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SURVEYS TO MEASURE PROGRAMME COVERAGE AND IMPACT: A REVIEW OF THE METHODOLOGY USED BY THE EXPANDED PROGRAMME ON IMMUNIZATION

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Introduction

In order to improve the health status of their populations, most countries are developing their ability to provide primary health care. This relies upon a capacity to manage the national health system which, in turn, is dependent upon information for purposes of planning, supervision and monitoring of health activities. Data are required to define the need for health services, the efficiency of existing services, as well as their impact on morbidity and mortality.

Although much of the necessary information can be obtained from routine sources, some can best be obtained through the use of surveys. The World Health Organization (WHO) and other international agencies have been active in promoting the use of such surveys. As an example, through its Expanded Programme on Immunization (EPI), WHO aims to ensure the availability of immunization for all the children of the world by the year 1990. This effort is considered a vital step towards the attainment of WHO's stated goal of health for all by the year 2000.

The impact of diseases such as neonatal tetanus, poliomyelitis, measles, whooping cough, diphtheria and tuberculosis upon children in the developing world is compounded by the fact that many of these children are severely weakened by malnutrition and repeated episodes of diarrhoea and malaria. Immunization programmes are seen as an important measure for reducing infant and childhood disability and deaths, which act as spurs to sustain high birth rates.

A major need of the EPI has been an appropriate system for gathering information. Reliable data are necessary to document the level of morbidity and mortality from specific target diseases as well as the level of immunization coverage against these diseases. Since this information is not readily available to health managers in many developing countries, EPI has worked to develop a method which could obtain accurate information quickly and cheaply. A method was sought which, in addition, could be implemented in a relatively standardized manner from one country to the next. This would permit training materials and operation manuals to be developed for widespread use, and would facilitate comparison of results between countries. In addition, standardized methodology makes possible the comparison from one time period to another within a particular country, which is needed to measure the impact of the immunization programme's efforts over time.

The primary purpose of the methodology developed by EPI was to assess the level of immunization coverage. Because of the operational success of this methodology, it has been adopted for a range of other purposes including assessments of population morbidity due to specific causes, service coverage and health service needs. It is the purpose of this article to review the EPI methodology, to consider its suitability for other purposes, and to suggest limitations, modifications and alternatives to meet the needs of different health programmes.

Theoretical basis

It is not possible to study every child in a target age range in order to assess the level of immunization coverage. As a result, a sampling scheme was sought which would be effective, relatively easy to carry out in a short period of time and inexpensive. Simple random sampling (SRS) would not be suitable: accurate lists of children could not be readily obtained or constructed, and travel between selected children, who might be widely scattered, could be both costly and time consuming.

An alternative sampling strategy, with considerable operational advantages, is known as "probability proportionate to size" (PPS) cluster sampling. The principle of this method is as follows: it is assumed that the population under consideration can be divided into M groups or "clusters" on the basis of geographic location. The population of each cluster must be known or estimable from census or other government figures.

Let N_i denote the population size of the i^{th} cluster, where i can take on any value between *1* and M. At the first stage of sampling, a subset of m clusters is selected from the complete list of M clusters. This selection is done in such a way that large clusters have a higher probability of being selected than do small clusters. The actual selection is carried out using random numbers, and it should be noted that it is possible for a large cluster to be selected more than once in making up the subset of m clusters.

As with any cluster sampling method, this procedure entails a big saving in time and expense, since detailed frames of children in the target age group need be compiled only for the m selected clusters rather than for the entire population. In fact the compilation of these frames can, if necessary, be delayed until the m clusters to be included in the sample have been identified.

At the second stage of sampling, $\bar{n}$ children are selected from the total population (N_i) of children living in each of the selected m clusters. This selection is prescribed to be carried out *at random*. When carried out in this manner there are a number of distinct advantages with PPS cluster sampling. First of all, resulting estimates are "self-weighting". In other words, the sizes of the clusters do not enter into computations of proportions or associated standard errors.

For example, to estimate the proportion of the population immunized, the following formula is used:

$$\hat{P} = \frac{1}{m}\left\{\sum_{i=1}^{m} y_{i*}/\bar{n}\right\} \qquad (1)$$

where $y_{i*} = \sum_{j=1}^{\bar{n}} y_{ij}$ = total number of children adequately immunized from among the $\bar{n}$ children studied in the i^{th} cluster and

$y_{ij} = \begin{cases} 1 \text{ if the } j^{th} \text{ child in the } i^{th} \text{ cluster is adequately immunized} \\ 0 \text{ if the } j^{th} \text{ child in the } i^{th} \text{ cluster is not adequately immunized.} \end{cases}$

Expression (1) is equivalent to:

$$\hat{P} = \sum_{i=1}^{m} \sum_{j=1}^{\bar{n}} y_{ij/m\bar{n}} \qquad (2)$$

which is simply the total number of immunized children divided by the total number of children studied. This estimate is called self-weighting, since there was no need to incorporate the N_i into the formula.

An estimate of the standard error of this estimated proportion (necessary for the construction of confidence intervals) is also relatively straightforward, with computation as follows:

$$\hat{SE}(\hat{P}) = \left[\frac{1}{m(m-1)} \sum_{i=1}^{m} (\hat{P}_i - \hat{P})^2 \right]^{1/2} \qquad (3)$$

where $\hat{P}_i$ is the proportion immunized in the i^{th} cluster and $\hat{P}$ is the proportion immunized over all sample clusters as given in (2) above. This expression is considerably easier to calculate than typical cluster sampling formulae, since only the variability between the estimated proportions in the sampled clusters is needed. An expression for the intracluster variability is not incorporated into this formula.

It should be noted that, strictly speaking, for these formulae to hold, it is necessary that the m sample clusters be selected with probability proportionate to size and that simple random samples of $\bar{n}$ children be selected from each of the m clusters.

Modifications for EPI

Since 1978, WHO/EPI has been advocating a PPS cluster sampling procedure for surveys of immunization coverages *(1, 2)*. The method adopted is a modification of a survey technique originally used for immunization coverage in the United States of America *(3)* and later updated for use in the smallpox eradication programme in West Africa *(4)*. By the end of 1982, at least 441 surveys of this type had been carried out worldwide *(5)*.

The EPI survey, as it is currently carried out for determining immunization coverage, involves the detailed review of immunization status of approximately 210 children by trained reviewers. The current convention is to identify and visit 30 "clusters" which may be in cities, towns, or villages, and to study 7 children in each. As a result, the EPI surveys are commonly referred to as "30 x 7" surveys.

The rationale for using 210 individuals is as follows. First of all, it was decided that it was necessary to be able to estimate immunization coverage to within 10 percentage points of the true population proportion. Using a population proportion of 50% coverage as the basis, and desiring to be 95% confident that the resulting estimate would be in the interval 40–60%, it was determined that a simple random sample of size 96 would be required. To select a simple random sample of this size from the population was not operationally feasible and, as a result, a cluster sampling strategy was deemed necessary.

In order to achieve the same precision with cluster sampling as would be possible with simple random sampling, experience suggested that a cluster sample of approximately twice the size ("design effect") of the simple random sample would be needed. Because of the economy afforded by the cluster sampling strategy, this larger sample can be studied both more conveniently and less expensively. As a result, the necessary sample size with cluster sampling was estimated to be 192.

Based on procedures adopted for use in the United States at that time *(3)* and, taking into account practical as well as logistic factors, it was decided that 30 clusters should be used. This meant that 7 children per cluster must be studied in order to attain the specified sample size. There is no particular statistical advantage in using 30 clusters and it is perfectly reasonable that the operational considerations should dictate the number to be selected. However, it should be noted that if one is satisfied to have an estimate which will be within 10 percentage points of the true P with 95% confidence, a different combination of m and n might result in significant savings in time and cost. On the other hand, for the same time and cost, alternative combinations of m and n could yield increased precision. Decisions as to the exact value of m and n to use in a particular country would have to be tailored to the specific characteristics of that country. These could involve numerous assumptions regarding costs likely to be incurred at each stage of sampling, as well as estimates of intracluster correlation coefficients which, in their own right, might be of questionable accuracy.

The classic PPS cluster sampling scheme as described above has certain features which may make implementation in field conditions difficult. In particular, the random selection at the second stage may not always be possible — particularly in scattered rural areas. The classic methodology has thus been modified, and the 30 x 7 survey currently being used by the EPI may be characterized as a PPS cluster sample without random selection at the second stage.

Field methodology

The first step in carrying out an immunization coverage survey is to identify precisely what the population is and which age groups within this population are of particular interest. Clusters are identified by compiling a list of all cities, sectors of cities, towns and villages included in the target area for which immunization coverage is to be evaluated.

Although detailed lists of individuals or families may well not exist for each of the clusters on the list, all that is really needed at this point is a reasonably accurate population estimate for each cluster so that PPS selection can take place. In practice, this may present some difficulties since it may be several years since the last census update was carried out. The most important assumption that must be made in this case is that any changes that have occurred in the population since the last figures were compiled affect equally all clusters in the country being studied. For example, if cluster A had 3 times as large a population as did cluster B at the time of the last census, that same 3:1 ratio is assumed to hold at the time of the survey. If census data are not available, intelligent estimates must be made of the population sizes in the various clusters. With PPS sampling, the relative sizes of the clusters are more important than the actual sizes.

Once the list of M clusters and associated population sizes has been compiled, m of the clusters can be selected with probability proportionate to size. This is accomplished by computing the cumulative population, cluster by cluster, with the cumulative total equalling the total population size, N. By dividing the total population size by the number of clusters sought (30 in this case), 30 "zones" are identified, each containing $N/30 = K$ individuals. K is referred to as the "sampling interval". By selecting a single random number between 1 and K (call this i), the first cluster to be sampled is identified as the one which includes the i-th individual on the cumulative

list. Starting from this position on the cumulative list, 29 further clusters are identified by successively adding the sampling interval, K. Thus, $i, i+K, i+2K, \ldots, i+29K$ define the 30 clusters. This type of sampling is called a systematic sample of clusters and the probability that a particular cluster will be included in the sample is directly related to the size of the cluster. In fact, large clusters can be selected more than once in this systematic sampling process.

Once the sample cluster is identified, it is then necessary to determine which individuals to study within the cluster. The method advocated by EPI is as follows: a household is selected "at random" from all households in the cluster. In practice, it may not be possible to make this selection truly at random, since the method used will depend upon the density of the population as well as other factors such as the availability of lists of households. When household lists are available, the households are numbered and 1 random number is selected to represent the first sampled household. If, on the other hand, the cluster is a small village and household lists do not exist, it may be possible to carry out a quick census of households, numbering them, and then selecting a random number to represent the first sampled household. In fact, the process of carrying out a census is to be preferred, since it is then reasonably assured that the list is complete.

Enumerating all households is often impossible in moderately large towns or widely scattered rural populations. In that case, EPI suggests that the interviewer go to some centrally located landmark, (such as a church, school or market) randomly select a direction to walk (e.g., north, south, east or west), count the number of households (call this L) found in that direction from the central point to the town boundary, and finally, select a random number between 1 and L. This number identifies the randomly selected starting household.

In urban areas, the process of identifying a random starting household may be much more difficult. One possibility is a 2-step process where the city is subdivided into geographically contiguous zones, a zone is selected at random, and a starting household is then identified within the zone. If household lists do not exist and if the zones are made small enough, it may be possible to carry out a census in the selected zone before selecting a household. Clearly, there is no single strategy which can be applied to all situations, and solutions must often be devised on the spot to deal with living arrangements such as multiple dwelling units and apartment buildings.

It should be noted that there are certain situations where valid lists of target populations are not available and cannot be reliably constructed. For instance, in certain societies there are large numbers of "street dwellers"; i.e., individuals who do not live in permanent dwellings and who do not, therefore, figure in local census data. In fact the very concept of household may have unclear meaning with such individuals, and compiling accurate lists to include them is rarely done since such lists are immediately obsolete.

Upon entering the first household, the interviewer must first determine whether there are any individuals living there who are in the target age group. If there are, the required information is collected for each such individual. As presently recommended by EPI, if no one is at home in the selected household, the interviewer moves on to the next household. There is no provision to revisit households.

After the first household is visited, whether there is an individual in the target group or not, the interviewer proceeds to the "next" household. This is defined as that residence whose front door is physically closest to the one just visited. This process of visiting households is repeated until a total of 7 individuals of the appropriate age have been studied in the sampled cluster. As an operating rule, all individuals in that household contributing the seventh individual to the sample are studied, even if that results in 8-10 individuals in the cluster rather than the target number of 7.

Potential problems with the EPI methodology

The EPI survey has proved to be a most useful tool for providing health managers with essential information for planning health programmes. Normally a survey will require about 5 days of work for 4-6 teams, and is routinely included as part of EPI management training. Results from these surveys have provided the incentive for resource allocation which has allowed immunization programmes to expand and increase their impact. Without these surveys, many national programmes would have no means for assessing their progress.

It should be evident from the preceding sections that the procedure followed by EPI has diverged from standard PPS cluster sampling methodology. This divergence occurs at the second stage of sampling (i.e., selection of households and individuals). From a statistical point of view this is a cause for concern since, in order for the formulae presented earlier in this article to hold, it is assumed that individuals studied at the second stage are the result of random selection, and the procedure advocated by EPI does not achieve this.

Theoretically, the households should be selected independently of each other and should be representative of the totality of households in the cluster. The EPI method, by selecting a starting household and then visiting geographically related households, ensures that this will not be the case. The effect of this is impossible to quantify but, intuitively, households which are spatially related may have other factors in common, including access to immunization facilities, water supply, disease exposure, etc. A particular example of this might be the pocketing of unimmunized households in inaccessible slum areas of cities. There is a risk that surveys of adjacent households could either over- or under-estimate the true population coverage depending upon where the starting household happens to fall. In fact, where such pocketing is suspected, and where the objective is to immunize individuals living in such pockets, other survey methods may give the manager more immediately useful data than would be possible even with a perfectly implemented PPS survey. In practice, in circumstances such as these, special arrangements are usually made by EPI managers.

In some situations, the distance between the central point and the edge of the community may be too large for the prescribed EPI procedure to be practical. Although this is not advocated by EPI, the interviewer may find it more realistic to simply select the direction to be taken from a fixed starting point and to pick out and visit a house at random in that direction without first counting the number of households in that direction. Strictly speaking, this method of selecting the starting household is not random, and although it does assure that the interviewer does not exercise personal judgement in the selection process, it still introduces statistical bias

Leaving selection of successive households to the interviewer presents another opportunity for bias. This may occur when an interviewer must decide which household is closest to the one just visited. If, for example, this choice is between one household in a slum area and another not in a slum area, there is a possibility that the

interviewer's preferences may result in one or the other not being adequately represented. Areas which are not easily accessible may also be underrepresented if left to the discretion of the interviewer.

A further potential for bias is inherent in the practice of not revisiting households found to be unoccupied at the time of the interviewer's visit since certain subgroups will not be adequately represented.

The selection of the starting household is also a cause of some concern since the exigencies of field operations may rule out the possibility of a truly random selection. This may result in households inadvertently being selected on grounds of convenience. In a scattered rural community, for example, the tendency may be to select the starting household in the area of densest population. Such selection is subject to bias since such centres may also be the focus for outreach services and other health care facilities. In these circumstances, even if the direction in which the interviewer is to walk is selected at random and the household is selected at random from all houses lying in that direction, considerable bias may still result.

Finally, there is a potential problem in having to use non-documented evidence of immunization status. This problem cannot be fully overcome but is often taken into account when analysing the data.

Other applications of the EPI methodology

The EPI survey was designed for the express purpose of measuring immunization coverage, either in the absence of any data, or when data of doubtful validity exist. In recent years, the sampling methodology developed for these surveys has been applied not only to assessments of immunization coverage but also to assessments of changes in immunization coverage over time. With some modifications, the methodology has also been applied to studies of the incidence of poliomyelitis, neonatal tetanus *(6)* and diarrhoea, as well as to studies of mortality due to measles. Recently, the same procedure has been used in surveys to assess various factors relating to the availability and use of health services.

Based on the above discussion, it should be clear that the particular methodology developed by EPI specifically for coverage surveys might not be appropriate for other surveys having different objectives. For example, for surveys designed to document the expansion of coverage of an immunization programme over time, sample size computation should incorporate such factors as the estimated coverage rate before the expansion, the anticipated coverage after the expansion as well as requirements for type I and type II error rates. The rationale underlying the number 210 does not take these factors into acount and if this number were used, the study would involve an extremely high type II error rate (i.e., the probability of failing to detect a difference or change which actually occurred).

Much of modern knowledge concerning neonatal tetanus and poliomyelitis in developing countries arises from information from cluster sample surveys *(7)*. Researchers studying the incidence of these diseases have recognized that the procedures advocated for coverage surveys needed to be modified and, in particular, that larger sample sizes were required. The commonly accepted practice is that in each of 30 clusters, 70 livebirths having occured within a stated recall period (usually 4 to 6 months) are sought. The deaths among these livebirths are investigated and the proportion due to neonatal tetanus is estimated. All children aged 5-9 years in the households visited during the survey are examined for evidence of lameness due to poliomyelitis. This usually results in at least 200 children studied in each of the 30 clusters. The continued use of 30 clusters is based more on tradition and intuition than on statistical theory.

The need to consider carefully the choice of sample size can be illustrated by another example: if a study was planned to estimate the prevalence of a comparatively uncommon disease such as leprosy, the total required sample size would be much greater than that required in either of the previously described surveys in order to make estimates with acceptable precision. Specifically, if the rate of leprosy in a country is 1 per 1000 (i.e., $P = .001$), then a simple random sample of size of 15 350 would be required to be 95% confident that the sample estimate would be within 50% of the true value (i.e., between .0005 and .002) *(8)*. An even larger sample would be required if a more complex sampling scheme were used or if greater precision is required. Clearly, for such uncommon diseases, planning the sample size so that the resulting estimate will be within 10 percentage points of the true P would be of little value.

For surveys of health service utilization and health status, there are often numerous and unrelated parameters being estimated in a wide age range. This difficulty is compounded by the fact that some of the parameters being studied are unlikely to be distributed homogenously within communities. This is particularly true for accessibility of water supplies, communications and health service facilities. Planning sample size requirements under these conditions is extremely difficult, but it is an issue which must be confronted by agencies or others needing the information which would result from such studies. Blind acceptance of the 30 x 7 sampling scheme may seriously compromise the results of surveys and mislead planners and managers, despite the intensive efforts which may have gone into the execution of such surveys.

Evaluation of the 30 x 7 EPI sampling strategy via computer simulation

Despite the concern expressed over the lack of random selection of household with the EPI method, no studies have been carried out which have objectively evaluated its performance compared to a method which selects households at random. To this end, a computer simulation was designed and implemented at WHO Headquarters by the Epidemiological and Statistical Methodology Unit of the Division of Epidemiological Surveillance and Health Situation and Trend Assessment in cooperation with EPI and the Diarrhoeal Diseases Control Programme (CDD). The details of this study which will be presented in a separate report, are summarized here. Populations were constructed to represent low, moderate, high or very high population density. Households within the populations were distributed in 1 of 4 ways: at random, along a road or stream, centred around a single focus such as a market, school or church, or centred around 2 foci. One out of every 7 households was designated as having a child in the target age range and a certain overall proportion of children was designated "immunized". Four schemes were used to mark the immunized children: at random among all children, restricted to 1 "pocket" in which 100% of children are immunized and outside of which no children are immunized, restricted to 2 pockets of 100% immunization and, finally, restricted to 5 pockets of 100% immunization. The overall immunization levels in the various strata were either 10%, 30%, 50%, 70%, or 90% which, in combination over 30 clusters, resulted in popu-

lations with coverage probabilities of 17%, 34%, 46%, 65% or 75%. Once the clusters were established, sampling took place either by selecting a single household as a starting point and then visiting adjacent households until 7 children had been obtained, or by selecting 7 random starting points and visiting adjacent households until 1 child was obtained. The sampling process was repeated 500 times for each of the established clusters.

The results of the simulation can be summarized as follows. Comparison of the EPI estimates to the SRS estimates over the 500 replications within the constructed populations suggests that the EPI estimates have greater bias and are more variable than the corresponding SRS estimates. However, with respect to the stated goal of the EPI to estimate the true immunization coverage to within 10 percentage points of the true population proportion with 95% confidence, the simulation provides strong evidence that this is being accomplished. Over the 500 replications within each of the artificially generated populations, the estimated coverage rate differed from the actual level by more than 10 percentage points in less than 5% of the replications.

At the individual cluster level, the EPI method was typically outdone by the SRS method. This was particularly evident for those clusters having pockets of immunized children where the EPI method demonstrated high bias and extremely high variability which was not the case with the SRS method. This can be explained as follows: if the randomly selected starting household was far from the pocket of immunized children, the estimated proportion with the EPI method would then be much too low, whereas if it was in or close to the pocket, the estimate would be much too large. Since the SRS method selects 7 random starts, pocketing did not result in the same errors in estimation.

It should be reemphasized that the EPI uses these surveys for overall population estimates and has never advocated estimation at the individual cluster level. Hence, even though the individual cluster estimates may, under some conditions, be poor, the averaging process over the 30 clusters produced estimates accurate to within 10 percentage points of the true level of immunization coverage in the vast majority of replications. This certainly provides reassuring testimony concerning the coverage surveys as currently being carried out. It also sets forth a warning to those users who might disaggregate the estimates for the purpose of making statements concerning particular communities, cities or regions. This disaggregation represents an unacceptable procedure within the cluster sampling framework.

Alternatives to the 30 x 7 EPI sampling strategy

Further research into possible alternatives to the currently used 30 x 7 EPI survey is certainly warranted and would be of great value to a wide range of investigators. Several alternatives have been proposed to date. For example the Diarrhoeal Diseases Control Programme group at WHO is currently testing survey designs which divide a population into M clusters of roughly equal size. When clusters are large, they are subdivided into smaller subsets, each with the desired number of individuals. From these M clusters, m are selected at random and *all* individuals residing in those selected clusters are studied. This eliminates the need to select n_i from the N_i in the selected clusters and, as a result, eliminates the need to incorporate a "within cluster" term into the variance estimate. In order for this method to be used, it is necessary for good estimates of population size to exist prior to creation and selection of clusters. This method is appropriate to the objective of the CDD since the desired sample size is large. It would not be appropriate for coverage or other surveys requiring smaller sample sizes.

Another sampling method which has been proposed but which needs much field testing is known as the "Lot Quality Assurance Sampling" (LQAS) technique. In effect, used in an immunization context, this method can be used to say whether or not the immunization coverage is below a predetermined target level. It has the advantage of being able to provide a programme manager with a local estimate of the effectiveness of his/her programme and, as such, could be most effective as a management tool in specific health districts or towns.

With the LQAS method a target level of coverage is specified for the population. A random sample of individuals in the population is then selected and, based on this sample, the population (or "lot") is "rejected" if it is determined that the coverage is below the target, or is "accepted" if the coverage achieves the target. The method parallels simple quality assurance methods used traditionally in industrial applications and is based upon the binomial distribution.

Specifically, let d = number of unimmunized children out of n children sampled and let Q = threshold value or the proportion of children unimmunized in the population. One may wish to test the null hypothesis $H_o: Q \geq .50$ (i.e., the population is not adequately immunized) versus the alternative $H_A: Q < .50$ (i.e., the population is adequately immunized). Note that the null hypothesis is that the proportion of unimmunized people in the population is at least as large as the threshold. If the null hypothesis H_o is rejected, it can be concluded that the level of coverage is adequate. Rejection of H_o is equivalent to "accepting the lot" from a quality assurance point of view and, similarly, failing to reject H_o is equivalent to "rejecting the lot". The type I error in this situation is the probability that the population is deemed adequately immunized on the basis of the survey when, in fact, the proportion immunized is actually less than Q. This very serious error can easily be controlled by choosing a small type I error rate. The type II error is the probability that the population is deemed not adequately immunized when in fact it is. For actual policy decisions, this would not appear to be as serious as an error of the first type.

The strategy with LQAS is to select a sample of n children, determine whether or not each has been immunized and, defining d as number of unimmunized children among the n studied. If $d \leq d^*$, H_o is rejected and the lot is accepted; if $d > d^*$, H_o is accepted and the lot is rejected. If the lot is rejected, a new immunization effort might be initiated. Choice of d^* and n depend upon the desired type I and type II error probabilities.

As an example, a district health officer wishes to know whether immunization coverage in his/her district is above 50%. To make this decision, a random sample of children in the target age group is selected and their individual immunization status is determined. (It should be noted here that the process of selecting a random sample will present a difficult barrier and, as a result, the LQAS method may not be practicable for even moderately large populations.) If the number of unimmunized children in the sample is less than or equal to a computed threshold (d^*), the lot is accepted and it is concluded that the coverage exceeds the target level. Otherwise, the lot is rejected. Specifically, if a random sample of $n = 50$ children is taken and, for a type I error of .05, the rule would be to reject H_o

(and accept the lot) if $d \leq d^* = 18$ since, then, $Pr(d \leq d^* P = .50) \leq .05$. If it were possible to select a sample of $n = 100$, the criterion would be to reject H_o if $d \leq 41$. Clearly, larger sample sizes will result in smaller type II errors against a range of alternatives.

Confidence interval estimates can be constructed within each of the areas for which the LQAS sample was selected and, using straightforward stratified sampling theory, estimates can be produced for the totality of the areas sampled. It should be noted that this is not a cluster sampling strategy and that LQAS is not likely to be a substitute for a sampling scheme which attempts to estimate a parameter for a large population.

Conclusions

In many situations the use of sample surveys is essential to provide some of the data necessary for the management of health programmes. The adoption of survey methodology by national programmes should be encouraged and supported. At the same time, there is great danger in taking for granted the idea that because a particular survey design works for one stated objective, that this same design can be successfully employed in other contexts. It is essential that prospective users tailor the available survey designs to suit their particular needs and, at very least, recognize the limitations inherent in the use of any survey methodology adopted — particularly when practical considerations force divergence from the ideal path. Programme managers cannot expect reliable results from surveys if the methodology is not adhered to and if the underlying assumptions of the sampling strategy are not pertinent to the survey in question.

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