

# Sampling Designs for Xerophthalmia Prevalence Surveys

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**Background.** The purpose of this study was to estimate the bias and design effects associated with the Expanded Program on Immunization's (EPI) sampling design when estimating xerophthalmia prevalence, and to estimate the savings associated with EPI in terms of distance travelled within selected clusters.

**Methods.** Computer simulation of the EPI sampling strategy was done using maps from a xerophthalmia survey of 40 wards in Sarlahi district, Nepal. Samples of fixed cluster sizes of 7, 10, 15, 20 and 25 were compared. The estimated prevalence using the EPI design was compared with the true prevalence in the 40 wards to estimate the bias. The design effect was estimated by taking the ratio of the variance under EPI sampling to that of stratified random sampling (SRS) with fixed cluster sizes. The EPI was also modified by increasing the distance between selected houses from nearest neighbour to skipping 1–4 houses between selected ones. The difference between the distance travelled within clusters using SRS compared with EPI was weighed against the bias and increased variance.

**Results.** The prevalence of xerophthalmia was 2.8%. The EPI design overestimated xerophthalmia prevalence by between 0.27% and 1.16%. The design effects of EPI cluster sampling within wards varied between 0.73 and 1.35. Neither the bias nor the design effect was related to distance between households or cluster size. Distance travelled within wards was always less for EPI designs with cluster sizes of 7 or 10. There was no saving in terms of distance travelled for designs with cluster sizes from 15 to 25 if there were two or more houses between selected ones. For fixed cluster sizes of 15 or fewer, the EPI sampling design using nearest or next nearest neighbours is a better choice than SRS in terms of minimizing the distance travelled and the mean square error.

**Conclusions.** The choice of an optimum method would need to account for the density of clusters and difficulty of travel between clusters, as well as the costs of travel within clusters. Based on certain assumptions, EPI with 15 children per cluster would be favoured over examining all children in selected wards unless the travel time between wards was more than 2 days.

**Keywords:** sampling, survey, vitamin A deficiency, xerophthalmia, Nepal

Xerophthalmia is the leading cause of childhood blindness worldwide.<sup>1–3</sup> Children with xerophthalmia have been shown to be at increased risk of mortality,<sup>4</sup> morbidity,<sup>5–7</sup> and poor growth.<sup>8</sup> In areas where xerophthalmia is a public health problem, vitamin A supplementation with capsules or through fortification can reduce mortality by an average of 23% and avert between one and two million deaths annually among children aged 1–4 years.<sup>9,10</sup> A number of existing national vitamin A supplementation programmes have been given renewed urgency by these findings, while new national programmes are beginning in other countries. Although xerophthalmia is only the tip of the iceberg in terms of underlying subclinical vitamin A deficiency, it is a relatively easy and non-invasive way to assess whether

vitamin A deficiency is a public health problem. Furthermore, a decline in xerophthalmia prevalence is one way to measure the impact of programmes. A xerophthalmia prevalence survey can be done to assess whether a problem exists. Such a survey can also act as a baseline from which to judge the progress of programmes through a comparison with follow-up surveys. For these purposes, ways in which to reduce the complexity, cost and time taken to complete surveys would be useful.

The Expanded Programme on Immunization (EPI) of the World Health Organization (WHO) advocates a sampling procedure for surveys of immunization coverage that does not require mapping or a census of selected clusters.<sup>11–19</sup> The purpose of this procedure is to make it simple enough to be used by health workers with minimal technical support in field settings. The approach has been adapted for surveys of many other health outcomes such as nutritional status and for morbidity surveys.<sup>20,21</sup> However, EPI has the potential

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to lead to increased variance and bias of the coverage or disease prevalence estimates.<sup>22–25</sup> In this study, we use maps drawn for a baseline xerophthalmia survey in Nepal to simulate stratified random sampling (SRS) and EPI sampling of this population so as to obtain estimates of the bias and increased variance associated with EPI sampling for xerophthalmia surveys. These estimates are then used, in conjunction with the difference between the distance travelled within clusters using EPI and SRS, to make recommendations about the optimal sampling design for xerophthalmia surveys.

## MATERIALS AND METHODS

### *Source of Data*

The data used in this study come from a baseline survey of xerophthalmia undertaken prior to a randomized community trial of vitamin A supplementation in Sarlahi district, Nepal.<sup>26</sup> Forty wards in 25 Village Development Committees were selected at random from the larger sample of 261 wards in 29 Village Development Committees that were included in the trial. A ward is a well defined administrative unit, with nine wards comprising each Village Development Committee. The mean number of households per ward was 72.7. The mean number of children per ward was 119.1, and the mean number of children per household was 1.6. Within neighbourhoods, houses are relatively close (5–10 metres on average), but there are often several neighbourhoods within wards that can be spaced up to half a kilometre apart. Sarlahi district is located in the lowland terai area bordering Bihar, and was selected because it was identified as an area with high rates of xerophthalmia in two previous national surveys.<sup>27,28</sup> The selected wards were mapped, all houses were numbered, and children under 5 years old were censused by interviewers who visited each household. Parents were invited to bring eligible children to a central site in the ward for an eye examination. Parents were asked about night blindness using local terms. An ophthalmologist or senior ophthalmic assistant examined the eyes for signs of xerophthalmia which were graded according to the WHO definition.<sup>29</sup> Prior inter-observer trials with 1001 children showed excellent agreement between the two graders, with a kappa statistic of 0.93.

### *Sampling Designs*

The comparison of all sampling designs assumed that the primary sampling units (wards) had already been selected proportionate to ward size, and that a fixed number of children would then be selected in varying ways from each ward. The first design selected a fixed number of children at random from each of the 40

wards, thereby guaranteeing equal probability of selection within each ward. Other simulations used the EPI cluster design or modifications of this design to select a fixed number of children per ward. The EPI cluster sampling method is described in detail in the WHO manual.<sup>11</sup> Briefly, the interviewer goes to the centre of the village and selects a direction at random (spinning a bottle has been suggested). The houses along this direction are numbered from the centre to the periphery and one is chosen at random to be the starting house in the sample. All children of eligible age are identified in this house and immunization coverage information is obtained. The interviewer then goes to the next nearest house to the right of the front door of the starting house. If a house has no eligible children, the next nearest house is visited. This continues until the immunization status of at least seven children has been determined. All children in selected houses are surveyed so that a cluster may contain more than seven children.

### *Simulation of EPI Sampling*

The maps of the 40 wards were digitized and the x-y coordinates of each house were stored in computerized format with the associated identifiers of the preschool children in that house. The coordinates were selected relative to some arbitrary origin. The coordinates of all the households were then averaged to identify an approximate centre of each ward. This location was then set as the origin and the household coordinates were adjusted to reflect this origin. A direction was chosen at random from the origin. A pair of parallel straight lines of a fixed width was drawn from the centre to the periphery. This was done because houses in the computerized database were dimensionless locations, and unless a path of a certain width was drawn, the likelihood of a house falling exactly on the line would be very low. After the starting house was selected, the next nearest neighbour from  $+90^\circ$  to  $-90^\circ$  was selected. This was done in order to simulate the EPI rule of going to the next nearest house to the right when facing North. Once the edge of the village was reached, the direction of search was rotated  $90^\circ$  to the right and selected the next nearest house on this path. If the selected house had already been visited or if the house did not have any eligible children, the next nearest house to its right was visited. Since xerophthalmia is a much rarer outcome than positive immunization status, fixed cluster sizes within each ward were varied to include 7, 10, 15, 20 and 25 children selected per ward by either SRS or EPI cluster sampling. If the ward had fewer children than the requisite number per ward, then all children in that ward were included. Only three wards had fewer than 25 children. Modifications to the EPI approach increased

the distance of households from nearest neighbour to the selection of every second, third, fourth and fifth house to the right of each selected house.

#### *Estimation of Prevalence and Design Effects*

The true prevalence was estimated by dividing the number of cases of xerophthalmia in the 40 wards by the total number of children examined. In an actual survey in which the primary sampling units (wards) were selected proportionate to size, the sample would be self-weighting. However, in this simulated study, there has been no selection proportionate to size, so the comparison of interest is between the 'true' prevalence in the 40 wards and the estimated prevalence based on sampling a fixed number of children in each of the 40 wards. Therefore the prevalence of xerophthalmia for each simulation sample was calculated by taking the average of the ward prevalences weighted by the ward size. One thousand simulated samples were drawn for each sampling design and each sample size. The mean prevalence and the empiric variance of the 1000 samples were calculated for each sampling design (variance =  $\sum (p_i - p)^2/999$  where  $p_i$  is the prevalence in the  $i$ th ward and  $p$  is the average prevalence across the 1000 simulations). The bias of the EPI design was estimated by taking the difference between the 'true' prevalence in the 40 wards and the mean prevalence of the 1000 samples selected according to EPI cluster sampling.

The design effect was calculated by taking the ratio of the variance from EPI sampling to that of stratified random selection of a fixed sample size per ward. This design effect is a measure of the extra variation generated by sampling a cluster or local neighbourhood of houses within a ward rather than sampling children at random within wards, where children would tend to be scattered across the ward. The mean square error associated with EPI sampling was calculated by taking the variance of EPI sampling plus the square of the bias. The mean square error of SRS was estimated by taking the variance under simple random sampling, since there should be no bias for this sampling design.

#### *Estimation and Comparison of Distance Travelled*

For EPI sampling, the simulated surveyor started at the centre of the ward, travelled in a straight line to the first selected house, and then travelled sequentially to each next nearest house to the right. The total distance travelled in the ward was calculated by adding the straight line distances along the path just described. This included visits to houses where no eligible children resided. For simple random sampling, the distance was calculated by taking the shortest straight line path from the first randomly selected house to the next nearest

house and from there to the next nearest house until all randomly selected houses had been visited. This excluded visits to houses without children because they were not included in the sampling frame. Because the maps of each ward were not drawn to the same scale, the ratio of the total distance travelled for the 1000 simulated EPI samples to that of SRS was calculated separately for each ward. The median of the ward ratios was used to compare the overall distances travelled using EPI sampling to that using SRS. The median was chosen because the distribution of the distance ratios was skewed to the right.

To compare sampling designs in terms of the trade-off between distance travelled and mean square error, the EPI mean square error multiplied by the median distance travelled under EPI sampling was divided by the SRS mean square error multiplied by the distance travelled under SRS. This assumes the cost of doubling the mean square error is the same as doubling the time. If this ratio was less than one then EPI would be favoured over SRS in terms of less distance travelled for comparable mean square error. In order to evaluate designs in terms of increased variance alone, the ratios of the EPI variance multiplied by distance to that of SRS variance multiplied by SRS distance was calculated. If this ratio was less than one, the EPI approach would be favoured over SRS in terms of less distance travelled for comparable variance.<sup>30</sup>

An alternative to cluster sampling using EPI or SRS would be to examine all children in selected villages, since the cost of travel to villages is greater than the cost of travel within villages. The cost of these different survey designs can be calculated in days to complete the survey using the following formula:  $mn/x + mt_1 [(n/y) - 1] + mt_2$  where  $m$  is the number of clusters,  $n$  is the number of children examined per cluster,  $x$  is the number of children that can be examined per day,  $y$  is the number of children per household,  $t_1$  is the average travel time between houses in days, and  $t_2$  is the average travel time between villages in days. The total sample size,  $N = nm$ , varies according to the design effect of cluster sampling, which is greater the larger the cluster size.<sup>31,32</sup>

## RESULTS

A total of 4920 households were censused in the 40 wards, and 57.4% of households were identified as having children under 5 years of age. The number of children under 5 living in these households was 4765. Of these, 4297 (90.2%) were interviewed about their morbidity in the past week and examined for xerophthalmia. These children formed the population from

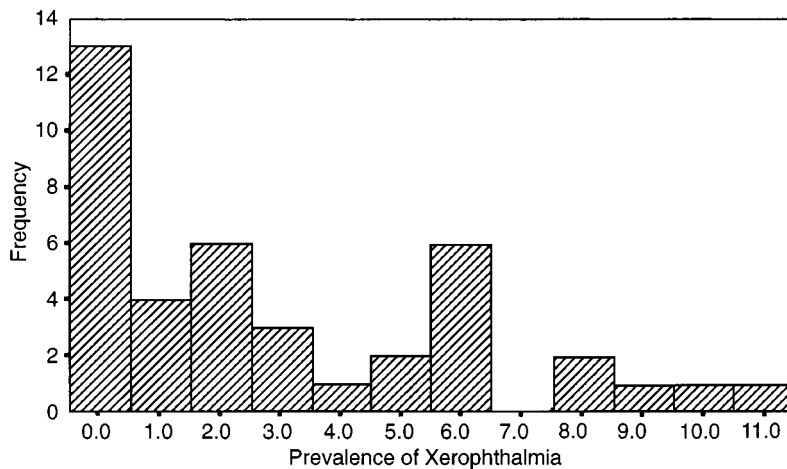


FIGURE 1 Frequency distribution of xerophthalmia prevalence by ward, Nepal, 1989

TABLE 1 Design effect at the second stage of cluster sampling ( $\text{Variance}_{EPI} / \text{Variance}_{SRS}$ ), Nepal, 1989

Cluster size	No. of houses skipped				
	0	1	2	3	4
7	1.11	0.90	1.12	1.20	1.16
10	1.19	0.91	1.11	1.35	1.15
15	1.13	1.15	1.00	1.10	1.15
20	1.20	1.00	0.80	0.93	0.83
25	0.90	0.80	0.73	0.83	0.83

TABLE 2 Bias (sample prevalence minus true prevalence in per cent), Nepal, 1989

Cluster size	No. of houses skipped				
	0	1	2	3	4
7	0.49	0.27	0.77	0.86	0.95
10	0.63	0.30	1.00	0.92	1.16
15	0.74	0.55	1.10	0.64	0.94
20	1.04	0.79	0.98	0.57	0.70
25	1.00	0.75	0.79	0.54	0.92

which repeat samples were drawn. The number of children in each ward ranged from 13 to 284 and the number of households ranged from 31 to 315. The prevalence of xerophthalmia was 2.8% among the 4297 children surveyed in the 40 wards. However, the ward prevalences ranged from 0% to 11% (Figure 1). The design effect at the second stage of sampling (variance of EPI divided by the variance of SRS of a fixed number of children per ward) ranged from 0.73 to 1.35 (Table 1).

The bias generated by EPI sampling ranged from 0.27% to 1.16% (Table 2). The EPI design consistently led to overestimation of xerophthalmia prevalence. The true prevalence was 2.8% but the EPI prevalences ranged from 3.1% to 4.0% (Figure 2). The bias was not related to sample size per cluster, nor to number of houses between selected houses.

As expected, the median distance ratios for EPI relative to SRS increased with distance between houses

and with the increase in fixed cluster size (Table 3). The larger the cluster size, the greater the proportionate increase in the distance ratio. For next nearest houses, the distance travelled was always less for EPI than SRS, even when sampling 25 children per cluster. However, for houses with at least two houses between them and for cluster sizes of 20 or more, the advantage of EPI in terms of distance travelled disappeared. For houses with four houses between them, EPI offered a clear advantage for cluster sizes of seven only. Based on minimizing the combination of mean square error and distance travelled, EPI was preferable for cluster sizes of 10 or fewer and for houses spaced close together (Table 5). Based on minimizing the combination of variance and distance travelled, EPI was preferred over SRS for all cluster sizes where no more than two houses were between selected houses (Table 6).

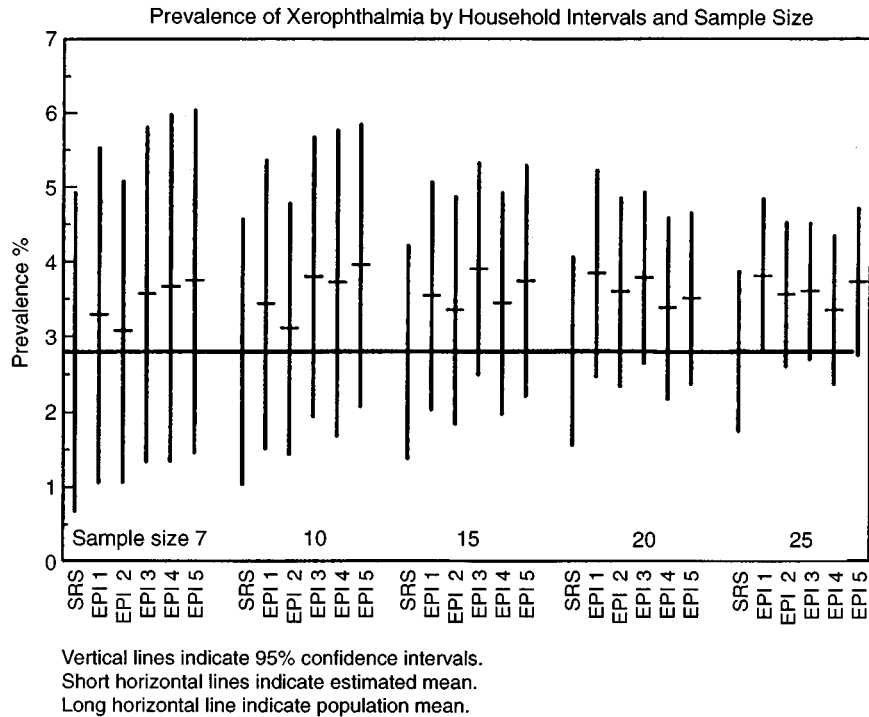


FIGURE 2 Prevalence and 95% confidence intervals for xerophthalmia using different sampling designs, Nepal, 1989

TABLE 3 Median ratios of distance<sup>a</sup> travelled within wards for Expanded Program on Immunization (EPI) versus Stratified Random Sampling (SRS), Nepal, 1989

Cluster size	No. of houses skipped				
	0	1	2	3	4
7	0.39	0.53	0.64	0.72	0.82
10	0.42	0.58	0.68	0.80	0.92
15	0.49	0.62	0.80	1.01	1.07
20	0.55	0.77	0.92	1.28	1.33
25	0.65	0.95	1.13	1.47	1.62

<sup>a</sup> Ratio of <1 means EPI favoured over SRS.

TABLE 4 Ratios of mean square error for Expanded Program on Immunization (EPI) to mean standard error for Stratified Random Sampling (SRS), Nepal, 1989

Cluster size	No. of houses skipped				
	0	1	2	3	4
7	1.31	1.02	1.63	1.83	1.93
10	1.69	1.02	2.37	2.41	2.84
15	2.18	1.73	3.33	1.88	2.85
20	3.84	2.51	3.17	1.71	2.02
25	4.23	2.66	2.80	1.80	3.67

Given the above results, EPI appears to be a good alternative to SRS at the second stage of sampling (at the ward level) since the cost in terms of distance travelled within wards was much lower for EPI using nearest neighbours. However, the estimates of prevalence generated by EPI for cluster sizes from seven to 25 were overestimated by between 11% and 36%. In

addition, other important costs come into play in the choice of design. The time it takes to travel from one cluster to another may make it more efficient to survey every child in the cluster. One disadvantage of this latter design is that the design effect increases with the size of the cluster. Figure 3 gives the time taken in days to complete a survey using EPI with fixed cluster sizes

TABLE 5 Efficiency<sup>a</sup> of Expanded Program on Immunization (EPI) relative to Stratified Random Sampling (SRS) = (median distance<sub>EPI</sub> × Mean Square Error [MSE]<sub>EPI</sub>)/(median distance<sub>SRS</sub> × MSE<sub>SRS</sub>) by cluster size and house proximity, Nepal, 1989

Cluster size	No. of houses skipped				
	0	1	2	3	4
7	<b>0.51</b>	<b>0.51</b>	1.04	1.32	1.58
10	<b>0.71</b>	<b>0.59</b>	1.61	1.93	2.61
15	1.07	1.07	2.66	1.90	3.05
20	2.11	1.93	2.92	2.19	2.69
25	2.75	2.53	3.16	2.65	5.94

<sup>a</sup> Ratio of <1 means EPI favoured over SRS.

TABLE 6 Median distance ratios of Expanded Program on Immunization (EPI) to Stratified Random Sampling (SRS) multiplied by the design effect<sup>a</sup> by cluster size and house proximity, Nepal, 1989

Cluster size	No. of houses skipped				
	0	1	2	3	4
7	<b>0.43</b>	<b>0.48</b>	<b>0.72</b>	<b>0.86</b>	<b>0.95</b>
10	<b>0.50</b>	<b>0.53</b>	<b>0.75</b>	1.08	1.06
15	<b>0.55</b>	<b>0.71</b>	<b>0.80</b>	1.11	1.23
20	<b>0.66</b>	<b>0.77</b>	<b>0.74</b>	1.19	1.10
25	<b>0.59</b>	<b>0.76</b>	<b>0.82</b>	1.22	1.34

<sup>a</sup> Ratio of <1 means EPI favoured over SRS.

of 10 or 15, compared with one in which all children in selected wards are examined. Sample size calculations indicate that approximately 1000 children need to be examined in order to estimate a prevalence of 2.8% (95% confidence interval : 1.8–3.8). This is the sample size needed if a truly simple random sample were selected from a list of all children in the population of interest. However, the design effect using EPI with clusters of size 10 is about 1.4, and about 1.5 for clusters of size 15. Hence, 1400 or 1500 children must be examined in 140 or 100 wards, respectively. If all children in selected wards are to be examined, then approximately 5500 children in 55 wards are needed because the design effect is 5.5 for clusters of size 100 (the average number of children per ward).<sup>31</sup> The calculation assumes that the travel time between houses is

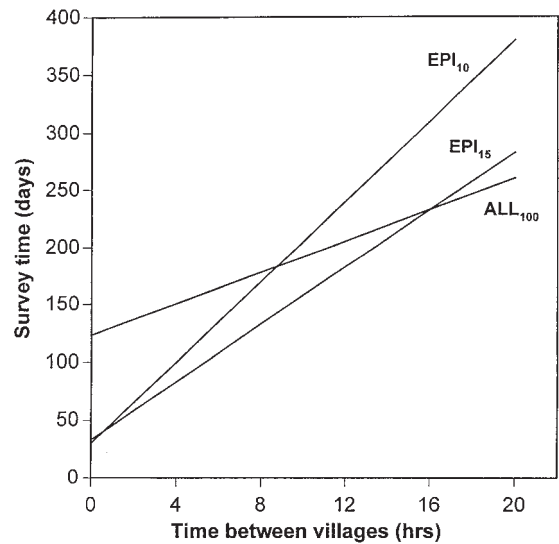


FIGURE 3 Total time to complete a survey plotted against the average travel time between clusters for the Expanded Program on Immunization (EPI) with cluster size 10 and 15, and for a design that examines all children in selected wards, Nepal, 1989

5 min, that there are 1.8 children per household who are under 5 years and that 8 h of work can be done in one day. If 60 children can be examined in 8 h, then EPI with 10 children per cluster takes less time than examining all children in selected wards, unless the travel time between wards is more than 8 h and 45 min. If 100 clusters of 15 children are examined, then the break point is about 2 days travel time between wards. Variations in the travel time between houses ( $t_1$ ) changes the intercept but not the slope of the cost lines. An increase in the travel time between houses would reduce the travel time between wards where EPI and 'all children examined' designs were comparable in terms of time costs. A decrease in  $t_1$  would have the opposite effect.

## DISCUSSION

Less expensive and more rapid ways of estimating the prevalence of xerophthalmia would be very helpful for population assessment and surveillance. Because of the logistics and cost of constructing a sampling frame of all children in a population, simple random sampling from the entire population is almost never an option. A sampling frame of communities such as villages, wards, or city blocks can often be constructed with estimates of community size based on census information already

available. However, variations in how to sample individuals from communities that have been selected proportionate to size can substantially alter the accuracy, speed and cost of surveys. The EPI sampling design reduces the cost of surveys by removing the need to map and census selected communities, and by reducing the time taken to travel within communities. This reduced cost needs to be balanced against the potential bias and increased variance generated by such a sampling design. To date, no estimates of the bias and increased variance have been available for xerophthalmia surveys where EPI sampling is used. The magnitude of the estimate of xerophthalmia prevalence in this population is comparable to other populations in which xerophthalmia is of public health importance; 3.9% in Malawi, 1.3% in Zambia, 1.9% in Indonesia.<sup>31</sup>

The magnitude of the bias and increased variance will depend on the geographical distribution of xerophthalmia within communities. If cases of xerophthalmia are evenly spread across the community, then there should be no bias or increased variance from EPI sampling. Increased variance in the absence of bias can result if clusters of cases are found in certain neighbourhoods. The selection of a cluster of households in a neighbourhood (as tends to be the case with EPI sampling) will produce prevalence estimates that are sometimes too low, and sometimes too high, but on average the prevalence will be accurately estimated. Bias arises if there is a systematic pattern of distribution of cases in the community, and if the starting household in the EPI design is not selected at random. A consistent overestimate of prevalence using EPI, as seen in these data, might arise if cases were concentrated in the centre of the ward and if EPI more often resulted in the selection of a starting house near the centre. This was not a likely explanation because, when the ward was divided into an inner and outer circle, one of which was selected at random to contain the starting house, the bias was not reduced. The bias was also not reduced by increasing the distance between surveyed houses, thereby spreading the sample across a larger area of the ward. The bias was smaller when cluster sizes were small and there were fewer houses between selected houses. These findings are contrary to those of Bennett for other health outcomes.<sup>22</sup> However, we found similar results to Bennett for different morbidity outcomes such as diarrhoea, so the bias pattern appears to vary with the health outcome of interest. In this population, the variance was increased by using EPI, but only for small cluster sizes and houses of close proximity, and the largest design effect would have resulted in a 35% increase in variance of the prevalence estimate (a 16% increase in the length of the confidence interval).

Obviously, a design in which fewer communities are visited but more children within the communities are examined is preferred because of the cost of travelling between communities. However, the more children examined per community, the larger the design effect and the greater the total number of children who need to be examined. For example, the design effect was 5.5 if all children in the 40 wards were examined.<sup>31</sup> This would result in the need to examine 5500 children in 55 wards (100 children per ward) to obtain a 95% confidence interval around the prevalence estimate of 1.8–3.8. To achieve the same level of precision, a design that sampled 15 children per ward would require only 1500 children but necessitate visiting 100 wards. Hence, the distance between communities is important in deciding which design is less costly. Other costs include reimbursement of trained examiners (the same per unit time for both designs) and the cost of mapping and census-taking (for SRS and 'all children examined' designs). In our study, the average ward took 3 days to map and census. The cost of trained examiners as a per cent of total costs is relatively small, but would vary by whether a local or expatriate ophthalmologist, or ophthalmic assistant was hired. In the context of our data, EPI with 15 children per cluster would be favoured over examining all children in a ward if travel time between wards did not exceed 2 days on average. With clusters of size 10, EPI would be favoured if time between wards did not exceed 8 h. If fewer children can be examined per day, if the distance between houses within communities is greater, or if there are fewer eligible children per household, then examining all children will become preferable with a shorter travel time between communities. While these calculations provide guidelines for the choice of sampling design, the situations may vary from one population to another. The design effect when sampling all children in selected villages ranged from 2.03 to 5.50 for two African and two Asian countries with a median number of children per cluster ranging from 25 to 99.<sup>31</sup> Hence, it is possible that there is some variation in disease clustering within neighbourhoods from one population to another and this might lead to a different optimal design in a different setting. Based on our data from Nepal, we favour the traditional EPI design with a fixed cluster size of 15, except when villages are on average further than 2 days apart, but we caution that the process of selecting an appropriate design should be done using as much local information as is available. It should also be cautioned that the EPI approach could overestimate the prevalence of xerophthalmia by up to 41% of the true prevalence, and that this is one of the disadvantages that must be weighed against the reduced survey costs attained using EPI.

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