivals or individuals not registered. The FGD participants observed that households may not respond if they thought the study team was conducting a census or if the study was perceived to be related to food rations or distributions. Some camp residents may also have Thai identification cards and may be reluctant to report these deaths as they may result in their refugee status being questioned. FGD participants felt some respondents may not know the name of particular diseases and so could not accurately identify causes of death.

4.1.3. Chiradzulu District

The FGD was held in Khongozo village in Chiradzulu district on 23 August 2008. There were 9 participants (see **Table 1**). The different sources of mortality information identified by the participants are summarized in **Table 5**.

Table 5. Sources of Information on Deaths Identified During the FGD in Chiradzulu District

Source	Advantages	Disadvantages	Notes
Village headman	Expected to attend all funeral ceremonies in the village. Role of informing people about deaths, including those in charge of graveyard. Some headmen keep a register of births and deaths. Responsibility as headman and so will tell truth. Major events are reported to headman.	May exclude information on families if unfriendly with them, or if families have not paid him a contribution. May be drunk. Could be away from village. Headman may not know about stillborn babies. May forget. May be new and so less knowledgeable.	Each village has a headman who is nominated by village elders; may be a male or female. Has overall responsibility for the running of the village, dealing with village disputes, and referring matters to the next administrative level (the Traditional Authority). Also responsible for allocating land in graveyards for burial.
Fumukazi (see Notes)	Informed about newborn deaths very quickly Good knowledge on deaths of children and newborns (responsible for burying 2-3 day old children). Expected to be present at all village funerals. Knows many women in the community. Works closely with headman so may also know of deaths from him. Trustworthy.	May forget about deaths in the village. May be drunk. May not know about all deaths if there is not unity in the village. Long procedure for reporting deaths to headman (may take several days).	The f*umukazi* is the village headwoman, sometimes deputy to the headman, and can also fill the role of village headman. The *fumukazi* is responsible for women's activities, specifically marriage, burying young babies, following-up of pregnancy, and cooking for funerals.

Church/ mosque leader	Lead funeral ceremony. Knowledgeable about deaths of church/mosque members (often keep written records). Informed of deaths by letters and also by members of church/mosque. Role of spreading information about deaths and funeral to other church/mosque members. Honest.	Frequent turnover of church/mosque leaders reduces knowledge. May be less informed about deceased who were not active in church/mosque. Newborn deaths are not registered in the church. May be hard to find as are often away. Often serve more than one village so knowledge may not be very detailed.	Churches (different denominations) and mosques serve a cluster of villages rather than being found in each and every village.
Azukuru (graveyard chairman)	Responsibility of grave-digging and burial means very good knowledge of deaths in village. Longstanding appointment so has good knowledge of deaths in village.	Do not have a written record so may forget. Not all people are buried in graveyard (e.g. some are drowned or go missing, or buried in other villages or at home). Poor cooperation with family of deceased may limit knowledge/sharing of knowledge.	In each village, a man from the village is appointed chairman of the *azukuru, and* leads a group of *ad hoc* volunteer gravediggers. The *azukuru* has overall responsibility for the digging of graves and burial of the dead in the village.
CHWs	Keep written records/books with all households listed. Able to know the cause of death.	Some CHWs don't live close to village. Some CHWs rarely visit the village at all. New CHWs don't know community well.	

The FGD participants described a number of ways in which people in the villages share information about deaths. These include orally in the village and when working; over the phone and through letters; and announcements on the radio. There are also specific signs giving notification of a death (for all deaths except for babies). These signs include a special beating of drums; ringing a bell (mainly for Christians); laying out freshly cut tree branches across the road next to village (most common); and blowing of whistles.

The potential challenges to obtaining information on deaths could be that people may hide information because of the circumstances of the death (examples cited included murder, drowning, and manslaughter). People may also give a different reason for the cause of death because of a sense of shame or embarrassment. For example, they may state malaria instead of HIV/AIDS. People may also be fearful which could lead to only partial responses. For very recent deaths, respondents may get very upset. However, this was thought to be a rare challenge. People may also not give information on premature deaths (neonatal, early deaths).

The FGD participants noted a number of challenges of using community members to give information about another household which recently suffered a death. For example, if the respondent had bad relations with the family of the deceased persons, they may not refer to that family. Others may be fearful in case it caused distress to the family of the deceased.

4.1.4. Lugufu and Mtabila Camps

The FGD in Lugufu was held in a meeting hall in the main health centre on 29 September 2008. There were 14 participants (see **Table 1**). The different sources of mortality information identified by the participants are summarized (see **Table 6**).

Table 6. Sources of Information on Deaths Identified During the FGD in Lugufu camp, Tanzania

Source	Advantages	Disadvantages	Notes
Community Leaders	Know of major events, including deaths (all causes, all ages). Prompt knowledge as help coordinate support for relatives of deceased (e.g., funds for burial, food etc). Often attend burial of deceased. Systematically informed by other community leaders (e.g., block leaders) and also informed by regular community members.	May want payment for providing information. May fear that information will be given to police and police will want to question them. May not always record because of low literacy or lack of materials. Families may ask them not to report deaths (because of food rations and repatriation).	Leaders are camp residents selected by other residents. Include block leaders (up to 30 per village) who report to village leaders (32 villages) who report to zone leaders (eight zones) who report to the camp chairman.
CHWs	Good knowledge of general health situation in their area, including deaths. Refer sick residents to camp hospital. Sometimes keep written records, including of deaths.	People may not admit death to CHW (especially if had ignored CHW advice). Know less about home deaths as not always reported to CHW. Use of traditional medical practices means CHW may not be aware of illnesses/deaths. Don't keep systematic records. May not report a death if they feel they themselves failed to adequately treat/refer deceased.	CHWs are volunteer camp residents who provide very basic health information, prevention and referral services for TRCS.
Religious Leaders	Involved in burials (all ages). Routinely visit community members so have knowledge of key events. Information on deaths is spread by them/to them in the church/mosque.	May only know about deaths of regular members of their congregation. Relatives of deceased who used traditional medical practices may not report to religious leader. May not keep accurate records. There are many religious leaders.	There are over one hundred Christian and Muslim religious leaders. The majority are Christian and represent many denominations.
Graveyard Keepers	Keep very good records as used to inform deaths for World Vision's camp registration system. Not possible to hold burial in graveyard without informing graveyard keepers. All ages are buried (including the newborns). Hold details about the deceased (name, address, age, cause of death). Involved in issuing the burial shroud for the deceased.	Deaths outside the camp may be buried elsewhere (but rare). A keeper may be away and burial takes place without him (although someone will probably act as an assistant and pass on information to the keeper upon his return).	There are three graveyards and each is attended by a graveyard keeper. They are supported by World Vision (camp management agency) and their reports of death are used for the camp register maintained by World Vision.
Traditional Birth Attendants (TBAs)	Knowledgeable on deaths of babies and mothers. Involved in burials of babies.	Knowledge mainly limited to deaths of babies/mothers. Most don't keep a written record. May not be told about deaths if relatives of deceased ignored their advice. May not report deaths if they feel they gave poor advice.	Camp residents providing voluntary services to pregnant mothers and newborns. Receive basic training from TRCS and NGOs.

The FGD participants in Lugufu camp described a number of ways in which people in the camp share information about deaths. These included through religious leaders who spread information on deaths to their congregations; village/block leaders; camp radio stations; word of mouth; mobile phones; visits to other households; using children to pass message and letters to other households; and public gathering points such as food distribution and water points.

A number of potential challenges to obtaining information on deaths in Lugufu camp were noted by the FGD participants. These included respondents being too sad to want to be interviewed or to give accurate information; households' perception that the research is tied to food aid or repatriation, leading them to deny that a death occurred; reluctance/shame by relatives of the deceased to admit to the death if they had ignored medical advice or used traditional medicine, or because of the cause of death (e.g., suicide or crime-related).

The FGD in Mtabila camp was held in a meeting hall in the main health centre on 6 October 2008. There were 22 participants (see **Table 1**). The different sources of mortality information identified by the participants are summarized in **Table 7**.

Table 7. Sources of Information on Deaths Identified During the FGD in Mtabila Camp, Tanzania

Source	Advantages	Disadvantages	Notes
Community Leaders	Street leaders very confident they knew about all deaths. Involved in coordinating support for relatives of deceased (e.g., funds for burial, food etc). Often attend burial of deceased. Often visit households in area and so know of key-events. If someone is very ill the relatives will inform the street leaders. Community members expected to inform street leaders of a death. Know about deaths of all ages. Well established reporting system of events from block leaders to village leaders and to the chairman. Expected to report deaths from unnatural causes to camp authorities.	May want payment for providing information. May not always write down information.	Community leaders are camp residents selected by other residents. These consist of street leaders (around 50 for each of the 10 zones in the camp) who report to zone leaders (1 man and 1 woman for each of the 10 zones) who report to the camp chairman.
Security Guards	Based in all zones. Some may keep records. Report to camp authorities major events, including deaths.	May not record all deaths (more likely to know about unnatural causes). Do not have as extensive knowledge of whole of the zone (compared with street leaders).	Camp residents who act as voluntary police in each of the zones (there can be up to 23 in each zone)
Religious Leaders	Involved in burials (all ages). Good contacts with community members so know about events.	Will only know about deaths of regular members of their congregation. There are many religious leaders in the camps (apparently there are nearly 40 denominations in the camp and hundreds of leaders).	There are over one hundred religious leaders, predominantly Christian. They represent many denominations.
Graveyard Keepers	Keep very good records about deceased (name, address, age). Not possible to hold burial in graveyard without informing graveyard keepers to have comprehensive knowledge. All ages are buried (including the newborns). Involved in issuing the burial shroud for the deceased. Employed by camp management and report to them so keep good records.	Won't know about deaths outside of the camp.	There are 3 graveyards and each is attended by a graveyard keeper. They are supported by IRC and their reports of death are used in the camp register compiled by the IRC.

A number of potential challenges to obtaining information on deaths were noted by the FGD participants. These included that informants may not know about deaths of refugees from the camp but who died outside of the camp. Participants also noted that households where a death occurred may have repatriated back to Burundi.

4.2. EXHAUSTIVE SEARCH FOR RECENT DEATHS

4.2.1. Choice of Key Informants

The selection of primary and secondary informants was made based upon the analysis of FGD findings. **Table 8** presents the key-informants selected, along with a summary explanation for their selection. Further information on these informants can be found in **Section 4.1**.

Table 8. Primary and Secondary Key Informants, by Study Site

Informant	District 1, Kabul	Mae La Camp	Chiradzulu District	Tanzania Camps
Primary	*Wakil-e-guzars*: Principal source of information on deaths in community as officially responsible for *guzar* and so should be informed about deaths. Attend many funerals in the district. All *wakils* were interviewed to give information on deaths in their *guzar*.	*Section leaders:* Responsible for official reporting of vital statistics Comprehensive coverage of entire camp.	*Village headman:* Responsible for allocating land in graveyards for burial of individual decedents. Expected to be present at all village funerals. Informed of key events in village. Role of sharing information on key events.	*Community leaders:* Camp residents selected by other residents. These consist of street leaders (around 50 for each of the 10 zones in the camp) who report to zone leaders (1 man and 1 woman for each of the 10 zones) who report to the camp chairman. The camp chairman and zone leaders were the main community leaders used.
Secondary	*Mullah:* Theoretically responsible for funeral prayers after any death. Give notification of death and funeral at the mosque. Each *wakil* sector contains at least one and typically up to five mosques, both Shi'a and Sunni; all mosques were visited.	*Karen Women's Organization members:* Members are female. Strong knowledge of deaths of women and children.	*Fumukazi*: Good knowledge on deaths of children and newborns. Expected to be present at all village funerals. Village elder and so informed of key events in the village. Works closely with village headman so informed of events.	Secondary informants were not used as available reliable sources were either not feasible (e.g., religious leaders as there are well over one hundred religious leaders in each camp) or because they were already being relied upon for information by the sources used for the capture-recapture analysis (e.g., graveyard diggers as they provided information on deaths to the management agencies in both camps).

4.2.2. Timeframe, Coverage and Study Attrition

Details on the population size (all ages and under five years) investigated, the data collection period and study attrition for the 4 study sites are given in **Table 9**. The exhaustive search was carried out over 11 working days in Kabul, five days in Mae La, 17 days in Chiradzulu and seven days in Tanzania (results for both camps combined).

All community informants who were found agreed to provide information. In Chiradzulu District, two households delayed their consent after consulting with family members or the headman. In Tanzania, one household refused to give consent.

Table 9. Timeframe, Population Covered by the Exhaustive Search, and Attrition of Key Informants, by Study Site

	District 1, Kabul	Mae La Camp	Chiradzulu District	Tanzania Camps
Population size (children under 5 years)	76,476 (13,790)	43,794 (5,384)	54,418 (9,462)	80,136 (16,028)
Timeframe of data collection	14-27 July 2008	11-17 July 2008†	26 August-16 September 2008	3-9 October 2008
Proportion of primary informants found (%)	26/26 (100%)	22/22 (100%)¶	91/96 (94.8%)*	15/18 (83.3%) §
Proportion of secondary informants found (%)	≈80%‡	22/22 (100%)	90/96 (93.8%)**	n/a (see **Section 0**)
Response rate (households found and giving consent)	100%	100%	100%	98%

‡ Data not collected systematically due to the large number of mullahs contacted: a rough estimate is provided.
† Two interviews were done on 27 July due to previous inability to contact the household.
¶ In practice, we consulted with the section leader as well as members of his/her office, who were usually present during the visit.
* In 5 (5.2%) of the 96 villages sampled in Chiradzulu district, the *fumukazi* was also the acting headman as the headman was away. In a further 7 villages (7.3%) the *fumukazi* was also the headman on a permanent basis (these are included among the 91 found).
** In the remaining 6 villages (6.3%) a deputy *fumukazi* was contacted as the *fumukazi* was away.
§ The three remaining primary informants were contacted by other primary informants and provided the required information.

4.2.3. Deaths Captured by the EM Method

Several deaths were excluded from the analysis after data entry, for the following reasons: (i) stillbirth (one in Kabul; four in Chiradzulu); (ii) death outside the 60 day recall period (two in Kabul; five in Mae La; three in Chiradzulu; six in Tanzania); (iii) not residing in the community (one in Mae La; seven in Chiradzulu). Note that an additional number of deaths were reported outside of the recall period, but were immediately discarded (we did not keep systematic records about referrals that, upon household visit, proved incorrect).

Table 10 provides details on analyzable deaths reported by the different informants over 60 day and 30 day recall periods.

Table 10. Number (Percentage) of Deaths Captured Using the EM Method, by Informant Type, Recall Period, and Study Site

Informant	District 1, Kabul		Mae La camp		Chiradzulu District		Tanzania camps	
	60 d	30 d	60 d	30 d	60 d	30 d	60 d	30 d
Primary and secondary community informants	55 (82.1)	11 (100.0)	20 (74.1)	11 (68.8)	90 (96.8)	35 (94.6)	42 (95.5)	20 (95.2)
Respondent households	11 (16.4)	0 (0.0)	9 (33.3)	4 (25.0)	6 (6.5)	5 (13.9)	9 (21.4)	4 (20.0)
Others	4 (6.0)	0 (0.0)	1 (3.7)	1 (6.3)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)
Total deaths, excluding multiple reports	67	11 (16.4% of 60 d)	27	16 (59.2% of 60 d)	93	37 (39.8% of 60 d)	44	21 (47.7% of 60 d)

In all four sites, primary and secondary informants provided the majority of reports. These were either provided as written lists or verbally, and the primary and secondary informants were also frequently probed to elicit information on more deaths. Few reports came from respondent households. However, of deaths reported by households, only 3/11 in Kabul, 3/9 in Mae La, 3/6 in Chiradzulu were also reported by community informants, considering a 60 day recall period.

The estimated mortality rates as measured by the EM method are reported in **Section 4.4.5** to facilitate comparison with the capture-recapture gold standard.

4.2.4. Profile of Deaths Captured by the EM Method

Key characteristics of deaths captured by the method are reported in **Table 11**. The sex ratio appeared balanced in all sites. About one third of deaths were among children under 5 years in Kabul and Chiradzulu. Most decedents received some medical treatment before death. However, the majority died at home, especially in Kabul. The breakdown of causes of death revealed expected patterns, with a predominance of infectious and/or neonatal conditions in Chiradzulu and proportionately more chronic disease elsewhere, with a higher contribution of injuries in Kabul, the urban site. There was no obvious difference in the profile of decedents according to recall period.

Table 11. Profile of Deaths Captured by the EM Method, by Recall Period and Study Site

	District 1, Kabul		**Mae La Camp**		**Chiradzulu District**		**Tanzania Camps**	
	60 d (n=67)	**30 d** (n=11)	**60 d** (n=27)	**30 d** (n=16)	**60 d** (n=93)	**30 d** (n=37)	**60 d** (n=44)	**30 d** (n=21)
Demographic characteristics (%)								
Median age in years (range)	35 (0-99)	50 (0-99)	55 (1-87)	63 (21-87)	35 (0-96)	35 (0-85)	4 (0-98)	25 (0-98)
Age below 5 years	20 (29.9)	1 (9.1)	2 (7.4)	0 (0.0)	26 (28.0)	12 (32.4)	22 (50.0)	8 (38.1)
Male sex	32 (47.8)	4 (36.4)	15 (55.6)	6 (37.5)	47 (50.5)	16 (43.2)	22 (50.0)	10 (47.6)
Place of death (%)								
In person's household	50 (74.6)	9 (81.8)	15 (55.6)	10 (62.5)	51 (54.8)	22 (59.4)	9 (20.5)	4 (19.0)
In a different household	1 (1.5)	0 (0.0)	2 (7.4)	5 (31.3)	2 (2.2)	0 (0.0)	3 (6.8)	2 (9.5)
In a health structure	12 (17.9)	2 (18.2)	8 (29.6)	1(6.3)	39 (41.9)	14 (37.8)	29 (65.9)	13 (61.9)
Elsewhere	4 (6.0)	0 (0.0)	2 (7.4)	0 (0.0)	1 (1.1)	1 (2.7)	3 (6.8)	2 (9.5)
Received any medical treatment before death (%)								
Yes	51 (76.1)	8 (72.7)	18 (66.7)	10 (62.5)	79 (84.9)	32 (86.5)	37 (84.1)	20 (95.2)
No / Don't know	16 (23.9)	3 (27.3)	9 (33.3)	6 (37.5)	14 (15.1)	5 (13.5)	7 (15.9)	1 (4.8)
Cause of death according to respondent (%)								
Neonatal (first month)	6 (9.0)	1 (9.1)	0 (0.0)	0 (0.0)	12 (12.9)	4 (10.8)	2 (4.5)	0 (0)
Probably infectious	10 (14.9)	0 (0.0)	4 (14.8)	3 (18.8)	33 (34.5)	10 (27.0)	18 (40.9)	8 (38.1)
Probably chronic	36 (53.7)	8 (72.7)	17 (63.0)	9 (56.3)	20 (21.5)	8 (21.6)	17 (38.6)	9 (42.9)
Probably injury-related	12 (17.9)	1 (9.1)	2 (7.4)	1 (6.3)	4 (4.3)	3 (8.1)	1 (2.3)	1 (4.8)
Unclear	3 (4.5)	1 (9.1)	4 (14.8)	3 (18.8)	24 (25.8)	12 (32.4)	6 (13.6)	3 (14.3)
Death recorded in writing by any organization, according to respondent (%)								
Yes	11 (16.4)	2 (18.2)	24 (88.9)	14 (87.5)	51 (54.8)	22 (59.5)	39 (88.6)	19 (90.5)
No / Don't know	56 (83.6)	9 (81.8)	3 (11.1)	2 (12.5)	42 (45.2)	15 (40.5)	5 (11.4)	2 (9.5)

The majority of deaths in Mae La and Chiradzulu, but not Kabul, were reported by respondents to have been recorded by one or more organizations. In Kabul, 7/67 (10.4%) deaths were reportedly recorded by hospitals and 4/67 (6.0%) by various administrative offices. In Mae La, according to respondents 21/27 (77.7%) of deaths were reported to the camp section leader, 12/27 (44.4%) to the AMI surveillance system home visitors and 2/27 (7.4%) to a church, mosque or temple. In Chiradzulu, 5/93 (5.4%) of deaths were reported to have been recorded by the District administration, 22/93 (23.7%) by religious leaders and 31/93 (33.3%) by health centers or hospitals. In Tanzania, according to respondents 32/44 (72.7%) of deaths were reported to have been recorded by health centre or hospital staff, 8/44 (18.2%) by community leaders, and 7/44 (15.9%) by religious officials.

4.3. POPULATION ESTIMATION

4.3.1. District 1, Kabul

Population estimation took place over two days, and all respondents consented to participate. Attrition was 0%. In the low-lying quadrant, 3/30 (10.0%) of quadrants sampled had in fact no residential structures; they were however retained in the analysis as zero values.

Stratum-specific population estimates were 48,723 (95%CI 39,783-56,780) in the low-lying stratum and 29,788 (95%CI 13,990-39,170) in the hill stratum considering all age groups; for children under 5 years, they were 9,387 (95%CI 6,705-13,410) and 4,112 (95%CI 2,404-8,180) respectively. Altogether, 76,476 (95%CI 58,494-89,523) people were estimated to live in District 1 at the time of the survey, of whom 13,790 (95%CI 10,224-19,004) or 18.0% were estimated to be children under 5 years. Further data on occupancy per household or residential structure are available upon request from the investigators.

4.3.2. Mae La Camp

The mid-period total population for Mae La camp was 43,794, of whom 5,384 (12.3%) were recorded as children under 5 years. This refers to the July 2008 update of population figures conducted by the AMI surveillance system.

4.3.3. Chiradzulu District

In three villages, only one structure count was performed. Structure counts in four neighboring villages were not handed over by the community, and had to be imputed (see **Section 3.3.6**). Overall, we estimated that the 96 villages sampled comprised 15,744 structures.

Residents of all structures selected for the survey of structure size consented and provided information: attrition was thus 0%. The estimated number of people per structure was 3.46 (95%CI 3.19-3.71, Deff=1.28), with 0.60 being children under 5 years (17.3%) (95%CI 0.49-0.73, Deff=1.44).

We estimated the population of the 96 sampled villages as 54,418 (95%CI 46,717-62,694), of whom 9,462 (95%CI 6,419-13,642) or 17.7% were estimated to be children under 5 years.

4.3.4. Lugufu and Mtabila Camps

We used population estimates at the mid-point of the recall period, based upon population data used by UNHCR. The mid-period population estimate for Lugufu camp was recorded as 38,363 for all ages and 7,673 (20.0%) for children below 5 years. The mid-period population estimate for Mtabila camp was recorded as 41,773 for all ages and 8,355 (20.0%) for children below 5 years. The combined population estimation was therefore 80,136 for all ages and 16,028 (20.0%) for children below 5 years.

4.4. VALIDITY

4.4.1. Comparison of Deaths Identified by the EM Method and Alternative Data Sources

A summary of the number, age and sex profile of the deaths captured by the three lists in each of the study sites is shown in **Table 12**. The EM method had the highest yield among the three lists in all sites except for Mae La, where surveillance identified more deaths.

Table 12. Demographic Profile of Deaths Captured by the EM Method and Alternative Lists, by Recall Period and Study Site

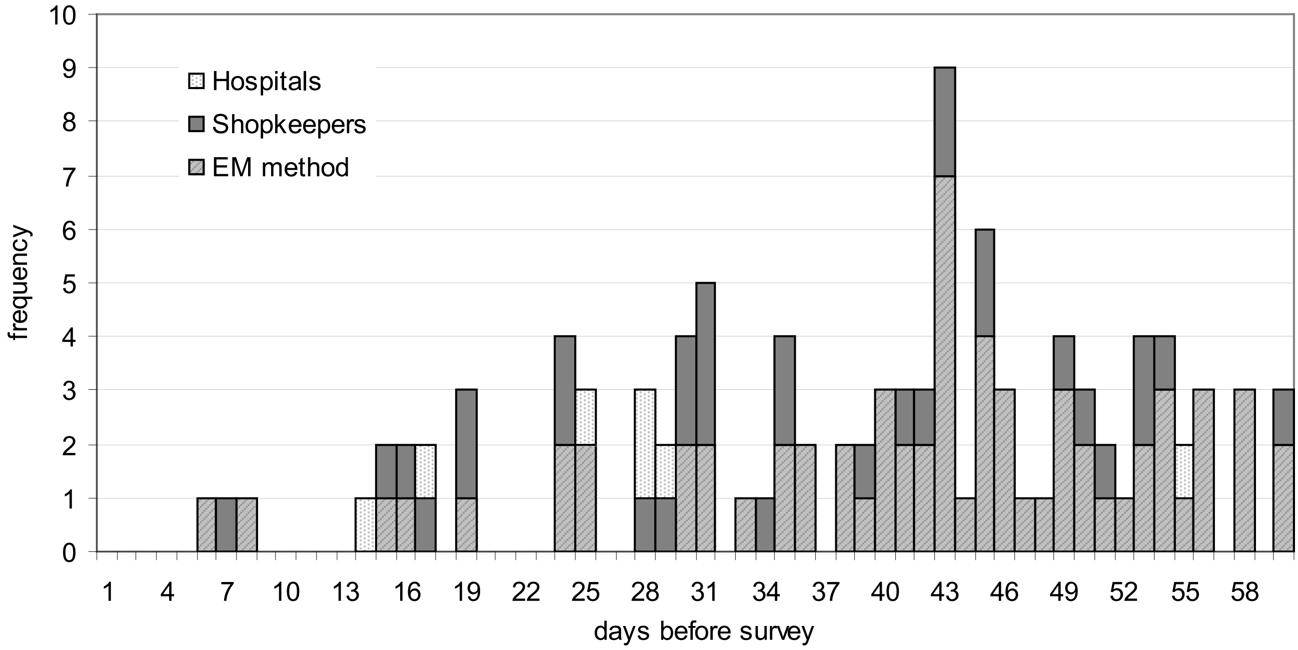
Site	List 1		List 2		List 3		All Lists Combined	
	60 d	30 d	60 d	30 d	60 d	30 d	60 d	30 d
District 1, Kabul	**EM Method**		**Convenience Stores, Bakeries**		**Hospital Records**		**Total**	
Number of deaths	67	11 (16.4% of 60 d)	33	12 (36.3% of 60 d)	8	6 (75.0% of 60 d)	82	18 (22.0% of 60 d)
Median age in years (range)	35 (0-99)	50 (0-99)	50 (0-99)	48 (0-99)	3 (0-36)	1 (0-6)	35 (0-99)	40 (0-99)
Age below 5 years (%)	20 (29.9)	1 (9.1)	9 (27.3)	2 (16.7)	5 (62.5)	4 (66.7)	26 (31.7)	5 (27.8)
Male sex (%)	32 (47.8)	4 (36.4)	16 (48.5)	6 (50.0)	8 (100.0)	6 (100.0)	43 (52.4)	10 (55.6)
Mae La Camp	**EM Method**		**Religious Leaders**		**Surveillance System**		**Total**	
Number of deaths (%)	27	16	23	15 (65.2)	41	18 (43.9)	52	23 (44.2)
Median age in years (range)	55 (1-87)	63 (21-87)	55 (1-85)	63 (45-85)	47 (0-87)	54 (0-87)	49 (0-87)	63 (0-87)
Age below 5 years (%)	2 (7.4)	0 (0.0)	2 (8.7)	0 (0.0)	15 (36.6)	4 (22.2)	16 (30.8)	4 (17.4)
Male sex (%)	15 (55.6)	6 (37.5)	14 (60.9)	8 (53.3)	27 (65.9)	11 (61.1)	33 (63.4)	13 (56.5)
Chiradzulu District	**EM Method**		**Graveyard Chairmen**		**Health Structure Records, HSAs**		**Total**	
Number of deaths (%)	93	37 (39.8)	72	27 (37.5)	44	18 (40.9)	110	43 (39.0)
Median age in years (range)	35 (0-96)	35 (0-85)	34 (0-96)	34 (0-85)	35 (0-96)	29 (0-85)	35 (0-96)	34 (0-85)
Age below 5 years (%)	26 (28.0)	12 (32.4)	20 (27.8)	9 (33.3)	11 (25.0)	7 (38.9)	30 (27.3)	15 (35.7)
Male sex (%)	47 (50.5)	16 (43.2)	34 (47.2)	9 (33.3)	25 (56.8)	9 (50.0)	55 (50.0)	19 (45.2)
Tanzania Camps	**EM Method**		**Camp Register**		**TRCS Surveillance**		**Total**	
Number of deaths (%)	44	21	35	16 (45.7)	29	13 (44.8)	63	28 (44.3)
Median age in years (range)	4 (0-98)	25 (0-98)	2 (0-87)	22 (0-87)	2 (0-78)	27 (0-78)	4 (0-98)	15 (0-98)
Age below 5 years (%)	22 (50.0)	8 (38.1)	17 (48.6)	6 (37.5)	17 (58.6)	4 (30.8)	33 (52.4)	12 (42.9)
Male sex (%)	22 (50.0)	10 (47.6)	22 (62.9)	11 (68.8)	16 (55.2)	9 (69.2)	34 (54.0)	16 (57.1)

The age composition of the three different lists was mostly similar in Chiradzulu and Tanzania; in Kabul, most of the deaths reported by hospitals were among children; in Mae La, children were much more prominent in the surveillance list than in lists provided by the EM method or religious leaders.

Gender ratios were mostly balanced, with the exception of hospital records in Kabul (only male deaths reported); Surveillance systems in Mae La and the Tanzanian camps may also have over-reported males, although this may also reflect gender-specific mortality differentials.

In Kabul, 78% of deaths were reported to have occurred between 30 and 60 days before the survey, contradicting expectations (conventional theory suggests “telescoping” of traumatic events, whereby people report them as having occurred more recently than they actually do; furthermore, recall would be expected to be better in the most recent period). As shown in **Figure 3**, deaths reported by the EM method’s informants are particularly skewed towards earlier dates. Reported deaths peak around 40-45 days before the survey date: in Islamic-majority countries, the 40-day mark is important as it marks the completion of the mourning period; however, in repeated conversations with numerous key informants about this odd timing pattern, no qualitative association between the mourning period and death reporting was mentioned. Some digit preference may also have occurred (deaths appear to be clustered around the 7-, 14- and 30-day mark).

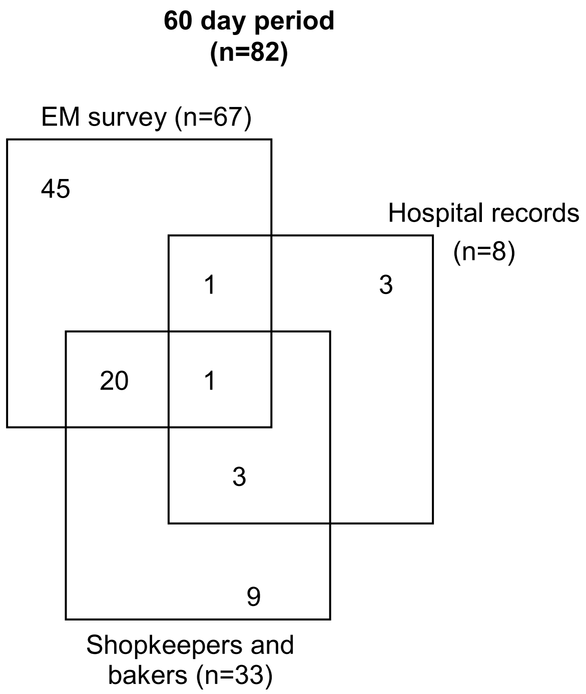
Figure 3. Reported Timing of Deaths Captured in Kabul, by Source



4.4.2. Overlap of Deaths Among Different Lists

Venn diagrams showing the overlap among the three lists, over 60 days and 30 days recall periods, are shown below for each site (**Figure 4**, **Figure 5, Figure 6** and **Figure 7**).

Figure 4. Overlap Among the Three Mortality Lists in District 1, Kabul, by Recall Period



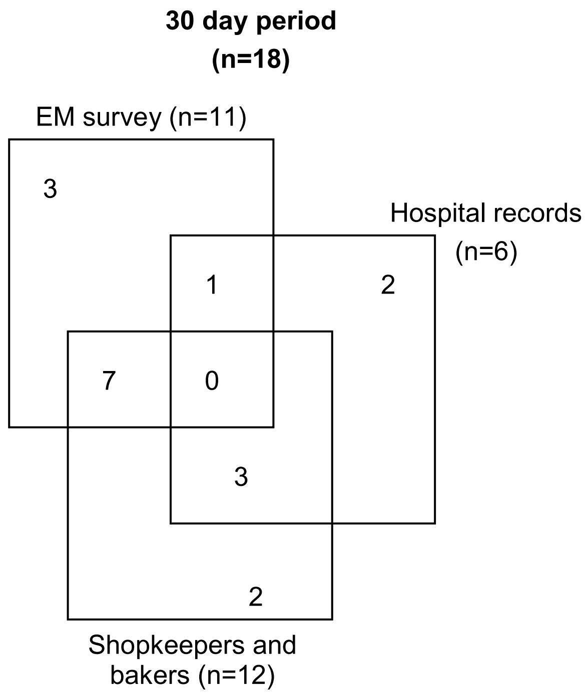
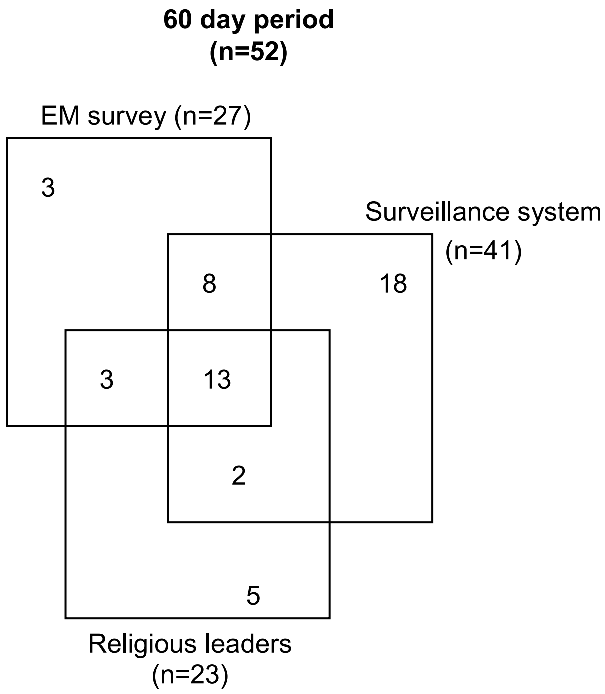


Figure 5. Overlap Among the Three Mortality Lists in Mae La Camp, by Recall Period



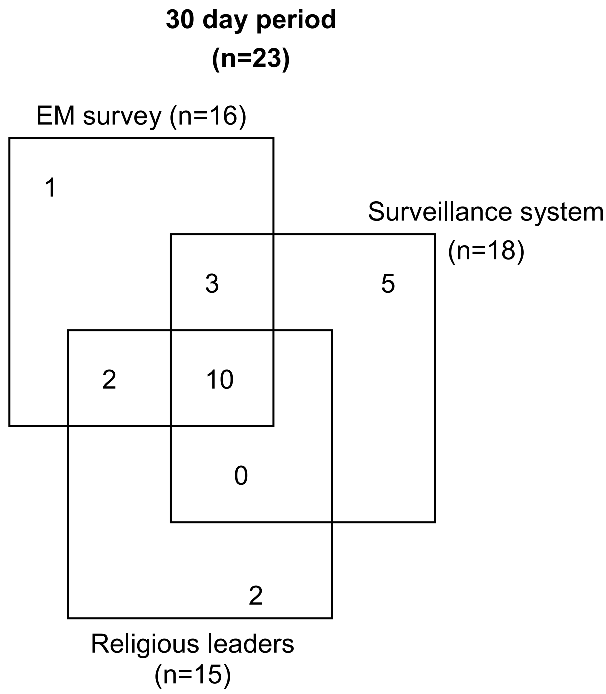


Figure 6. Overlap Among the Three Mortality Lists in Chiradzulu District, by Recall Period

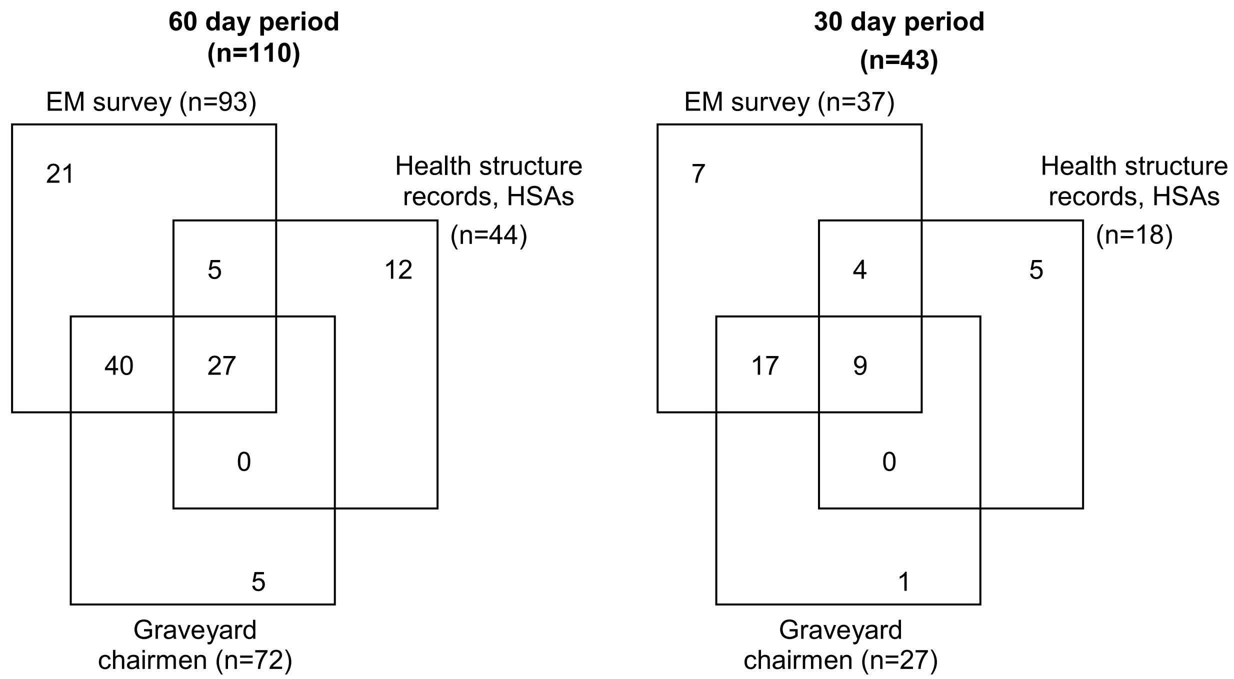
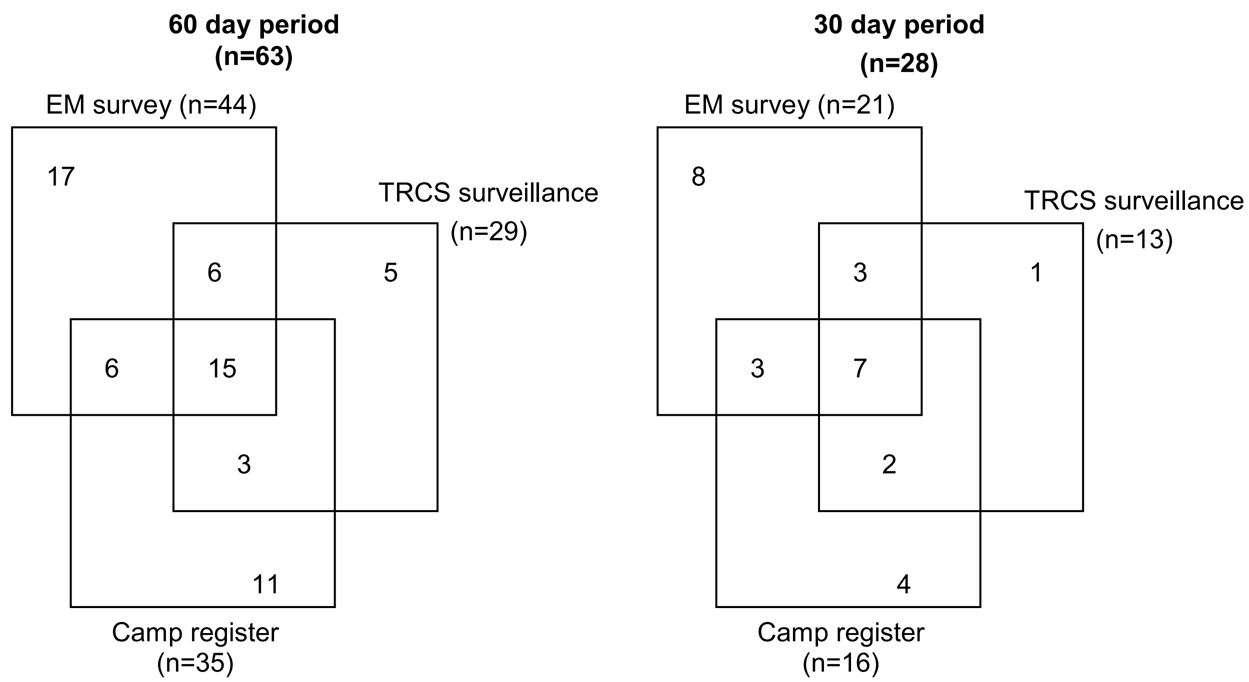


Figure 7. Overlap Among the Three Mortality Lists in Lugufu and Mtabila Camps, by Recall Period



4.4.3. Capture-Recapture Analysis

District 1, Kabul

Table 13 shows various results for possible log-linear models fit to the Kabul data considering a 60 and 30 day recall period. Considering the 60 day period, models 7 and 8 showed evidence of over-fitting and were thus excluded from BMA. The BMA estimated of uncaptured deaths was 25 (95%CI 10-86). Models for the 30 day period suggested a consistently low number of uncaptured deaths.

Table 13. Log-Linear Models and Possible Capture-Recapture Estimates of Uncaptured Deaths in District 1, Kabul, By Recall Period

Model		Model parameters				
	Posterior Probability	Uncaptured Deaths (95%CI)	Degrees of Freedom	Chi-2 P-Value	BIC	Pearson Adjusted Chi-2
60 days recall period						
1. No interactions	0.290	28 (13-55)	3	0.086	-6.62	2.20
2. [EM x shops]	0.041	44 (9-184)	2	0.047	-2.71	3.05
3. [EM x hospitals]	0.494	18 (8-40)	2	0.568	-7.68	0.57
4. [shops x hospitals]	0.070	31 (15-62)	2	0.080	-3.77	2.52
5. [EM x shops] + [EM x hospitals]	0.082	9 (1-54)	1	0.578	-4.10	0.31
6. [EM x shops] + [shops x hospitals]	0.023	135 (17-2761)	1	0.092	-1.57	2.84
7. [EM x hospitals] + [shops x hospitals]	Excluded	20 (8-46)	1	0.999	-4.41	0.00
8. [EM x shops] + [EM x hospitals] + [shops x hospitals] (saturated)	Excluded	10 (1-758)	0	1.000	0.00	Infinite
Bayesian model average		*25 (10-86)*				
30 days recall period						
1. No interactions	0.143	3 (1-10)	3	0.056	-1.11	2.52
2. [EM x shops]	0.050	6 (1-34)	2	0.034	0.99	3.38
3. [EM x hospitals]	0.486	1 (0-6)	2	0.329	-3.56	1.11
4. [shops x hospitals]	0.037	3 (0-11)	2	0.025	1.59	3.69
5. [EM x shops] + [EM x hospitals]	0.116	1 (0-12)	1	0.138	-0.69	2.20
6. [EM x shops] + [shops x hospitals]	0.012	6 (0-149)	1	0.009	3.88	6.77
7. [EM x hospitals] + [shops x hospitals]	0.157	1 (0-5)	1	0.208	-1.30	1.59
8. [EM x shops] + [EM x hospitals] + [shops x hospitals] (saturated)	Excluded	(no convergence)	0	1.000	0.00	Infinite
Bayesian model average		*2 (0-11)*				

Alternative approaches (see **Section 3.4.3**) yielded estimates of 24 uncaptured deaths (combination of the three lists), 26 (EM method vs. shopkeepers), 195 (EM method vs. hospitals) and 29 (shopkeepers vs. hospitals) over a 60 day period. Over a 30 day period, these estimates were 1, 3, 50 and 9 respectively.

Over a 60 day period, most estimates yield around 20-30 uncaptured deaths, comforting the BMA estimates. Over a 30 day period, uncaptured deaths are below 10 in all models with the exception of the simple comparison of the EM method and hospital records (see **Discussion**, **Sections 6.2** and **6.4**).

Among children under 5 years and over a 60 day period, BMA estimated 12 (95%CI 2-91) uncaptured deaths. The low number of child deaths precluded analysis over a 30 day period.

Mae La camp

Over a 60 day period, we estimated 8 uncaptured deaths (**Table 14**) based on BMA. Alternative approaches yielded 12 (all lists combined), 5 (EM method vs. religious leaders), 6 (EM method vs. surveillance) and 14 (religious leaders vs. surveillance) uncaptured deaths.

Over a 30 day period, models were consistent, and alternative approach estimates were 3, 1, 1 and 3 respectively.

Estimates for children under 5 years could not be computed due to the low number of child deaths.

Table 14. Log-Linear Models and Possible Capture-Recapture Estimates of Uncaptured Deaths in Mae La Camp, by Recall Period

Model		Model parameters				
	Posterior Probability	**Uncaptured Deaths (95%CI)**	**Degrees of Freedom**	**Chi-2 P-Value**	**BIC**	**Pearson Adjusted Chi-2**
60 days recall period						
1. No interactions	0.076	5 (2-11)	3	0.002	2.81	4.89
2. [EM x religious]	0.764	9 (4-20)	2	0.047	-1.81	3.05
3. [EM x surveillance]	0.021	8 (3-22)	2	0.001	5.42	6.66
4. [religious x surveillance]	0.019	3 (1-10)	2	0.001	5.54	6.72
5. [EM x religious] + [EM x surveillance]	Excluded	45 (9-328)	1	0.603	-3.68	0.27
6. [EM x religious] + [religious x surv.]	0.117	7 (1-25)	1	0.015	1.95	5.90
7. [EM x surv.] + [religious x surv.]	0.003	5 (1-32)	1	<0.001	8.99	12.94
8. [EM x religious] + [EM x surveillance] + [religious x surveillance] (saturated)	Excluded	73 (6-1035)	0	1.000	0.00	Infinite
Bayesian model average		*8 (4-21)*				
30 days recall period						
1. No interactions	0.155	1 (0-3)	3	0.008	2.43	3.95
2. [EM x religious]	0.611	2 (0-7)	2	0.051	-0.31	2.58
3. [EM x surveillance]	0.054	1 (0-6)	2	0.004	4.55	5.41
4. [religious x surveillance]	0.040	0 (0-3)	2	0.003	5.12	5.70
5. [EM x religious] + [EM x surveillance]	Excluded	(no convergence)	1	0.718	-3.01	0.13
6. [EM x religious] + [religious x surv.]	0.129	2 (0-15)	1	0.015	2.80	5.94
7. [EM x surv.] + [religious x surv.]	0.012	1 (0-14)	1	0.001	7.55	10.69
8. [EM x religious] + [EM x surveillance] + [religious x surveillance] (saturated)	Excluded	(no convergence)	0	1.000	0.00	Infinite
Bayesian model average		*2 (0-9)*				

Chiradzulu District

In Chiradzulu District, two families of models were apparent, some supporting an estimate of very few uncaptured deaths, and others, more supported by the data, a considerable higher figure (**Table 15**).

Non-model approaches yielded estimates of 9 (all lists combined), 2 (EM method vs. graveyards), 23 (EM method vs. health structures and HSAs) and 28 (graveyards vs. health structures and HSAs) considering the 60-day period, and 2, 0, 10 and 16 considering 30 days.

Table 15. Log-Linear Models and Possible Capture-Recapture Estimates of Uncaptured Deaths in Chiradzulu District, by Recall Period

Model		Model parameters				
	Posterior Probability	Uncaptured Deaths (95%CI)	Degrees of Freedom	Chi-2 P-Value	BIC	Pearson Adjusted Chi-2
60 days recall period						
1. No interactions	0.000	5 (3-9)	3	<0.001	20.76	11.62
2. [EM x graveyards]	0.691	25 (12-49)	2	0.018	-1.39	4.01
3. [EM x health sources]	0.000	3 (1-7)	2	<0.001	21.76	15.58
4. [graveyards x health sources]	0.000	5 (2-10)	2	<0.001	25.42	17.41
5. [EM x graveyards] + [EM x health]	Excluded	(no convergence)	1	0.047	-0.77	3.93
6. [EM x graveyards] + [grave. x health]	0.309	50 (17-169)	1	0.027	0.22	4.92
7. [EM x hospitals] + [grave. x health]	0.000	3 (1-7)	1	<0.001	26.27	30.97
8. [EM x graveyards] + [EM x health] + [graveyards x health] (saturated)	Excluded	(no convergence)	0	1.000	0.00	Infinite
Bayesian model average		*33 (13-84)*				
30 days recall period						
1. No interactions	0.037	2 (1-5)	3	0.002	3.48	4.90
2. [EM x graveyards]	Excluded	10 (3-30)	2	0.648	-6.61	0.43
3. [EM x health sources]	0.030	1 (0-3)	2	0.003	3.90	5.69
4. [graveyards x health sources]	0.009	1 (0-4)	2	0.001	6.29	6.88
5. [EM x graveyards] + [EM x health]	Excluded	(no convergence)	1	0.791	-3.67	0.07
6. [EM x graveyards] + [grave. x health]	0.914	12 (2-65)	1	0.362	-2.91	0.83
7. [EM x hospitals] + [grave. x health]	0.009	0 (0-2)	1	0.002	6.27	10.01
8. [EM x graveyards] + [EM x health] + [graveyards x health] (saturated)	Excluded	(no convergence)	0	1.000	0.00	Infinite
Bayesian model average		*11 (2-55)*				

Among children under 5, 9 (95%CI 2-66) uncaptured deaths were estimated by BMA over 60 days. No such estimate could be obtained for a 30 day period due to small numbers.

Lugufu and Mtabila Camps

Over 60 days, 20 uncaptured deaths were estimated in Lugufu and Mtabila camps combined, and only 3 over 30 days (**Table 16**). Non-model approaches yielded roughly comparable estimates of 22 (all lists combined), 15 (EM method vs. camp register), 9 (EM method vs. surveillance) and 10 (camp register vs. surveillance) over 60 days, and 6, 7, 3 and 3 over 30 days.

Table 16. Log-Linear Models and Possible Capture-Recapture Estimates of Uncaptured Deaths in Lugufu and Mtabila Camps, by Recall Period

Model		Model parameters				
	Posterior Probability	Uncaptured Deaths (95%CI)	Degrees of Freedom	Chi-2 P-Value	BIC	Pearson Adjusted Chi-2
60 days recall period						
1. No interactions	0.197	8 (4-16)	3	0.005	0.31	4.25
2. [EM x register]	0.027	7 (2-18)	2	0.002	4.31	6.30
3. [EM x surveillance]	0.116	13 (6-28)	2	0.008	1.38	4.83
4. [register x surveillance]	0.168	12 (5-25)	2	0.012	0.63	4.46
5. [EM x register] + [EM x surveillance]	0.017	18 (4-98)	1	0.002	5.23	9.38
6. [EM x register] + [register x surveil.]	0.022	14 (4-51)	1	0.003	4.67	8.82
7. [EM x surveil.] + [register x surveil.]	0.454	31 (11-99)	1	0.095	-1.36	2.78
8. [EM x register] + [EM x surveil.] + [register x surveillance] (saturated)	Excluded	130 (18-1099)	0	1.000	0.00	Infinite
Bayesian model average		*20 (7-58)*				
30 days recall period						
1. No interactions	0.368	3 (1-7)	3	0.150	-4.68	1.77
2. [EM x register]	0.113	1 (0-7)	2	0.114	-2.31	2.17
3. [EM x surveillance]	0.113	4 (1-13)	2	0.114	-2.32	2.17
4. [register x surveillance]	0.314	4 (1-13)	2	0.316	-4.36	1.15
5. [EM x register] + [EM x surveillance]	0.026	2 (0-25)	1	0.047	0.62	3.96
6. [EM x register] + [register x surveil.]	0.066	3 (0-23)	1	0.150	-1.26	2.07
7. [EM x surveil.] + [register x surveil.]	Excluded	11 (2-61)	1	0.913	-3.32	0.01
8. [EM x register] + [EM x surveil.] + [register x surveillance] (saturated)	Excluded	12 (0-303)	0	1.000	0.00	-Infinite
Bayesian model average		*3 (1-13)*				

Among children under 5, BMA estimated 8 (95%CI 3-28) uncaptured deaths over 60 days, and 5 (95%CI 1-39) over 30 days (note that the latter estimate is higher than that for all deaths over the same period; note also that the estimation in any stratum, such as children, is unconstrained from that in the entire sample and can thus yield discordant results depending on model fit).

4.4.4. Estimated Sensitivity of the EM Method and Other Sources

Sensitivities of the EM method and other sources are presented in **Table 17**, with gold standard provided by the total number of ascertained deaths, plus the estimated number of uncaptured deaths, as computed through BMA.

The EM method showed a consistently modest sensitivity, though in Mae La it was roughly comparable to that of a long-standing active surveillance system over the 30 day period, and higher than both surveillance and camp register sources in Tanzania. While sensitivity over 60 days was expected to be lower due to date misclassification, it was not appreciably higher when considering the main period of interest of 30 days.

Interestingly, the combination of all three sources appeared to achieve good sensitivity in all sites.

Table 17. Estimated Sensitivity of the EM Method, Other Sources and All Lists Combined, by Study Site and Recall Period

	List 1		List 2		List 3		All lists Combined		Total Estimated Deaths
	n	% (95%CI)	n	% (95%CI)	n	% (95%CI)	n	% (95%CI)	N (95%CI)
District 1, Kabul	**EM Method**		**Shopkeepers and Bakers**		**Hospital Records**				
60 days recall	67	62.6 (39.9-72.8)	33	30.8 (19.6-35.9)	8	7.5 (4.8-8.7)	82	76.6 (48.8-89.1)	107 (92-168)
30 days recall	11	55.0 (37.9-61.1)	12	60.0 (41.4-66.7)	6	30.0 (20.7-33.3)	18	90.0 (62.1-100.0)	20 (18-29)
Mae La Camp	**EM Method**		**Religious Leaders**		**Surveillance System**				
60 days recall	27	45.0 (37.0-48.2)	23	38.3 (31.5-41.1)	41	68.3 (56.2-73.2)	52	86.7 (71.2-92.9)	60 (56-73)
30 days recall	16	64.0 (50.0-69.6)	14	56.0 (43.8-60.9)	18	72.0 (56.2-78.3)	23	92.0 (71.9-100.0)	25 (23-32)
Chiradzulu District	**EM Method**		**Graveyard Chairmen**		**Health Structure Records, HSAs**				
60 days recall	93	65.0 (47.9-75.6)	72	50.3 (37.1-58.5)	44	30.8 (22.7-35.8)	110	76.9 (56.7-89.4)	143 (123-194)
30 days recall	37	72.5 (46.8-82.2)	27	52.9 (34.2-60.0)	17	35.3 (22.8-40.0)	42	84.3 (54.4 -95.6)	51 (45-79)
Tanzania Camps	**EM Method**		**Camp Register**		**Surveillance System**				
60 days recall	44	53.0 (36.4-62.9)	35	42.2 (28.9-50.0)	29	34.9 (24.0-41.4)	63	75.9 (52.1-90.0)	83 (70-121)
30 days recall	21	67.7 (51.2-72.4)	16	51.6 (39.0-55.2)	13	41.9 (31.7-44.8)	28	90.3 (68.3-96.6)	31 (29-41)

Among children under 5, the EM method's estimated sensitivity was 20/38 (52.6%, 95%CI 17.1-71.4) in Kabul (60 days period), 26/39 (66.7%, 95%CI 27.1-81.2) in Chiradzulu (60 days), and, in Tanzania, 22/41 (53.7%, 95%CI 36.1-61.1) over 60 days and 8/17 (47.1%, 95%CI 15.7-61.5) over 30 days. Other sensitivity estimates could not be computed due to the low number of child deaths, precluding capture-recapture analysis. However, even considering the total number of ascertained deaths as the minimum denominator, sensitivity was no better than 1/5 (20.0%) in Kabul (30 days), 2/16 (12.5%) and 0/4 (0.0%) in Mae La (60 and 30 days respectively), and 12/15 (80.0%) in Chiradzulu (30 days).

4.4.5. Estimated Mortality Rates

Mortality rates were consistent with non-emergency situations in all sites, and reflected expected patterns given the underlying epidemiological and demographic profile (**Table 18**). Mortality rates among children under 5 were approximately double the all-age CMR, as typically observed in developing country settings.

Table 18. Estimated Crude and Under 5 Years Mortality Rates (as Deaths per 10,000 Person-Days) Based on the EM Method, All Lists Combined and the Capture-Recapture Estimate, by Study Site and Recall Period

	Crude mortality rate (95%CI)			Under 5 years mortality rate (95%CI)		
	EM Method	All Lists Combined	Capture-Recapture	EM Method	All Lists Combined	Capture-Recapture
District 1, Kabul						
60 days recall	0.15 (0.12-0.19)	0.18 (0.15-0.23)	0.24 (0.19-0.34)	0.24 (0.17-0.33)	0.31 (0.22-0.43)	0.49 (0.29-1.30)
30 days recall	0.05 (0.04-0.06)	0.08 (0.07-0.10)	0.09 (0.08-0.12)	0.02 (0.02-0.03)	0.12 (0.09-0.17)	n/a
Mae La Camp						
60 days recall	0.10 (0.09-0.11)	0.20 (0.18-0.22)	0.23 (0.20-0.28)	0.06 (0.06-0.07)	0.49 (0.45-0.55)	n/a
30 days recall	0.12 (0.11-0.13)	0.18 (0.16-0.19)	0.19 (0.17-0.23)	0.00 (0.00-0.00)	0.25 (0.22-0.28)	n/a
Chiradzulu Dist.						
60 days recall	0.30 (0.23-0.39)	0.39 (0.29-0.52)	0.51 (0.38-0.67)	0.54 (0.30-0.93)	0.61 (0.36-1.06)	0.84 (0.48-1.64)
30 days recall	0.26 (0.17-0.39)	0.32 (0.22-0.46)	0.38 (0.25-0.59)	0.55 (0.25-1.31)	0.69 (0.34-1.37)	n/a
Tanzania Camps						
60 days recall	0.09 (0.09-0.10)	0.13 (0.12-0.14)	0.18 (0.15-0.24)	0.23 (0.21-0.24)	0.34 (0.32-0.37)	0.43 (0.38-0.54)
30 days recall	0.09 (0.08-0.09)	0.12 (0.11-0.13)	0.13 (0.12-0.15)	0.17 (0.15-0.18)	0.25 (0.23-0.27)	0.39 (0.30-0.71)

4.5. FEASIBILITY

4.5.1. Economic Feasibility

There was significant variance in the time-inputs among the study sites (**Table 19**). For example, the study in Mae La camp required only 168 person-hours to be completed compared to 2,295 in Chiradzulu District. This variance reflects the different situations in which the study took place.

Comparisons of time-inputs with retrospective surveys are provided in **Table 19**. The EM method appeared to require less time than retrospective surveys in three of the four study sites. The EM method was estimated to require 656 (31%) fewer person-hours in Kabul, 432 (72%) in Mae La camp, and 230 (34%) in the Tanzania camps. These time savings reflect the reduced training, data collection and analysis time required for the EM method in comparison with retrospective surveys. However, in Chiradzulu district, the EM method was estimated to require 561 (32%) *more* person-hours than a survey method. This was mainly due to collecting data for the population estimation, an activity that accounted for 1,003 person-hours.

Respondent time should be treated with caution: opportunity costs for respondents may not be very substantial, as they are unlikely to have had to leave work to participate in the two methods being compared (it should be noted, however, that some survey teams request the population to remain at home during the time when the survey team is due to visit: this is probably unsound from an ethical perspective).

Table 19. Comparison between EM Method and Retrospective Survey in Terms of Time Inputs (Person-Hours), by Activity

Activity/Staff Type	**District 1, Kabul**		**Mae La Camp**		**Chiradzulu District**		**Tanzania Camps**	
	EM	**Survey**	**EM**	**Survey**	**EM**	**Survey**	**EM**	**Survey**
Preparation								
Study investigators	90	90	8	8	45	45	24	24
Data collectors	25	25	4	4	62	62	6	6
Drivers	59	59	0	0	50	50	0	0
Collaborators	25	25	4	4	28	28	29	29
Sub-total	*199*	*199*	*16*	*16*	*185*	*185*	*59*	*59*
Population estimation †								
Study investigators	36	0	0	0	27	0	0	0
Other study staff	6	0	0	0	849	0	0	0
Data collectors	63	0	0	0	68	0	0	0
Respondents	2	0	0	0	1	0	0	0
Drivers	0	0	0	0	58	0	0	*0*
Sub-total	*105*	*0*	*0*	*0*	*1,003*	*0*	*0*	*0*
Focus Group Discussion *								
Study investigators	34	0	9	0	8	0	5	0
Other study staff	22	0	9	0	0	0	10	0
Participants	24	0	67	0	33	0	83	0
Data collectors	87	0	3	0	21	0	15	0
Collaborators	6	0	0	0	0	0	5	0
Drivers	6	0	0	0	5	0	0	0
Sub-total	*179*	*0*	*88*	*0*	*67*	*0*	*118*	*0*
Training								
Study investigators	27	32	2	32	29	32	18	32
Data collectors	146	384	2	192	131	192	53	192
Other study staff	0		2	0	14	0	0	0
Sub-total	*171*	*416*	*6*	*224*	*174*	*224*	*71*	*224*

(continued)

Data collection								
Study investigators	133	78	14	24	120	108	19	24
Other study staff	76	0	3	0	0	0	8	0
Data collectors	402	936	14	144	424	648	57	144
Key informants	58	15	14	11	94	15	47	18
Drivers	119	156	0	0	175	216	30	24
Respondents	22	240	8	120	24	240	16	120
Sub-total	*810*	*1,425*	*53*	*299*	*837*	*1,227*	*177*	*330*
Data entry/analysis								
Study investigators	8	82	1	45	21	82	9	45
Sub-total	*8*	*82*	*1*	45	*21*	*82*	9	*45*
Local report production								
Study investigators	8	16	4	16	8	16	10	16
Sub-total	*8*	*16*	*4*	*16*	*8*	*16*	*10*	*16*
Total person-hours	**1,482**	**2,138**	**168**	**600**	**2,295**	**1,734**	**444**	**674**

Data only for conducting the EM method. Excludes time spent on validating the EM method.
† Population estimation only included if required in study. * FGD includes field-based analysis.

Table 20 presents time inputs by staff type. The variance among study sites reflects the different scenarios noted above. The EM method required substantially less time inputs for data collectors and particularly respondents in all four study sites when compared with surveys. Less time was generally spent by study investigators on the EM method compared with a retrospective survey, with the exception of Kabul where two investigators were present as this was the first site in which the EM method was tested. The ‘other study staff’ category recorded more time input for the EM method than a survey. This was particularly the case in Chiradzulu district due to the time input by village residents hired to perform structure counts. The staff categories of FGD participants and population respondents also had time inputs for the EM method which were not required for the survey method.

Table 20. Time Inputs into the EM Method, by Staff Type and Site, and Comparison with Surveys

Staff	**District 1, Kabul** (person-hours)		**Mae La camp** (person-hours)		**Chiradzulu District** (person-hours)		**Tanzania camps** (person-hours)	
	EM	**Survey**	**EM**	**Survey**	**EM**	**Survey**	**EM**	**Survey**
Study investigators	336	298	38	125	258	283	85	141
Other study staff	104	0	14	0	863	0	18	0
Data collectors	721	1,345	23	340	706	902	131	342
Key-informants	58	15	14	11	94	15	47	18
Drivers	184	215	0	0	288	266	30	24
Collaborators	31	25	4	4	28	28	34	29
FGD participants	24	0	67	0	33	0	83	0
Respondents	22	240	8	120	24	240	16	120
Population respondents	2	0	0	0	1	0	0	0
Total person-hours	**1,482**	**2,138**	**168**	**600**	**2,295**	**1734**	**444**	**674**

Monetary costings (USD) were attached to the staff time inputs in the four study sites (**Table 21**). The EM method was projected to incur less monetary costs than retrospective surveys in all four study sites. The EM method was estimated to cost $1,119 (9%) less than a survey method in Kabul, $3,325 (76%) in Mae La camp, $158 (1%) in Chiradzulu, and $1,818 (41%) in Tanzania.

Table 21. Comparison between EM Method and Retrospective Survey in Terms of Monetary Inputs (USD), by Activity and Study Site

Activity/Staff Type	District 1, Kabul		Mae La Camp		Chiradzulu District		Tanzania Camps	
	EM	**Survey**	**EM**	**Survey**	**EM**	**Survey**	**EM**	**Survey**
Preparation								
Study investigators	2,034	2,034	181	181	1,017	1,017	542	542
Data collectors	94	94	18	18	143	143	19	19
Drivers †	275	275	0	0	1348	1348	0	0
Collaborators	135	135	40	40	160	160	102	102
Sub-total	*2,538*	*2,538*	*239*	*239*	*2,668*	*2,668*	*663*	*663*
Population estimation								
Study investigators	802	0	0	0	610	0	0	0
Other study staff	32	0	0	0	264	0	0	0
Data collectors	243	0	0	0	156	0	0	0
Drivers †	0	0	0	0	1,564	0	0	0
Sub-total	*1,077*	*0*	*0*	*0*	*2,594*	*0*	*0*	*0*
Focus Group Discussion								
Study investigators	768	0	203	0	181	0	113	0
Other study staff	116	0	41	0	0	0	33	0
Data collectors	335	0	14	0	48	0	47	0
Collaborators	32	0	0	0	0	0	18	0
Drivers †	28	0	0	0	135	0	0	0
Sub-total	*1,279*	*0*	*258*	*0*	*364*	*0*	*211*	*0*
Training								
Study investigators	610	723	45	723	655	723	*407*	723
Data collectors	554	1,478	9	864	301	442	164	595
Other study staff	0	0	9	0	0	0	0	0
Sub-total	*1,164*	*2,201*	*63*	*1,587*	*956*	*1,165*	*571*	*1,318*
EM data collection								
Study investigators	3,006	1,763	316	542	2,712	2,441	*429*	542
Other study staff	399	0	14	0	0	0	26	0
Data collectors	1,548	3,604	63	648	975	1,490	177	446
Drivers †	560	735	0	0	4720	5832	105	84
Sub-total	*5,513*	*6,102*	*393*	*1,190*	*8,407*	*9,763*	*737*	*1,072*
EM data entry/analysis								
Study investigators	181	1,844	23	1,012	475	1,844	203	1,012
Sub-total	*181*	*1,844*	*23*	*1,012*	*475*	*1,844*	*203*	*1,012*
Local report production								
Study investigators	181	362	90	362	181	362	226	362
Sub-total	*181*	362	*90*	*362*	*181*	362	226	362
Total cost (USD)	**11,933**	**13,047**	**1,066**	**4,390**	**15,646**	**15,802**	**2,610**	**4,428**

Details on costing calculations provided in Annex 8.11. Excludes time spent on validating the EM method.
† Includes costs of vehicles hire, driver fees, petrol.

Table 22 presents cost inputs by staff type. These reflect the findings from the time-inputs in **Table 20**.

Table 22. Cost Inputs (USD) into the EM Method, by Staff Type and Site, and Comparison with Surveys

Staff Type	District 1, Kabul (USD)		Mae La Camp (USD)		Chiradzulu District (USD)		Tanzania Camps (USD)	
	EM	Survey	EM	Survey	EM	Survey	EM	Survey
Study investigators	7,582	6,726	859	2,820	5,831	6,387	1,921	3,182
Other study staff	546	0	63	0	264	0	59	0
Data collectors	2,774	5,178	104	1,530	1,624	2,075	406	1,060
Drivers †	864	1,013	0	0	7,767	7,182	105	84
Collaborators	167	135	40	40	160	160	119	102
Total	11,933	13,052	1,066	4,390	15,646	15,804	2,610	4,428

Details on costing calculations provided in Annex 8.11. The number of hours worked by each staff type is provided in Table 20.
† Includes costs of vehicles hire, driver fees, petrol.

Table 23 presents the time and cost inputs per death recorded for the four study sites. The time required per death recorded using the EM method varied between 5 hours in the Tanzania camps and 25 hours in Chiradzulu district. This time per death in Chiradzulu district reflects the large distances covered to collect the data, including population estimation data. Time per death is much lower in the densely populated camps in Mae La and Tanzania where existing population data were available. The cost per death using the EM method again reflected the different scenarios of the sites.

Table 23. Comparison between the Four Study Sites for the Time and Cost per Death Recorded

	District 1, Kabul	Mae La Camp	Chiradzulu District	Tanzania Camps
Number of deaths recorded	67	27	93	44
Time (person-hours)	1,482	168	2,295	444
Cost (USD)	11,933	1,065	15,645	2,610
Time per death (hours)	**22**	**6**	**25**	**5**
Cost per death (USD)	**178**	**39**	**168**	**28**

4.5.2. Ethical Implications of the EM Method

The ethical issue of burden to respondent can be explored by comparing the time input by respondents between the EM method and a retrospective survey. The type of information requested from households is similar for the EM and survey method. However, the time input by respondents was considerably less in all four study sites for the EM method than estimated for a survey. In Kabul, the EM method was estimated to require 218 (91%) fewer respondent-hours than a survey method; corresponding figures were 112 (93% fewer) in Mae La camp, 104 (87% fewer) in Chiradzulu, and 216 (90% fewer) in the Tanzania camps.

Other key ethical issues include confidentiality arrangements. Both the EM method and survey involve household collection of data and so face the same challenge of ensuring confidentiality. However, the EM method's reliance on key informants means that other members of the community (the key informants as well as any bystanders who may be involved in the data search) will be aware of interviews taking place in the identified households. We found that it was often difficult to persuade the key informant to not be present during the interview, and thus ensure full confidentiality (on the other hand, the EM method would not result in involuntary disclosure of deaths, since it by definition relies on existing community knowledge). In routine application of the method, the information requested of the household could be very limited (e.g., age, sex), and thus not exceed that already known to the key informant, reducing the risk of disclosure of confidential information to persons other than the study team. However, there is a separate risk that one household could have told another household about a death in confidence, and when the latter household refers interviewers to the former household, the former could feel like their information had been disclosed against their will.

A potential risk of the EM method concerns willingness to take part in the study. The method exploits hierarchical social structures, the dynamics of which may be poorly comprehensible to a naïve investigator. Deference towards and/or fear of authorities may mean that households might be unable to refuse participation in the study, or to decide which information about the decedent they wish to disclose (while introducing bias into estimation, deliberately false answers may be a justifiable way for the household to protect itself against the consequences of sharing certain information with strangers). For example: (i) in a community where leaders must be politically aligned to a given warring faction, key informants relied upon by the EM method might force households to disclose certain kinds of deaths, such as killings perpetrated by an opposing faction; (ii) households who have experienced a stigmatizing HIV/AIDS death might prefer to report the cause of death as malaria, but would be prevented from doing so from fear that their information will be cross-checked with a community authority. During our site validation we did not observe or hear of incidents suggesting any of these dynamics, although we recognize that such delicate issues are difficult to gauge in the short amount of time we spent in each study location. To some extent these issues also arise in standard retrospective surveys, although in the latter the relationship between investigators and respondents is more direct, with less mediation by community authorities.

4.5.3. Feasibility of Adding Verbal Autopsy to EM Method

Verbal autopsy interviews were conducted in the Chiradzulu district site. If a death was recorded by the EM questionnaire interview as taking place within the 30 day recall period, the respondent was asked to also participate in a separate interview for the verbal autopsy questionnaire. This verbal autopsy interview took place in the same location immediately after the EM questionnaire was completed and was conducted by the interviewer trained in administering verbal autopsy questionnaires. Respondents rarely had to wait long to be interviewed for the verbal autopsy questionnaire. The verbal autopsy interviews also did not impede the data collection for the EM method.

Each interview for the verbal autopsy questionnaire took an average of 38 minutes, and 56 verbal autopsy questionnaires were completed. The analysis was conducted after the data collection exercise by the verbal autopsy interviewer and the MSF-F doctor. Each verbal autopsy questionnaire took an average of 44 minutes to analyze (includes time input by both questionnaire reviewers and in them reaching agreement). The total time to conduct the verbal autopsy component was therefore 76.5 person-hours. The results from this study suggest that the additional time required for the verbal autopsy was feasible given the reduced time required for conducting the EM method when compared to a retrospective survey.

5. Mathematical Simulation

The following simulation results apply only to the routine application of the EM method (however, measuring mortality based on sources other than the EM exhaustive search would be expected to entail similar statistical requirements). Simulations were performed mainly to (i) recommend an amount of person-time investigated sufficient to minimize the influence of stochastic error, under various assumptions about the method's implementation and the underlying mortality levels; and (ii) guide inference based on the method's findings. A fundamental assumption about the properties of the typical distribution of deaths over any discrete time unit underpins both of the above: we first sought to verify this assumption.

5.1. DISTRIBUTION OF DEATH COUNTS IN THE TIME DIMENSION

To guide further simulation work, we first sought to verify whether mortality is distributed over time according to the classical Poisson assumption (Poisson distributions are generally very appropriate for rare events such as deaths). We obtained six existing datasets of death counts from emergency prospective mortality surveillance systems implemented among camp-residing displaced populations by Epicentre and Médecins Sans Frontières France in Murnei, Niertiti, and Zalingei (West Darfur, 2004), and by the UNHCR in Lugufu and Mtabila (Tanzania, 2008) and Fugnido (Ethiopia, 2007-2008). Details of the datasets are provided in **Table 24**.

Table 24. Details of Six Mortality Surveillance Datasets Used to Study the Distribution of Death Counts Over Time

Dataset	Implementing Agency	Time Unit	Time Span (Duration)	Average Population Under Surveillance
Fugnido, Ethiopia	UNHCR	month	January 2007-May 2008 (17 months)	21,700
Lugufu, Tanzania	UNHCR	month	January 2006-May 2008 (29 months)	50,300
Mtabila, Tanzania	UNHCR	month	January 2006-May 2008 (29 months)	20,600
Murnei, West Darfur	Epicentre / MSF-F	week	January-August 2004 (30 weeks)	65,800
Niertiti, West Darfur	Epicentre / MSF-F	week	May-September 2004 (16 weeks)	16,600
Zalingei, West Darfur	Epicentre / MSF-F	week	April-October 2004 (27 weeks)	52,300

We first standardized counts to deaths per 10,000 person-weeks (Darfur) or person-months (Tanzania, Ethiopia) by dividing them by the population reported under surveillance. We then de-trended each time series by considering only the residuals from a simple ordinary least-squares model (i.e., after fitting a straight line to the time series, we worked only with the errors between observations and fitted values). Finally, we tested by maximum likelihood the goodness of fit of a Poisson distribution applied to each time series of residuals.

The goodness of fit was moderate, probably due to short duration of the time series (**Table 25**). On balance, however, there was little evidence to reject the Poisson assumption, and this was further evidenced graphically (see **Annex 8.13**).[2] Based on this analysis, we retained the Poisson assumption for further simulation work aiming to guide determination of minimum person-time to be investigated and inference decisions for the method.

[2] The smaller the p-value, the smaller the chance that the Poisson distribution fits the observed data by chance alone, i.e. the better the fit of the Poisson distribution.

Table 25. Goodness of Fit of a Poisson Model Applied to Six Mortality Surveillance Datasets

Dataset	Likelihood Ratio	Degrees of Freedom	P-Value †
Fugnido, Ethiopia	2.089	1	0.148
Lugufu, Tanzania	4.162	2	0.125
Mtabila, Tanzania	3.055	4	0.549
Murnei, West Darfur	10.290	5	0.067
Niertiti, West Darfur	7.274	4	0.122
Zalingei, West Darfur	8.565	4	0.073

† The smaller the p-value, the smaller the chance that the Poisson distribution fits the observed data by chance alone, i.e. the better the fit of the Poisson distribution.

5.2. SIMULATION TO DETERMINE PERSON-TIME REQUIREMENTS FOR THE EM METHOD

We used R freeshare software to implement mathematical simulations designed to inform minimum person-time requirements for the EM method. The simulations assumed a true mortality rate, and computed the probability of classifying the mortality as being equal to or above a threshold of interest set by the investigator, given the true mortality rate and various assumptions about the amount of person-time investigated.

5.2.1. Scenarios for Implementation of the EM Method

We considered two scenarios in which the EM method might be implemented routinely:

- An *exhaustive scenario* in which the entire population is surveyed: this would occur when the population is relatively small (e.g., less than 100,000) and/or regimented, thus reducing travel distances and times, as in Mae La, Kabul, Lugufu and Mtabila;

- A *sampling scenario* in which a representative sample of the entire population is selected, and the method is applied therein, with projection of the findings to the entire sampling universe; this would be the expected choice for a large and/or very scattered population, as in Chiradzulu District, where it would have been unfeasible to survey the more than 500 villages comprising the district's population.

5.2.2. Classification Tests

We considered that a user of the method would wish to classify mortality in the surveyed population as equal to or above a given threshold of interest (e.g., ≥1 per 10,000 person-days) with high sensitivity (i.e. high probability of classifying true crisis situations as crises) and acceptable specificity (i.e. relatively low probability of misclassifying non-crisis situations as crises and thereby spending resources on a partially unnecessary relief operation). Accordingly, we defined the null hypothesis as "the true mortality rate is lower than the threshold of interest", and applied to this hypothesis the following three alternative tests:

- A *Poisson test*, whereby the probability of finding less than the observed number of deaths was computed based on a Poisson probability distribution; this distribution was centered around the expected number of deaths given the threshold rate of interest and the investigated person-time (population x recall period). If the test probability is lower than the specified alpha level, the null hypothesis is rejected and mortality is classified as equal to or greater than the threshold;

- A *sequential sampling test*, consisting of a slight modification of the above; here, the minimum number of observed deaths that would lead to a rejection of the null hypothesis at the specified alpha level is computed ahead of data collection. If this minimum is exceeded, mortality is classified as equal to or greater than the threshold. Note that this approach allows for early survey stopping and thus potential resource and time savings (This option was not explored further);

- A *CI test*, whereby the mortality rate point estimate and CI at the specified alpha level are computed using exact methods: if the point estimate is higher than the threshold and the CI also does not include the threshold, mortality is classified as equal to or greater than the threshold.

In practice, rejecting the null hypothesis would lead to a recommendation to deploy health interventions appropriate for a crisis situation as defined by the threshold of interest (e.g. if the test classifies mortality as being ≥5 deaths per 10,000 person-days, one would conclude conditions are catastrophic, warranting exceptional and urgent intervention). Failure to reject the null hypothesis merely indicates that the threshold of interest is not exceeded.

5.2.3. Expression of Person-Time Requirements

Throughout this document, we express mortality as deaths per 10,000 person-days.

Person-time sample size for a mortality survey is composed of the number of people being investigated, and the recall period over which deaths are ascertained; it varies according to whether the indicator of interest is CMR or U5MR. U5MR is sometimes recommended as a more sensitive and timely crisis indicator, but there are equally compelling reasons to monitor CMR. Simulating different population sizes, recall periods and choices of threshold mortality rates under two scenarios (exhaustive and sampling) whilst also considering two possible indicators (CMR or U5MR) would have entailed a large amount of partly redundant simulations, difficult to summarize graphically. Instead, we merely considered minimum person-time requirements in terms of (i) person-time to be investigated and (ii) the threshold of interest. For example, a recommendation to survey at least 500,000 person-days if one wishes to accurately classify mortality according to a threshold of 1 death per 10,000 person-days would mean that the user of the method could choose to survey 50,000 people over a 10 day recall period, 10,000 people over 50 days, 100,000 people over 5 days, etc.

If the survey primarily aims to measure U5MR, the threshold would usually be doubled (see Sphere Project guidelines[1]), and a sufficiently large population and/or recall period should be sampled to capture the minimum recommended person-time under 5 years.

5.2.4. Parameter Values for Simulation

Table 26 lists the chosen values for the simulation's parameters. In general, we decided to privilege sensitivity (probability that the survey will correctly classify mortality as being equal to or above the threshold of interest when indeed it is) over specificity (probability that the survey will correctly classify mortality as being below the threshold when indeed it is), i.e. minimize the risk of overlooking a true crisis rather than the risk of needlessly expending resources. To this end, after preliminary simulations (results omitted for brevity's sake) we settled upon an unconventionally large alpha level of 0.40 (**Table 26**), meaning an inherent acceptance of up to 40% risk that the survey's classification of mortality as being equal to or greater than the threshold of interest might be the result of chance alone.

Exhaustive Scenario

In the exhaustive scenario, we kept population size constant at 100,000, and evaluated different amounts of person-time investigated by varying the recall period. We assumed that the population size would ordinarily be subject to error (being the sum of sampling and non-sampling errors), normally distributed around the true population.

Sampling Scenario

In the sampling scenario, we assumed that the population would be divided into PSUs such as villages, and that a spatial sampling design would be adopted, such as the CSAS (see sampling design for the Chiradzulu District survey). Further, we assumed that the total population size of the sampled PSUs would be estimated using the following method: (i) residential structure count within each PSU (featuring an error of ±3.5%), combined with (ii) systematic sampling of every nth structure counted to estimate the number of people per structure (error ±7.5%).

Because of long computation times, we were restricted to only a few simulations: (i) we assumed that the sampling universe would consist of approximately 100,000 people split among 300 villages; (ii) we evaluated three possible numbers of PSUs sampled, 30 being the minimum considered statistically robust; and (iii) we looked at a single recall period of 30 days.

We performed multiple iterations of each simulation to incorporate uncertainty in the population estimates and, in the sampling scenario, random selection of PSUs and variability in PSU population size. In each iteration, we sampled randomly from each uncertainty distribution to compute the three test statistics. Results presented below are the median of 5,000 iteration results.

Table 26. Parameter Values for the Exhaustive and Sampling Scenario Simulations

Parameter	Value(s)	Notes / Assumptions
Parameters specific to the exhaustive scenario		
Population	100 000	
Error in population estimate	Standard deviation = ±7.5%	Normally distributed around the true population; based on expert consensus about typical magnitude of error†
Recall period	1, 5, 10, 20, 30, 50, 100 days	
Parameters specific to the sampling scenario		
Number of PSUs (villages) in sampling universe	300	
Number of PSUs sampled	30, 45, 60	
PSU population	Sampled randomly from a log-normal distribution with median = 300; minimum = 150; maximum = 5000; ln(standard deviation) = 3	Yields a population of about 100 000, given 300 villages
Error in estimation of mean residential structure size	±3.5%	Systematic error; applies to all sampled PSUs combined; normally distributed
Error in estimation of number of residential structures	±7.5%	Random error; applies to each PSU individually; normally distributed
Recall period	28 days	
Parameters common to both scenarios		
Threshold mortality rate of interest	0.5, 1, 2, 4, 8 per 10 000 person-days	
True mortality rate	Range = [threshold rate/2, threshold rate x 2] Unit increment = range/20	Unit increment chosen to provide sufficient resolution
Alpha	0.40 (i.e. 60%CI)	See text
Number of iterations for each simulation (combination of recall period, threshold and true mortality rates)	5000	Only median of distribution of iterations is reported
† http://documents.wfp.org/stellent/groups/public/documents/ena/wfp111670.pdf		

5.2.5. Simulation Results: Exhaustive Scenario

Figure 8, Figure 9, Figure 10, **Figure 11** and **Figure 12** show operating characteristic (OC) curves summarizing the performance of the three alternative tests over increasing person-time sampled (as person-days) for different threshold mortality rates of interest. Summary sensitivity and specificity results are shown in **Annex 14**.

The OC curves show the probability that an EM survey will classify mortality as equal to or above the threshold (y axis), as a function of the true underlying mortality rate (x axis), for a given amount of person-days sampled and threshold of interest. The steeper the curve, the smaller the probability of both false negatives and false positives, and thus the higher both sensitivity and specificity.

As expected, OC curves become steeper as person-time and/or the underlying mortality rate increase. For any mortality threshold, OC curves became appreciably steep around 1,000,000 person-days sampled: this amount of person-time investigated is associated with sensitivity no worse than 90%, with consistently high specificity (>95%). Sensitivity deteriorates rapidly below about 500,000 person-days.

The three alternative tests behave in a similar fashion. However, the Poisson test is consistently more conservative than the others.

On balance, this simulation suggests that the EM method, if implemented according to the exhaustive scenario, should attempt to survey no less than 1,000,000 person-days: this is equivalent to 100,000 people with a 10 day recall period, 50,000 with 20 days, or 25,000 with 40 days.

5.2.6. Simulation Results: Sampling Scenario

Figure 13, **Figure 14**, **Figure 15**, **Figure 16** and **Figure 17** show OC curves for increasing amounts of person-time (number of PSUs) sampled and different mortality thresholds of interest, under the sampling scenario.

All three choices of person-time perform reasonably, though the amount of classification error at low mortality rates is probably unacceptable. For this reason, we recommend a simple rule of sampling at least 30 PSUs (this is also advisable on statistical robustness grounds), and that the PSUs sampled comprise a total minimum population of at least 20,000 people. Clearly, sampling more PSUs would be preferable, given the same number of people surveyed, as it would reduce design effects. In our simulation we did not explore the effect of high design effect, as one might expect after an epidemic or acts of war.

Figure 8. Simulation Results for the Exhaustive Scenario (Threshold: 0.5 per 10,000 Person-Days)

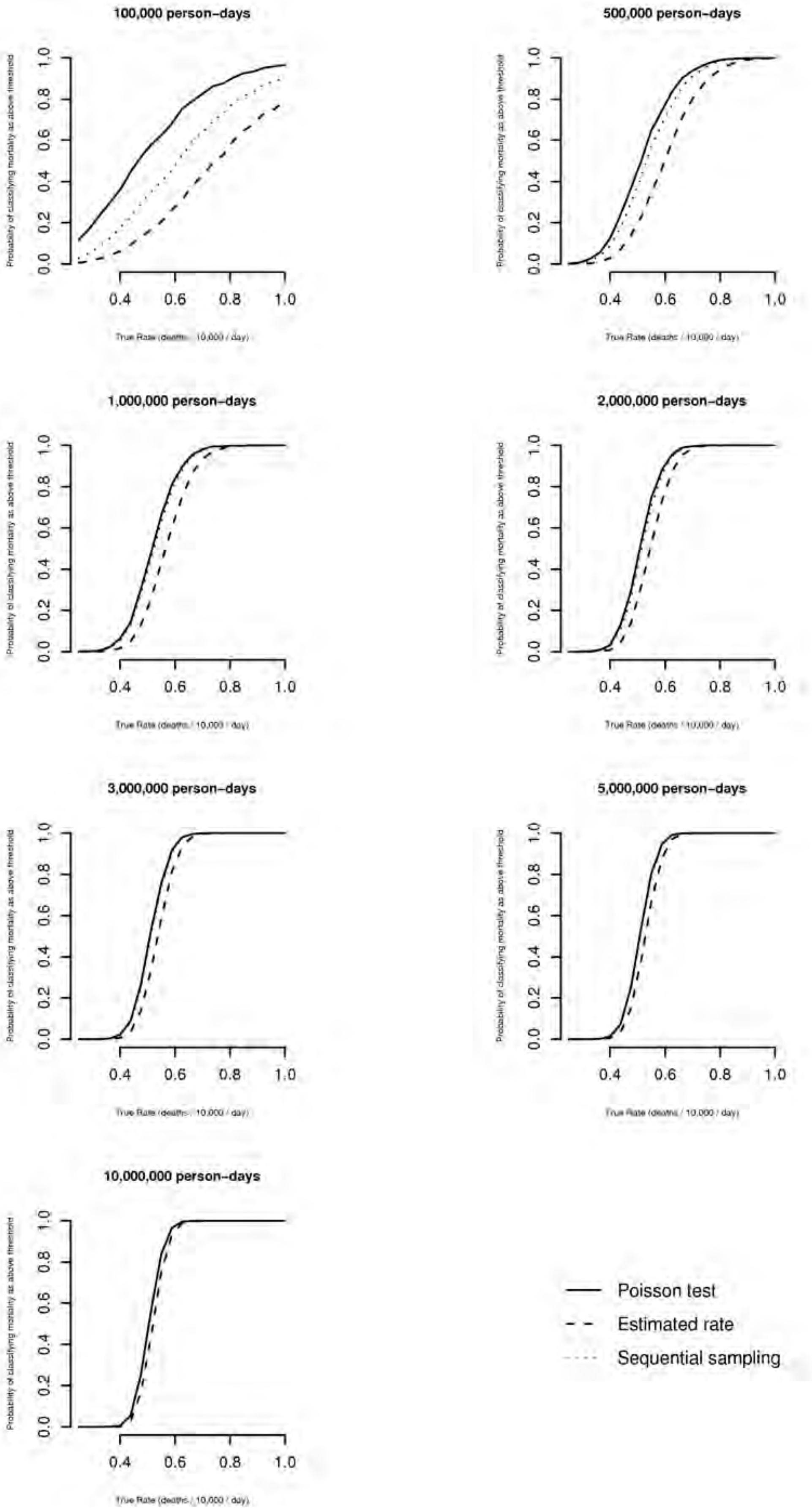


Figure 9. Simulation Results for the Exhaustive Scenario (Threshold: 1 per 10,000 Person-Days)

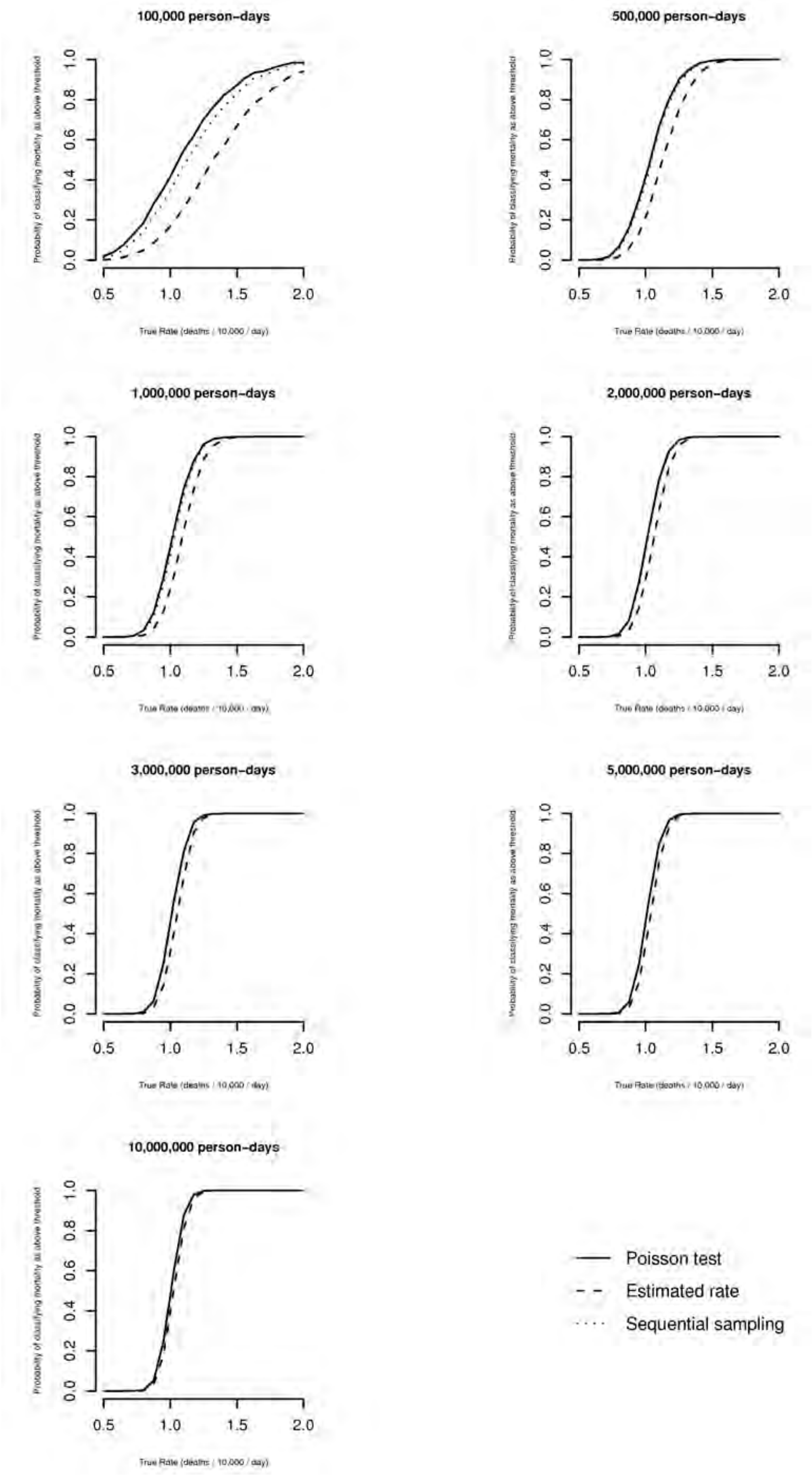


Figure 10. Simulation Results for the Exhaustive Scenario (Threshold: 2 per 10,000 Person-Days)

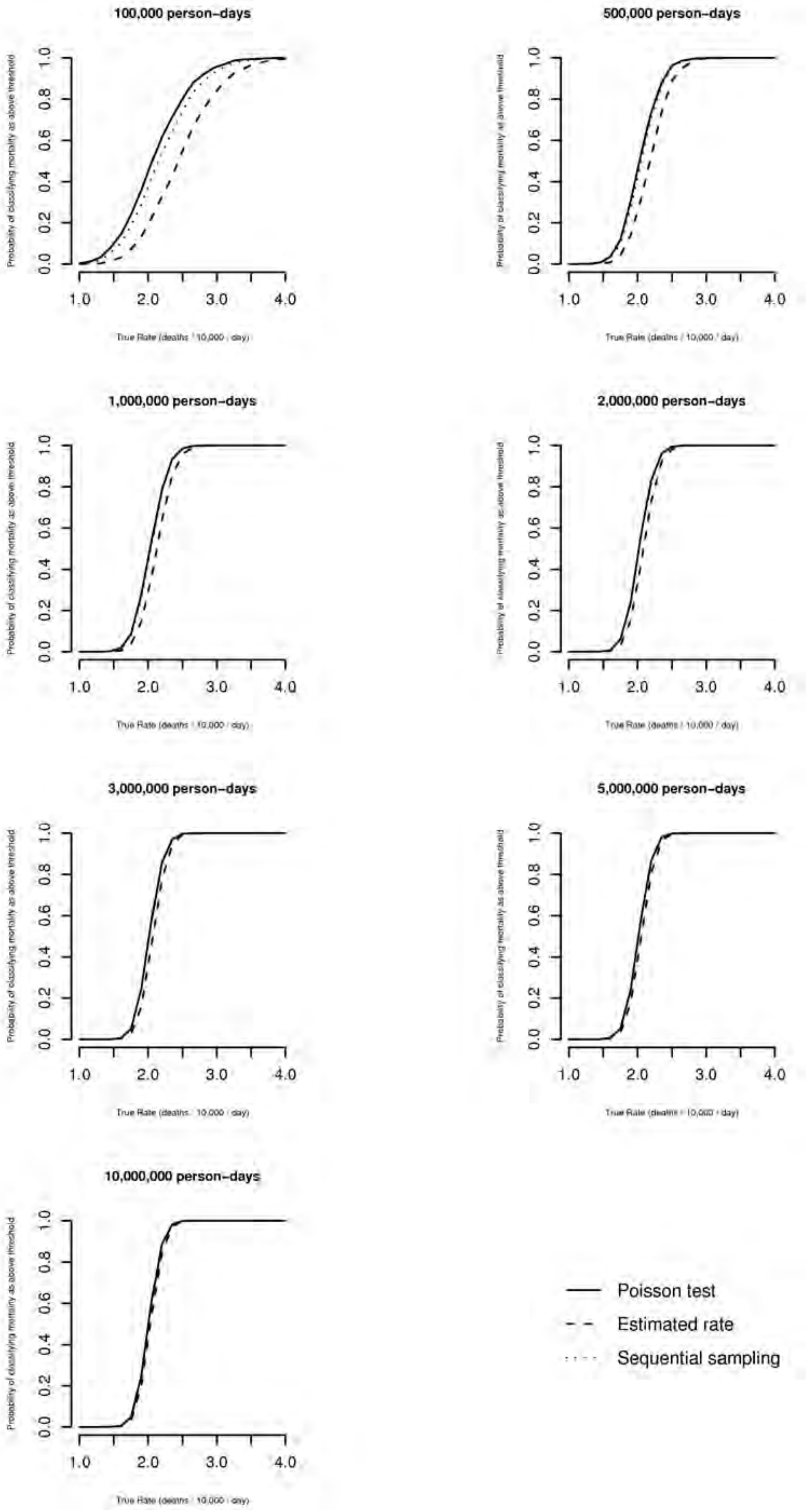


Figure 11. Simulation Results for the Exhaustive Scenario (Threshold: 4 per 10,000 Person-Days)

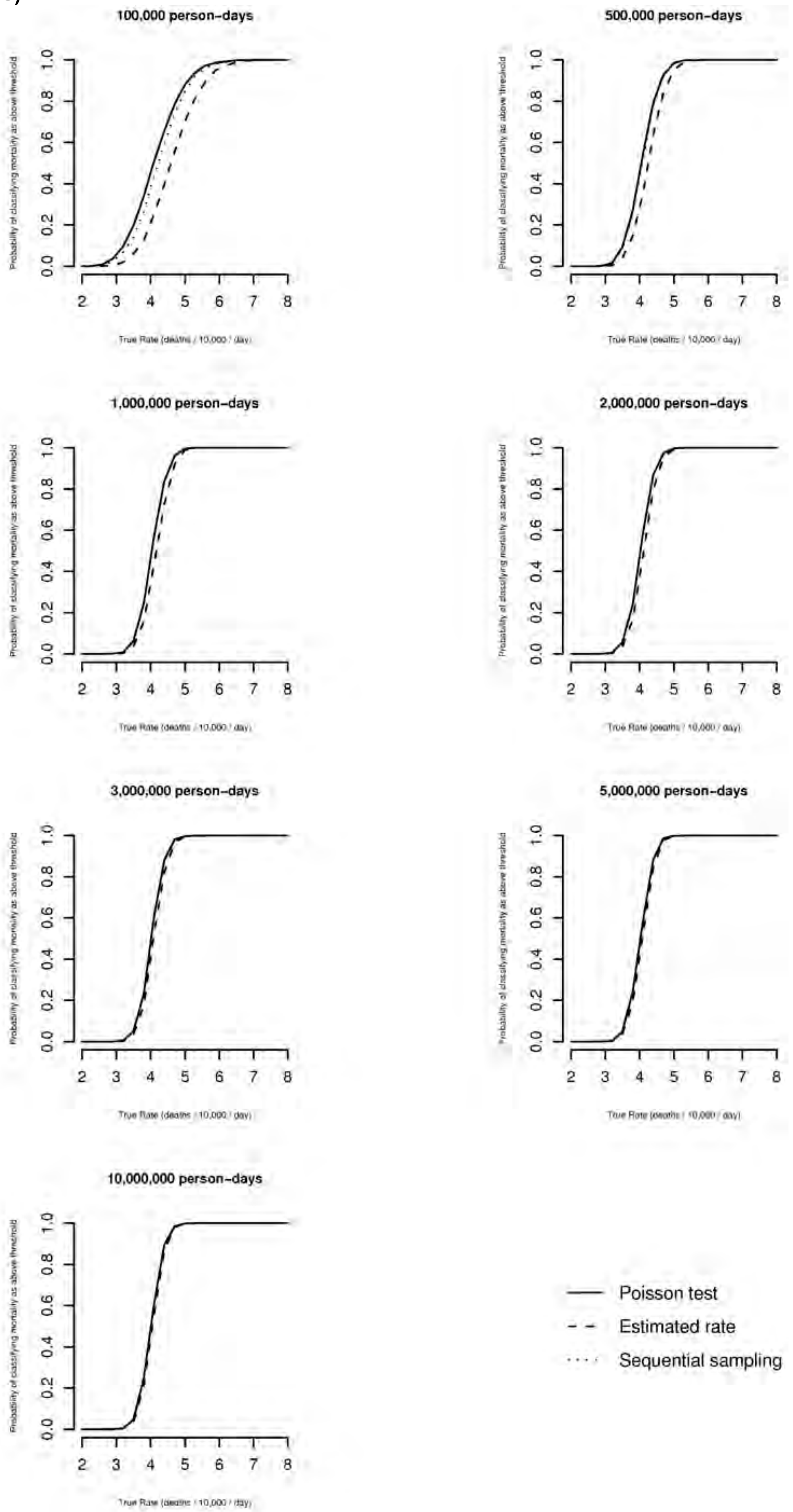


Figure 12. Simulation Results for the Exhaustive Scenario (Threshold: 8 per 10,000 Person-Days)

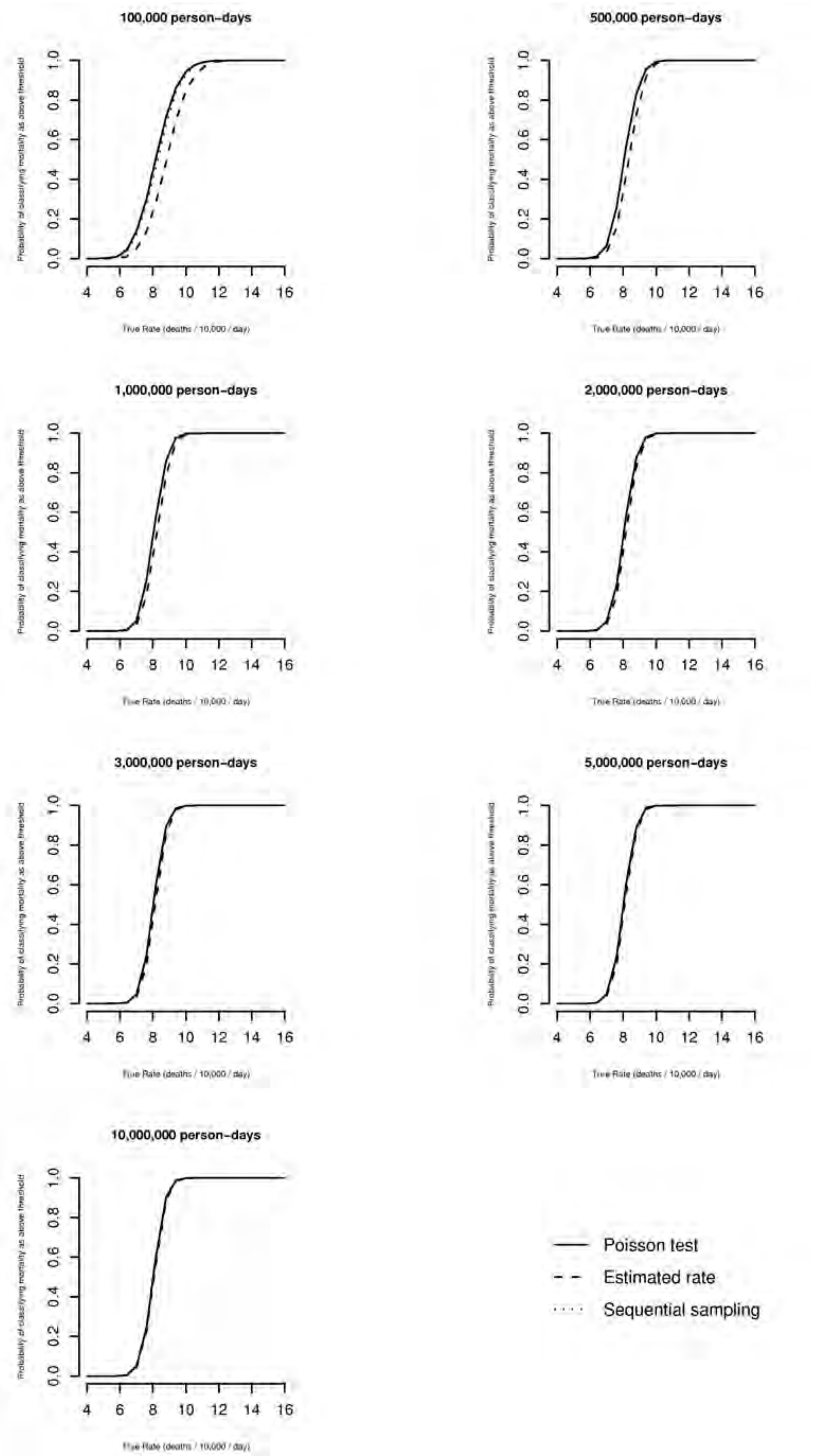


Figure 13. Simulation Results for the Sampling Scenario (Threshold: 0.5 per 10,000 Person-Days)

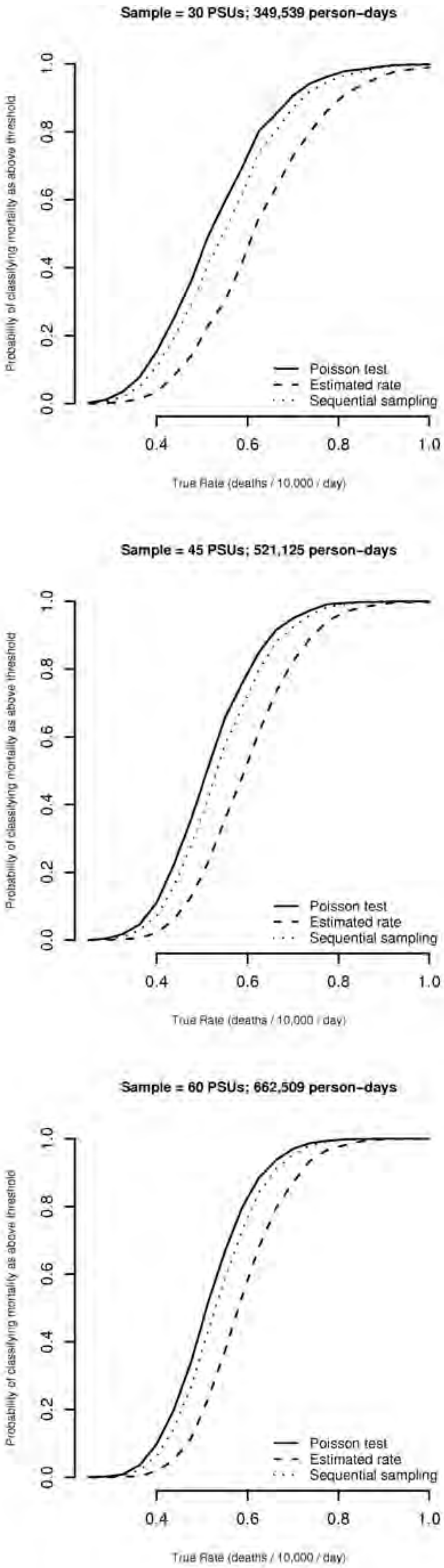


Figure 14. Simulation Results for the Sampling Scenario (Threshold: 1 per 10,000 Person-Days)

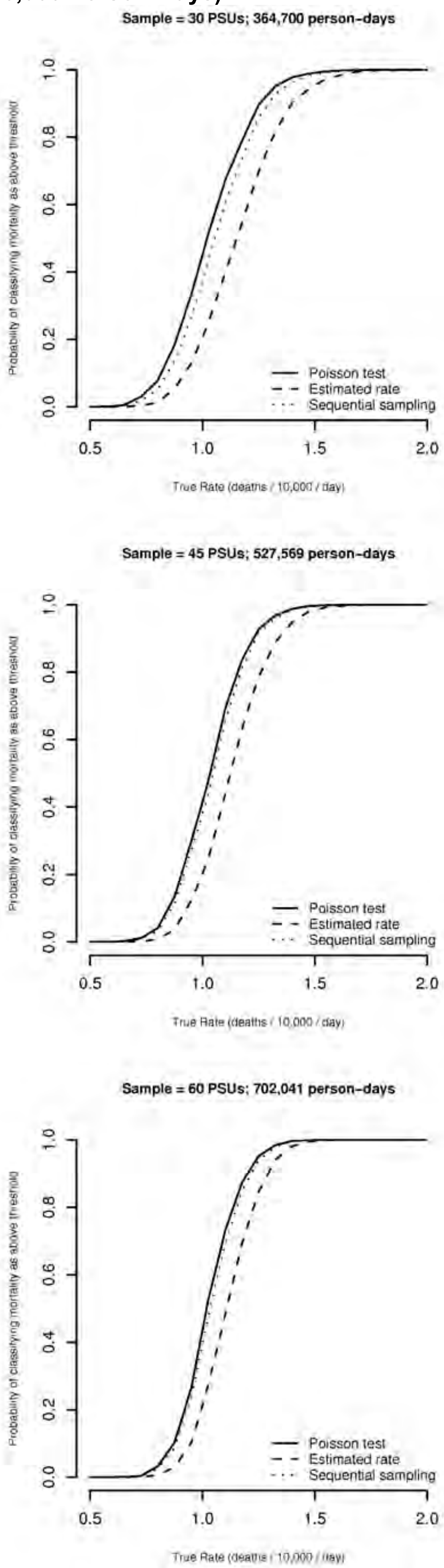


Figure 15. Simulation Results for the Sampling Scenario (Threshold: 2 per 10,000 Person-Days)

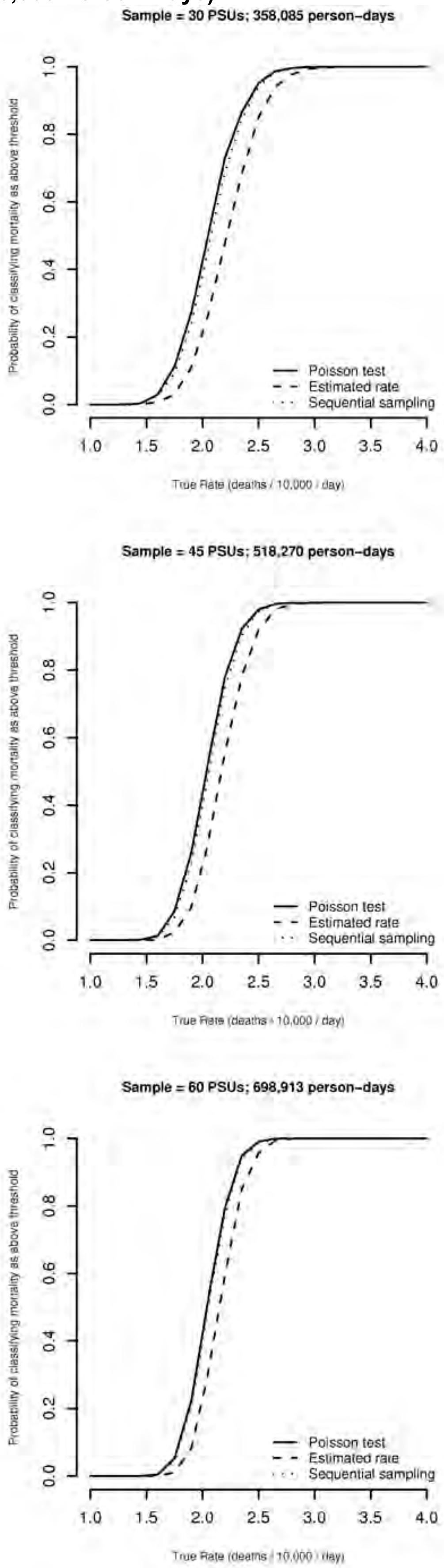


Figure 16. Simulation results for the Sampling Scenario (Threshold: 4 per 10,000 Person-Days)

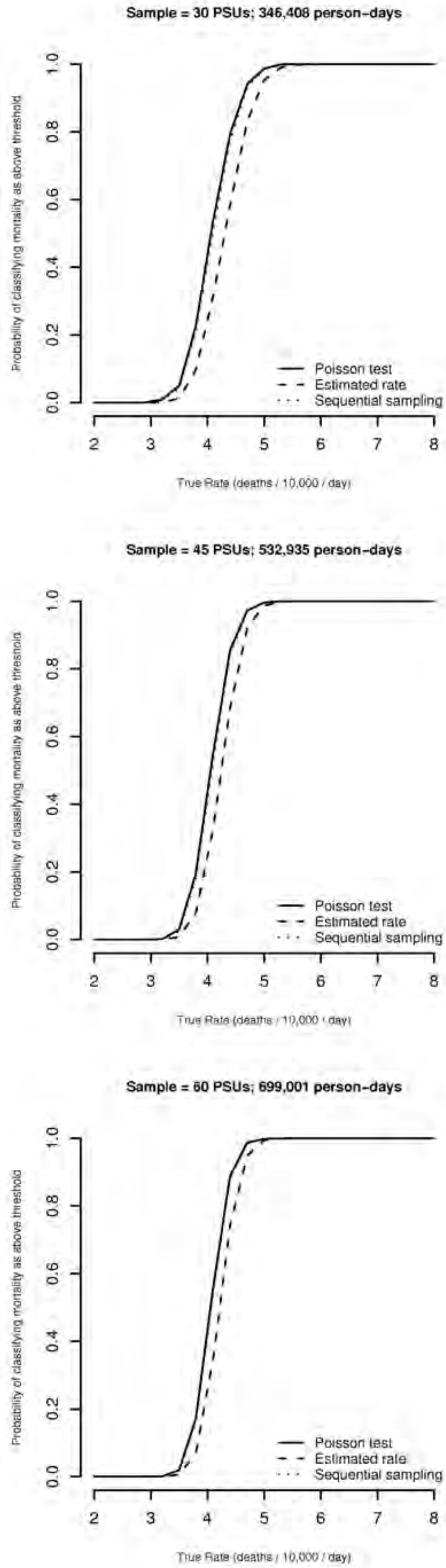
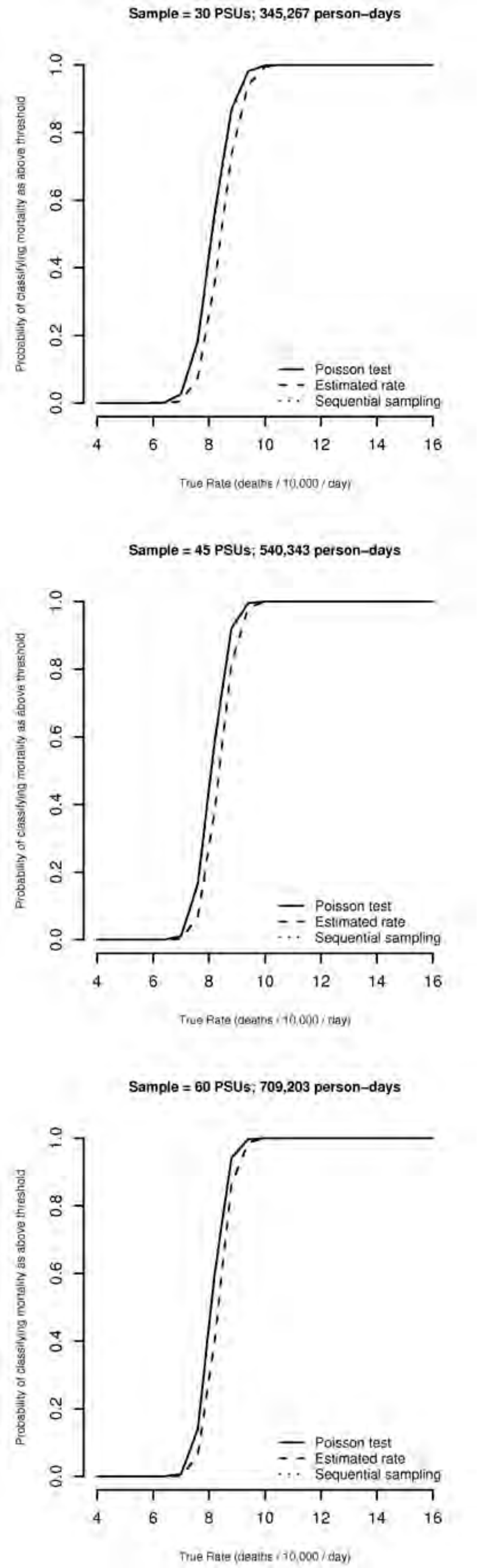


Figure 17. Simulation Results for the Sampling Scenario (Threshold: 8 per 10,000 Person-Days)



5.3. CALCULATORS TO GUIDE INFERENCE DECISIONS BASED ON THE METHOD

Because the Poisson test proved to be the most conservative (i.e. maximized sensitivity), we adopted it for inference decisions. Accordingly, we developed an open-access HTML calculator, applicable to the exhaustive scenario detailed above, that classifies mortality according to a set threshold of interest, given the population size and recall period investigated. The calculator is available at http://www.fantaproject.org/publications/EM_method.shtml, and attached to the electronic version of this report as an offline HTML page. Users are given the option to specify an alpha level, depending on whether they would like the classification to privilege sensitivity or specificity.

We also developed a calculator that computes the 95%CI for the mortality rate estimated by the EM method. The CI is based on a Poisson assumption, to provide users with a somewhat more familiar estimation result (http://www.fantaproject.org/publications/EM_method.shtml). Note that this CI is *not* a reflection of error arising from sampling a portion of the population (in the exhaustive scenario, the entire population is surveyed). Rather, it indicates the degree to which chance could have affected the observed mortality rate: as person-time investigated decreases, the CI widens.

Due to time constraints we did not develop similar calculators for the sampling scenario: it should be noted that the analysis of EM surveys done in a sample of PSUs will depend heavily on the sampling design (e.g., a different number of PSUs may be sampled; weights may be needed if the sample is not self-weighting, as in CSAS; standard errors may need adjustment for design effect), and that, given that the numerator and denominator arise from different sampling processes, a bootstrapping method (see analysis of Chiradzulu survey: **Section 3.3.7**) is probably best. These complexities are difficult to incorporate in a single calculator. However, we believe that, in the majority of scenarios, failure to adjust for non-self-weighting samples and possible clustering of deaths will result in only mild error (sampling designs requiring heavy weighting or surveys with high design effects are on the whole infrequent, with most measured design effects in mortality surveys being relatively small, i.e. <2 [24]). Thus, the calculators above should perform reasonably even if applied to the sampling scenario.

6. Discussion

6.1. OVERALL IMPORTANCE OF STUDY

This study presents findings on the validity and feasibility of a new method to estimate mortality in crisis-affected populations. To our knowledge this is the only recent study that aimed to develop a new mortality estimation method based on primary data collection, and targeted to field users with limited epidemiological and statistical skills.

Importantly, our study evaluated the proposed EM method in different settings and populations, enhancing the generalizability of our findings. We sought to identify and implement a rigorous gold standard measure (capture-recapture analysis) for the validation. Corollary mathematical simulation work explored the statistical foundations of the method, and developed recommendations and online calculators for study design and inference.

Below we discuss validity and feasibility findings, appraise the limitations of this study, and suggest future steps for the development of the method.

6.2. VALIDITY

The EM method showed consistently modest sensitivity in the four study sites. While sensitivity over 60 days was expected to be lower due to date misclassification, it was not appreciably higher when considering the main period of interest of 30 days, and below 73% in all sites. The method compared favorably with existing surveillance systems in Tanzania, but was slightly less sensitive than surveillance in Mae La.

Sensitivity was particularly disappointing among children under 5 years, with the partial exception of Chiradzulu. In Mae La, the method captured no more than 13% of child deaths (in this camp, the surveillance system is favored over other methods as it includes all health centers, where nearly all child deaths are reported to take place). This poor performance clearly precludes utilization of the method, as currently devised, for measurement of mortality under 5 years.

Comparisons cannot easily be made with retrospective household surveys, as none were performed in the study populations during a comparable period; furthermore, surprisingly little is known about the validity of standard survey methods as put forth by the SMART initiative and similar agency guidelines. While the EM method appears to miss a considerable proportion of deaths, it is difficult to say whether it misses more than a standard survey would, if performed in the same population.

What explains the moderate sensitivity of the EM method? Qualitative observations as well as theoretical reasoning suggest the following main explanations:

- **The choice of key informants was sub-optimal:**
    - The FGDs were consistently attended by community leaders, who were the majority of participants in Mae La, Kabul, and Tanzania camps. This unequal representation was difficult to avoid, as organizing the FGDs themselves would have been difficult without the leaders' facilitation. However, participants may not have reflected the diversity of opinions in the community, and the presence of community leaders, due to their seniority, may have stifled other participants from expressing their views, resulting in over-reliance on the knowledge of community leaders by the study team and a lack of awareness of the potential limitations of community leaders as community informants.
    - We arbitrarily settled on only two key informants (primary and secondary), judging this *a priori* to be sufficient. However, more (and more diverse) sources may have been warranted. Furthermore, in both Kabul and Chiradzulu key informants were mutually dependent for information on deaths (mullahs and *wakils* informed each other about recent deaths, as did village headmen and *fumukazi*).

- **Key informants had limited knowledge about deaths in their sectors**. In addition to mere forgetfulness, this may have been due to the following:

    - Key informants were unable to learn about all deaths:

        - They covered far too large a population. In Kabul, this was in the order of 3,000 people per *wakil-e-guzar*; section leaders controlled about 2,000 people each in Mae La, and 4,500 in Tanzania.

        - Most informants were community leaders who had considerable knowledge of the community, and whose involvement generally helped to gain the trust of respondents. However, the degree of trust between a community leader and community members clearly varied and could have affected his/her knowledge of deaths, willingness to support the study team, and willingness of households to share information with the community leader. For example, relations between community members and some of the *wakil-e-guzars* in District 1 of Kabul appeared strained; it should be noted that *wakils* requested that we delay our study by a few days due to community dissatisfaction about an insufficient food distribution: this may have further reduced reporting of recent deaths to some of the *wakil-e-guzars*, or their ability to solicit information.

        - Other contextual factors. In Kabul, old age appeared to impair some *wakils*' ability to interact with community members. Although prayers are said over each decedent before burial, some mullahs in Kabul (secondary informants) reported that in certain households prayers were performed by literate family members or by Shi'a minority leaders within the sector. Both *wakils* and mullahs reported difficulty in learning about deaths of small children. In Mae La, section leaders had only been elected about one month before our survey and may not have had good knowledge of deaths during a 60-day period, while members of the Karen Woman's Organization, our secondary informants, had only recently started recording information about deaths in the community.

    - Key informants deliberately did not mention certain deaths, or households deliberately did not report certain deaths to key informants:

        - Household food rations in Mae La and the Tanzania camps are reduced after a household member dies, which may lead households to under-report deaths (note however that we systematically informed households that the survey was not connected to a registration or distribution process). In Tanzania, reporting a death may also affect the amount of repatriation assistance. Some refugees in Mae La are in possession of Thai identification cards and may not wish to invite attention. Note that these issues would probably also affect alternative mortality measurement methods such as surveys and surveillance.

        - Cases of suicide or addiction-related death may be cultural taboos and may thus not be reported. This issue would probably also affect alternative mortality measurement methods.

- **Household respondents contributed relatively little to the exhaustive search process**, providing only a minority of all referrals (we had originally expected that households would primarily lead the search through a snowball process used successfully for other epidemiological studies performed on hard-to-find sub-groups [25, 26]). Possible reasons include:

    - Limited knowledge of other households' circumstances:

        - Given the relatively low mortality in the study sites where we tested the method, it may be inherently difficult for households to know about events in a sufficient number of other families: for example, in the Tanzania camps, over 60 days a death occurred for about every 200 households, meaning a household would have to routinely hear about happenings in at least 200 households in order to not interrupt the process of snowball chain referral.

- Population movement and ethnic segregation within the communities may mean households may know little about their neighbors. In Kabul, many inhabitants were short-term renters or recent immigrants from rural areas; in Mae La, during the year before our study almost a third of the refugees in the camp had been resettled to other countries and many households had recently arrived in the camp. However, household referrals were infrequent even in more stable settings such as Chiradzulu.

o Unwillingness to refer the study team to other bereaved households:

- Households may have been reticent to share such information with strangers, fearing to upset the community leaders or the bereaved families themselves, and/or preferring not to override the information already provided by the key informants.

- Cultural and/or religious barriers to speaking about deaths in other households.

o Unwillingness by the interviewers to probe about other deaths in the community. In Kabul and Chiradzulu, during many interviews we attended this seemed apparent, and we preferred not to interfere.

In all four sites, most deaths captured by the EM method were reported as having occurred 60 to 30 days before the survey date, rather than in the last 30 days. This may be a spurious finding, a reflection of seasonal patterns (e.g., in Tanzania and Chiradzulu, the dry season may have corresponded with a lower incidence of malaria, a major contributor to mortality), or an artefact due to systematically mistaken recall of dates (we attempted to minimize this through use of a detailed calendar). The pattern was most striking in Kabul, where only 16% of deaths took place in the last 30 days, suggesting some form of bias. Potential explanations for low sensitivity in Kabul have been mentioned above: furthermore, study team members felt that some *wakils* were receiving information from the community with a considerable delay, thus explaining these observations.

6.3. FEASIBILITY

There was significant variance in the time-inputs among the study sites, reflecting their different conditions. The studies in Mae La camp in Thailand and Lugufu and Mtabila camps in Tanzania required only 168 person-hours and 444 person-hours respectively (i.e. about three person-weeks). This was because very little travel and data collection time was required due to the low number of deaths recorded and high population density of these camp settings. They also did not require population estimation to be conducted. The study in Tanzania required greater time inputs than Mae La camp principally because two camps were included. By contrast, the study in Chiradzulu District recorded 93 deaths in 96 villages covering an area of approximately 875 km^2, and required population estimation, which contributed about 40% of total person-hours (most of which recorded by villagers hired to count structures). The Kabul site also entailed high person-time inputs: this was partially attributable to requiring additional data collection staff to ensure same-sex interviews and FGDs, and also having to conduct population estimation.

Comparisons between the EM method and retrospective surveys suggest that the EM method could offer considerable economic benefits over surveys. The EM method required less time than retrospective surveys in three of the four study sites, with person-time savings of up to 72%. It also showed considerable monetary savings, with costs estimates in all four study sites lower for the EM method than for a survey. Generally, we believe that the EM method would be most feasible in camps and concentrated populations, though not appreciably less feasible than surveys in scattered populations. In chaotic situations where no population estimates are available, feasibility would be considerably lessened; however, in post-emergency camps or other communities where population size is being monitored, feasibility results from Mae La and the Tanzania camps represent what might be expected routinely.

The EM method has potentially considerable ethical benefits over surveys. The time input by respondents was approximately 90% less in all four study sites for the EM method when compared with a survey, representing a substantial reduction in burden to respondents. However, the referral by key informants who are also community leaders may entail a risk that households will not be able to decline participating in the study.

The study also explored the feasibility of including verbal autopsy questionnaires as part of the routine application of the EM method. In the Chiradzulu District site, this took a total additional time of 76.5 hours, 3.3% of the total person-time input in that site. This suggests that verbal autopsy questionnaires could be used alongside the EM method routinely. The addition of verbal autopsy questionnaires when measuring mortality rates provides an extremely important way of increasing the accuracy of recording the causes of mortality and so helping to inform appropriate health interventions and responses.

6.4. STUDY LIMITATIONS

6.4.1. Implementation of the EM Method

The main limitation when implementing the EM method concerned establishing the correct date of death, crucial to the validation effort as well as the routine implementation of the method. Qualitatively, it was apparent that many household respondents and informants had difficulties pinpointing the actual date of death: a gold standard for this was not available, and we attempted to minimize error through a series of questions and a detailed calendar containing local salient events; indeed, most of the interview time was spent establishing the date of death. Systematic reporting of dates as having occurred more recently than in reality would have biased our findings by making the EM method seem more sensitive than it actually is. Likewise, systematic reporting of dates as having occurred less recently than in reality would have biased our findings by making the EM method seem less sensitive than it is.

There were no instances in which a household was empty, or had no next of kin 18 years of age or older available to be interviewed. Therefore, although absenteeism is a potential risk for the EM method (as with retrospective surveys), it did not affect the results of the EM study in any of the study sites.

6.4.2. Choice of the Gold Standard Measurement Tool

The mortality rates we estimated after adjustment for uncaptured deaths were consistent with non-emergency situations in all sites, and reflected expected patterns given the underlying epidemiological and demographic profile. Mortality rates among children under 5 were approximately double the all-age CMR, as typically observed in developing country settings. In **Table 27** we compare our estimated mortality rates over a 60-day recall period with rates from broadly comparable regions and time periods: these mostly corroborate our gold-standard measurements. It should be noted that both camp-based populations we investigated enjoy long-standing humanitarian assistance and comparatively good health services, which may explain their low mortality (a typical developing country population not experiencing a crisis would have a CMR of about 0.3 to 0.6 deaths per 10,000 person-days).

Table 27. Recent Survey-Based Estimates of Mortality (as Deaths per 10,000 Person-Days) from Regions Surrounding the Four Study Sites, by Year(s) Covered by the Survey

	Kabul District (since ousting of Taleban government; residents or returnees only; includes rural areas)	**Tak Province, Thailand** (including resident population)	**Southern and Central Regions, Malawi** (after food crisis in 2002)	**Tanzania Refugee Camps**
CMR	1999-2002: 0.16 (Bartlett et al [27]) 2001: 0.20 2003: 0.50 2004: 0.30 2006: 0.76 **This study: 0.24**	2007: 0.17 (Thailand demographic surveillance; pers. comm., Oliver Morgan) **This study: 0.23**	2005: 0.45 2006: 0.10, 0.26, 0.30, 0.40, 0.40, 0.41, 0.41, 0.80, 0.90, 1.90, 2.20, 2.50 (median: 0.41) 2007: 0.38 **This study: 0.51**	[no survey found] **This study: 0.18**
Under 5 Mortality Rate	2001: 0.54 2003: 0.59 2004: 0.63 2006: 0.73 2001-2006: 0.45 (calculated based on data in Mashal et al [28]) **This study: 0.49**	[no survey found] **This study: ≥0.49**	2003: 1.57 2005: 1.20 2006: 0.40, 0.78, 1.60, 2.10, 3.40, 3.80 2007: 0.71, 0.82 **This study: 0.84**	[no survey found] **This study: 0.43**

Unless otherwise referenced, all data are as reported by the Centre for Research on the Epidemiology of Disasters' Complex Emergency Database (www.cedat.be), after excluding reports of zero mortality on plausibility grounds.

Nevertheless, the use of capture-recapture analysis proved challenging. Obtaining additional lists of deaths entailed difficulties: in Kabul, hospital records were often incomplete, leaving us with a very small list that decreased the precision of the capture-recapture estimate; in Mae La and Chiradzulu respectively, remote collection of data through religious leaders and HSAs did not enable us to properly supervise data quality, particular as relates to:

- Date of death: both systematic and random error in date recall would have biased the capture-recapture analysis and thus the sensitivity estimation (any listing of deaths wrongfully reported as taking place within the recall period would have resulted in an underestimate of sensitivity; the converse however does not apply, since omission of deaths from lists would have mainly reduced the estimate's precision, unless the wrongly omitted deaths were also those that weren't captured by the EM method).

- Residence of the decedent: whereas through the EM method's questionnaires we were able to establish whether the deceased person was truly a resident of the community, we had less information on this variable in the additional capture-recapture lists, and, unless clearly indicated, assumed that everyone on them had been a resident of the community, which is unlikely (for example, in Chiradzulu many villagers are in fact urban migrants to the city of Blantyre who return only occasionally): this bias would have resulted in an underestimation of sensitivity.

Capture-recapture analysis by its nature entails some arbitrary decisions in the choice of models to exclude, though we attempted to minimize these through Bayesian averaging procedures. It should be noted that alternative choices of models would not have significantly altered the main sensitivity findings. Imprecision in the estimates of uncaptured deaths might have been reduced through stratification, but the scant nature of most lists did not allow for this.

Alternative gold standard options included prospective surveillance systems, but these either did not exist in the study sites or were suspected to have imperfect sensitivity, as suggested by a review of camp-based surveillance [29]. Similarly, retrospective household surveys are subject to methodological limitations and biases, including common under-reporting of deaths, and to our knowledge have not been validated sufficiently to provide a reliable gold standard option [2, 3, 30], [31].

6.4.3. Study Generalizability

Clearly, evidence from four sites only is not optimal to draw recommendations about the usefulness of the EM method, although we believe it is sufficient to conclude that the method requires further development to improve its sensitivity (see below).

Most importantly, due to the short duration of this project and the difficulty to organize proper field research in unstable settings, we were unable to test the method in settings of high mortality, most consistent with the conditions for which we believe the method would be most useful. We do not know whether the method would have proven more or less valid and feasible in such settings. In terms of sensitivity, a chaotic, high mortality setting might favor the EM method, as respondent households might feel less constrained to refer deaths due to a less structured political hierarchy, and/or might have better knowledge of other deaths simply due to their sheer number; alternatively, the method might not perform as well due to population movement and difficulty to find adequate key informants.

6.5. ROUTINE APPLICATION OF THE EM METHOD

Notwithstanding its limited sensitivity, we believe the EM method could be a useful and sometimes superior alternative to both retrospective surveys and surveillance. At this stage, we believe that its routine application depends on increasing sensitivity to an acceptable level (e.g. >85%) are found (see **Section 6.6.1**). However, based on the present study we can already recommend a number of procedures that should accompany any implementation of the method in the future:

- At least 1,000,000 person-days should be investigated: in practice, the recall period could consist of one month in a population of 50,000 and two weeks in a population of 100,000.

- If the population is very large and/or scattered, sampling of at least 30 PSUs is appropriate; the total population in the PSUs should be at least 20,000.

- Recall periods longer than about 30 days are probably inappropriate, although the exhaustive search should target a period longer than the actual recall period of interest to maximize sensitivity (e.g., if the recall period of interest is 20 days, the team could ask about deaths occurring in the last 40 days, and then restrict the analysis to 20 days: in Islamic majority countries this would correspond well with the religious mourning period; elsewhere, a salient event could be identified).

- Error in date recall, especially if systematic, is a major potential limitation of the EM method, and should be pre-empted through questionnaires that cross-check information on dates through different probing questions and rely on detailed calendars including salient events.

- Household referrals may not yield a large number of deaths, but should remain a feature of the method, as they take little extra time and appear to capture mostly deaths not reported by community informants.

- Population estimation should be performed when population figures are either unavailable or unreliable. In camps or concentrated populations, we believe the easiest method is to (i) perform two independent counts of all residential structures, and average the two; (ii) in at least 50 structures, sampled systematically (i.e. every n^{th} structure), count the number of people and children under 5 years who spent the previous night in the structure; (iii) multiply the number of structures by the mean number of people (children under 5) per structure to estimate the total population size.

- For the purposes of this first validation study, we wished to calculate mortality rates very precisely, partly to maximize the benefit of our study to populations in the study sites; further complexity was introduced by the capture-recapture validation process and the difficult sampling conditions in Kabul. However, for the routine application of the method, we envisage much simplified data management and analysis: (i) records can be single-entered, with double entry and validation of the most important variables (e.g., age, sex, date of death); (ii) if population estimation is done, only the point estimate of population size need be computed, provided appropriate sample sizes are used for the estimation; (iii) inference can be based on either of the calculators developed in **Section 5.3**, which require little analysis aside from tallying the number of deaths, calculating the population size point estimate, and specifying a threshold of interest if the investigator wishes to classify rather than estimate mortality.

The situations in which we believe the EM method could or could not be applied as a useful alternative to surveys and surveillance are summarized in **Table 28**.

Table 28. Summary of Scenarios in Which the EM Method Could be Applied

Appropriate Situations to Use the EM Method	Inappropriate Situations to Use the EM Method
No adequate surveillance system	Adequate surveillance system
No massive in/out migration in recall period	Extremely large geographic area
Aim of estimating real time mortality (operational)	Age and sex profile of population required (unless simultaneously conducting population estimation)
Require detailed information on deaths (e.g. using verbal autopsy questionnaires)	Aim of estimating mortality over long recall period (advocacy/documentation)
Supportive community leaders or other informed members of the community	Small population over a short period (due to stochastic oscillation – see **Section 2.3.2** and **Section 5**)
Communities in which knowledge of key events such as deaths exists and is shared through social networks Places with good population estimates, e.g., population in camps or concentrated	Situations in which there is very large and rapid population movement and social networks and community structures do not exist

The EM method has a number of advantages and disadvantages in comparison with alternative prospective surveillance and retrospective survey methods. Unlike surveys, the EM method furnishes nearly real-time mortality data and thus should be of significant operational value to humanitarian agencies. It required fewer monetary resources in all sites and less time inputs in 3 out of 4 sites, allowing for more time to add additional research components such as verbal autopsy questionnaires to obtain more reliable information on the causes of death. The EM method is also statistically more precise and the analysis is far simpler than for retrospective surveys in most scenarios of implementation. However, it does require some skills in social research skills and mapping of settlements and populations.

The main disadvantage of the EM method may be its reliance on well-informed and supportive key informants and social networks in the community to yield reliable information on deaths. Without this support and knowledge, the EM method will yield a potentially severe underestimate. Communities which have experienced massive in-and out-migration during the short recall period may not have the required social networks and knowledge required for the study. In addition, high migration rates would violate the assumption that population size is equal at the beginning and end of the recall period. Conducting the EM method also requires that information on potentially marginalized population groups is actively collected to avoid further underreporting.

Furthermore, the EM method may not be very feasible for large population sizes or geographic areas. While there is no methodological obstacle to applying the EM method in very large communities (e.g., an entire crisis-affected region such as North Darfur, all internally displaced persons [IDP] camps in northern Uganda, eastern Kivu province in the Democratic Republic of Congo), logistically this might require simultaneous implementation of the method by several teams that, after centralized training, would be dispatched to different sites. Furthermore, data collection in very scattered communities (e.g., hamlets in southern Sudan) would entail considerable transport and walking times. While the above is theoretically feasible, it might negate some of the benefits of the EM method, mainly speed and the ability of investigators to directly supervise all or most data collection.

Finally, the EM method does not provide an age-sex profile of the population. Because of this, the size of the population under 5 years, necessary to compute the under-5 mortality rate, may not immediately be known. This would then need to be estimated by applying the typical range in the region (e.g., 17-22% in Sub-Saharan Africa), applying findings from any recent survey, or performing an ad hoc mini-survey (e.g., 60 households) to estimate the proportion of the population aged less than 5 years (this could easily be nested with population estimation).

6.6. FURTHER RESEARCH AND DEVELOPMENT

6.6.1. Future Development of the EM Method

While sensitivity of the EM method as implemented in this study was unsatisfactory, it was not far from acceptable levels: improvements of about 15-30% would result in sensitivity >85%. This seems like an achievable aim, warranting further development of the method. Future steps should focus on improving the method's validity (we believe this study shows that it compares very favorably with surveys in terms of feasibility), and could include the following:

- Ensuring that the FGD participants represent a mix of men and women, ages, occupations and economic and social hierarchies. Using additional qualitative methods to complement the FGD could also be considered, such as informal individual discussions with a diverse range of community members.
- Using more than two types of key informant. This appears very feasible, judging by the ease with which we collected additional lists of deaths (e.g., from shopkeepers in Kabul, from religious leaders in Mae La, from graveyard chairmen in Chiradzulu). It is thus conceivable that application of the EM method with four or five different sources would take only marginally more time, but offer the improvements in sensitivity needed to make this a viable method.
- Relying on less formal types of informant. For example, instead of identifying community leaders or "figures" as the only informants, the method could rely on questioning people at gathering points (e.g., boreholes where people stand in line; shops and bakeries, as done in Kabul; traditional gatherings).

A further point concerns ethics: although we did not observe or hear of any incidents involving breach of confidentiality or forced participation of households in the study, follow-up work could be conducted to explore any untoward effects of the referral system that underpins the EM method, and find ways to strengthen the confidentiality and consent arrangements.

Much of the above work is clearly in the remit of social science and anthropology, not epidemiology and demography: we thus recommend that future development of the EM method be spearheaded by researchers trained in qualitative and ethnographic techniques.

6.6.2. The Potential Applicability of Capture-Recapture Analysis

Despite its moderate sensitivity, the EM method was consistently able to identify deaths that were not found on either of the other two lists, and especially on those derived from existing registers or surveillance systems. This suggests an inherent value of the EM method whether used alone or as part of a capture-recapture approach.

Indeed, we found that capture-recapture analysis may have a value in routine measurement of mortality and/or evaluation of ongoing surveillance systems. Clearly, this approach is too complex for its routine use by investigators without adequate statistical training. However, we believe that it could have a place in studies implemented by experienced epidemiologists; for example, the EM method could be used in parallel to a survey or surveillance, and the lists obtained from these alternative methods could be combined through capture-recapture analysis to yield realistic mortality estimates.

6.7. CONCLUSIONS

This study suggests that a new method to estimate mortality in crisis-affected populations (the EM method), based on information provided by community informants, may have moderate sensitivity, i.e. detect a disappointingly low proportion of all deaths. Sensitivity appears particularly low among children under 5 years. However, the method's performance is comparable to that of existing surveillance systems.

The EM method appears more feasible in terms of time and financial inputs, as well as ethics, than the main alternative, namely retrospective surveys. Addition of verbal autopsy questionnaires is cost- and time-efficient.

On balance, we believe the method shows sufficient promise to warrant further development, particularly given the paramount importance of mortality measurement in crisis-affected populations, the current paucity of methods to measure mortality, and the advantages of the EM method over both surveys and surveillance, including its ability to monitor mortality on a real-time basis. The main outstanding issue is how to improve sensitivity through selection of appropriate key informants: this should be the focus of future research and development.

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8. Report Annexes

8.1 FOCUS GROUP DISCUSSION TOPIC GUIDE

CHIRADZULU: FIRST FOCUS GROUP DISCUSSION TOPIC GUIDE

Agenda

- Introduction
- Agreement to participate
- Questions and discussion – should take a bit less than an hour and half

Introduction

We are here to have a discussion on how information is shared on deaths in the community. This discussion seeks to share knowledge and exchange different opinions. We very much hope you will speak as open and freely as possible. The meeting is expected to last between around one and a half hours. Most of the time will be spent having a discussion.

Now we can give you some background on what we are doing here in Chiradzulu district. We are undertaking a study of deaths in the community. The study is run by the University of London in the United Kingdom, in conjunction with the DHO. The main purpose of the study is to try out a new method of measuring the number of deaths in a community.

The new method we want to try out uses key persons in the community who have a good knowledge of deaths that have occurred within the past few weeks in that community. These ‘key-informants’ will lead us, the researchers, to households in the community that have been recently suffered a death. We will then briefly interview someone in the household about the death. We will then ask the person in the household we interviewed to identify another household in the community that has also recently experienced a death. We would then find that household and conduct another interview. The process will continue until we think we have identified all the households in the community that have experienced a recent death. The primary community informants would help us throughout this process.

[Prompt: offer to repeat if method is not clear to group.]

This study we are doing here will help us to decide whether this method works, and whether we can use it in the future in other places to help have a better understanding of the number of deaths occurring in those communities.

The study will also provide information about the health conditions here, and the main health problems people face.

We have asked you to come today because we would like to have a group discussion on how information on deaths is shared on deaths in the community in order to help us conduct the study as well as possible. We have selected you because of your knowledge of the community.

Before we carry on, it would be helpful for us to get to know each other

[Introduce yourself, and then ask one participant to give their name and role continue with all participants/study staff.]

[Write down name on seating plan]

Outputs and Confidentiality

The findings from this discussion will be included in public reports. To make sure we accurately reflect the discussion, we will be recording it. The discussion will then be typed up and analyzed. The recording will be securely kept by project staff and will not be shared with anyone. We want to keep this discussion private and confidential so your names will not appear in any documents. Instead, any statements you make will be written as "one of the respondents stated that…"

Informed Consent

We have given you an information sheet for this discussion. This information sheet outlines key points:

- Who we are and why we are here
- Why you have been selected to come
- The topics we would like to discuss
- The voluntary nature of your participation

[Make sure all participants have information sheet.]

You will now be asked if I can mark a consent form that you agree to participate in this discussion. A witness will also sign each of these forms to mark an information sheet to show that you agree to participate in this discussion.

Ground Rules

There are a number of ground rules we would like us all to respect to make this discussion as effective and agreeable as possible.

- Please feel like you can speak freely and don't feel embarrassed about what other people may think. There are no right or wrong answers. All your opinions are important and valuable to us and we ask you to participate as fully as possible.
- We are here to share knowledge and exchange opinions. Please respect the views and opinions of other participants and allow them to speak freely.
- The discussion is private and confidential. Please do not share the content of this discussion with anyone outside of this group.

Before we begin, does anyone have questions about the arrangements for the discussion?

Discussion Questions

[Try and preface each participant's comment's by saying "please, [name], what would you like to say?". Although there is a risk of stifling the discussion, it will aid the transcription and analysis by making clear who says what... If necessary, repeat that the names will not be used in the final report.]

1. If someone from Chiradzulu district left for 6 months and then came back to Chiradzulu district, and wanted to know about things that had happened here in the district, like babies being born and people who had died, where do you think he or she would go to obtain the best information on these births and deaths in the district in the past six months?

[Prompt: Focus on deaths.]

[Prompt: e.g., religious leader, community leader, health worker etc].

*[Prompt: **Why** do you think he would choose these people? e.g., comprehensive knowledge; accuracy; trust in community; community contacts; professional knowledge; experience; wide geographic coverage; etc.]*

[Prompt: why did you think they would be effective at sharing their knowledge? E.g., duty; job; experience; trust etc.]

2. Do you think there are any particular advantages or disadvantages of relying on any of these people that have been mentioned?

[Prompt: e.g., unable to access information on certain types of deaths; not willing or able to share information on all deaths; limited geographic coverage; timing of deaths (e.g., recent deaths not reported); age of deceased (e.g., newborns); lack of trust by community members in these people; these people not being helpful in providing information to study team]

[Prompt: probe for disadvantages]

3. How would the selection of these people as sources of knowledge change if deaths were of children rather than adults?

[Prompt: why would it change/not change?]

4. As our survey method also relies on community members who have experienced a recent death in the family sharing information on other households in the community that have experienced a death, we are keen to understand how information on death is shared and spread in the community by other community members. What are the ways in which information on recent deaths is shared among community members?

[Prompt: e.g., with relatives, friends and neighbors, at a place of work, worship, or at the market?]

[Prompt: How does information on a death spread in the community? e.g., networks of friends or relatives; fellow worshipers.]

[Prompt: How willing do you think households would be to share information about deaths in other households in the community? Explore why.]

5. What challenges do you think may exist in trying to collect information on recent deaths from community members?

[Prompt: e.g., Mourning; wariness of outside researchers; some deaths may be hidden/stigma; key-informants may not be helpful to study team.]

[Prompt: How could these challenges affect the sharing of knowledge about the death? e.g., alter cause of death; adjust timing of death.]

[Prompt: Are documents given out for deaths (e.g. death certificate)? How widely used are these? What are the challenges of using these documents (e.g., only given for certain causes of death/population types/locations of deaths etc.?]

6. If the household and key-persons could not identify any more deaths in the community, what other sources in the community could be useful to help identify a household that had recently suffered a death?

[Prompt: e.g., market place (where/who in particular), water point etc.; shops; why would they be useful?]

[Prompt: What advantages and disadvantages do these other sources have?]

7. I would now like to ask you a broader question about how community members view the causes of death and illness in the community here. What do you think a typical community members would list as the main causes of death in the community?

[Prompt: we are keen to understand community perceptions, so encourage participants not to introduce their own, perhaps more biomedical explanations.]
[Prompt (only use if really necessary): e.g., fever, diarrhea, cough, bad water or food, spirits, etc.]

[Prompt: How could these views change between different types of community members? E.g., older, younger, different ethnic groups, women and men.]

8. If someone was seriously ill in the community, apart from the hospital where do you think they would go?

[Prompt: where would a child go, where would an elderly person go, etc.]

9. Finally, to help us make sure that the deaths we record in our study are accurate, we need to know some key events that have taken place within the 2 months, as it may help the community members we will be interviewing to more accurately record when the deceased died.

[Prompt: either an event that virtually everyone in Malawi was aware of; or specific to the district which virtually everyone in the district was aware of]

[Prompt: e.g., religious day; community event.]

Concluding Statement

[Conclude by briefly summarizing the key-points from the discussion.]

Does anyone else have anything else they would like to add?

The discussion has given us very useful information to help ensure our study is as effective as possible. We thank you for giving your time and expertise. We are extremely grateful. If you have any questions about the discussion today or our work, please feel free to ask them afterwards. You can also contact us using the details given on your information sheet.

[End]

* * *

Facilitator Role

- Mediation between participants
- Ensure participation of all participants
- Prevent single participants/groups of participants dominating conversation
- Encourage reserved members to become involved and share views
- Encourage space for discussion/conversation on the topics
- Maintain balance between steering the group and moderating it
- Active listening
- Being non-judgmental
- Not-interrupting
- Developing rapport

Tips

- Use research assistants to take notes, check recording, provide an on-going oral translation to study investigators.
- Names could be given by participants before they speak to help the transcriber separate out comments. These names will obviously be omitted from the final transcription documents and all project outputs.
- Ensure comfortable, private and quiet location for discussion.
- Ensure refreshments are available for all participants.
- Could sketch out the seating plan to help recall who sat where and who said what.
- Address issue of local hierarchies/encouraging everyone to feel free to speak.

8.2. HOUSEHOLD REGISTER

A new method to estimate mortality in crisis-affected populations: Validation study

HOUSEHOLD REGISTER **TA:** __________ **Village:** __________

No.	Address and Details	Outcome
1.	Address: □ within village □ in a different village □ outside District (do not visit) Referred by: □ village headman □ fumukazi □ household □ other:	□ already visited via other referral □ no recent death □ no respondent found □ consent refused □ interview done Cross box to confirm referral has been followed up: **HOUSEHOLD VISITED** Related questionnaire ID: I__I__I I__I__I I__I__I__I
	Notes:	

No.	Address and Details	Outcome
2.	Address: □ within village □ in a different village □ outside District (do not visit) Referred by: □ village headman □ fumukazi □ household □ other:	□ already visited via other referral □ no recent death □ no respondent found □ consent refused □ interview done Cross box to confirm referral has been followed up: **HOUSEHOLD VISITED** Related questionnaire ID: I__I__I I__I__I I__I__I__I
	Notes:	

No.	Address and Details	Outcome
3.	Address: □ within village □ in a different village □ outside District (do not visit) Referred by: □ village headman □ fumukazi □ household □ other:	□ already visited via other referral □ no recent death □ no respondent found □ consent refused □ interview done Cross box to confirm referral has been followed up: **HOUSEHOLD VISITED** Related questionnaire ID: I__I__I I__I__I I__I__I__I
	Notes:	

No.	Address and Details	Outcome
4.	Address: □ within village □ in a different village □ outside District (do not visit) Referred by: □ village headman □ fumukazi □ household □ other:	□ already visited via other referral □ no recent death □ no respondent found □ consent refused □ interview done Cross box to confirm referral has been followed up: **HOUSEHOLD VISITED** Related questionnaire ID: I__I__I I__I__I I__I__I__I
	Notes:	

No.	Address and Details	Outcome
5.	Address: □ within village □ in a different village □ outside District (do not visit) Referred by: □ village headman □ fumukazi □ household □ other:	□ already visited via other referral □ no recent death □ no respondent found □ consent refused □ interview done Cross box to confirm referral has been followed up: **HOUSEHOLD VISITED** Related questionnaire ID: I__I__I I__I__I I__I__I__I
	Notes:	

No.	Address and Details	Outcome
6.	Address: □ within village □ in a different village □ outside District (do not visit) Referred by: □ village headman □ fumukazi □ household □ other:	□ already visited via other referral □ no recent death □ no respondent found □ consent refused □ interview done Cross box to confirm referral has been followed up: **HOUSEHOLD VISITED** Related questionnaire ID: I__I__I I__I__I I__I__I__I
	Notes:	

8.3. HOUSEHOLD VISIT PROCEDURES

A new method to estimate mortality in crisis-affected populations: Validation study

HOUSEHOLD VISIT PROCEDURES

Step 1. Verifying whether the death has indeed taken place.

Ask for the head of the household. If absent, ask to speak to a responsible member of the household aged 18 years or older, preferably a female.

If no respondent is found, revisit household later or on the following days, based on when the respondent is most likely to be present.

If the household appears to be permanently abandoned or the family is reported as being away on a trip, find an adult neighbour.

If a respondent is found (or a neighbour in case of an abandoned or absent household), proceed:

"**Hello. We work with the University of London, in collaboration with the District Health Office. We are doing a survey of health conditions in this community. The community leaders have been informed of our work.**

We were told that someone may have died in this household during the last two months. Is that correct?"

- If YES, start a new Mortality Questionnaire and fill out the Interview Checklist section. If a respondent is found, go to Step 2 (Informed Consent).
- If NO, ASK: "**So no one living here has died in the last two months, including any adults, old people, children, newborns, orphans, visitors or homeless people?**"
  - If YES, start a new Mortality Questionnaire, fill out the Interview Checklist section. If a respondent is found, go to Step 2 (Informed Consent).
  - If NO: "**Can you direct us to any other household around here where someone may have died in the last two months? This could include adults, old people, children, newborns, orphans, visitors, and homeless people.**"
    - If the respondent or neighbour mentions no household, refer back to community informant.
    - If the respondent or neighbour mentions one or more households, ask for directions and note the details of each household on Household Register.
    - ASK: "**Do you know of any others?**"

Step 2. Informed Consent.

Read out the Information Sheet and answer any questions.

- If the respondent gives verbal consent: sign the Consent Form, give a copy of the Information Sheet to the respondent, and proceed to Step 3 (Mortality Questionnaire).
- If the respondent does not give consent: "**That is no problem, thank you very much.**" Refer back to community informants.

Step 3. Mortality Questionnaire.

Administer Questions section of Mortality Questionnaire.

ASK: "**Has there been any other death in this household during the last two months?**"

- If YES, administer a new Mortality Questionnaire.

ASK: "**Can you direct us to any other household around here where someone may have died in the last two months?**"

Step 4. Verbal Autopsy.

Once all Mortality Questionnaires for the household have been completed, check whether the date of death falls within the last month.

- If YES, locate a respondent suitable for answering the Verbal Autopsy Questionnaire. Ask this respondent if (s)he wishes to proceed with the Verbal Autopsy Questionnaire.
  - If YES, arrange for a visit on a separate day to carry out the Verbal Autopsy Questionnaire.
  - If NO: "**That is no problem, thank you very much.**"

8.4. CONSENT FORMS AND PARTICIPANT INFORMATION SHEETS

London School of Hygiene & Tropical Medicine
In case of any questions, please contact the researchers:
Francesco Checchi, Bayard Roberts
LOCAL ADDRESS AND PHONE NUMBER
E-mails francesco.checchi@lshtm.ac.uk , bayard.roberts@lshtm.ac.uk

Study title: "A new method to estimate mortality in crisis-affected populations: Validation study"

Study approved by the ethics committees of the London School of Hygiene and Tropical Medicine and [insert local ethics body].

This sheet provides information on a study which you have been asked to participate in.

Survey Participant Information Sheet

We are undertaking a survey of deaths in the community. The survey is run by the London School of Hygiene and Tropical Medicine in the United Kingdom and [insert local collaborating partner]. The survey has two purposes:

1) The first is to try out a new method of measuring the degree to which people are dying in communities such as these. The method is based on families and people in the community leading the researchers to all the households that have been recently bereaved. This study will help us to decide whether this method works, and whether we can use it in the future so as to see how healthy people in this community are, what the main health problems are, and therefore what improvements can be done.

2) The second is to provide information about the health conditions here, and the main health problems people face.

We were referred to this household by someone else in the community who thought there had been a death in this household in the past month. We would like to ask some questions about this death, including a detailed list of questions that will help us understand what may have caused the death. The interview will take 20 minutes to half an hour. If the person died after hospital treatment, we would also like to consult his/her medical records to learn more about the cause of death.

Participation is completely voluntary, so you should feel completely free to refuse to participate, to stop the interview, or to not answer questions you do not feel comfortable with.

The study does not give any benefits for you or other people in the household. However, our findings could be beneficial for the community because they will provide useful information about the number of people dying in the community, and the causes of death. This could lead to better health services and other responses in the future.

Because we will be asking you about a recent loss in your household, the study may cause emotional distress for you and your family.

All the answers you provide are confidential, and we will keep all the forms with us in London for the next ten years, and not share them with anyone outside of the research team. We will not write down any names so answers cannot be connected to individual persons. No quotes or other results arising from your participation in this study will be included in any reports, even anonymously, without your agreement. We will however publish a report with our findings about mortality in the community as a whole, and will also present them to people responsible for health services to this community.

If you have any questions on this study in the future or would like further information you can contact us on the details at the top of this sheet. Thank you for your kind help.

London School of Hygiene & Tropical Medicine
In case of any questions, please contact the researchers:
Francesco Checchi, Bayard Roberts
LOCAL ADDRESS AND PHONE NUMBER
E-mails francesco.checchi@lshtm.ac.uk, bayard.roberts@lshtm.ac.uk

Study title: "A new method to estimate mortality in crisis-affected populations: Validation study"

Survey Participant Consent Form

I have understood the verbal explanation about this survey, the information sheet has been read out to me, and I have a copy of it for me to keep. I understand what is required of me for this survey and what will happen to me and my household if I take part in it.

My questions concerning this study have been answered by

…………………………………………………
[name of investigator performing interview].

I understand that I can stop participating in this survey at any time without giving a reason, and that if I do there will be no effect, positive or negative, on me or my household.

I agree to take part in this study.

I DO DO NOT agree to quotes or other results arising from my participation in the study being included, even anonymously in any reports about the study.
[*please circle chosen answer*]

Signature of data collector: ……………………………………………..

Date: ……../……../……..
[dd / mm / yyyy]

Signature of investigator: …………………………………………………

London School of Hygiene & Tropical Medicine
In case of any questions, please contact the researchers:
Francesco Checchi, Bayard Roberts
LOCAL ADDRESS AND PHONE NUMBER
E-mails francesco.checchi@lshtm.ac.uk , bayard.roberts@lshtm.ac.uk

Study title: "A new method to estimate mortality in crisis-affected populations: Validation study"

Study approved by the ethics committees of the London School of Hygiene and Tropical Medicine and [insert local ethics body].

This sheet provides information on a study which you have been asked to participate in.

Focus Group Participant Information Sheet

We are undertaking a survey of deaths in the community. The survey is run by the London School of Hygiene and Tropical Medicine in the United Kingdom and [insert local collaborating partner]. The survey has two purposes:

1) The first is to try out a new method of measuring the degree to which people are dying in communities such as these. The method is based on families and people in the community leading the researchers to all the households that have been recently bereaved. This study will help us to decide whether this method works, and whether we can use it in the future so as to see how healthy people in this community are, what the main health problems are, and therefore what improvements can be done.

2) The second is to provide information about the health conditions here, and the main health problems people face.

To help develop this research, we are conducting a focus group discussion with people with a good knowledge of the community. The discussion is estimated to take about one to two hours, and will cover the following topics:

1. The type of people in the community that are the most effective in sharing and collecting information about recent deaths.
2. The ways in which information on recent deaths is shared among community members.
3. What problems there may be in collecting information on recent deaths through the proposed method.
4. The concepts of causes of illness and death in the community.

Participation is completely voluntary, so you should feel completely free to refuse to participate, to stop participating even during the discussion, or to not answer questions you do not feel comfortable with.

The study does not give any benefits for you. However, our findings could be beneficial for the community because they will provide useful information about the number of people dying in the community, and the causes of death. This could lead to better health services and other responses in the future.

The discussion will be recorded and noted down on paper. The recording and notes will be securely stored by the project team in London for ten years and no one else will be able to listen to or read them. The discussion will be analysed, and a report produced, which will be shared with you. No names will be written down in the report. No quotes or other results arising from your participation in this study will be included in any reports, even anonymously, without your agreement.

If you have any questions on this study in the future or would like further information you can contact us on the details at the top of this sheet.

London School of Hygiene & Tropical Medicine
In case of any questions, please contact the researchers:
Francesco Checchi, Bayard Roberts
LOCAL ADDRESS AND PHONE NUMBER
E-mails francesco.checchi@lshtm.ac.uk , bayard.roberts@lshtm.ac.uk

Study title: "A new method to estimate mortality in crisis-affected populations: Validation study"

Focus Group Discussion Participant Consent Form

I have read and understood the information sheet about this focus group discussion, and I have a copy of it for me to keep. I understand what is required of me for this focus group discussion and what will happen to me if I take part in it.

My questions concerning this study have been answered by

……………………………………………………
[name of investigator conducting focus group discussion].

I understand that I can stop participating in this focus group discussion at any time without giving a reason, and that if I do there will be no effect, positive or negative, on me.

I agree to take part in this study.

I DO DO NOT agree to quotes or other results arising from my participation in the study being included, even anonymously in any reports about the study.
[*please circle chosen answer*]

Signature of data collector: ……………………………………………..

Date: ……../………/……..
[dd / mm / yyyy]

8.5. MORTALITY QUESTIONNAIRE

A new method to estimate mortality in crisis-affected populations: Validation study

MORTALITY QUESTIONNAIRE

Interview Checklist

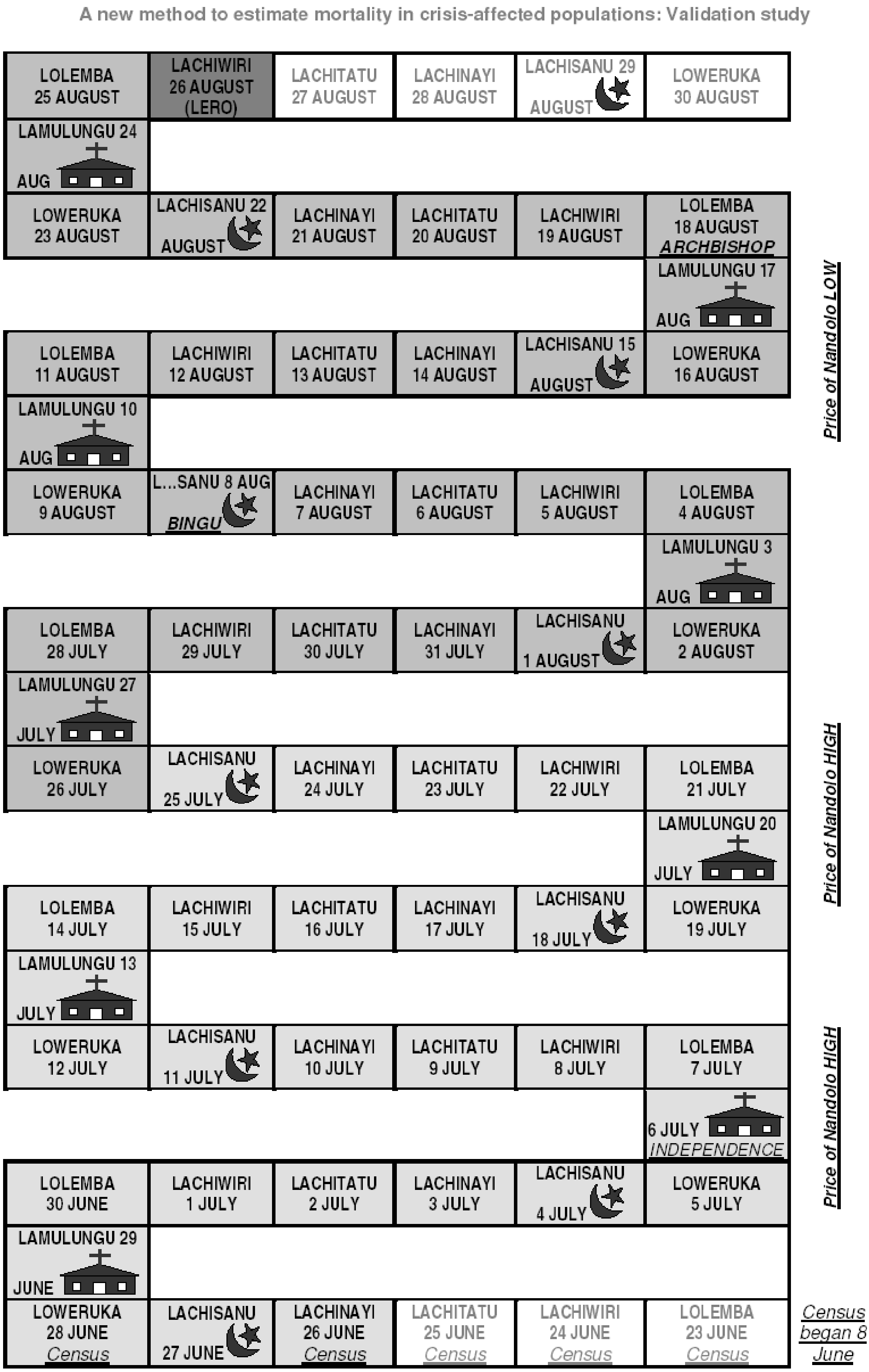
0.a	Questionnaire ID number:	|__0__|__2__| Malawi=02 |______|______| Village number |______|______|______| Number interviews consecutively from 001 onwards, including invalid ones.	
0.b	Interview date: |_D_|_D_|/|_M_|_M_|/|_Y_|_Y_|_Y_|_Y_|	0.c	Interview start time: |_h_|_h_|:|_m_|_m_|
0.d	Location: CHIRADZULU DISTRICT	0.e	Village:
0.f	Interviewer name:	0.g	Investigator name:
0.h	Household referred by: ☐ village headman ☐ fumukazi ☐ household ☐ other [specify:] ……………………………… Can tick more than one answer.		
0.i	Respondent found on first visit? ☐ yes ☐ no	0.j	If no, respondent found on second visit? ☐ yes ☐ no
0.k	Respondent aged 18 years or older? ☐ yes ☐ no		
0.l	Information sheet read out, copy given? ☐ yes ☐ no	0.m	Consent statement signed? ☐ yes ☐ no
0.n	If no information recorded from household, state reason here:		

Questions

1.	**Could you tell me, to the best of your recollection, how many days ago the deceased person died?** Prompt with **Calendar**.	|______| more than 60=88 don't know=99
2.	**How many Sundays have passed since the deceased person died?**	|______| more than 8=88 don't know=99
3.	**Could you tell me the exact date the deceased person died on?** If respondent is uncertain, reassure and say approximate date is OK. Cross-check Q1, Q2 and Q3 for discrepancy. A discrepancy arises if the questions give different answers about when the death occurred. If there is a discrepancy, use the **Calendar** and additional questions to resolve.	|_D_|_D_|/|_M_|_M_|
4.	***Report most likely date of death based on above answers:***	|_D_|_D_|/|_M_|_M_|
5.	**Did someone record the death of the deceased person in writing?**	☐ yes ☐ no ☐ don't know
► ***SKIP: If Yes go to Q6. If No or Don't Know, go to Q7.***		
6.	**Which organisations to your knowledge recorded the death in writing?** If respondent mentions only one organisation, ASK: any other organisations?	Tick one or more answers: ☐ District government ☐ religious officials ☐ health centre/hospital [give name:] …………………………… ☐ other [specify:] …………………………… ☐ don't know
7.	**Was the deceased person male or female?**	☐ male ☐ female
8.	**What was his/her age when (s)he died?** Record age in years or months if aged below 1 year. In case of a neonatal death (age below 1 month), record as 0 months.	|______| years (if 1 year old or older) or |______| months (if below 1 year)
► ***SKIP: If age at death was 4, 5 or 6 years go to Q9. If age at death was 0 months go to Q10. Otherwise, go to Q11.***		
9.	**Do you remember if the child was born more or less than 5 years ago?** How many Christmases (or Eid-el-Fitrs for Muslims) did the child live? How many New Years did the child live? Significant events: (i) general election 4.5 years ago; (ii) hunger period 6-7 years ago Cross-check Q8 and Q9 for discrepancy in age, and probe to verify.	☐ more than 5 years ago ☐ less than 5 years ago ☐ don't know
► ***SKIP: Go to Q11.***		
10.	**Was the baby breathing when (s)he was born?**	☐ yes ☐ no ☐ don't know

11.	**Where exactly did the deceased person die?** If answer is unclear, give definition of **household**: **people that eat from the same food pots**.		Tick only one answer: ☐ within this household ☐ in a different household ☐ in a health structure [give name:] …………………………………… ☐ elsewhere [specify:] …………………………………… ☐ don't know
► *SKIP: If person died in a different household, go to Q12. Otherwise, go to Q13.*			
12.	**Where is the household in which the deceased died?**		Tick only one answer: ☐ within this village ☐ outside of this village ☐ don't know
13.	**During the month before his/her death, did the deceased person live and stay mostly in this household, or mostly elsewhere?** Reiterate definition of **household**: **people that eat from the same food pots**.		☐ mostly in this household ☐ mostly elsewhere ☐ don't know
14.	**How many nights in the 1 week before death did the deceased person spend in this household?** If necessary, reiterate definition of **household**.		|______| don't know=99
15.	**What do you think was the cause of death?** Write down answer exactly as given by respondent.		…………………………………………… ……………………………………………
16.	**Did the deceased person receive any treatment in the 1 week prior to death for the condition which caused the death?**		☐ yes, at home ☐ yes, from a traditional healer ☐ yes, from government clinic ☐ yes, from government hospital ☐ yes, from private clinic ☐ yes, from private hospital ☐ yes, at pharmacy, drug seller or store ☐ yes, at another place or facility ☐ no ☐ don't know
► *SKIP: If Yes or Don't Know go to Q18. If No, go to Q17.*			
17.	**As far as you know, why did the deceased person not receive any treatment?** Write down answer exactly as given by respondent.		…………………………………………… ……………………………………………
18.	**Could I please have the name of the deceased person?** Explain why this is useful for the study.	**IN PENCIL ONLY!**	……………………………………………
19.	**Could I please have the name of the father of the deceased?** Explain why this is useful for the study.		……………………………………………
Has there been any other death in this household during the last two months? **Can you direct us to any other household around here where someone may have died in the last two months? This could include adults, old people, children, newborns, orphans, visitors, and homeless people.** **This is the end of the questionnaire. Thank you for taking the time to answer our questions.** ***Check whether death falls within the last month: If YES, arrange for follow-up visit for Verbal Autopsy.***			
0.o	Interview end time: |_h_|_h_| : |_m_|_m_|		
0.p	Interviewer/investigator notes:		

8.6. VISUAL CALENDAR AID



8.7. REGISTER FOR ADDITIONAL CAPTURE-RECAPTURE SOURCES

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No.	Sex M / F	Age at death y (m if<1y, 0 m if <1m)	Village where person lived before death	Name First, last name	Name of father First, last name	How many days ago? more than 60=88 don't know=99	How many Sundays? more than 8=88 don't know=99	Reported date of death Exact or approximate	Most likely date of death Date of death known exactly? Y / N	Other information
1.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	
2.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	
3.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	
4.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	
5.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	
6.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	
7.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	
8.						|___|	|___|	|D|D|/|M|M|	|D|D|/|M|M| □ yes □ no	

CHIRADZULU DISTRICT, MALAWI

MORTALITY SOURCE: ______________________________

8.8 RESIDENTIAL STRUCTURE TALLY SHEET

A new method to estimate mortality in crisis-affected populations: Validation study

RESIDENTIAL STRUCTURE TALLY SHEET – CHIRADZULU DISTRICT

MUDZI:		DZINA LA OPANGITSA MAYESO:
TSIKU: |_D_|_D_|/|_M_|_M_|/|_Y_|_Y_|_Y_|_Y_|		KODI NTCHITO YOWERENGA INATENGA MA OLA ANGATI?

CHIWERENGERO CHA NYUMBA:

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ZONSE PAMODZI:	|___|___|___|___|___|	

SAINILANI: ______________________________

8.9. INTERNATIONAL FORM OF MEDICAL CERTIFICATE OF DEATH

	Cause of death	Approximate interval between onset and death
I Disease or condition directly leading to death*	a)____________________ due to (or as a consequence of)	__________
Antecedent causes Morbid conditions, if any, giving rise ot the above cause, stating the underlying condition last.	b) ____________________ due to (or as a consequence of)	__________
	c) ____________________ due to (or as a consequence of)	__________
	d) ____________________	__________
II Other significant conditions contributing to the death, but not related to the disease or conditions causing it	____________________	__________
	____________________	__________
* This does not mean the mode of dying, e.g heart failure, respiratory failure, it means the disease, injury or complication that caused death.		
Notes (explaining answers):		
Signatures: __________________ Date:	__________________ Date:	

8.10. BOOTSTRAPPING PROGRAMS FOR MORTALITY ESTIMATION

District 1, Kabul

```
rm(list=ls(all=TRUE))

###--->>>USER INPUT REQUIRED HERE<<<---###
###Input data
##Population data for Low-Lying stratum
low.quadrants<-894
sampled.quadrants<-30
low.data<-read.csv(file="lowlying.csv")

##Population data for Hill stratum
est.structures<-1640
sd.structures<-55
sampled.structures<-20
hill.data<-read.csv(file="hill.csv")

##Mortality data
#Ascertained deaths
captured.deaths<-18
captured.u5deaths<-5

#Estimates of uncaptured deaths
uncaptured.deaths.pars<-dget("uncapturedkabul30d")

#Estimates of uncaptured under 5 deaths
uncaptured.u5deaths.pars<-dget("uncapturedu5kabul60d")

##Specify other parameters
recall.period<-30
crc<-1 ##0 means don't add estimate of uncaptured deaths to CMR
u5crc<-0 ##0 means don't add estimate of uncaptured deaths to U5MR
iterations<-1000  #Number of iterations for bootstrap
###--->>>USER INPUT ENDS<<<---###

##Creating empirical distributions from population data
low.ecdf.size<-ecdf(low.data$People)
a<-knots(low.ecdf.size)
b<-low.ecdf.size(a)
low.dist.size<-approx(a,b,xout=c(0:max(a)),rule=2)

low.ecdf.u5size<-ecdf(low.data$Under5)
a<-knots(low.ecdf.u5size)
b<-low.ecdf.u5size(a)
low.dist.u5size<-approx(a,b,xout=c(0:max(a)),rule=2)

hill.ecdf.size<-ecdf(hill.data$People)
a<-knots(hill.ecdf.size)
b<-hill.ecdf.size(a)
hill.dist.size<-approx(a,b,xout=c(0:max(a)),rule=2)

hill.ecdf.u5size<-ecdf(hill.data$Under5)
a<-knots(hill.ecdf.u5size)
b<-hill.ecdf.u5size(a)
hill.dist.u5size<-approx(a,b,xout=c(0:max(a)),rule=2)


##Functions that generate a random number of uncaptured deaths

#Function to calculate m000 based on model coefficients
m000.est<-function(pars){
  m111<-prod(pars)
  m110<-prod(pars[1],pars[2],pars[3],pars[5])
  m101<-prod(pars[1],pars[2],pars[4],pars[6])
  m100<-prod(pars[1],pars[2])
  m011<-prod(pars[1],pars[3],pars[4],pars[7])
  m010<-prod(pars[1],pars[3])
  m001<-prod(pars[1],pars[4])
  m000<-(m111*m100*m010*m001)/(m110*m101*m011)
  invisible(round(m000))
```

```
}
#Function to generate a random number of uncaptured deaths
uncaptured.deaths.gen<-function(pars,m000.est){

sim.dummies<-as.data.frame(pars[[1]])

rand.pars<-as.data.frame(array(0,c(7,length(sim.dummies))))
for(i in 1:length(sim.dummies)){
for(j in 1:length(sim.dummies[[1]])){
index<-sum(sim.dummies[[i]][1:j])
rand.pars[[i]][j]<-
ifelse(sim.dummies[[i]][j]==1,exp(pars[[3]][[i]][index]+rnorm(1,mean=0,sd=pars[[4]]
[[i]][index])),1)
}
}

sim.m000s<-rep(0,times=length(sim.dummies))
for(i in 1:length(sim.dummies)){
sim.m000s[i]<-m000.est(rand.pars[[i]])
}

sim.m000<-round(sum(sim.m000s*pars[[2]]))
invisible(sim.m000)
}



##~~~~~Bootstrap implementation~~~~~~~~~~~~~~~~~~~~~
results<-replicate(iterations,{

#Random population size for low-lying stratum
low.rand.size<-
median(replicate(sampled.quadrants,findInterval(runif(1),low.dist.size$y,rightmost.
closed=TRUE)))
low.rand.u5size<-
median(replicate(sampled.quadrants,findInterval(runif(1),low.dist.u5size$y,rightmos
t.closed=TRUE)))
low.rand.pop<-round(low.rand.size*low.quadrants)
low.rand.u5pop<-round(low.rand.u5size*low.quadrants)

#Random population size for hill stratum
rand.n.structures<-round(rnorm(1,est.structures,sd.structures))
hill.rand.size<-
median(replicate(sampled.structures,findInterval(runif(1),hill.dist.size$y,rightmos
t.closed=TRUE)))
hill.rand.u5size<-
median(replicate(sampled.structures,findInterval(runif(1),hill.dist.u5size$y,rightm
ost.closed=TRUE)))
hill.rand.pop<-round(rand.n.structures*hill.rand.size)
hill.rand.u5pop<-round(rand.n.structures*hill.rand.u5size)

#Total random population size
rand.pop<-low.rand.pop+hill.rand.pop
rand.u5pop<-low.rand.u5pop+hill.rand.u5pop

#Generating random number of uncaptured deaths
if (crc==1)
{uncaptured.deaths.rand<-uncaptured.deaths.gen(uncaptured.deaths.pars,m000.est)}
else
{uncaptured.deaths.rand<-0}

#Generating random number of uncaptured under 5 deaths
if (u5crc==1)
{uncaptured.u5deaths.rand<-
uncaptured.deaths.gen(uncaptured.u5deaths.pars,m000.est)}
else
{uncaptured.u5deaths.rand<-0}
```

```
#CMR and U5MR per 1 000 000 per day
cmr<-(captured.deaths+uncaptured.deaths.rand)*1000000/(rand.pop*recall.period)
u5mr<-
(captured.u5deaths+uncaptured.u5deaths.rand)*1000000/(rand.u5pop*recall.period)

iteration.results<-
c(low.rand.pop,low.rand.u5pop,hill.rand.pop,hill.rand.u5pop,rand.pop,rand.u5pop,cmr
,u5mr)
invisible(iteration.results)
}
)

results<-as.data.frame(t(results))
colnames(results)<-
c("low.pop","low.u5pop","hill.pop","hill.u5pop","pop","u5pop","cmr", "u5mr")

pop.point<-round(median(results$pop))
pop.lci<-round(quantile(results$pop,0.025))
pop.uci<-round(quantile(results$pop,0.975))

u5pop.point<-round(median(results$u5pop))
u5pop.lci<-round(quantile(results$u5pop,0.025))
u5pop.uci<-round(quantile(results$u5pop,0.975))

cmr.point<-round(median(results$cmr)/100,2) #per 10000 per day
cmr.lci<-round(quantile(results$cmr,0.025)/100,2)
cmr.uci<-round(quantile(results$cmr,0.975)/100,2)

u5mr.point<-round(median(results$u5mr)/100,2) #per 10000 per day
u5mr.lci<-round(quantile(results$u5mr,0.025)/100,2)
u5mr.uci<-round(quantile(results$u5mr,0.975)/100,2)

###~~~~~Results~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
results.summary<-
rbind(c("variable","point.est","95%lci","95%uci"),c("pop",pop.point,pop.lci,pop.uci
),c("under5.pop",u5pop.point,u5pop.lci,u5pop.uci),c("cmr",cmr.point,cmr.lci,cmr.uci
),c("u5mr",u5mr.point,u5mr.lci,u5mr.uci))
results.summary
```

Mae La Camp

```
rm(list=ls(all=TRUE))

###--->>>USER INPUT REQUIRED HERE<<<---###
##Enter data

#Mortality data
#Ascertained deaths
captured.deaths<-52
captured.u5deaths<-16
#Estimates of uncaptured deaths
uncaptured.deaths.pars<-dget("uncapturedmaela60d")
#Estimates of uncaptured under 5 deaths
uncaptured.u5deaths.pars<-dget("uncapturedu5maela60d")

#Population data
pop.est<-43794
u5pop.est<-5384
pop.sd=round(0.05*pop.est)
u5pop.sd=round(0.05*u5pop.est)

#Specify other parameters
recall.period<-60
crc<-1 ##0 means don't add estimate of uncaptured deaths to CMR
u5crc<-0 ##0 means don't add estimate of uncaptured deaths to U5MR
iterations<-1000
###--->>>USER INPUT ENDS<<<---###

##Functions that generate a random number of uncaptured deaths

#Function to calculate m000 based on model coefficients
m000.est<-function(pars){
  m111<-prod(pars)
  m110<-prod(pars[1],pars[2],pars[3],pars[5])
  m101<-prod(pars[1],pars[2],pars[4],pars[6])
  m100<-prod(pars[1],pars[2])
  m011<-prod(pars[1],pars[3],pars[4],pars[7])
  m010<-prod(pars[1],pars[3])
  m001<-prod(pars[1],pars[4])
  m000<-(m111*m100*m010*m001)/(m110*m101*m011)
  invisible(round(m000))
}
#Function to generate a random number of uncaptured deaths
uncaptured.deaths.gen<-function(pars,m000.est){

sim.dummies<-as.data.frame(pars[[1]])

rand.pars<-as.data.frame(array(0,c(7,length(sim.dummies))))
for(i in 1:length(sim.dummies)){
for(j in 1:length(sim.dummies[[1]])){
index<-sum(sim.dummies[[i]][1:j])
rand.pars[[i]][j]<-
ifelse(sim.dummies[[i]][j]==1,exp(pars[[3]][[i]][index]+rnorm(1,mean=0,sd=pars[[4]]
[[i]][index])),1)
}
}

sim.m000s<-rep(0,times=length(sim.dummies))
for(i in 1:length(sim.dummies)){
sim.m000s[i]<-m000.est(rand.pars[[i]])
}

sim.m000<-round(sum(sim.m000s*pars[[2]]))
invisible(sim.m000)
}


##~~~~~Bootstrap implementation~~~~~~~~~~~~~~~~~~~~~
```

```
results<-replicate(iterations,{

#Random population size
pop.rand<-round(pop.est+rnorm(n=1,mean=0,sd=pop.sd))
u5pop.rand<-round(u5pop.est+rnorm(n=1,mean=0,sd=u5pop.sd))

#Generating random number of uncaptured deaths
if (crc==1)
{uncaptured.deaths.rand<-uncaptured.deaths.gen(uncaptured.deaths.pars,m000.est)}
else
{uncaptured.deaths.rand<-0}

#Generating random number of uncaptured under 5 deaths
if (u5crc==1)
{uncaptured.u5deaths.rand<-
uncaptured.deaths.gen(uncaptured.u5deaths.pars,m000.est)}
else
{uncaptured.u5deaths.rand<-0}

#CMR and U5MR per 1 000 000 per day
cmr<-(captured.deaths+uncaptured.deaths.rand)*1000000/(pop.rand*recall.period)
u5mr<-
(captured.u5deaths+uncaptured.u5deaths.rand)*1000000/(u5pop.rand*recall.period)

iteration.results<-c(pop.rand,u5pop.rand,cmr,u5mr)

invisible(iteration.results)
}
)

results<-as.data.frame(t(results))
colnames(results)<-c("pop","u5pop","cmr", "u5mr")

pop.point<-round(median(results$pop))
pop.lci<-round(quantile(results$pop,0.025))
pop.uci<-round(quantile(results$pop,0.975))

u5pop.point<-round(median(results$u5pop))
u5pop.lci<-round(quantile(results$u5pop,0.025))
u5pop.uci<-round(quantile(results$u5pop,0.975))

cmr.point<-round(median(results$cmr)/100,2) #per 10000 per day
cmr.lci<-round(median(quantile(results$cmr,0.025))/100,2)
cmr.uci<-round(median(quantile(results$cmr,0.975))/100,2)

u5mr.point<-round(median(results$u5mr)/100,2) #per 10000 per day
u5mr.lci<-round(median(quantile(results$u5mr,0.025))/100,2)
u5mr.uci<-round(median(quantile(results$u5mr,0.975))/100,2)

###~~~~~Results~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

results.summary<-
rbind(c("variable","point.est","95%lci","95%uci"),c("pop",pop.point,pop.lci,pop.uci
),c("under5.pop",u5pop.point,u5pop.lci,u5pop.uci),c("cmr",cmr.point,cmr.lci,cmr.uci
),c("u5mr",u5mr.point,u5mr.lci,u5mr.uci))

results.summary
```

Chiradzulu District

```
rm(list=ls(all=TRUE))
library(survey)

###--->>>USER INPUT REQUIRED HERE<<<---###
#Input data on ascertained deaths and structure counts, per cluster
mortality.data<-read.csv("chiradzulu mortality summary data.csv")

#Specify which dataset you want to analyse
dataset<-mortality.data$all.30d

#Estimates of uncaptured deaths
uncaptured.deaths.pars<-dget("uncapturedchz30d")

#Mini-survey of structure size
size.survey<-read.csv("structure size survey.csv")

#Specify some parameters
recall.period<-30
age.group=0 ##0 means all ages, 1 means children under 5
crc<-1 ##0 means don't add estimate of uncaptured deaths
iterations<-1000 ##Number of iterations for bootstrap
###--->>>USER INPUT ENDS<<<---###


##Functions that generate a random number of uncaptured deaths

#Function to calculate m000 based on model coefficients
m000.est<-function(pars){
  m111<-prod(pars)
  m110<-prod(pars[1],pars[2],pars[3],pars[5])
  m101<-prod(pars[1],pars[2],pars[4],pars[6])
  m100<-prod(pars[1],pars[2])
  m011<-prod(pars[1],pars[3],pars[4],pars[7])
  m010<-prod(pars[1],pars[3])
  m001<-prod(pars[1],pars[4])
  m000<-(m111*m100*m010*m001)/(m110*m101*m011)
  invisible(round(m000))
}
#Function to generate a random number of uncaptured deaths
uncaptured.deaths.gen<-function(pars,m000.est){

sim.dummies<-as.data.frame(pars[[1]])

rand.pars<-as.data.frame(array(0,c(7,length(sim.dummies))))
for(i in 1:length(sim.dummies)){
for(j in 1:length(sim.dummies[[1]])){
index<-sum(sim.dummies[[i]][1:j])
rand.pars[[i]][j]<-
ifelse(sim.dummies[[i]][j]==1,exp(pars[[3]][[i]][index]+rnorm(1,mean=0,sd=pars[[4]]
[[i]][index])),1)
}
}

sim.m000s<-rep(0,times=length(sim.dummies))
for(i in 1:length(sim.dummies)){
sim.m000s[i]<-m000.est(rand.pars[[i]])
}

sim.m000<-round(sum(sim.m000s*pars[[2]]))
invisible(sim.m000)
}


##~~~Specifying survey designs~~~

#Specifying survey design for structure size minisurvey
size.survey.design<-
svydesign(id=~cluster,variables=NULL,data=size.survey,weights=size.survey$weight)
```

```
#Specifying survey design for mortality survey
used.data<-
data.frame(cluster=mortality.data$cluster,weight=mortality.data$weight,deaths=datas
et,structures=mortality.data$structures)
mortality.survey.design<-
svydesign(id=~cluster,variables=NULL,data=used.data,weights=used.data$weight)


##~~~Parameters for estimate of people per structure~~~

#Fitting Poisson model to parametrise distribution of people per structure,
adjusted for deff
glm.size<-svyglm(people~NULL, size.survey.design,family=poisson())
summary(glm.size)
ln.size.est<-summary(glm.size)$coefficients[1]
ln.size.se<-summary(glm.size)$coefficients[2]

#Fitting Poisson model to parametrise distribution of under 5 children per
structure, adjusted for deff
glm.u5size<-svyglm(under5s~NULL, size.survey.design,family=poisson())
summary(glm.u5size)
ln.u5size.est<-summary(glm.u5size)$coefficients[1]
ln.u5size.se<-summary(glm.u5size)$coefficients[2]


##~~~Parameters for mortality estimation~~~

##Structure count data
structures.est<-used.data$structures
structures.sd<-0.03*structures.est

##~~~~~Bootstrap implementation~~~~~~~~~~~~~

results<-replicate(iterations,{
#Generating random structure counts
structures.rand<-rep(0,times=length(structures.est))
for(i in
1:length(structures.est)){structures.rand[i]=round(structures.est[i]+rnorm(n=1,mean
=0,sd=structures.sd))}

#Fitting Poisson model to parametrise distribution of deaths per structure,
adjusted for deff
glm.deaths<-
svyglm(deaths~NULL,design=mortality.survey.design,family=poisson(),offset=log(struc
tures.rand))
ln.deaths.est<-summary(glm.deaths)$coefficients[1]
ln.deaths.se<-summary(glm.deaths)$coefficients[2]

#Generating random value of people per structure, depending on age group of
interest
if (age.group==0) {size.rand<-exp(rnorm(n=1,mean=ln.size.est,sd=ln.size.se))} else
{size.rand<-exp(rnorm(n=1,mean=ln.u5size.est,sd=ln.u5size.se))}

#Generating random number of captured deaths per structure
captured.deaths.rand<-exp(rnorm(n=1,mean=ln.deaths.est,sd=ln.deaths.se))

#Generating random number of uncaptured deaths
if (crc==1)
{uncaptured.deaths.rand<-uncaptured.deaths.gen(uncaptured.deaths.pars,m000.est)}
else
{uncaptured.deaths.rand<-0}

#Computing mortality rate as deaths per 10 000 person-days
ptime<-recall.period*sum(structures.rand)*size.rand
mr.rand<-
round(captured.deaths.rand*sum(structures.rand)+uncaptured.deaths.rand)*1000000/pti
me
```

```
#Computing total population sampled, considering age group of interest
pop.rand<-round(sum(structures.rand)*size.rand)

iteration.results<-c(mr.rand,pop.rand)
invisible(iteration.results)
}
)

results<-as.data.frame(t(results))
colnames(results)<-c("mr","pop")

mr.point<-round(median(results$mr))
mr.lci<-round(quantile(results$mr,0.025))
mr.uci<-round(quantile(results$mr,0.975))

pop.point<-round(median(results$pop))
pop.lci<-round(quantile(results$pop,0.025))
pop.uci<-round(quantile(results$pop,0.975))

###~~~~~Results~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
results.summary<-
rbind(c("var","point.est","95%lci","95%uci"),c("population",pop.point,pop.lci,pop.u
ci),c("rate",mr.point/100,mr.lci/100,mr.uci/100))

results.summary
```

Lugufu and Mtabila Camps

```
rm(list=ls(all=TRUE))

###--->>>USER INPUT REQUIRED HERE<<<---###
##Enter data

#Mortality data
#Ascertained deaths
captured.deaths<-63
captured.u5deaths<-33
#Estimates of uncaptured deaths
uncaptured.deaths.pars<-dget("uncapturedtz60d")
#Estimates of uncaptured under 5 deaths
uncaptured.u5deaths.pars<-dget("uncapturedu5tz60d")

#Population data
lugufu.pop.est<-38363
lugufu.u5pop.est<-7673
lugufu.pop.sd=round(0.05*lugufu.pop.est)
lugufu.u5pop.sd=round(0.05*lugufu.u5pop.est)

mtabila.pop.est<-41773
mtabila.u5pop.est<-8355
mtabila.pop.sd=round(0.05*mtabila.pop.est)
mtabila.u5pop.sd=round(0.05*mtabila.u5pop.est)

#Specify other parameters
recall.period<-60
crc<-0 ##0 means don't add estimate of uncaptured deaths to CMR
u5crc<-0 ##0 means don't add estimate of uncaptured deaths to U5MR
iterations<-1000
###--->>>USER INPUT ENDS<<<---###

##Functions that generate a random number of uncaptured deaths

#Function to calculate m000 based on model coefficients
m000.est<-function(pars){
  m111<-prod(pars)
  m110<-prod(pars[1],pars[2],pars[3],pars[5])
  m101<-prod(pars[1],pars[2],pars[4],pars[6])
  m100<-prod(pars[1],pars[2])
  m011<-prod(pars[1],pars[3],pars[4],pars[7])
  m010<-prod(pars[1],pars[3])
  m001<-prod(pars[1],pars[4])
  m000<-(m111*m100*m010*m001)/(m110*m101*m011)
  invisible(round(m000))
}
#Function to generate a random number of uncaptured deaths
uncaptured.deaths.gen<-function(pars,m000.est){

sim.dummies<-as.data.frame(pars[[1]])

rand.pars<-as.data.frame(array(0,c(7,length(sim.dummies))))
for(i in 1:length(sim.dummies)){
for(j in 1:length(sim.dummies[[1]])){
index<-sum(sim.dummies[[i]][1:j])
rand.pars[[i]][j]<-
ifelse(sim.dummies[[i]][j]==1,exp(pars[[3]][[i]][index]+rnorm(1,mean=0,sd=pars[[4]]
[[i]][index])),1)
}
}

sim.m000s<-rep(0,times=length(sim.dummies))
for(i in 1:length(sim.dummies)){
sim.m000s[i]<-m000.est(rand.pars[[i]])
}

sim.m000<-round(sum(sim.m000s*pars[[2]]))
invisible(sim.m000)
```

```
}

##~~~~~Bootstrap implementation~~~~~~~~~~~~~~~~~~~~~

results<-replicate(iterations,{

#Random population size
pop.rand<-round(lugufu.pop.est+rnorm(n=1,mean=0,sd=lugufu.pop.sd))+
round(mtabila.pop.est+rnorm(n=1,mean=0,sd=mtabila.pop.sd))
u5pop.rand<-round(lugufu.u5pop.est+rnorm(n=1,mean=0,sd=lugufu.u5pop.sd))+
round(mtabila.u5pop.est+rnorm(n=1,mean=0,sd=mtabila.u5pop.sd))


#Generating random number of uncaptured deaths
if (crc==1)
{uncaptured.deaths.rand<-uncaptured.deaths.gen(uncaptured.deaths.pars,m000.est)}
else
{uncaptured.deaths.rand<-0}

#Generating random number of uncaptured under 5 deaths
if (u5crc==1)
{uncaptured.u5deaths.rand<-
uncaptured.deaths.gen(uncaptured.u5deaths.pars,m000.est)}
else
{uncaptured.u5deaths.rand<-0}

#CMR and U5MR per 1 000 000 per day
cmr<-(captured.deaths+uncaptured.deaths.rand)*1000000/(pop.rand*recall.period)
u5mr<-
(captured.u5deaths+uncaptured.u5deaths.rand)*1000000/(u5pop.rand*recall.period)

iteration.results<-c(pop.rand,u5pop.rand,cmr,u5mr)

invisible(iteration.results)
}
)

results<-as.data.frame(t(results))
colnames(results)<-c("pop","u5pop","cmr", "u5mr")

pop.point<-round(median(results$pop))
pop.lci<-round(quantile(results$pop,0.025))
pop.uci<-round(quantile(results$pop,0.975))

u5pop.point<-round(median(results$u5pop))
u5pop.lci<-round(quantile(results$u5pop,0.025))
u5pop.uci<-round(quantile(results$u5pop,0.975))

cmr.point<-round(median(results$cmr)/100,2) #per 10000 per day
cmr.lci<-round(median(quantile(results$cmr,0.025))/100,2)
cmr.uci<-round(median(quantile(results$cmr,0.975))/100,2)

u5mr.point<-round(median(results$u5mr)/100,2) #per 10000 per day
u5mr.lci<-round(median(quantile(results$u5mr,0.025))/100,2)
u5mr.uci<-round(median(quantile(results$u5mr,0.975))/100,2)

###~~~~~Results~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

results.summary<-
rbind(c("variable","point.est","95%lci","95%uci"),c("pop",pop.point,pop.lci,pop.uci
),c("under5.pop",u5pop.point,u5pop.lci,u5pop.uci),c("cmr",cmr.point,cmr.lci,cmr.uci
),c("u5mr",u5mr.point,u5mr.lci,u5mr.uci))

results.summary
```

8.11. ECONOMIC FEASIBILITY COSTING

Table 29. Costing by Study Site

Staff	Kabul Hourly Rate (USD)	Thailand Hourly Rate (USD)	Chiradzulu Hourly Rate (USD)	Tanzania Camps Hourly Rate (USD)
Study investigators †	22.6	22.6	22.6	22.6
Other study staff *	5.25	4.5	264 * *	3.3
Collaborators *	5.40	9.9	5.7	3.5
Data collectors *	3.85	4.5	2.3	3.1
Drivers ***	4.71		27	2.2

Costs based on costs incurred during data collection for EM method. Same costs applied for survey estimations.
Costs for key informants, FGD participants, EM respondents and population respondents not included.
† Average cost for different study staff in each study site.
** In Chiradzulu a one-off payment was made to other study staff (household enumerators for population data) amounting to $264 in total.
*** Includes costs of vehicles hire, driver fees, petrol.
Exchange rates recorded on first data of data collection in each study site: 1 USD=50.20 Afghani; 1 USD=33.87 Thai Baht; 1 USD=143.29 Malawi Kwacha; 1 USD=1186.53 Tanzanian shillings.

Table 30. Assumptions for Time Inputs for a Retrospective Survey, by Study Site

Activity/Staff Type	District 1, Kabul	Mae La Camp	Chiradzulu District	Tanzania Camps
Preparation				
Investigators	Same as for EM	Same as for EM	Same as for EM	Same as for EM
Data collectors	Same as for EM	Same as for EM	Same as for EM	Same as for EM
Drivers	Same as for EM	Same as for EM	Same as for EM	Same as for EM
Collaborators	Same as for EM	Same as for EM	Same as for EM	Same as for EM
Population estimation	Not applicable for survey			
FGD	Not applicable for survey			
Training				
Investigators	1 investigator x 4 d	1 investigator x 4 d	1 investigator x 4 d	1 investigator x 4 d
Data collectors	12 interviewers (teams of 2) x 4 days	6 interviewers x 4 days	6 interviewers x 4 days	6 interviewers x 4 days
Data collection				
Investigators	person-time for data collectors / n of data collectors (12)	person-time for data collectors / n of data collectors (6)	person-time for data collectors / n of data collectors (6)	person-time for data collectors / n of data collectors (6)
Data collectors	1 h preparation/cluster 15 min/household 3 min to select each new household 2 data collectors /household	15 min/household 3 min to select each new household 1 data collector /household	1 h preparation/cluster 2 h drive/cluster 15 min/household questionnaire 3 min to select each new household 2 data collectors /household	15 min/household 3 min to select each new household 1 data collector /household

Key informants*	30 min x 1 informant/cluster	30 min x 22 section chiefs	30 min x 1 informant/cluster	30 min x 35 section chiefs
Drivers	2 drivers x person-time for study investigators	No driving necessary	2 drivers x person-time for study investigators	1 driver x person-time for study investigator
Respondents	1 person /household x 15 min	1 person /household x 15 min	1 person /household x 15 min	1 person /household x 15 min
Data entry/analysis				
Investigators	entry: 3 min /questionnaire + 20% double entry analysis: 1 investigator x 3 d	entry: 3 min /questionnaire + 20% double entry analysis: 1 investigator x 2 d	entry: 3 min /questionnaire + 20% double entry analysis: 1 investigator x 3 d	entry: 3 min /questionnaire + 20% double entry analysis: 1 investigator x 2 d
Report production				
Investigators	1 investigator x 2 d	1 investigator x 2 d	1 investigator x 2 d	1 investigator x 2 d

*To guide the team around the community.
Working day assumed to be 8 hours.
For general assumptions please see Section 3.5.1.

8.12. PROGRAM FOR CAPTURE-RECAPTURE ANALYSIS

```
### ~~~ R CODE FOR THREE-LIST CAPTURE-RECAPTURE ANALYSIS ~~~ ###

# Written by Francesco Checchi (October 2008), partly based on code supplied by
# Patrick Ball (Benetech Initiative)

rm(list=ls(all=TRUE))
library("MASS")
library("BMA")
library("leaps")

###--->>>USER INPUT NEEDED HERE<<<---###
###Enter data from three-list contingency table as an integer vector: x111, x110, x101, x100,
x011, x010, x001
kabuldata.60d<-c(1,20,1,45,3,9,3)
kabuldata.30d<-c(0,7,1,3,3,2,2)
thaidata.60d<-c(13,3,8,3,2,5,18)
thaidata.30d<-c(10,2,3,1,0,2,5)
kabuldatau5.60d<-c(1,4,1,14,1,3,2)
chzdata.60d<-c(27,40,5,21,0,5,12)
chzdata.30d<-c(9,17,3,7,0,1,5)
chzdatau5.60d<-c(7,12,1,6,0,1,3)
tzdata.60d<-c(15,6,6,17,3,11,5)
tzdata.30d<-c(7,3,3,8,2,4,1)
tzdatau5.60d<-c(9,3,3,7,0,7,4)
tzdatau5.30d<-c(2,1,1,4,0,3,1)
###Specify which dataset to analyse:
used.data<-tzdatau5.30d

###Choose plausibility cut-off criteria for models to be included in model averaging
procedure
plausibility.1<-100
#meaning that if the model predicted x000 (m000) is more than X times the total number of
#ascertained deaths, the model is discarded (note: default of 100 essentially means only
#models that do not converge are discarded
plausibility.2<-0.6
#meaning that if the model has a Pearson Chi-square P-value>X it is discarded
#on grounds of possible overfitting (note: this is optional; setting it >=1 means no models
#are discarded)
###--->>>USER INPUT ENDS<<<---###


###~~~LOG-LINEAR MODELLING APPROACH~~~~~~~~~~

##Model names
model.names<-c("no
interactions","l1xl2","l1xl3","l2xl3","l1xl2,l1xl3","l1xl2,l2xl3","l1xl3,l2xl3","l1xl2,l1xl3,
l2xl3")

##Coefficients for the 8 different models
x1 = c(1,1,1,1,0,0,0)
x2 = c(1,1,0,0,1,1,0)
x3 = c(1,0,1,0,1,0,1)
x12 = ifelse(x1==1 & x2==1, 1, 0)
x13 = ifelse(x1==1 & x3==1, 1, 0)
x23 = ifelse(x2==1 & x3==1, 1, 0)
indepvars = cbind(x1,x2,x3,x12,x13,x23)
rm(x1,x2,x3,x12,x13,x23)

##Formulas for the 8 different models
indep = "x ~ x1+x2+x3"
formulas = c(
  indep,
  paste(indep, "x12", sep="+"),
  paste(indep, "x13", sep="+"),
  paste(indep, "x23", sep="+"),
  paste(indep, "x12", "x13", sep="+"),
  paste(indep, "x12", "x23", sep="+"),
  paste(indep, "x13", "x23", sep="+"),
```

```
  paste(indep, "x12", "x13", "x23", sep="+"))
rm(indep)

##Function that applies log-linear model to each possible model
crc.models <- function(crc.data,formulas) {

crc.data <- as.data.frame(cbind(indepvars, crc.data))
colnames(crc.data) <- c(colnames(crc.data)[1:6], "x")

crc.glm <- function(formula){
glm(formula=as.formula(formula), family=poisson(), data=crc.data, maxit=1000)
}

models <- lapply(formulas, crc.glm)
return(models)
}

##Calling the above function so as to fit the 8 models:
models <- crc.models(used.data,formulas)


##Computing point estimates of m000 for each model

#Function to calculate m000 based on model coefficients
m000.est<-function(pars){
  m111<-prod(pars)
  m110<-prod(pars[1],pars[2],pars[3],pars[5])
  m101<-prod(pars[1],pars[2],pars[4],pars[6])
  m100<-prod(pars[1],pars[2])
  m011<-prod(pars[1],pars[3],pars[4],pars[7])
  m010<-prod(pars[1],pars[3])
  m001<-prod(pars[1],pars[4])
  m000<-(m111*m100*m010*m001)/(m110*m101*m011)
  invisible(round(m000))
}

dummies<-as.data.frame(cbind(c(1,1,1,1,0,0,0), c(1,1,1,1,1,0,0), c(1,1,1,1,0,1,0),
c(1,1,1,1,0,0,1), c(1,1,1,1,1,1,0), c(1,1,1,1,1,0,1), c(1,1,1,1,0,1,1), c(1,1,1,1,1,1,1)))

pars.point<-as.data.frame(array(0,c(7,length(models))))
for(i in 1:length(models)){
for(j in 1:length(dummies[[1]])){
index<-sum(dummies[[i]][1:j])
pars.point[[i]][j]<-ifelse(dummies[[i]][j]==1, exp(models[[i]]$coefficients[index]), 1)
}
}
point.estimates<-rep(0,times=length(models))
for(i in 1:length(models)){
point.estimates[i]<-m000.est(pars.point[[i]])
}


##Computing profile 95% confidence intervals for m000 for each model

pars.lci<-as.data.frame(array(0,c(7,length(models))))
pars.uci<-as.data.frame(array(0,c(7,length(models))))

#Obtaining profile CIs for each independent variable for each model
cis1<-rep(NA,times=7)
cis2<-rep(NA,times=7)
cis3<-rep(NA,times=7)
cis4<-rep(NA,times=7)
cis5<-rep(NA,times=7)
cis6<-rep(NA,times=7)
cis7<-rep(NA,times=7)
cis8<-rep(NA,times=7)

cis1<-confint(models[[1]],level=0.95)
```

```
cis2<-confint(models[[2]],level=0.95)
cis3<-confint(models[[3]],level=0.95)
cis4<-confint(models[[4]],level=0.95)
cis5<-confint(models[[5]],level=0.95)
cis6<-confint(models[[6]],level=0.95)
cis7<-confint(models[[7]],level=0.95)
cis8<-confint(models[[8]],level=0.95)

cis<-list(cis1,cis2,cis3,cis4,cis5,cis6,cis7,cis8)

for(i in 1:length(models)){
for(j in 1:length(dummies[[1]])){
index<-sum(dummies[[i]][1:j])
pars.lci[[i]][j]<-ifelse(dummies[[i]][j]==1, exp(cis[[i]][index]), 1)
}
for(j in 1:length(dummies[[1]])){
index<-sum(dummies[[i]])+sum(dummies[[i]][1:j])
pars.uci[[i]][j]<-ifelse(dummies[[i]][j]==1, exp(cis[[i]][index]), 1)
}
}

lcis<-rep(0,times=length(models))
for(i in 1:length(models)){
lcis[i]<-m000.est(pars.lci[[i]])
}

ucis<-rep(0,times=length(models))
for(i in 1:length(models)){
ucis[i]<-m000.est(pars.uci[[i]])
}


##Computing degrees of freedom for each model
dfs<-rep(0,times=length(models))
for(i in 1:length(models)){
dfs[i]<- models[[i]]$df.residual
}

##Computing Pearson Chi-square probabilities for each model
pearson.chi.probs<-rep(0,times=length(models))
for(i in 1:length(models)){
pearson.chi.probs[i]<-
round(pchisq(models[[i]]$deviance,models[[i]]$df.residual,lower.tail=FALSE),digits=3)
}
for(j in 1:length(pearson.chi.probs)){
if (is.nan(pearson.chi.probs[j])==TRUE) pearson.chi.probs[j]=1
}

##Computing the Bayesian Information Criterion for each model
bics<-rep(0,times=length(models))
for(i in 1:length(models)){bics[i]<-models[[i]]$deviance-
models[[i]]$df.residual*log(sum(used.data))}
bics<-round(bics,digits=2)


##Computing the Pearson-adjusted Chi square for each model
pearsons<-rep(0,times=length(models))
for(i in 1:length(models)){
pearsons[i]<- round(models[[i]]$deviance/models[[i]]$df.residual,digits=2)
}


###~~~BAYESIAN MODEL AVERAGING~~~~~~~~~~~

##Selecting eligible models for averaging, based on plausibility criteria supplied by user
possible.models<-
data.frame(as.data.frame(rbind(c(1,1,1,0,0,0),c(1,1,1,1,0,0),c(1,1,1,0,1,0),c(1,1,1,0,0,1),c(
```

```
1,1,1,1,1,0),c(1,1,1,1,0,1),c(1,1,1,0,1,1),c(1,1,1,1,1,1))),point.estimates,pearson.chi.probs
,rep(1,times=length(models)))
colnames(possible.models)<-c("x1","x2","x3","x12","x13","x23","point","pchi","eligible")

for(i in 1:length(possible.models$eligible)){
if (possible.models$point[i]>(plausibility.1*sum(used.data)) |
possible.models$pchi[i]>plausibility.2) possible.models$eligible[i]=0
}
selected.models<-subset(possible.models,eligible==1,select=c(x1,x2,x3,x12,x13,x23))

n.models<-sum(possible.models$eligible)


##Implementing BMA
bma.output<-
glib(y=used.data,x=indepvars,error="poisson",link="log",phi=1,models=selected.models)

bma.dummies<-as.data.frame(rbind(rep(1,times=n.models),t(selected.models)))

#Calculating posterior probabilities of averaged models from their BICs
#note: not using values given by glib package but rather calculating based on Raftery (1995)
#calculating model degrees of freedom (bma.dummies[[i]]
bma.dfs<-rep(0,times=n.models)
for(i in 1:n.models){bma.dfs[i]<-length(bma.dummies[[i]])-sum(bma.dummies[[i]])}
#calculating model BICs
bma.bics<-rep(0,times=n.models)
for(i in 1:n.models){bma.bics[i]<-bma.output$bf$deviance[i]-bma.dfs[i]*log(sum(used.data))}
#calculating model posterior probabilities
bma.totprob<-sum(exp(bma.bics*(-0.5)))
bma.postprobs<-rep(0,times=n.models)
for(i in 1:n.models){bma.postprobs[i]<-exp(bma.bics[i]*(-0.5))/bma.totprob}

##Recalculating point estimates of m000 for each BMA model

#Obtaining coefficients of averaged models
bma.coefficients<-bma.output$glim.est$coef

bma.pars.point<-as.data.frame(array(0,c(7,n.models)))
for(i in 1:n.models){
for(j in 1:length(bma.dummies[[1]])){
index<-sum(bma.dummies[[i]][1:j])
bma.pars.point[[i]][j]<-ifelse(bma.dummies[[i]][j]==1, exp(bma.coefficients[[i]][index]), 1)
}
}

bma.point.estimates<-rep(0,times=n.models)
for(i in 1:n.models){
bma.point.estimates[i]<-m000.est(bma.pars.point[[i]])
}

##Recalculating 95%CIs of m000 for each BMA model, this time in a simpler fashion
##i.e multiplying SEs x 2

#Obtaining standard errors of coefficients of averaged models
bma.ses<-bma.output$glim.est$se

bma.pars.lci<-as.data.frame(array(0,c(7,n.models)))
for(i in 1:n.models){
for(j in 1:length(bma.dummies[[1]])){
index<-sum(bma.dummies[[i]][1:j])
bma.pars.lci[[i]][j]<-ifelse(bma.dummies[[i]][j]==1, exp(bma.coefficients[[i]][index]-
2*bma.ses[[i]][index]), 1)
}
}

bma.lcis<-rep(0,times=n.models)
for(i in 1:n.models){
bma.lcis[i]<-m000.est(bma.pars.lci[[i]])
```

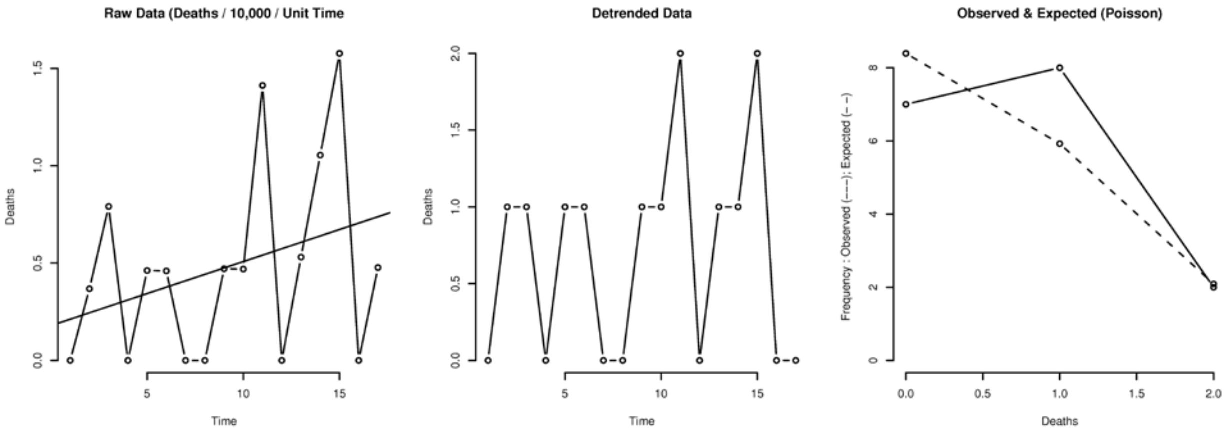
```
}

bma.pars.uci<-as.data.frame(array(0,c(7,n.models)))
for(i in 1:n.models){
for(j in 1:length(bma.dummies[[1]])){
index<-sum(bma.dummies[[i]][1:j])
bma.pars.uci[[i]][j]<-ifelse(bma.dummies[[i]][j]==1,
exp(bma.coefficients[[i]][index]+2*bma.ses[[i]][index]), 1)
}
}

bma.ucis<-rep(0,times=n.models)
for(i in 1:n.models){
bma.ucis[i]<-m000.est(bma.pars.uci[[i]])
}


##Computing BMA estimates of m000

bma.m000.est<-round(sum(bma.point.estimates*bma.postprobs))
bma.m000.lci<-round(sum(bma.lcis*bma.postprobs))
bma.m000.uci<-round(sum(bma.ucis*bma.postprobs))
results.bma<-data.frame(bma.m000.est,bma.m000.lci,bma.m000.uci)
colnames(results.bma)<-c("m000_est","m000_lci","m000_uci")


###~~~ALTERNATIVES TO LOG-LINEAR MODELLING~~~~~~~~~~~

###Creating dataset for simple alternatives
simple.reshape<-function(d){
#x10 x01 and x11 for list 1 vs list 2; then for list 1 vs list 3; then for list 2 vs list 3
d12<-c(d[3]+d[4],d[5]+d[6],d[1]+d[2])
d13<-c(d[2]+d[4],d[5]+d[7],d[1]+d[3])
d23<-c(d[2]+d[6],d[3]+d[7],d[1]+d[5])
dall<-c(d12,d13,d23)
invisible(dall)
}
data.simple<-simple.reshape(used.data)

### ~~~ Alternative approach (Marks, Seltzer and Krotki) ~~~ #
##m000=(x100x010 + x100x001 + x010x001)/(x110 + x101 + x011)

m000=(used.data[4]*used.data[6]+used.data[4]*used.data[7]+used.data[6]*used.data[7])/(used.da
ta[2]+used.data[3]+used.data[5])
results.marks<-round(m000)

### ~~~ Simple approach (only two list comparisons) ~~~ #
##m00=x10x01/x11

#List 1 vs List 2
l1l2<-round(data.simple[1]*data.simple[2]/data.simple[3])
#List 1 vs List 3
l1l3<-round(data.simple[4]*data.simple[5]/data.simple[6])
#List 2 vs List 3
l2l3<-round(data.simple[7]*data.simple[8]/data.simple[9])

results.simple<-cbind(c("list 1 vs list 2", "list 1 vs list 3", "list 2 vs list 3"),
c(l1l2,l1l3,l2l3))


###~~~ESTIMATING SENSITIVITY OF THE THREE LISTS
#Numbers of events captured by any list or all lists combined
list.1<-used.data[1]+used.data[2]+used.data[3]+used.data[4]
list.2<-used.data[1]+used.data[2]+used.data[5]+used.data[6]
list.3<-used.data[1]+used.data[3]+used.data[5]+used.data[7]
all.lists<-sum(used.data)

#Estimated total number of events, with 95%CI
```

```
all.est<-results.bma+all.lists

#Estimated sensitivities, with 95%CI
sens.list.1<-round(100*list.1/all.est,digits=1)
sens.list.2<-round(100*list.2/all.est,digits=1)
sens.list.3<-round(100*list.3/all.est,digits=1)
sens.all.lists<-round(100*all.lists/all.est,digits=1)

sensitivities<-data.frame("List.1_n"=list.1,"List.1_sens     "=paste("   ",sens.list.1[[1]],
"% (",sens.list.1[[3]],"-",sens.list.1[[2]],")",sep=""), "List.2_n"=list.2,"List.2_sens
"=paste("   ",sens.list.2[[1]], "% (",sens.list.2[[3]],"-",sens.list.2[[2]],")",sep=""),
"List.3_n"=list.3,"List.3_sens     "=paste("   ",sens.list.3[[1]], "% (",sens.list.3[[3]],"-
",sens.list.3[[2]],")",sep=""), "All.Lists_n"=all.lists,"All.Lists_sens     "=paste("
",sens.all.lists[[1]], "% (",sens.all.lists[[3]],"-",sens.all.lists[[2]],")",sep=""))

###~~~DISPLAYING RESULTS~~~

##Essential results from each log-linear model
results.glm<-
data.frame(model.names,point.estimates,lcis,ucis,dfs,pearson.chi.probs,bics,pearsons,possible
.models$eligible)
colnames(results.glm)<- c("model","m000_point","m000_lci","m000_uci","df","Chi2 P-
value","BIC","Pearson adj Chi2","eligible?")
#m000_est is the estimated number of uncaptured cases, m000_lci and m000_uci are the lower
and upper 95%CIs
#other parameters: residual degrees of freedom, P-value for model, Bayesian Information
#Criterion, adjusted Chi2 (Chi2/df), whether model is eligible for BMA

##Displaying results

#Results of log-linear modelling approach
results.glm

#Results of Bayesian Model Averaging
summary(bma.output,n.models=n.models)

#Actual BICs of the averaged models:
round(bma.bics,digits=2)

#Actual posterior probabilities of the averaged models:
round(bma.postprobs,digits=3)

#Estimated m000 based on Bayesian Model Averaging:
results.bma

#Estimated m000 based on Marks et al. method (simple approach):
results.marks

#Estimated m000 based on two-list pair comparison method (simple approach):
results.simple

#Total estimated number of events:
all.est

#Sensitivity of the different lists and all lists combined:
sensitivities



###~~~SAVING PARAMETERS FOR BOOTSTRAPPING ESTIMATION OF MORTALITY RATES~~~
sim.pars<-list(bma.dummies,bma.postprobs,bma.coefficients,bma.ses)
dput(sim.pars,"sim.pars")
```

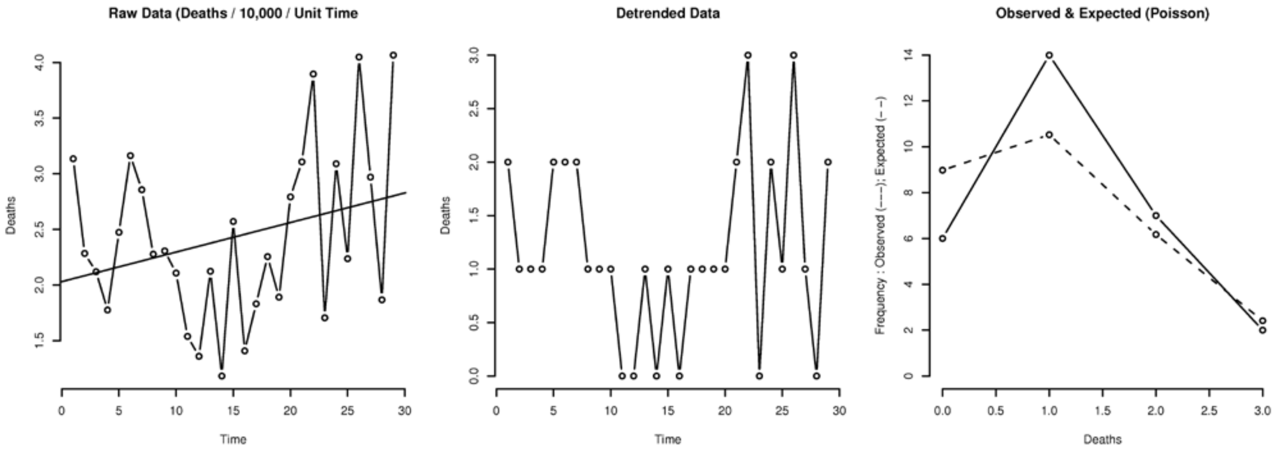
8.13. VERIFICATION OF THE POISSON ASSUMPTION ON THE DISTRIBUTION OF DEATH COUNTS OVER TIME

Each panel shows, from left to right, (i) observed mortality rates with linear trend, (ii) detrended data, and (iii) the frequencies of death counts per time unit, both observed and expected based on the Poisson assumption.

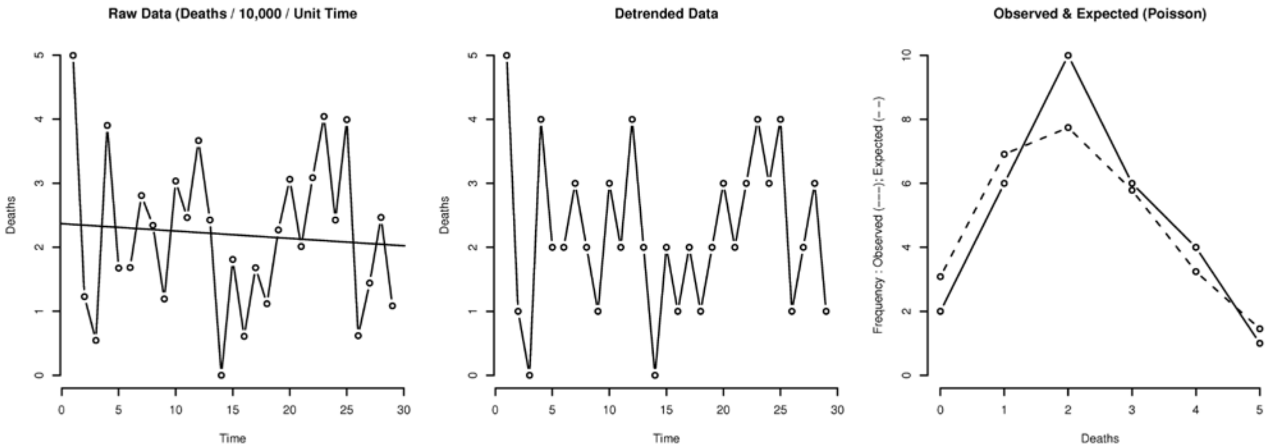
Fugnido, Ethiopia



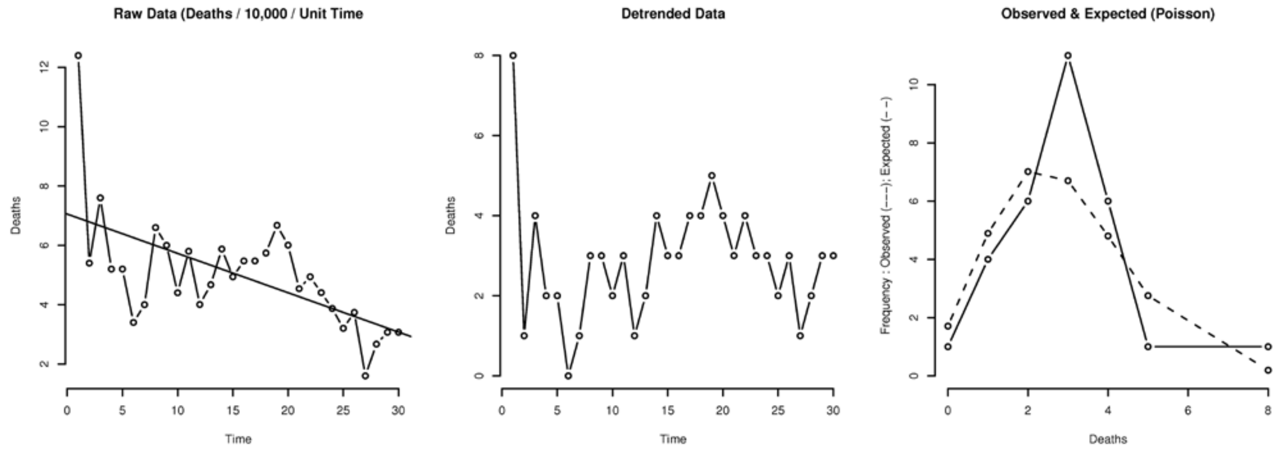
Lugufu, Tanzania



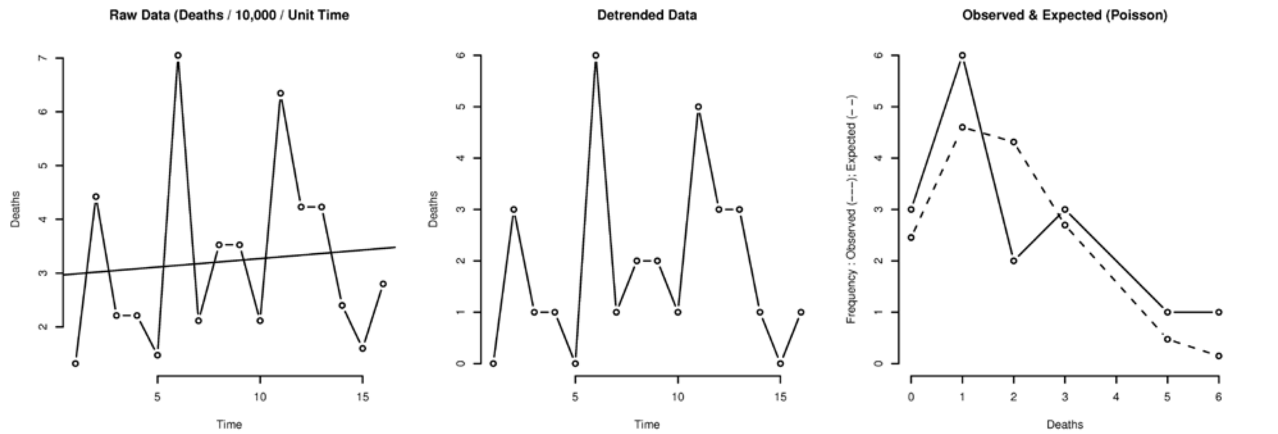
Mtabila, Tanzania



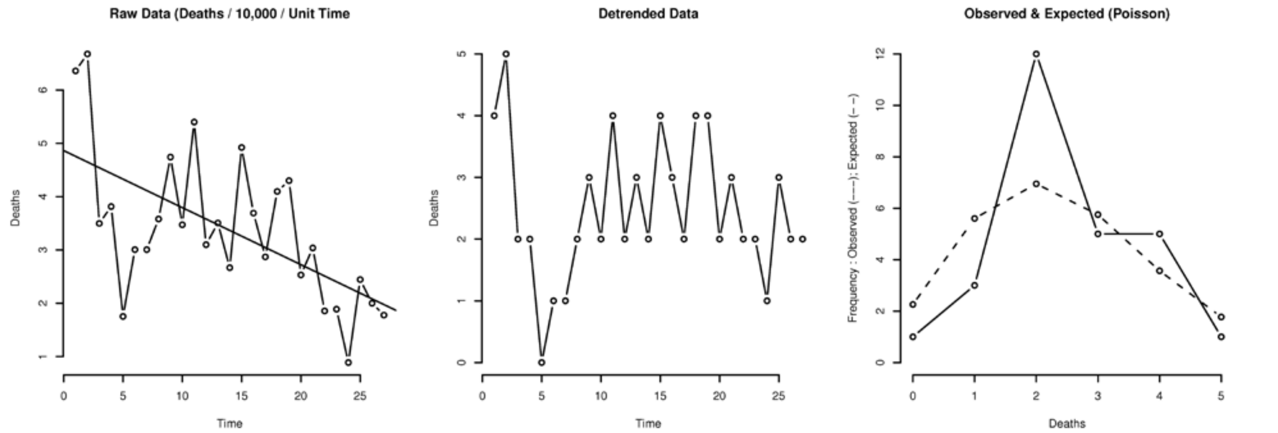
Murnei, West Darfur



Niertiti, West Darfur



Zalingei, West Darfur



8.14. SUMMARY RESULTS OF MATHEMATICAL SIMULATIONS

Exhaustive Scenario

Standard = 0.5 / 10,000 / day

	Sequential sampling		Rate & CI		Poisson test	
PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
100,000	67.70	85.92	49.88	94.73	83.11	69.36
500,000	86.56	91.22	77.45	96.40	89.29	88.31
1,000,000	90.41	93.19	84.25	97.18	91.24	92.37
2,000,000	92.75	94.34	88.45	97.27	93.57	93.41
3,000,000	93.76	95.00	90.34	97.45	94.12	94.61
5,000,000	94.63	95.36	92.11	97.30	94.85	95.16
10,000,000	95.35	95.71	93.63	97.24	95.45	95.56

Standard = 1.0 / 10,000 / day

	Sequential sampling		Rate & CI		Poisson test	
PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
100,000	77.65	88.22	63.28	95.58	81.77	84.53
500,000	90.38	93.00	84.22	97.00	91.16	92.15
1,000,000	92.77	94.47	88.54	97.47	93.54	93.49
2,000,000	94.22	95.12	91.35	97.42	94.52	94.68
3,000,000	94.90	95.62	92.61	97.46	95.16	95.31
5,000,000	95.43	95.74	93.71	97.29	95.54	95.53
10,000,000	96.01	95.78	94.83	96.96	96.07	95.70

Standard = 2.0 / 10,000 / day

	Sequential sampling		Rate & CI		Poisson test	
PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
100,000	84.93	90.68	74.69	96.39	87.93	87.67
500,000	92.83	94.26	88.58	97.26	93.54	93.32
1,000,000	94.32	95.01	91.45	97.33	94.59	94.54
2,000,000	95.27	95.74	93.29	97.33	95.40	95.60
3,000,000	95.64	95.82	94.10	97.32	95.75	95.70
5,000,000	95.88	95.75	94.71	96.91	95.94	95.69
10,000,000	96.24	95.75	95.42	96.70	96.29	95.72

Standard = 4.0 / 10,000 / day

	Sequential sampling		Rate & CI		Poisson test	
PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
100,000	89.13	92.78	82.33	96.98	90.89	90.29
500,000	94.30	95.01	91.25	97.30	94.65	94.56
1,000,000	95.34	95.61	93.35	97.19	95.47	95.47
2,000,000	95.82	95.75	94.44	97.13	95.91	95.69
3,000,000	96.15	95.95	95.06	96.95	96.17	95.87
5,000,000	96.10	95.82	95.29	96.72	96.12	95.79
10,000,000	96.31	95.69	95.75	96.39	96.33	95.66

Standard = 8.0 / 10,000 / day

	Sequential sampling		Rate & CI		Poisson test	
PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
100,000	92.02	93.95	87.30	97.21	92.92	93.01
500,000	95.26	95.45	93.25	97.29	95.40	95.27
1,000,000	95.79	95.65	94.39	96.98	95.86	95.58
2,000,000	96.00	96.04	95.07	96.98	96.04	95.99
3,000,000	96.32	95.89	95.50	96.79	96.35	95.86
5,000,000	96.34	95.98	95.80	96.67	96.35	95.96
10,000,000	96.42	95.79	96.04	96.23	96.43	95.79

Sampling Scenario

Standard = 0.5 / 10,000 / day

		Sequential sampling		Rate & CI		Poisson test	
PSUs	Mean PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
30	349,539	83.76	90.47	73.33	96.09	87.06	87.30
45	521,125	87.18	92.18	78.52	96.90	89.91	89.21
60	662,509	88.63	92.86	81.06	97.32	91.02	90.26

Standard = 1.0 / 10,000 / day

		Sequential sampling		Rate & CI		Poisson test	
PSUs	Mean PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
30	364,700	89.22	93.28	82.15	97.15	91.25	91.01
45	527,569	91.14	93.92	85.06	97.61	91.99	93.07
60	702,041	92.35	95.00	87.19	98.06	93.26	94.27

Standard = 2.0 / 10,000 / day

		Sequential sampling		Rate & CI		Poisson test	
PSUs	Mean PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
30	358,085	92.22	94.86	87.04	97.90	93.07	94.04
45	518,270	93.62	95.68	89.40	98.26	94.29	94.88
60	698,913	94.32	96.43	90.70	98.68	94.68	96.01

Standard = 4.0 / 10,000 / day

		Sequential sampling		Rate & CI		Poisson test	
PSUs	Mean PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
30	346,408	94.29	96.31	90.52	98.41	94.71	95.86
45	532,935	95.24	97.01	92.30	98.73	95.55	96.69
60	699,001	95.72	97.38	93.11	98.90	95.96	97.21

Standard = 8.0 / 10,000 / day

		Sequential sampling		Rate & CI		Poisson test	
PSUs	Mean PDAR	Sensitivity	Specificity	Sensitivity	Specificity	Sensitivity	Specificity
30	345,267	95.57	97.17	93.02	98.85	95.79	96.96
45	540,343	96.28	97.62	94.11	99.00	96.44	97.49
60	709,203	96.59	98.01	94.63	99.11	96.69	97.90