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APPROACHES TO THE MEASUREMENT OF CHILDHOOD MORTALITY: A COMPARATIVE REVIEW

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FOREWORD

It is generally recognized that a good civil registration system, which the author describes as one with "complete vital registration of births and deaths, with minimal delayed registration, good reporting of age at death, adequate identification of cause of death through medical certification, and a high degree of geographic disaggregation" meets most needs for measures of child mortality. However, few developing countries have adequate civil registration systems. Consequently, a variety of alternate methods for providing measures of child mortality have been developed in recent decades for use until such time there is a flow of adequate vital statistics in the developing countries.

This paper reviews the available methods for measuring childhood mortality, and ascertains the extent to which they meet the needs for mortality data for children. This article first appeared in Population Index, 57(3) Fall 1991, and is reprinted here with the kind permission of Population Index, Office of Population Research, Princeton University, 21 Prospect Avenue, Princeton, New Jersey 08544.

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APPROACHES TO THE MEASUREMENT OF CHILDHOOD MORTALITY: A COMPARATIVE REVIEW

Kenneth Hill*

Abstract. In the developing world, measures of child mortality are needed for a variety of purposes, and estimates of child mortality can be obtained by a variety of approaches. In this paper, the author reviews the characteristics that child mortality measures should have for particular purposes, and then examines the available measurement approaches to determine the extent to which they provide accurate measures with the required characteristics. Particular emphasis is put on the comparative performance of different approaches in different settings to produce estimates of recent levels and trends in child mortality. He concludes that no single approach can satisfy all measurement purposes and that all approaches are sensitive to the quality of data collection, but that many needs can be met by relatively inexpensive data collection and analysis methods.

1. Reasons for Measuring Childhood Mortality

Measures of child mortality, taken here to mean measures of risks of dying up to the age when such risks reach their minimum values, typically around 10, are used for a number of purposes. Child mortality, and particularly infant mortality, is often used as a broad indicator of social development or as a more specific indicator of health status. Measures of child mortality are necessary for making population projections. Characteristics of child mortality, such as its age pattern, socioeconomic differentials, cause of death structure, and subnational distribution, are used to seek causal explanations and to design interventions. Information on trends, both national and disaggregated, is used to evaluate the impact of interventions. No one data collection system or analytical methodology provides the ideal information base for all these diverse objectives. Before discussing individual systems or methods, it is thus necessary to review the criteria by which these systems or methods may be judged.

2. Criteria for Evaluating Systems or Methods

A single measure of child mortality is sufficient as a broad socioeconomic or health status indicator. The measure most often used is the infant mortality rate (IMR), conventionally calculated as deaths under age one in a year divided by births in the year, or, almost equivalently, as the probability of dying by first birthday. However, in most developing-country contexts of high child mortality, as many as 50 percent of all child deaths may occur after infancy, and a broader measure, such as the probability of dying by age five, christened by UNICEF the "Under-5 Mortality Rate," is preferable to the infant mortality rate. Whichever measure is employed, in order to be useful it should be accurate and refer to as recent a time point as possible.

For population projection purposes, it is important to measure trends over time, including trends in the recent past, to obtain a sound basis for forecasting future trends. To evaluate intervention programs, the accurate measurement of recent trends is essential, and measurement of age patterns and differentials is valuable. For intervention targeting, geographic differentials, requiring estimates for small areas, and information on age patterns will be needed, and the cause structure of deaths (for example, diarrhea versus measles versus respiratory infections) will be very useful. For research into the causes of levels, trends, and differentials, measures of child mortality by socioeconomic, cultural, and environmental variables, preferably at the individual level, will be needed.

To meet all possible needs, we need accurate and recent measures of levels, accurate indicators of trends over a substantial time period, accurate measures of age patterns and differentials by social,

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economic, geographic, cultural, and environmental factors, and at least approximate indications of the broad cause structure of child mortality.

In the rest of this paper, I will discuss and illustrate the extent to which alternative sources of data and methods of analysis measure up to these needs. The review will in parts be rather superficial: there are a substantial number of methods, and a still larger number of versions of methods; a thorough examination of the evidence relating to the performance of each one would be beyond the scope of a review paper. Instead, I will offer my own assessment of the performance of the various methods and illustrate these conclusions by showing examples of child mortality estimates from countries for which a variety of the necessary data sources exist. More specialized evaluations can be found in Hill (1984), evaluating the performance of indirect methods;[1] Cochrane et al. (1982), reviewing possible methodologies for measuring child mortality in an in-depth survey of living standards; and Preston (1985), comparing the performance of indirect versus direct (maternity history) methods for measuring child mortality.

It should be stressed that this paper is not intended as a guide to data collection or analysis procedures. Rather, it is intended as a guide to the selection of an appropriate methodology for measuring some particular aspect of child mortality within some resource constraint. It must be remembered throughout that both data collection and data analysis are difficult and demanding activities. The best of data collection techniques will produce poor results if inadequate attention is given to questionnaire design, sample design (if applicable), interviewer training, and supervision. For further advice, the reader is referred to the United Nations (1984) *Handbook of Household Surveys*, and for questionnaire design for maternity history surveys to Jemai and Singh (1984). Poor analysis can also lead to a failure to make the best of available data, and sometimes more seriously to the drawing of incorrect inferences. For indirect analytical techniques, the reader is referred to Manual X (United Nations, 1983) and a more recent step-by-step guide (United Nations, 1990), but should remember that there is more to complete analysis than the rote application of standard methods; evaluation and comparison of multiple data sets are of particular value.

3. Review of Collection Systems and Analytical Procedures

(i) Vital Registration

The continuous registration of births and deaths is potentially the richest source of data about child mortality. If registration is complete, the IMR for each year can be calculated in the conventional manner directly from the system's data, thus providing information on level and detailed trend in the IMR. Ideally, all events are recorded and there is no sampling variance, though analysis may still be affected by small numbers of events. If events are recorded and tabulated by place of normal residence of mother, the IMR can be calculated for small areas, and if extensive information about the parents is collected, socioeconomic mortality differentials can be obtained. If all deaths are medically certified, good information on cause of death will be available. Under perfect conditions, the only two shortcomings of vital registration data as a basis for measuring child mortality are the restrictions on the amount of socioeconomic data that can realistically be collected, limiting the depth of potential analysis, and the limitation to directly measuring only infant mortality, rather than broader child mortality; to measure child mortality, additional data on exposure to risk in later childhood, typically obtained from censuses, are needed.

Unfortunately, very few less-developed countries (LDCs) have vital registration systems that approach the ideal. In many LDCs, particularly in Sub-Saharan Africa and South Asia, only small percentages of births and infant deaths are registered. Coverage levels may differ for births and infant deaths, and those events that are registered cannot be regarded as representative of all deaths in terms of socioeconomic characteristics or cause-of-death structure. In such circumstances, the IMR is likely to be biased in level and probably also in trend and cause structure. Unfortunately, there are no fully satisfactory ways for rehabilitating incomplete registration of births and infant deaths.

In some countries with largely complete coverage, delayed registration may be a problem that affects the timeliness of the measures; if the births in some year are not all registered until four or five years later, a final calculation of the infant mortality rate for the year will be delayed by those four or five years. Delayed registration may also affect completeness of registration: it is very unlikely that a birth that initially goes unregistered will later be registered if the child dies in infancy. Even in countries with complete and timely coverage, the ultimate timeliness of the measures may be reduced by delays in publication; even in the United States, detailed publication from vital registration data lags by three or four years. The detail available is also limited in most developing countries by a failure to publish information for small geographic units or by characteristics of the parents. Information on cause of death may be of limited quality because a high proportion of child deaths are not certified by a physician.

Attempts have been made in many parts of the world to improve deficient vital registration systems, but these attempts have by no means all been successful. A typical strategy is to introduce a new or improved registration system, generally using active rather than passive registrars, in a small sample of areas, focusing intensive effort on these areas, and then gradually expanding the system to a larger number of areas. A successful example of this strategy is the Indian Sample Registration System, which now provides apparently satisfactory estimates of vital rates for almost all the states of the country. An unsuccessful example was the attempt in the 1980s to upgrade the Kenya vital registration system; Gil (1989?) wryly chronicles the frustrations involved in this U.N.-sponsored effort, and shows that the initial gains in registration completeness were largely lost five years after the start of the program. Relatively few countries have made the transition from incomplete to complete vital registration coverage of child deaths in the last three decades, an indication of how difficult this transition is to make. In a recent world review of child mortality (United Nations, 1988), measures of such mortality were based largely on vital registration data for only 17 LDCs; in a further 16 countries, vital registration data were used in combination with other types of data; and in 73 LDCs, estimates were based exclusively on other types of information. This breakdown should be taken as an indication of how difficult it is, and how expensive it is likely to be, to introduce a satisfactory vital registration system in a developing country rather than as an indication of the enormous potential for introducing such systems.

(ii) Multi-Round Surveys and Surveillance Systems

As the name implies, multi-round surveys use repeated visits to households in sample areas to record demographic events. A typical format is for a baseline survey to establish initial household and population structure, and then follow-up rounds typically at intervals of six months or a year to collect information about demographic events since the previous round. Omission is kept to a minimum by probing to account for apparent changes in household structure not accounted for by reported events; for births and infant deaths, women are often asked about their pregnancy status at each round, and pregnancies that end between rounds are then matched against reported inter-round births and against either additions to household membership or to infant deaths.

With careful fieldwork, multi-round surveys can produce accurate measures of child mortality levels and age patterns. They cannot, however, provide estimates of trends prior to the period of fieldwork unless combined with other types of data collection. The amount of background information that can be collected is limited only by the availability of resources. Extensive information about the parents, the family, their living conditions, environmental conditions, and access to and use of health services can be collected at the initial round; information about the children, such as anthropometric data, nutritional practices, recent illnesses, and use of preventive and curative medical services, can then be collected at each round. Information on the causes of deaths identified between rounds can be obtained through the use of verbal autopsy methods.[2] Such longitudinal data offer extraordinary potential for research. The Cebu study (Cebu Study Team, 1991) in the Philippines provides an example of this type of system. An example of a less-detailed study using a national sample is the Honduras National Demographic Survey 1972-74 (Maccio et al., 1977).

A close relative of the multi-round survey is the surveillance system, whereby a geographically defined population is kept under detailed study for an extended time period. The best-known example of a surveillance scheme is the International Centre for Diarrhoeal Disease Research study of Matlab thana in Bangladesh (for a methodological description, see Cholera Research Laboratory, 1978; for an example of the use of the data, see Koenig et al., 1991), but smaller systems also exist in Senegal, the Gambia, and other places. The key characteristic of a surveillance system is a defined geographical area, the population of which is visited at frequent intervals, even as often as once a week, to inquire about demographic events, morbid episodes, contraceptive use, or whatever may be the focus of a particular study. From time to time, often several years apart, censuses of the system's population are taken, to check the population denominators needed to calculate age- and sex-specific rates and to evaluate registration coverage.

The surveillance system can generate excellent measures of levels, age patterns, and differentials of child mortality for the study population. Trends can also be measured over the life of the system, but they may be affected by other activities, such as health intervention trials, in the study area. Information on cause of death can be obtained by verbal autopsy methods, and only resource availability limits the amount of other information that can be collected. The system provides an ideal framework for field trials, other experimental interventions, or studies of specific issues. In the absence of usable vital registration data, a surveillance system may be the only source of data that can reveal sharp discontinuities in child mortality (see the Bangladesh example in Section 4).

Multi-round surveys and surveillance systems are excellent approaches to detailed research activities. However, both are extremely expensive, their longitudinal nature makes them susceptible to political or economic instability, and they are unable to provide quick measures because time is needed to build up sufficient events and exposure to risk to calculate stable rates. Further, the surveillance system is unable to produce national estimates because of its limited geographic coverage and the risk that it will become less and less representative as time goes by; this latter trend is likely to be exacerbated by the ethical requirement to provide medical help to those identified by the surveillance as needing treatment. The multi-round system may be relatively inefficient at producing national estimates because sample clusters need (for logistical reasons) to be large. Neither system can produce nationwide estimates for small areas or small population subgroups, and both will be difficult to implement in populations experiencing high levels of mobility, such as urban squatter populations.

(iii) Survey Questions on Child Survivorship

The most widely used substitute for a complete and timely vital registration system is the incorporation of questions in a cross-sectional data collection procedure asking women about the survival of some or all of the children they have had. There are a variety of ways in which the data can be collected, ranging from the very simple, such as permitting inclusion in a national population census, to the highly complex, requiring intensive interviewing of a small sample of women. We will review each in turn, starting with the simple. With all these approaches, it should be remembered that the data are affected by a selection process, namely the survival to interview of the mother. The children of mothers who have died or migrated away or are simply missed for interview will not be included. This selection problem is likely to become particularly acute in populations heavily affected by acquired immune deficiency syndrome (AIDS), since deaths from pediatric AIDS will not be reported if the mother herself has died from the disease.

(a) The Brass questions: aggregate numbers of children ever borne and children dead. (See United Nations, 1990.) All women of reproductive age (in some cultures, only ever-married women) are asked the number of children they have given birth to, and the number of those children that have died. (The preferred form of the question is in three parts: "Of the children you have ever borne alive, how many are now living with you? How many are living elsewhere? And how many have died?") Proportions of children dead born to women in five-year age (or marriage, or time since first birth) groups are converted into probabilities of dying by exact ages of childhood by allowing for the distribution of the children by lengths of exposure to risk of dying. This conversion is carried out using indicators of the age or duration of marriage or time since first birth pattern of fertility. This pattern of fertility can also be used to estimate an approximate reference period in the past (roughly, the average time before the survey that the child deaths occurred) to which each estimate refers, under conditions of steady child mortality change.

This method has many appealing characteristics. The questions needed are short and simple, so they can be included in a large-scale exercise such as a census, thus providing small-area or small population subgroup estimates. The method has been found to estimate child mortality levels and trends over roughly a 10-year period rather well. However, the approach cannot estimate age patterns of child mortality (indeed, these patterns have to be assumed), and cannot investigate causes of death or biosocial correlates of child mortality such as birth intervals because of the aggregate nature of the basic data. Nor, in practice, can the method measure recent levels and trends of child mortality, say in the last three years. The reason for this is that the most recent estimates of child mortality are derived from reports of the youngest women (when using data classified by age), but these youngest mothers tend to come from the more disadvantaged social strata and their births are predominantly first births. Thus, their children tend to have higher mortality risks than the average for the population as a whole and the estimates based on reports of these young mothers (particularly those aged 15-19) tend to be biased upwards. The problem is somewhat less severe if the data are classified by duration of marriage or time since first birth, but even in these cases the most recent exposure group includes a disproportionate number of high-risk first births.

The estimation procedure used to obtain probabilities of dying in childhood from proportions dead typically assumes a certain age pattern (but not level) of child mortality and assumes that fertility has been roughly constant in the recent past (Trussell, 1975). In estimating the time reference of the estimates, it is further assumed that child mortality has been changing linearly in the recent past (Coale and Trussell, 1977). Deviations from these assumptions will result in estimates that are biased to a greater or lesser extent. Choice of an inappropriate age pattern of mortality will result in misestimation of trends, but the estimate of the under-five mortality rate derived from women aged 30-34 (or duration of marriage or time since first birth group 10-14) referring to about six years before the survey will be very little affected. If fertility is falling, average exposure times will be underestimated and child mortality will tend to be overestimated, but the error can be avoided if data are available from two sequential surveys, allowing true cohort fertility patterns to be used. Changes in fertility that result from changing age at marriage (or other union) will have relatively little effect on methods classifying women by duration of marriage or time since first birth but a larger effect on the method classifying women by age. Changes in mortality that are not linear over time will tend to be smoothed out by the trends estimated by the analysis. Omission of children that have died relative to children still alive will of course lead to underestimates of child mortality.

(b) Survival of most recent birth or births in the last 12 months. Another simple approach is to ask each parous woman of reproductive age about her recent childbearing (either the date of her most recent live birth or whether she had a birth in the last 12 months) and also to ask whether the child is still alive. If the date of the most recent live birth is collected, analysis is limited only to those births in the last 12 months to avoid selection effects. The proportion dead of these children will be approximately equal to the life table function $1 - {}_1L_0/l(0)$, that is, a portion of the IMR, for the year before the survey. The data from this type of question have sometimes been collected or tabulated for periods not equal to 12 months, for example since the beginning of the calendar year. The conversion of the proportion dead of births in such a nonstandard time interval into a standard measure of child mortality such as the IMR is by no means straightforward.

This set of questions is simple to include in a data-collection instrument and has been included in a number of censuses, as well as in large-scale surveys. In terms of our criteria, the estimates indicate the level of part of child mortality (mortality after infancy is entirely excluded) for the very recent past, the last year. However, taken alone, the measure gives no indication of trends or age patterns of child mortality; indeed, the age pattern of mortality must be assumed in order to estimate the IMR. Differentials could be examined across any other variables collected by the survey. Cause of death could be ascertained for a small survey through verbal autopsies administered to the mothers of identified infant deaths, but such a procedure would not be feasible for a large survey or a census.

The most unsatisfactory feature of this approach is that it has been found in a number of applications (for example, Guatemala 1973 and Honduras 1972) to give substantial underestimates of child mortality (Hill, 1984). This underestimation may be related to other widely encountered errors, such as the underreporting of births in the last year and a deficit of children aged under one. In other applications, the approach seems to have given estimates consistent with other available measures (for example, the Bangladesh Retrospective Survey of Fertility and Mortality 1974 and the 1980 Census of Indonesia). There is some indication in the accumulated experience with this method that the order of the questions is important; Blacker (1990) has suggested that asking first about the survival of the last child and then about its date of birth (the order used in the Bangladesh 1974 survey) gives better results than vice versa. Sullivan et al. (1981) have proposed an extended series of questions about the last live birth, including numerous probes about the penultimate pregnancy and any subsequent pregnancy, and internal checks. The extended questions appear to give better results; for example, in a 1979 survey in Mexico, the questions about a pregnancy following the initially reported last birth identified additional infant deaths that increased the proportion dead by almost 20 percent (Sullivan et al., 1982), but at the expense of having more questions than can be included in a census or large household survey.

(c) Survival of the previous birth. This method, first proposed by Brass and Macrae (1984), was developed as a way of estimating child mortality from information collected from women at the time of delivery. A woman about to deliver or having just delivered a child is asked whether she has had a previous birth; if the answer is yes, she is asked whether the most recently born of her previous children is still alive. The proportion dead of such previous births is approximately equal to the probability of dying by an exact age of childhood equal to 80 percent of the mean birth interval. Thus, if the mean birth interval is 2.5 years, the proportion dead of previous births is approximately equal to the probability of dying by age two. When mortality is changing, the estimate is of this probability for a time period slightly less than two years before the time of observation.

This methodology provides an estimate of the mortality level for a fairly recent period; the data processing and analysis protocols are so simple that there need be very little delay between data collection and estimation. Some indication of socioeconomic differentials can be obtained. However, the method gives no indication of trend unless the data collection is maintained over a long period, gives no measure of the age pattern of child mortality (which indeed has to be assumed), and is unlikely to be of value for identifying cases for a verbal autopsy, since the deaths will mostly have occurred more than 12 months before they are reported. The other major drawback of the method is selection bias: data are collected from women at the time of delivery in some health institutions, so no information is collected about those children born prior to the births that do not take place in a health institution of some sort. This selection is likely to work in favor of the low-risk children of better-off mothers who live close to a health facility. The level of mortality in the population is thus likely to be underestimated unless a high proportion of births occur in health facilities. It is sometimes argued that the selection biases are likely to be less serious for trends than for levels of child mortality, but there seems to be no very obvious reason for assuming that trends will vary less with socioeconomic status than will levels. In particular, since the method has been suggested as a way of monitoring the impact of health interventions, there seems to be no sound theoretical reason for assuming that program impact will not vary by socioeconomic status or, even more forcefully, by distance from a health facility. The great advantage of the approach is its opportunistic data collection system: data collection and analysis costs are trivial because respondents come to interview (to give birth) rather than the interviewer having to go to the respondents, as in normal surveys; the questions are extremely simple and the analysis is straightforward.

The previous-birth method has also been proposed for inclusion in household surveys (David et al., 1990). Asking (ever-married) women of childbearing age about their previous child in a household survey appears to avoid the underreporting problems associated with information about the most recent child (perhaps because the dead child has been replaced by a surviving most recent child), and also avoids the selection problems associated with interviewing only women giving birth in a health facility. However, it is hard to see what advantages this approach has over the Brass questions. The survey estimate obtained is not as recent in its period of reference as an estimate obtained from information collected from women at the time of delivery (because the previous births are more than half an average birth interval older and thus exposed to mortality risks farther into the past), and is unlikely to be more recent than an estimate based on Brass-type reports of women married less than five years or whose first birth was in the last five years. Further, the previous-child approach provides no indication of trend unless repeated at intervals, whereas Brass-type questions provide useful indicators of trends over 10 years. Nor is there any evidence that the previous-birth technique as used in surveys provides more accurate results than the Brass approach; indeed, in the application to data from Jordan given by David et al. (1990), it is clearly the other way around: the Brass-type estimates are clearly superior to the previous-birth estimate.

(d) Truncated maternity histories. In this approach, each (ever-married) woman of reproductive age (but preferably up to age 54) is asked about the date of birth and, if dead, the age at death of each child born since some cutoff point such as a date five years or so before the survey. The normal starting point is the most recent birth, working backwards in time from that. The data from such histories can be used to calculate recent levels of child mortality, but trends cannot be examined directly. Age patterns of early child mortality can be observed up to the age corresponding to the time before the survey of the truncation date, but no further. Mortality differentials by any factors included in the survey, including biosocial factors such as birth interval or breast-feeding, can be analyzed. The truncated history can be used to identify cases of child deaths, such as those in the year before the survey, to which a verbal autopsy can be applied, in order to identify causes of death.

The Demographic and Health Surveys (DHS) program experimented with truncated maternity histories versus complete histories in two surveys and found relatively little difference between the results (Goldman et al., 1989). However, not all experiences have been so favorable. The Bangladesh Diarrheal Morbidity and Treatment Survey of 1987 used truncated histories; it was found hard to train interviewers with the questions, and comparisons of the mortality measures with estimates based on Brass questions suggested that the truncated histories were underestimating mortality by a great deal, apparently because child deaths were clustered into the period immediately preceding that covered by the truncated history (Osinski and Mitra,1989).

(e) Complete maternity histories. In this approach, each (ever-married) woman of reproductive age (or better, up to at least age 54) is asked for the date of birth of each and every live-born child she has had; for each child she is asked whether the child is still alive and, if not, the age of the child at death (the DHS program has reported age at death in days in the first month, in months up to two years, and in years thereafter, but for some analytical purposes it would be preferable to have the age at death reported in months at least up to age five). Maternity histories have now been widely used in many developing countries, in both the World Fertility Survey (WFS) program and the DHS. The data can be used to calculate life tables for children for time periods up to 15 years before the survey date (when using longer time periods, selectivity by age of mother at the birth and in some cases recall error become problems). Thus, the data provide estimates of recent levels of child mortality, trends over 15 years or so, age patterns, and differentials, by any variables included in the survey; of particular interest may be differentials by biosocial factors such as birth interval that are difficult to measure with any other methodology. The maternity history can also be used to identify targets, such as births that occurred in the 12 months before the survey, for the application of a verbal autopsy.

There are potential problems with maternity histories, however. Careful fieldwork is required since the data are quite complex; attention to questionnaire design, training, and supervision are crucial. Even the generally high-quality surveys of the WFS included one case (Ghana) in which child mortality was clearly underestimated (Rutstein, 1991). A more pervasive problem is the tendency, almost universal in developing country surveys, to round ages at death of children to exact numbers or convenient fractions of years. Thus, in the DHS surveys, the numbers of deaths at ages 12 or 18 months are often substantially larger than the numbers reported at neighboring ages. Such rounding makes the IMR particularly difficult to calculate satisfactorily: excluding all deaths reported as occurring at 12 months underestimates the IMR, but redistributing deaths to different ages introduces an essentially arbitrary element to the calculations. It may also be noted that different approaches can be taken to the calculation of period life tables in childhood, according to the assumptions used to allocate deaths and exposure to risk to time periods, and different results will be obtained. Cohort probabilities of dying by specified ages of childhood can be calculated more simply, but such probabilities are hard to locate in time.

Maternity history surveys have proved to be a very important source of information on levels, trends, age patterns, and, perhaps most significantly, associations with social, economic, and biological factors (see, for example, Hobcraft et al., 1984) of child mortality. Experience suggests that with careful training, supervision, and fieldwork, maternity histories can be collected in third-world settings with sufficient accuracy to measure important aspects of child mortality.

The only measurement dimension on which the maternity history is less than satisfactory is the spatial. The complexity of the training, supervision, and fieldwork involved in a maternity history survey is such that only small numbers of interviewers can be used and cost considerations keep the sample size small (rarely more than 10,000 women); as a result nationwide estimates for small areas or population subgroups cannot be obtained.

(iv) Retrospective Survey Questions on Household Deaths

An attempt to fill the gap left by inadequate vital registration is sometimes made in censuses and large household surveys by collecting information on household deaths by age and sex during some reference period, typically 12 or 24 months, prior to the survey. In theory, such information could provide levels and age patterns of childhood mortality for the year or two before the survey and could indicate child mortality differentials along any dimensions included elsewhere in the survey. However, this method could not provide information on trends except by repetition and would not, in a large-scale enquiry, be very useful as a way of identifying deaths for cause-of-death investigation through the application of verbal autopsies.

In practice, the household-deaths approach has given very disappointing results for both child and adult deaths. With adult deaths, there is at least some prospect of adjustment by comparison with the population age distribution, but in the case of childhood deaths, there is no obvious comparator for either consistency checks or adjustment. Mortality measures for childhood may be affected by errors both in numbers or ages of deaths and also in the population numbers used as denominators, given the poor quality of child age distributions in many LDCs.

4. Illustrative Applications

In this section, child mortality estimates are presented for a number of countries for which several of the data sources or methods outlined above can be used. It should be noted that the countries have been chosen because of the variety of data sources they offer; comparisons as elaborate as those presented are uncommon. In general, these illustrations are reassuring, since they tend to show that in most cases different methods or data sources give broadly similar results. It should be borne in mind, however, that measures based on registration data have not been presented for the countries where such data are generally recognized as being hopelessly incomplete. It should also be noted that the countries chosen as illustrations cannot be regarded as comprehensive or even necessarily representative, although each one offers a good variety of methods and sources. In order to permit cross-methodology comparisons, child mortality measures are all presented in the form of the probability of dying by age five for both sexes combined; in some cases, available measures have been converted into this comparable index by the use of a suitable model life table or by combining sex-specific measures using an assumed sex ratio at birth.

Figure 1 shows child mortality estimates for Bangladesh from around 1960 to the late 1980s. The sources and methods used are (i) the Bangladesh Retrospective Survey of Fertility and Mortality 1974 (Brass estimates, household infant deaths in the year before the survey, and survival of the last live birth); (ii) the Bangladesh Fertility Survey (BFS) 1975 (maternity history measures); (iii) the Contraceptive Prevalence Survey 1983 (Brass estimates); (iv) the ICDDR,B Matlab surveillance system; (v) the Bangladesh Fertility Survey (BFS) 1989; and (vi) the Diarrheal Morbidity and Treatment Survey (DMTS) 1987-88 (truncated maternity history). Conversions have been carried out using the West family of Coale-Demeny model life tables (Coale and Demeny, 1966) and a sex ratio at birth of 105 males per 100 females. The ${}_5q_0$ from the 1975 BFS are period measures, but those from the 1989 BFS are cohort measures located in time (for comparison purposes) 1.5 years after the midpoint of the period defining the births of the cohort (for example, the ${}_5q_0$ for the cohort born in 1979 to 1983 has been plotted at the beginning of 1983); the ${}_5q_0$ from the DMTS is a mixed cohort/period measure computed from a truncated maternity history.

The two series of Brass estimates are highly consistent, showing a ${}_5q_0$ roughly constant at around 0.23 from 1960 to 1980, with perhaps some slight upward disturbance around 1970. The 1975 BFS measures show similar levels, except rather lower in the late 1960s. The 1974 data on survival of most recent births and on infant deaths in the household in the preceding year give similar results, both slightly lower than other estimates for the period (household deaths in the preceding year at ages one to four would have given substantially lower estimates still; infant deaths were apparently well reported relative to deaths at other ages). The 1989 BFS gives results similar to those from other

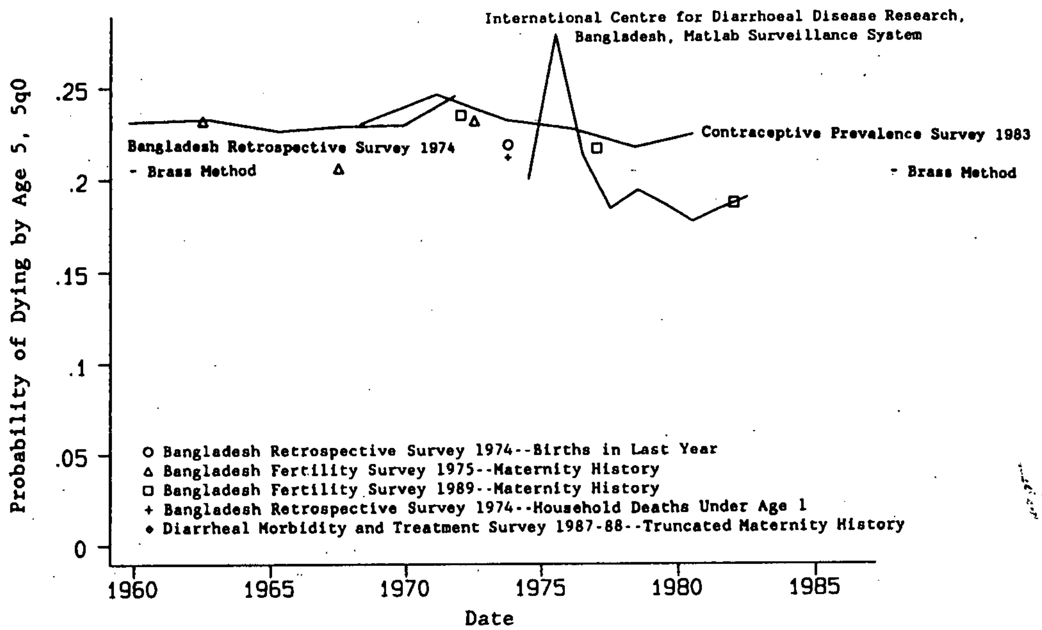

Figure 1. Child mortality estimates from various sources: Bangladesh, 1960-1987.

sources for the 1970s but shows quite a strong downward trend for the 1980s. The series of annual measures from the Matlab surveillance area shows mortality rather lower than other estimates throughout most of the period, but with a sharp spike in 1975, the famine year. The data from the surveillance system are the only indication of the severity of the child mortality response to the famine; either the response was less sharp in other parts of Bangladesh, or the other sources have smoothed out the effect to such an extent that it is imperceptible. The surveillance system's ability to show such a short-term response is a strength of the approach, but the generally lower level of the mortality measures points to a weakness, namely that the area is not representative of the country as a whole. The child mortality estimate from the truncated maternity histories of the DMTS is clearly unsatisfactory, some 50 per 1,000 below any of the other measures. It is interesting to note that the Brass estimates from the DMTS data are consistent with the earlier CPS Brass estimates, suggesting that any distortion in the life table estimates from the truncated histories results from dating errors rather than omissions of dead children.

Figure 2 shows child mortality estimates for Indonesia from (i) the 1971 census (Brass

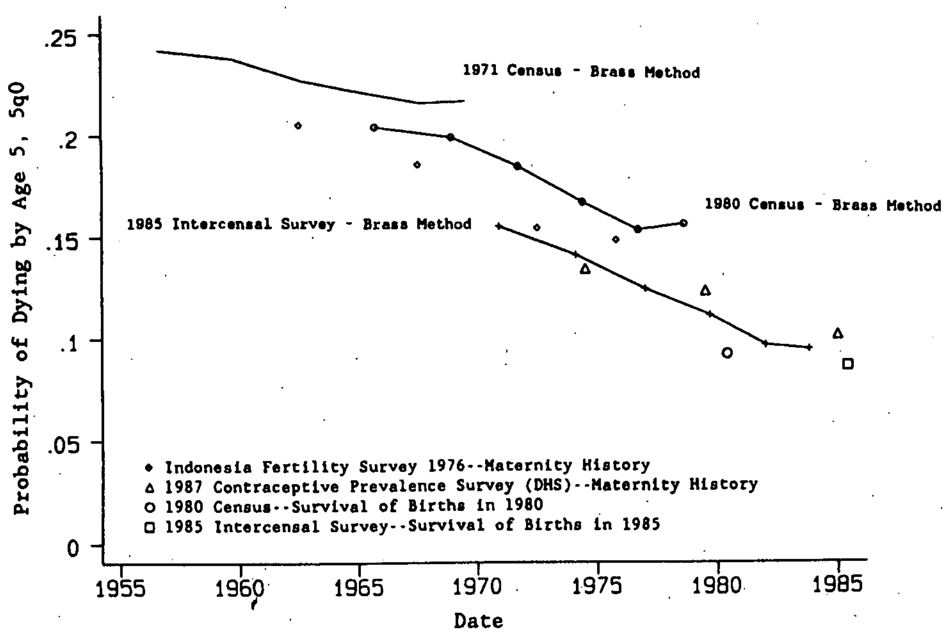

Figure 2. Child mortality estimates from various sources: Indonesia, 1955-1985.

estimates); (ii) the 1980 census (Brass estimates, and an estimate based on survival of births in 1980 reported in the census); (iii) the 1985 Intercensal Survey (Brass estimates, and survival of births in 1985 reported by the survey); (iv) the 1976 Indonesia Fertility Survey (IFS), part of the WFS (maternity history measures); and (v) the 1987 Contraceptive Prevalence Survey (CPS) (maternity history measures). Conversions into $_5q_0$ have been made using the South family of Coale-Demeny model life tables and a sex ratio at birth of 105 males per 100 females.

The Brass estimates from the 1971 and 1980 censuses show a fairly consistent pattern of decline, especially if the points for youngest (20-24) and oldest (45-49) women are excluded from both series. The Brass estimates from the 1985 survey are clearly substantially lower, as are the measures from the 1976 IFS (the IFS was limited in geographical coverage to Java and Bali; these more-developed areas of the country probably have lower child mortality than the national average, explaining at least part of the difference from the Brass sequences). The CPS maternity history measures for the late 1970s and 1980s are reasonably consistent with other available estimates, but the estimate for the early 1970s is clearly below all other estimates for the period; omission of child deaths in the distant past is a possibility. The two estimates based on survival of births in the year of the survey are rather below other available estimates for comparable time periods. It may be noted in passing that the fieldwork for both the 1980 census and the 1985 intercensal survey was conducted on an effective date of October 31. The survivorship statistic represented by the proportion surviving of births in the year of the survey is thus $_{0.83}L_0/0.83*l(0)$, not an easy measure to convert into an intelligible index of child mortality. Here, I assumed survivorship was a function of the cube root of age, fitted such curves to l(0) and l(1) at various levels of the Coale-Demeny models, and found the areas under such curves from birth to age 10 months.

There has been continuing discussion about the relative merits of direct (maternity history) approaches versus indirect (aggregate child survival) methods for measuring child mortality. In drawing lessons from the WFS, Preston (1985) wrote, "One of the important uses of WFS data has been to provide a repeated clear demonstration of the biases in the indirect estimation procedures...." He goes on to say, "WFS analyses to date give a clear edge [over indirect questions] to...direct questions in establishing levels and recent trends in child mortality." One of the analyses on which Preston bases this conclusion is that by Sullivan and Wilson (1982) of the 1976 Indonesia Fertility Survey (IFS). It is of interest to examine their analysis here.

The comparison that Sullivan and Wilson make is between the infant mortality rate for birth cohorts for the years 1957-1961, 1962-1966, 1967-1970, and 1971-1974, calculated from the IFS maternity histories, with indirect estimates based on the reports (in the same maternity histories) of women aged 20-24, 25-29, and 30-34 concerning number of children ever borne and number dead, using the Sullivan (1972) variant of the Brass technique with the Coale-Demeny North model life table family and the Coale and Trussell (1977) method for dating the indirect estimates. The indirect IMR estimates are higher than the direct ones for comparable time periods by amounts ranging from 3 to 23 percent. The authors review possible explanations for the discrepancy. They conclude that the indirect estimate based on reports of women aged 20-24 was too high because of the high proportion of the children ever borne by such women that are borne by them before age 20,[3] and that the estimate based on reports of women aged 30-34 was too high because child mortality trends in the 15 years preceding the survey had not been linear, with the result that the "procedure which imputes an indirect estimate to a point in time [was] badly in error" (Sullivan and Wilson, 1982, p. 103).

There is no question that indirect estimates based on reports by women aged 15-19 and to a lesser extent 20-24 are often distorted by selection effects. Nor is there any question that indirect estimates will fail to track correctly a nonlinear decline in child mortality. There are some questions, however, about the magnitude of the effects reported by Sullivan and Wilson. First, as can be seen in Figure 3, their cohort estimates[4] of IMR are all below--sometimes substantially so--the IMR estimates for time periods given by Rutstein (1983) on the basis of the same data. Second, Rutstein's period IMRs show a smooth change over time, quite unlike the stepped pattern shown by Sullivan and Wilson's cohort IMRs. Also shown in Figure 3 are indirect estimates based upon both age (groups 20-24 to 45-49) and time since first marriage (duration groups 0-4 to 20-24) of mother.[5] It can be seen that the duration-based indirect estimates track Rutstein's measures very closely, as do the age-based estimates for all but two age groups. The high indirect estimate based on reports of women aged 20-24 (the right-hand-most point of the series) is probably biased upwards by selection problems as described by Sullivan and Wilson; however, the bias relative to Rutstein's series is about 10 percent, well below the 23 percent they report. The second high indirect estimate, for women aged 30-34, is also about 10 percent above the Rutstein series, but the bias can be clearly seen not to be due to nonlinear trends in child mortality in the past, since the estimate based on reports of women aged 35-39 is right back on the Rutstein series; sampling error seems the most likely explanation for this aberrant point. Thus, if the Rutstein measures of period IMR are correct, the indirect estimates provide reasonably good indicators of both levels and trends of infant mortality. The method using age at marriage to allow for exposure to risk of the children seems to give particularly good estimates, including the one with a reference period closest to the survey date.

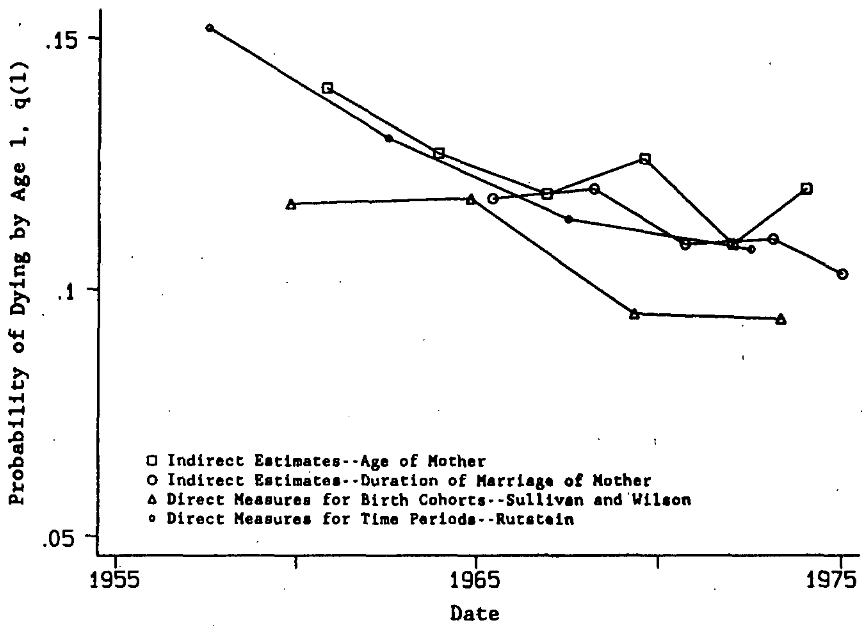

Figure 3. A comparison of direct and indirect estimates of the infant mortality rate: Indonesia Fertility Survey 1976.

The third example, in Figure 4, is from Guatemala. The data sources are: (i) the census of 1973 (Brass estimates and survival of births in the last year); (ii) the census of 1981 (Brass estimates); (iii) the 1987 DHS (maternity history measures); and (iv) vital registration from 1960 to 1987 (the IMR has been used, dividing infant deaths in each year by the number of births in the same year). The West family of Coale-Demeny models has been used for conversions.

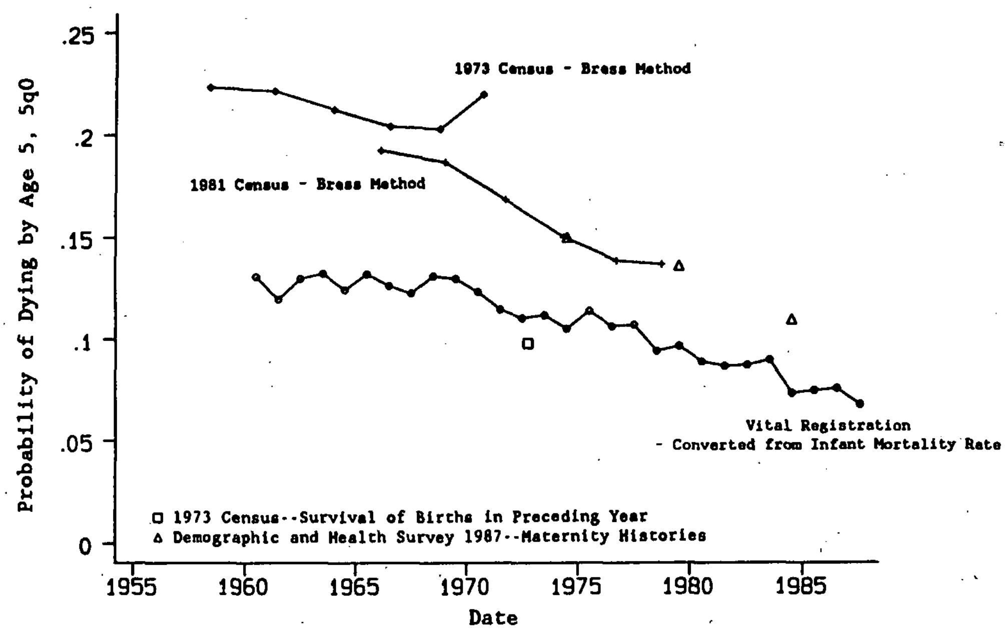

Figure 4. Child mortality estimates from various sources: Guatemala, 1955-1987.

The two series of Brass estimates are again quite consistent, particularly if the points for oldest and youngest women are excluded. They show a gradual decline in child mortality during the 1960s, accelerating during the 1970s. The maternity history measures from the 1987 DHS are remarkably consistent with these Brass estimates. The estimate based on survival of births in the year before the 1973 census is clearly below any other available estimates. The registration data give estimates below those of the Brass or maternity history approaches and also show a less pronounced trend; the registered IMR shows apparently constant child mortality through the 1960s and only a gradual downward trend in the 1970s and 1980s. It is possible that improvement in registration completeness has concealed the true pace of the decline.

Data from the DHS program make possible a broader comparison of measures of child mortality based on maternity histories with Brass estimates based on the same reported births. The two sets of estimates should be very similar, since the same births with the same survival status are being used. The differences, if any, must arise from differences between the direct and indirect allocation of deaths and exposure to risk to specific time periods. Figure 5 shows comparisons for 12 DHS countries between the direct and the Brass estimates. Though the Brass sequences tend to vary more, probably because of smaller sample sizes (especially for younger women, the amount of exposure to risk of child death represented by each proportion dead is substantially smaller than that underlying a five-year period measure), there are no obvious patterns of differences of level or trend.

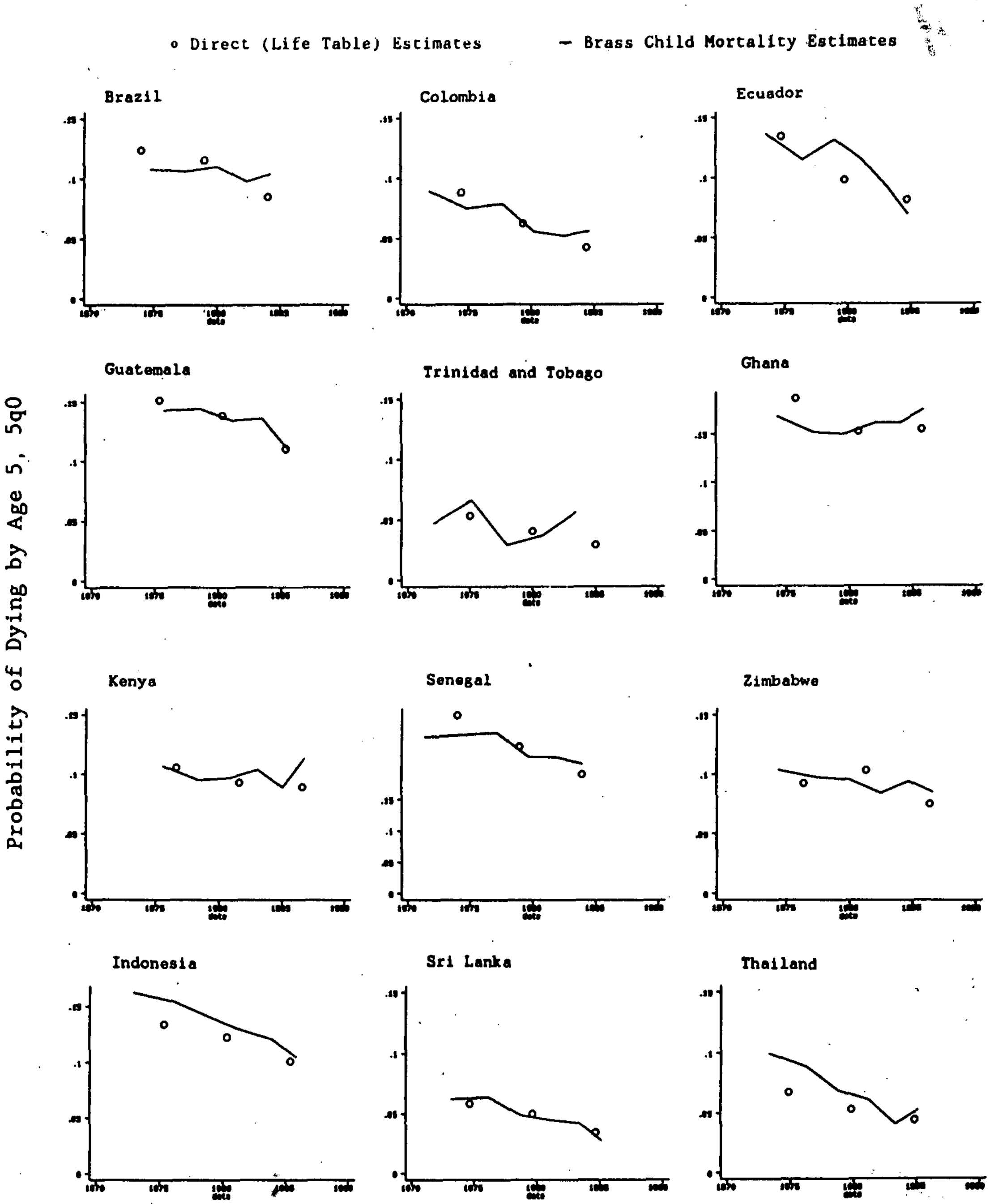

Figure 5. A comparison of direct (life table) and Brass child mortality estimates: 12 DHS countries.

5. Conclusions

Table 1 summarizes the methods discussed in this paper by their performance according to the evaluation criteria established in Section 2: *A* indicates excellence, *B* good, *C* acceptable, *D* mediocre, and *E* very poor.

No single data collection or analysis method meets all the needs for measures of child mortality. Complete vital registration of births and deaths, with minimal delayed registration, good reporting of age at death, adequate identification of cause of death through medical certification, and a high degree of geographic disaggregation, provides for most needs, though age patterns of mortality, socioeconomic differentials, and biosocial factors will require alternative data sources to be documented fully. Virtually no developing countries have such well-developed vital registration systems, and the cost of developing such systems would be prohibitive, even if it were possible. Complete maternity histories also fulfill many requirements, the only shortcomings being imposed by relatively high costs and thus practical limitations on sample size, affecting the detail of trends and of geographic variation. Truncated maternity histories have proved difficult to implement satisfactorily. Multi-round surveys and surveillance systems are ideal for intervention trials and other studies in which selection bias is not a serious shortcoming, but they are rather inflexible, with limited geographic detail; in the case of multi-round surveys, they provide no information on trends unless maintained for a long period. Brass questions are adequate in most categories (and are thus a good value, given their low cost), but give only smoothed estimates of trends, little information on biosocial differentials or age patterns of mortality, and are not suitable for cause-of-death add-ons; the duration of marriage variant and, if it proves satisfactory, the time since birth variant are marginally preferable to the age variant as giving slightly more stable recent estimates. The survival of previous birth method, as applied at health facilities or at some other independently motivated contact with an informant, provides a bargain-basement opportunity for data collection, though the data are limited in some respects and are potentially biased by selection; as a survey method, it has little to offer. Collection of information on deaths in the previous year, whether in the household or most recent birth reported in the last year, has a checkered history, giving results that are sometimes seriously out of line with alternative estimates.

A final note of caution is in order. No measurement method works all the time, and in developing countries at least, checks on data quality and consistency are very important. Data quality depends on fieldwork quality, which in turn depends on careful questionnaire design, thorough training, and continuous supervision. Though the methods discussed above vary in their robustness, they can all fail if inadequate attention is paid to the data collection process.

NOTES

[1] An indirect method for estimating mortality is one that obtains an estimate of mortality from some observed statistic which in large part (but not exclusively) is determined by mortality, after allowing for non-mortality factors.

[2] A verbal autopsy uses lay interviewers to identify cause of death or groups of causes of death. A structured questionnaire collects information from the mother or other informed respondent on the signs and symptoms that preceded death. See Gray et al. (1990).

[3] Sullivan and Wilson rather misleadingly give this proportion as 71 percent, and Preston quotes this figure. However, the 71 percent excludes all births in the year before the survey, since such births were excluded from the cohort calculations of infant mortality. The relevant proportion for indirect estimation includes all births, and for these the proportion born before age 20 is 59 percent (see Sullivan and Wilson, 1982, footnote to Table 6.19).

[4] The cohort IMRs of Sullivan and Wilson have been plotted at the midpoint of the birth cohort plus four months, to allow for nonlinearity of risk in the first year of life.

[5] The Coale-Demeny South family of model life tables was used here, because Rutstein's infant and child mortality measures suggest a closer fit to this than to the North family; using North would not materially affect the conclusions. The Trussell, and Coale and Trussell, methodologies described in Manual X (United Nations, 1983) have been used for both age- and duration-based estimates.

Table 1. Summary of child mortality measurement methods by performance on evaluation criteria.

Method	Criterion											
	Level		Trends			Age Patterns	Differentials			Cause of Death		Cost per unit of exposure
	Accuracy	Recency	Yes/No	Detail	Time Range	Yes/No	Socio-Econ	Geographic	Biosocial	Certification	Verbal Autopsy	
Vital Registration	E-A	C-B	Y	A	A[1]	Y	B	E-A	C-B	Y	N	D-B
Multi-Round Survey	B-A	B	N	N/A	N/A	Y	A	C	B-A	N	Y	C
Surveillance	B-A	A	Y	A	A[1]	Y	A	E	A	Y	Y	D
Brass questions:												
Age	C-B	C-B	Y	Smthd	15-5	N	B	A	D	N	N	B
Duration of marriage	C-B	B	Y	Smthd	15-2	N	B	A	D	N	N	B
Time since first birth[2]	?	B	Y	Smthd	15-2	N	B	A	D	N	N	B
Survival, most recent birth	D-B	A	N	N/A	N/A	N	B	A	D	N	Y	B
Survival, previous birth	C-B	B	N	N/A	N/A	N	C	C-B	C	N	N	A
Truncated maternal history	C-B	A[3]	Y	N/A	N/A	Y	A	C	B	N	Y	C
Complete maternal history	B-A	A[3]	Y	B[3]	15-0	Y	A	C	A	N	Y	C
Household deaths	D-C	A	N	N/A	N/A	Y	A	A	D	N	Y	B

[1] Time range depends on length of time for which system is maintained at high quality
[2] Methodology not yet fully developed
[3] Will depend on sample size

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