# Bias in Epidemiological Studies of Conflict Mortality[*]

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**ABSTRACT:**

Cluster sampling has recently been used to estimate the mortality in various conflicts around the world. The Burnham et al. study on Iraq employs a new variant of this cluster sampling methodology. The stated methodology of Burnham et al. is to (1) select a random main street, (2) choose a random cross street to this main street, and (3) select a random household on the cross street to start the process. The authors show that this new variant of the cluster sampling methodology can introduce an unexpected, yet substantial, bias into the resulting estimates as such streets are a natural habitat for patrols, convoys, police stations, road-blocks, cafes and street-markets. This bias comes about because the residents of households on cross-streets to the main streets are more likely to be exposed to violence than those living further away. Here the authors develop a mathematical model to gauge the size of the bias and use the existing evidence to propose values for the parameters that underlie the model. The research suggests that the Burnham et al. study of conflict mortality in Iraq may represent a substantial overestimate of mortality. Indeed, the recently published Iraq Family Health Survey covered virtually the same time period as the Burnham et al. study, used census-based sampling techniques and produced a central estimate for violent deaths that was one fourth of the Burnham et al. estimate. The authors provide a sensitivity analysis to help readers to tune their own judgements on the extent of this bias by varying the parameter values. Future progress on this subject would benefit from the release of high-resolution data by the authors of Burnham et al. study.

## Introduction

Recent studies of conflict mortality, such as the one on Iraq (Burnham et al., 2006) survey

households using a cluster sampling methodology. Cluster sampling itself is not

problematic but the micro-level details on how households are selected at the final stage of

sampling are crucial and widely overlooked (see Appendix 1 for details). As described by

the EPI sampling methodology (e.g., Spiegel & Salama, 2000; Depoortere et al., 2004;

Coghlan et al., 2006), these studies often initiate the sampling process from some easily

accessible geographical feature, such as the centre of a village, in order to economize

resources and ensure staff safety. The stated procedures in Burnham et al. (2006) call for

selecting a 'constituent administrative unit' and then selecting a main street from 'a list of

all main streets. A residential street was then randomly selected from a list of residential streets crossing the main streets.' (Figures 1 & 2). The field team would enumerate the households on the street, select one at random and initiate the interviewing from this household, proceeding to 39 further 'adjacent' households.[1] This *cross-street sampling algorithm* (CSSA) introduced by Burnham et al. (2006) is a new variant of the final stage of the EPI sampling methodology. In this article we examine the potential bias that can arise from the cross-street sampling algorithm.

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For conflicts like the one in Iraq, violent events tend to be focused around cross-streets since they are a natural habitat for patrols, convoys, police stations, parked cars, road-blocks, cafes and street-markets.[2] Major highways would not offer such a wide range of potential targets -- nor would secluded neighbourhoods (Figure 2, Gourley et al., 2006). Note that although interviews may progress away from the initial household on a cross street to a main street, such progress is limited by the number of adjacent households visited, in this case 39, in moving from one household to the next one (Figure 2).

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Figure 2 in here

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Here we gauge the potential bias resulting from the cross-street sampling algorithm. This bias is an example of noncoverage bias, which in turn is a special case of nonresponse bias (Cochran, 1977; Thompson, 1997). Such bias is notoriously difficult to assess; Cochran (1977: 361), summarizes that 'We are left in the position of relying on some guess about the size of the bias, without data to substantiate the guess.' There are three main approaches in the literature to assess such noncoverage bias, namely weighting (for an overview see Groves, 1989), modelling (Little, 1982), and imputation (Rubin, 1987). We apply a modelling approach since the data that has been released by the authors of Burnham et. al (2006) so far is insufficient for either weighting or imputation.

The structure of the article is as follows. In the next section we present our model and propose a set of parameter values for it that we believe are reasonable for the Burnham et al. (2006) study based on the information that has been released. The model and estimated parameter values suggest that the study has considerably overestimated conflict mortality in Iraq. We then discuss the mechanics of the model and elaborate further on the meaning of the parameters. Next we show how the results of the model vary with the underlying parameters. After the conclusion we offer two appendices. In the first we give background on cluster sampling and the final-stage sampling methods that have been applied in Burnham et al. (2006) and some other recent conflict surveys. We derive our sampling-bias formula in the second appendix.

## Model and parameter estimation

The cross-street sampling algorithm of Burnham et al. (2006) divides the underlying population into two distinct groups, namely, those who can be sampled under the CSSA methodology, and those who cannot. Using the following model we estimate the exposure to violence for each group and quantify the potential bias resulting from the CSSA.

Let us consider a population of size $N$, where $N_i$ people reside in households *inside* the survey space (denoted $S_i$), which means that they are reachable through the selection scheme; $N_o = N - N_i$ people reside in households *outside* the survey space (denoted $S_o$) and are hence unreachable (e.g. Figure. 2). Note that $S_i$ and $S_o$ can be spatially fragmented and inter-dispersed. Daily human movement is modelled via the model parameter $f_i$, the probability of an $S_i$ resident being present in $S_i$, and $f_o$, the probability of an $S_o$ resident being present in $S_o$. Probabilities of death for anyone present in $S_i$ or $S_o$ are, respectively, $q_i$ and $q_o$, regardless of the location of the households of these individuals. We define the *bias factor R* as the ratio of the expected number of deaths obtained by restricting the survey to $S_i$ households to the expected number of deaths in the entire population (i.e. $S_i$ and $S_o$); in the context of nonresponse bias similar approaches can be found, for example, in Kish & Hess (1958), and in Groves (1989). Setting $q = q_i / q_o$ and $n = N_o / N_i$ (see Appendix 2 for a derivation) we obtain

$$R = \frac{(1+n)(1+qf_i-f_i)}{(q-1)(f_i-f_on)+qn+1}.$$

(1)

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Figure 3 in here

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Figure 3 shows the parameter regimes where $R>1$ and $R<1$. For the Iraq study (Burnham et al., 2006) the following regimes are likely:

(1) The relative probability of death for *anyone present* in $S_i$ (regardless of their zone of residence) to that of $S_o$ is $q=q_i/q_o$. It is likely that the streets that define the samplable region $S_i$ are sufficiently broad and well-paved for military convoys and patrols to pass, are highly suitable for street-markets and concentrations of people and are, therefore, prime targets for improvised explosive devices, car bombs, sniper attacks, abductions and drive-by shootings. Given the extent and frequency of such attacks, a value of $q=5$ is plausible. Indeed, many cities worldwide have homicide rates which vary by factors of ten or more between adjacent neighbourhoods (Gourley et al., 2006).

(2) The proportion of population resident in $S_o$ to that resident in $S_i$ is $n=N_o/N_i$. Street layouts in Iraq are mostly irregular, hence the cross-street sampling algorithm will miss any

neighbourhood not in the immediate proximity of a cross-street (Fig. 1(a) and (b)). Analysis of Iraqi maps suggests $n = 10$ is plausible (Gourley et al., 2006).

(3) Intuitively, the probability $f_i$ is roughly the average fraction of time spent by residents of $S_i$ in $S_i$. Similarly, $f_o$ is roughly the average fraction of time spent by residents of $S_o$ in $S_o$. Given the nature of the violence, travel is limited; women, children and the elderly tend to stay close to home. Consequently, mixing of populations between the zones is minimal. Using the time people spend in their homes as a lower bound on the time they must spend within their zones, we can obtain rough estimates for $f_i$ and $f_o$. Assuming that there are two working-age males per average household of seven (Burnham et al., 2006), with each spending six hours per 24-hour day outside their own zone, yields $f_i = f_o = 5/7 + 2/7 \times 18/24 = 13/14$. Here we emphasize that $f_i$ and $f_o$ refer to the fractions of time spent *anywhere* within $S_i$ and $S_o$ respectively, i.e., they are *not* simply the fractions of time spent at home. Likewise, the respective probabilities $q_i$ and $q_o$ of being killed refer to the probability of people being killed *anywhere* within their zone, i.e., they are *not* simply probabilities of being killed at home.[3] In other words, our model makes no assumption whatsoever with regards to people being killed in their homes or not.

These specific values yield $R = 3.0$ (Figure 3), suggesting that the Iraq estimate (Burnham et al., 2006) provides a substantial overestimate. Burnham et al. (2006) assume that R=1. If R=3 actually is the true ratio, then they over-estimate the number of deaths by a factor of 3. Indeed, the survey of Iraq Family Health Study Group (2008) covered virtually the same time period as the Burnham et al. (2006) study, used census-based sampling techniques and

produced a central estimate for violent deaths that was one fourth of the Burnham et al. (2006) estimate. Of course, other parameter values, and other values of $R$, are possible. In Section 4 we provide a sensitivity analysis to clarify how $R$ varies with the underlying parameters. A lack of precise information about the implementation in Burnham et al., (2006) and hence values for $f_i$, $f_o$, $q$ and $n$, prevents a firmer quantification at this stage -- nor do we rule out the existence of additional biases in the Burnham et al., (2006) study. Conflict surveys are undoubtedly difficult, and may necessitate adapted methodologies. For this reason, quantitative tools such as Eq. (1) should prove invaluable in gauging any unexpected biases resulting from the cross-street sampling algorithm.

## Discussion of the model

Our model has been designed to be as simple as possible while capturing the relevant aspects of the bias phenomenon. It could be argued that the value of the parameter $q$, which is a ratio of the probabilities of being killed in the two zones, might differ between types of individuals, such as working-age males and children and, in addition, these values should perhaps depend on the time of the day. As there is presently insufficient information to estimate these aspects of the problem, we decided in favour of simplicity. Consequently, our parameters are to be viewed as averaged over time and over different types of individuals (see footnote 2).

We now examine the behaviour of Equation (1) in some detail. The 'no-bias' limit is equivalent to setting $R = 1$ in the above equation, corresponding to those values of the

parameters $q$, $n$, and $f$ that result in the same expected number of deaths for sampling from $S_i$ only and for sampling from both $S_i$ and $S_o$. In other words, under these circumstances sampling only from $S_i$ yields an unbiased estimate of the underlying population death rate and, therefore, sampling only from $S_i$ would be justified. After simplification it follows that $R = 1$ if and only if $n(q-1)(2f-1) = 0$, yielding altogether three different solutions, namely, $n = 0$ (independently of the values of $q$ and $f$), $q = 1$ (independently of $n$ and $f$), and $f = 1/2$ (independently of $q$ and $n$).

- The solution $n = N_o/N_i = 0$ corresponds to the entire population being in the samplable region ($N_o = 0$).

- The solution $q = q_i/q_o = 1$ corresponds to having equal death rates in the samplable and non-samplable region ($q_i = q_o$).

- The solution $f = 1/2$ yields $R = 1$ regardless of the values of $q$ and $n$ and corresponds to perfect mixing of populations between the zones. This means the entire population divides its time evenly between the two zones.

The last two solutions, $q = 1$ and $f = 1/2$, are interesting conceptually. In general, the interpretation of $q$ and $f$ can be recast in terms of localization of violence and people, respectively. Localization of violence is captured by the condition $q \neq 1$. If $q = 1$, violence is not localized in either the samplable or the non-samplable region, but is uniformly present everywhere, yielding $R = 1$. However, if $q \neq 1$, violence becomes localized and predominates in either of the two regions. In particular, when $q > 1$ the samplable region $S_i$ has a higher rate of violence than the non-samplable region $S_o$. Similarly, localization of

people is captured by the condition $f \neq 1/2$, since if $f = 1/2$, people are equally likely to be in either subsystem regardless of where they are resident, so residence loses its meaning. In particular, as $f \rightarrow 1$, people are increasingly more localized in their residential areas.

Qualitatively speaking, the bias in this framework emerges from having simultaneously partial localization of violence and partial localization of people. Both of these conditions are needed for the bias to emerge, since if $f = 1/2$, we have $R = 1$ regardless of the values of $q$ and $n$, and if $q = 1$ we have $R = 1$ regardless of the values of $n$ and $f$. To illustrate the idea of localization, one could suggest that the spreading of an airborne disease corresponds to $q = 1$, since everyone would have a similar chance of being infected. Similarly, if the movement of people were unconstrained, corresponding to $f = 1/2$, the time people spend in low or high violence zones would be uncorrelated with the location of their residence. With the suggested parameter values, $q = 5$ and $f = 13/14$, it is clear that both violence and people are highly localized and, consequently, a bias is introduced.

## Sensitivity analysis

In this section we conduct a sensitivity analysis of the model, which allows us to determine how sensitive the bias factor $R$ is to variations in parameters. Such analysis is especially important since the details of the implementation followed in Burnham et al. (2006) are unclear and the authors have not released data with sufficient resolution to resolve the ambiguity regarding appropriate parameter values.

The bias factor $R = R(f_i, f_o, q, n)$ given by Eq. (1) depends on four parameters, i.e., it is a function from a subset of $R^4$ to a subset of $R^1$. In what follows we explore the sensitivity of the model to different parameter values. Note that the regions of the parameter space that are plausible depend on the context in which the model is applied. Since it is not possible to visualize $R$ plotting its range versus its domain, we focus below on some of the regions of the parameter space that result in an over-estimate $(R > 1)$. We emphasize that some of the explored parameter values are not appropriate for the present study, but are shown here for the purpose of exposing the model to a wide readership. However, if the details and high-resolution data for Burnham et al. (2006) are disclosed in the future, it will be possible to obtain estimates for $q$ and $n$.

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Table I in here

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In Table I we tabulate the values of $R$ for different values of the parameters $(q, n, f_i, f_o)$. The values of $q$ vary along the main horizontal axis over the set $\{2, 4, 6\}$, and $n$ varies over the main vertical axis over the set $\{4, 8, 12\}$. Parameters $f_i$ and $f_o$ vary within each panel along the minor horizontal and minor vertical axes, respectively, running over the set $\{0.6, 0.7, 0.8, 0.9, 1.0\}$.

## Conclusion

In this article we have examined the final stage of the sampling procedures stated in Burnham et al. (2006), here referred to as the cross-street sampling algorithm (CSSA), in

which sets of interviews were initiated from random cross streets to random main streets. We argue that such locations are particular targets for violent attacks such as car bombs, drive- by shootings, attacks on patrols, street-market bombings and abductions. Proceeding to 39 further adjacent households, interviewers could only progress a relatively short distance from the initial starting point (Figure 2). Consequently, the interviewers include households whose residents, because of their location, are more likely to be exposed to violence than those residing elsewhere. We model the potential bias resulting from these final stages of the sampling procedure and derive a simple formula that can be used to both gauge and adjust for the bias. We suggest plausible values for the parameters underlying the model and give justification for them. We conclude that the bias may be quite large. This conclusion is bolstered by Iraq Family Health Survey Study Group (2008) which suggests that the Burnham et al. (2006) study may have overestimated violent mortality in Iraq by even a factor of four. We perform a sensitivity analysis on the parameter values to help readers form their own judgements. Release of high-resolution data by the authors of Burnham et al. (2006) would facilitate progress on the issue of bias.

## Appendix 1: Cluster sampling and the EPI method

Cluster sampling methodology has been applied frequently in recent years to estimate conflict mortality (e.g., Spiegel & Salama, 2000; Depoortere et al., 2004; Coghlan et al., 2006). Cluster sampling offers substantial benefits relative to surveying alternatives such as simple random sampling (see Thompson, 2002 for an overview). Simple random sampling of households at a national level requires a complete national list of households

from which a sample is then drawn at random.[4] Even when this is feasible the households that are selected will be widely scattered so that it will cost much time and money for field teams to visit all of them. Moreover, travel is risky during an ongoing conflict, so high travel time translates into high risk. Under household cluster sampling, in contrast, researchers select groups of households in close proximity to one another, reducing travel time between households. Another key advantage of this approach is that it can proceed without a full national listing of households; household lists can be developed after the selection of lower-level sampling units. Indeed, the SMART Methodology (2006: 35 & 52), an important attempt to standardize epidemiological surveys of mortality and nutrition in emergency situations, states simply that cluster sampling is applied when researchers lack a sufficiently complete household listing.[5] A third reason for employing cluster sampling is that this method can be designed so as to lower sampling variance (Thompson 2002: 138), although the SMART Methodology (2006: 36) would practically rule out such sampling techniques as violating a principle, stressed by this handbook, that each household should have an equal chance of selection. In practice cluster sampling is used primarily for reasons of convenience, practicality and safety. These are important concerns and cluster sampling is a vital and useful tool in conflict mortality surveys.

Since the absence of a reliable national listing of households is prime motivation for using cluster sampling a large and unresolved issue remains; how do researchers locate the households to be interviewed? Burnham et al. (2006) proceeded as follows according to their stated methodology. They used population estimates of Governorates (analogous to provinces, counties or states) to allocate clusters to Governorates, with the number of

clusters roughly proportional to estimated populations. They choose as locations of these clusters 'constituent administrative units' (CAUs) within each Governorate, where the CAUs were selected proportional to their estimated population; a CAU may receive more than one cluster. We have already discussed how at the next stage a random cross street to a random main street was selected. The field team would enumerate the households on the street, select one at random from this newly created list and initiate the interviewing from this household, proceeding to 39 further 'adjacent' households. The key point here is that this procedure requires a listing of households only at its final stage, after a cross street to a main street has already been selected. The sampling procedure is economical.

While the specific street-off-the-main-street scheme of Burnham et al. (2006), which we have referred to in the article as the cross-street sampling algorithm, is unusual for a conflict mortality study, it is really a variation on a last-stage sampling approach known as the EPI method, which has been used increasingly in conflict mortality studies in recent years (e.g., Spiegel & Salama, 2000; Depoortere et al., 2004; Coghlan et al., 2006). The experimental properties of this method, originally designed to measure vaccination coverage, are poorly understood at present.[6] Yet its easy applicability makes it a highly attractive option for survey researchers. Under this approach one can draw a sample from, for example, a village by going to the village center, spinning a pen or bottle, walking in the direction the bottle points to the edge of the village, enumerating the households along the way and choosing one of them at random for the first interview. In an urban environment movement in a random direction from the center of a cluster must be consistent with the street layout. The approach of Burnham et al. (2006) is a logical extension of the EPI

method to such an environment. In this case the center of the village or refugee camp corresponds to the main street and the selected cross street corresponds to the random direction.

The SMART Methodology (2006: 57) notes that the standard EPI approach when applied to a circular village gives higher selection probability to households near the center than to households near the edge and suggests a variation on the usual approach. Under this modification a team follows one randomly chosen direction from the center to the edge of a village and then chooses another random direction back into the interior. The team enumerates households and sampling along this second direction into the interior. Again, experimental comparisons of this method against other sampling alternatives would be welcome.

## Appendix 2: Derivation of the model

We consider a constant population size with $N_i + N_o = N$. The probability of an $S_i$ ( $S_o$) resident being present in $S_i$ ( $S_o$) at a given point in time is $f_i$ ( $f_o$). Since $N_i/(N_i + N_o)$ and $N_o/(N_i + N_o)$ give the probabilities that a randomly chosen person is resident in $S_i$ or $S_o$, it follows that $q_i f_i N_i/(N_i + N_o)$ is the probability that a randomly chosen person is resident in $S_i$ and gets killed in $S_i$, whereas $q_i(1 - f_o)N_o/(N_i + N_o)$ is the probability that the person is resident in $S_o$ and gets killed in $S_i$. Similarly, $q_o(1 - f_i)N_i/(N_i + N_o)$ is the probability that a randomly chosen person is resident in $S_i$ and gets killed in $S_o$, while $q_o f_o N_o/(N_i + N_o)$ is

the probability that the person is resident in $S_o$ and gets killed in $S_o$. Hence the probability that a randomly chosen person gets killed, is

$$\frac{q_i f_i N_i + q_i(1-f_o)N_o + q_o(1-f_i)N_i + q_o f_o N_o}{N_i + N_o} = \frac{(q_i-q_o)(f_i N_i - f_o N_o) + q_i N_o + q_o N_i}{N_i + N_o}.$$

Therefore the expected number of deaths in a population of size $N$ is

$$(q_i-q_o)(f_i N_i - f_o N_o) + q_i N_o + q_o N_i. \tag{2}$$

By contrast, the probability that a randomly chosen person who is a resident of $S_i$ gets killed is

$$q_i f_i + q_o(1-f_i).$$

Hence the expected number of deaths for a population of size $N$, based on the death rate for $S_i$ only, would be

$$(N_i + N_o)[q_i f_i + q_o(1-f_i)]. \tag{3}$$

The ratio of these expectations (Eq. (3) divided by Eq. (2)) defines the *bias factor*:

$$R = \frac{(N_i + N_o)[q_i f_i + q_o(1-f_i)]}{(q_i-q_o)(f_i N_i - f_o N_o) + q_i N_o + q_o N_i}. \tag{4}$$

For surveys that only sample from $S_i$, $R>1$ suggests an overestimate of conflict mortality on average, whereas $R<1$ suggests an underestimate on average. Assuming that $N_i \neq 0$ and $q_o \neq 0$, and setting $q = q_i/q_o$ and $n = N_o/N_i$ in Eq. (4), yields Eq. (1) in the main text:

$$R = R(f_i, f_o, q, n) = \frac{(1+n)(1+qf_i-f_i)}{(q-1)(f_i-f_o n) + qn + 1}.$$

Hence the bias factor $R$ depends only on $f_i$, $f_o$ and the ratios $q = q_i/q_o$ and $n = N_o/N_i$.

When $f_i = f_o = f$, Eq. (1) simplifies to

$$R = R(f,q,n) = \frac{(1+n)(1+qf-f)}{f(q-1)(1-n)+qn+1}. \tag{5}$$

The 'no-bias' limit of $R=1$ requires either (1) $n=0$ (i.e. $N_o = 0$) implying no individual is resident outside the survey space $S_i$, or (2) $q=1$ (i.e. $q_i = q_o$) implying equal death rates inside and outside the survey space, or (3) $f = 1/2$ which suggests that residents of $S_i$ spend on average 12 hours per day in $S_o$, and vice versa. Although permissible mathematically, these solutions would be difficult to justify for a conflict like the one in Iraq (see the discussion in the article). Setting $R(f,q,n) = r$ for general $r$ and solving for $q$ in terms of $n$ and $f$, yields
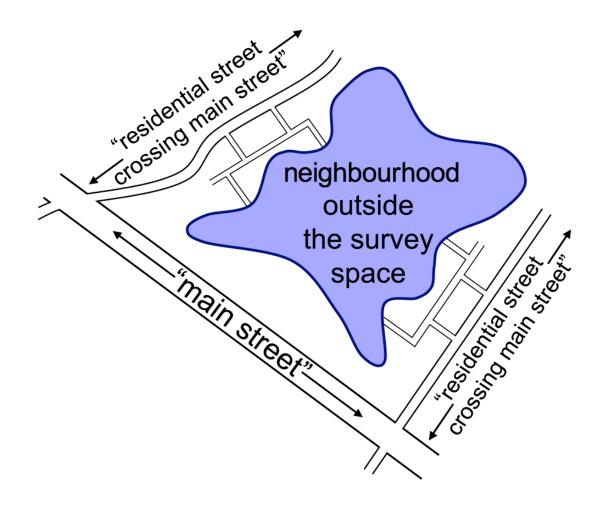
$$q(f,n,r) = \frac{f(1+n+nr-r)+r-n-1}{f(1+n+nr-r)-nr}. \tag{6}$$

In general the location of the contour $R(f,q,n) = r$ in Fig. 1(c) will depend on the mobility factor $f$, except for the special case $R(f,q,n) = 1$, which is independent of $f$.

Figure 1. Example of Iraqi Street Layout in Baghdad



Source: GoogleEarth™ at http://earth.google.com/).

Figure 2.  Schematic Diagram of Sampling Scheme



The 'street-off-main-street' selection criterion (footnote 1) can miss neighbourhoods with lower conflict mortality.
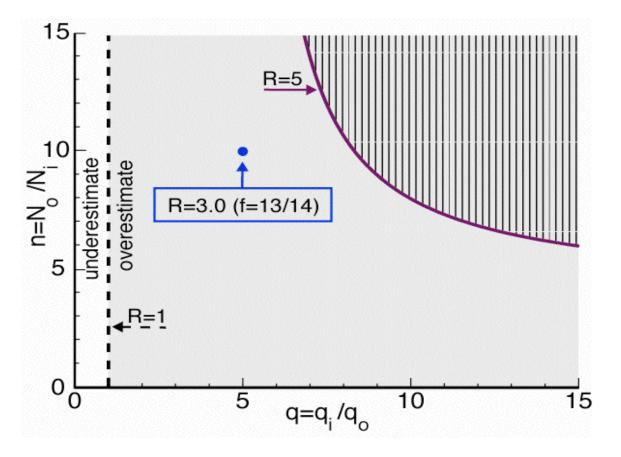
Figure 3. Bias for Baseline and Other Parameters.



Dashed vertical line separates regions with bias factor $R > 1$ (shaded, grey) and $R < 1$ (unshaded). In the shaded region, $R$ can exceed five ( $f = 1$ on the contour with vertical lines). Parameter values in main text yield $R = 3.0$ (solid circle). Here $f_i = f_o = f$.

Table I.  Sensitivity Analysis in Tabular Form



|  | q=2 | q=4 | q=6 |
|---|---|---|---|
| **n=12** | 1.53 1.61 1.70 1.78 1.86<br>1.41 1.48 1.56 1.64 1.71<br>1.30 1.37 1.44 1.52 1.59<br>1.21 1.28 1.34 1.41 1.48<br>1.13 1.19 1.26 1.32 1.38 | 2.46 2.67 2.87 3.06 3.25<br>1.98 2.16 2.33 2.49 2.65<br>1.65 1.81 1.96 2.10 2.24<br>1.42 1.56 1.69 1.82 1.94<br>1.25 1.37 1.48 1.60 1.71 | 3.25 3.55 3.82 4.09 4.33<br>2.36 2.60 2.83 3.04 3.25<br>1.86 2.05 2.24 2.42 2.60<br>1.53 1.70 1.86 2.01 2.17<br>1.30 1.44 1.59 1.72 1.86 |
| **n=8** | 1.50 1.58 1.65 1.73 1.80<br>1.38 1.46 1.53 1.60 1.67<br>1.29 1.35 1.42 1.49 1.55<br>1.20 1.26 1.33 1.39 1.45<br>1.12 1.19 1.25 1.31 1.36 | 2.33 2.51 2.68 2.85 3.00<br>1.91 2.07 2.22 2.36 2.50<br>1.62 1.75 1.89 2.02 2.14<br>1.40 1.52 1.65 1.76 1.87<br>1.24 1.35 1.46 1.56 1.67 | 3.00 3.24 3.46 3.67 3.86<br>2.25 2.45 2.65 2.83 3.00<br>1.80 1.98 2.14 2.30 2.45<br>1.50 1.65 1.80 1.94 2.08<br>1.29 1.42 1.55 1.68 1.80 |
| **n=4** | 1.43 1.49 1.55 1.61 1.67<br>1.33 1.39 1.45 1.51 1.56<br>1.25 1.31 1.36 1.42 1.47<br>1.18 1.23 1.29 1.34 1.39<br>1.11 1.16 1.22 1.27 1.32 | 2.06 2.18 2.30 2.40 2.50<br>1.75 1.87 1.98 2.08 2.17<br>1.52 1.63 1.73 1.83 1.92<br>1.35 1.45 1.55 1.64 1.72<br>1.21 1.30 1.39 1.48 1.56 | 2.50 2.65 2.78 2.89 3.00<br>2.00 2.14 2.27 2.39 2.50<br>1.67 1.80 1.92 2.04 2.14<br>1.43 1.55 1.67 1.77 1.88<br>1.25 1.36 1.47 1.57 1.67 |

Value of the bias factor $R$ for different values of parameters $(q,n,f_i,f_o)$. Each of the $3 \times 3$ panels in the table corresponds to a fixed set of values for $q = q_i/q_o$ and $n = N_o/N_i$. The values of $f_i$ increase from left to right, and those of $f_o$ from bottom to top, running over the set {0.6, 0.7, 0.7, 0.8, 0.9, 1.0}, as implied by the arrows in the bottom-left panel.

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[1] 'The third stage consisted of random selection of a main street within the administrative unit from a list of all main streets. A residential street was then randomly selected from a list of residential streets crossing the main street. On the residential street, houses were numbered and a start household was randomly selected. From this start household, the team proceeded to the adjacent residence until 40 households were surveyed.' (Burnham et al., 2006).

[2] These ideas were first put forward by some authors of this article in Bohannon (2006).

[3] See the page entitled 'clarifications' of Gourley et al. (2006) for details on the parameters $f_i$ and $f_o$.

[4] For simplicity we will write of national surveys but all the same arguments apply to sub-national surveys.

[5] This goes too far in our opinion since, as we indicate, there are other reasons for using cluster sampling even when other methods are feasible.

[6] For example, SMART Methodology (2006: 56), recommends that the EPI method only be used in cases where simple or systematic random sampling is impossible, suggesting that the EPI method

'results in a somewhat biased sample… There have not been sufficient studies where the two sampling methods have been compared at "cluster" level … to determine the extent to which this bias influences the results of the survey… research to determine the extent of the bias introduced into nutritional and mortality surveys is urgently needed.'