

I. SCOPE AND METHODOLOGY

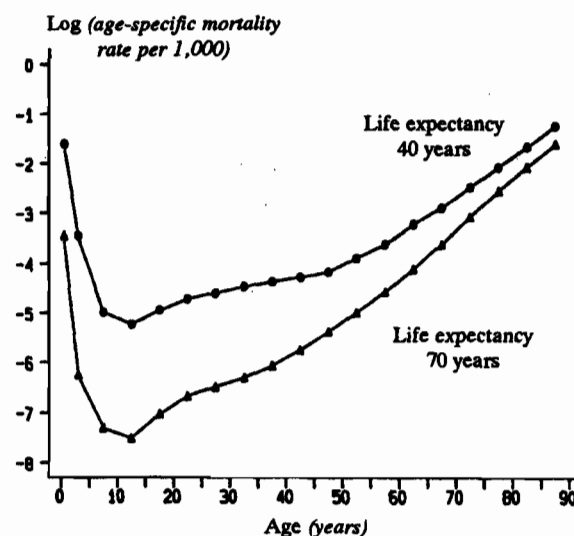
The main objective of this publication is to simplify the task of achieving consensus about recent levels and trends of child mortality in the developing countries. Chapter IV presents databases on which measures of child mortality can be based for every developing country¹ with an estimated 1990 population of 1 million or more and with data relevant to the estimation of child mortality during the period 1960-1990. Although 101 countries meet the population criterion for inclusion, 19 of those countries are not included because of a lack of available national-level data covering the period 1960-1990. Thus, the data set presented consists of 82 countries. For each country, the available national-level measures of child mortality from as many different sources as have been identified are presented graphically in a comparable manner. The data are presented without adjustment or smoothing as mentioned in the Preface. For each country, these graphs can be used to evaluate the estimates of child mortality drawn from different sources, in order to arrive at a plausible description of recent levels and trends based on all available sources rather than on one source or another taken individually. It should be noted, however, that this evaluation is not attempted here, although guidelines on how such evaluations can be carried out are given. The graphs also provide a convenient tool for assessing the plausibility of new estimates as they become available over the next few years. Any new estimate or series of estimates can simply be added to the appropriate graph to obtain a rapid visual assessment of the consistency of the new and existing information. Across countries, the graphs can be used as a methodological guide to which methods of measuring child mortality appear to work satisfactorily, or unsatisfactorily, in particular regions. It is useful first, however, to discuss briefly the nature of child mortality and the methods used to measure it. More detailed descriptions of these methods are given in the manual for estimation of child mortality (United Nations, 1990).

A. CHARACTERISTICS OF CHILD MORTALITY

The mortality risks faced by human beings vary sharply with age but exhibit an age pattern that does

Figure I. Age pattern of mortality at different life expectancies

(Based on Coale-Demeny West model life-tables for females)



Source: Ansley J. Coale and Paul Demeny, *Regional Model Life Tables and Stable Populations* (Princeton, New Jersey, Princeton University Press, 1966).

not vary greatly with the level of mortality. Figure I shows the mortality risks by age (on a logarithmic scale) typical of high- and low-mortality populations. Mortality risks are high in the period immediately after birth; they decline, often rapidly, to a minimum between ages 10 and 15, then rise gradually to about age 40, after which they rise steeply to high levels in old age. Although this broad pattern is very similar for all human populations, there are some variations between populations, particularly in the relationship of mortality risks prior to the adolescent minimum to those after that minimum (roughly corresponding to child mortality and adult mortality, respectively) and in the proportion of deaths during the first five years of life that occur in the first year. Since the focus in this volume is on child mortality, the discussion concentrates on patterns of mortality during childhood, primarily up to age 5.

B. MEASURES OF CHILD MORTALITY

The simplest single measure of mortality is the crude death rate, calculated as the ratio of deaths during a year in a population to the mid-year population, the ratio being multiplied by a conventional constant of 1,000. Thus, a crude death rate of 10.2 in 1990 means that there were 10.2 deaths per 1,000 population in that year. This measure clearly does not distinguish between deaths in childhood and those in adulthood and so cannot be used as an indicator of child mortality. Furthermore, the crude death rate is strongly affected by the age distribution of the population: given the same risks at every age, a population with a high proportion of elderly persons at high risk of mortality will have a higher crude death rate than another population with a much lower proportion of high-risk older people.

To avoid the effects of age distribution, mortality risks are calculated on an age-specific basis, often for single years of age or for five-year age groups. An age-specific mortality rate can be calculated as the ratio of deaths during a year in an age group to the person-years of exposure lived in that age group during that year, typically approximated by the mid-year population. Since mortality risks change little within an age group (except in the first five years of life), even extreme variations in age distribution within an age group can have little effect on such an age-specific rate. Age-specific rates can then be combined and summarized through the calculation of a life-table. First, those rates are converted into probabilities of dying between one age and another; for example, the age-specific mortality rate for age 4, between exact ages 4 and 5, is converted into a probability of dying between the fourth and fifth birthdays. The probabilities of dying between exact ages are then applied sequentially to a hypothetical group of births in order to calculate the number of survivors at each exact age. Thus, the probability of dying between age 0 (birth) and age 1 is applied to the hypothetical number of births to obtain the number of deaths between birth and age 1; the number of survivors to age 1 is obtained as the number of births less the number of deaths under age 1. The probability of dying between ages 1 and 2 is then applied to the number of survivors to age 1 to obtain the number of deaths between ages 1 and 2, and the number of survivors to age 2 is then obtained by subtracting this number of deaths from the survivors to age 1. This

process of chaining probabilities of dying continues until all the hypothetical births have died, at which point the life-table is complete. In the conventional notation of the life-table, the survivors at exact age x are denoted by $l(x)$, the deaths between age x and age $x+n$ are denoted by ${}_n d_x$, the probability of dying between x and $x+n$ is denoted by ${}_n q_x$, and the mortality rate between x and $x+n$ is denoted by ${}_n m_x$.

This publication is concerned primarily with three measures of child mortality: the probability of dying by age 1, ${}_1 q_0$, often called the "infant mortality rate"; the probability of dying by age 5, ${}_5 q_0$, called "under-five mortality"; and the probability of dying between the ages of 1 and 5, ${}_4 q_1$. In human populations, almost all deaths in childhood occur before age 5. Thus, the probability of dying by age 5 can be regarded as a good index of the overall level of child mortality that is readily comparable across populations. In low-mortality populations, mortality risks between ages 1 and 5 are also low, so that the infant mortality rate can also be used as a reasonable single index of child mortality. In populations with higher mortality, however, the infant mortality rate is a less satisfactory index of overall child mortality, because high proportions of child deaths occur between ages 1 and 5, and the proportions also vary substantially among populations, depending mainly upon the cause-of-death structure.

C. MODEL AGE PATTERNS OF CHILD MORTALITY: MODEL LIFE-TABLES

Age patterns of mortality are broadly similar across human populations even for very different overall levels of mortality, as is shown in figure I. This regularity makes it possible to create model mortality patterns, called "model life-tables", within which the mortality level varies while the age pattern remains broadly similar. Several model life-table systems have been proposed, but only two are considered here: the Coale-Demeny regional model life-tables (Coale and Demeny, 1966, 1983); and the United Nations model life-tables for developing countries (United Nations, 1982). Both systems are based on empirically observed life-tables selected after careful evaluation for accuracy; statistical methods were used to relate mortality at one age to mortality at another age for a range of mortality levels. As used here, both systems are single-parameter (mortality level) models for different groupings of similar age patterns of mortality.

The Coale-Demeny models are based on a large number of life-tables, principally from the countries of Europe in the nineteenth and twentieth centuries. Analysis showed that the life-tables fell into one of four typical age patterns of mortality, determined largely by the geographical location of the population. Each of these four patterns had a characteristic pattern of child mortality. In the Eastern European countries (contributing to the East family), infant mortality was very high in relation to child mortality. The Nordic countries (contributing to the North family) had high child mortality in relation to infant mortality. The countries of Southern Europe (contributing to the South family) had very high levels of child mortality in relation to infant mortality at high overall mortality levels, but rather low child mortality in relation to infant mortality at low mortality levels. The countries of Western Europe and most of the non-European populations included in the database (contributing to the West family) showed a pattern intermediate between the North and East patterns. In addition to variations in age patterns of child mortality, the four families also vary in their relationships between child, overall and adult mortality, but this type of variation is beyond the scope of this chapter.

The United Nations models are based on a smaller number of observed life-tables from developing countries that, after careful evaluation, were deemed to be accurate. As with the Coale-Demeny tables, the observed life-tables were found to fall into typical mortality pattern groups, again loosely associated with geographical region. Five patterns were identified: Latin American; Chilean; South Asian; Far Eastern; and a general pattern constructed as an average of all the observations. Variations between the groups were largely limited to the relationship between child and adult mortality: the only regional grouping with a distinctive pattern of child mortality is the Chilean, with high infant mortality in relation to child mortality.

Model life-tables are used for a number of purposes in the estimation of child mortality. First, age patterns of child mortality observed for some population can be compared with model patterns as a test of plausibility and thus of data quality. Model patterns are also used to convert one measure of child mortality into an equivalent alternative measure to compare estimates available from different sources. For example, an estimate of the infant mortality rate can be compared with an estimate of the probability of dying between ages 1 and 5 by finding the infant

mortality rate associated with the latter probability in some system of model life-tables. Thirdly, indirect estimation of child mortality based on aggregate information on child survival (see the discussion of indirect estimation or Brass methods given below) requires that the age pattern of child mortality be specified; model life-tables are used in developing the methods and a particular life-table system must be chosen in order to apply them.

D. DATA SOURCES AND ESTIMATION METHODS

The traditional source: vital registration

In the currently developed countries, measures of child mortality at the national level have traditionally come from the registration of births and deaths. The infant mortality rate is conventionally calculated as the ratio of deaths in the first year of life during a calendar year to births in the same year; this rate approximates the life-table probability of dying by age 1, ${}_1q_0$. If births and infant deaths are completely recorded, the measure will be unbiased. If the compilation and publication of statistics from the registration system are timely, the infant mortality rate thus obtained will provide a detailed and up-to-date time-series of a major component of childhood mortality. Such an annual series is available for Sweden from the middle of the eighteenth century to the present. Information from a registration system on deaths by age after infancy can be used in combination with information on the size of the population exposed to risk to calculate life-table measures of child mortality after infancy. The usual source of information on exposure to risk is a population census or a population projection from such a census.

The major problem with vital registration as a data source for estimating child mortality in developing countries is its quality. In many countries, birth registration is incomplete, biasing the denominator of the infant mortality rate; and in a still larger number of countries, the recording of infant deaths is incomplete, biasing the numerator. With potential errors in both the numerator and the denominator, the resulting ratio is subject to quite large errors, although underestimation is the usual outcome. Registration of deaths after infancy is, in general, more complete than in infancy, but the recording of the population by age (needed to provide denominators for rates after infancy) in childhood is also subject to error.

A second problem that affects the utilization of vital registration data in some countries is the delay in compilation and publication. Registration takes place at local administrative offices, and records have to be assembled and transmitted up the administrative ladder to some central processing location. In a number of developing countries, this process is slow and uncertain, and has in some cases even been blamed for coverage errors owing to the loss of records. Such administrative delays are compounded by the late registration of events. If a substantial proportion of events are registered after the year in which they occur and events are then tabulated by year of registration, the reported events for one year represent an average of demographic conditions over a number of preceding years. If, on the other hand, events are tabulated by year of occurrence, the data cannot be used until the large majority of late registrations have actually taken place and appeared in tabulations. Delayed registration also brings with it selection problems: the only children whose births are likely to be registered late are those still living, because there is little incentive to register the birth of a child who has died.

Experience with programmes to improve vital registration coverage has been rather discouraging. Several experiments in Africa and Latin America have attempted to improve the performance of the vital registration system through a step-by-step approach, upgrading the system in selected areas before moving on to additional areas with the ultimate intention of achieving national upgraded coverage. In Kenya, the improvement achieved in this way was not maintained (Gil and Ronoh, n.d.). Although the need for school admission or health-care provision will improve birth registration in general, it is unlikely to further the registration of children that die in the neonatal period. In many urban areas, burial permits that are obtainable only for registered deaths are required for disposal of a body. This type of measure, which has led to high levels of death registration in urban areas in many parts of the world, might improve the numerator of the infant mortality rate by more than its denominator, biasing the rates upward. Such a measure, however, has limited impact in rural areas. Experience suggests that complete vital registration evolves over time with general administrative development and as the importance of records becomes evident to the population at large.

Experience with the improvement of vital registration systems has therefore been largely negative. Programmes directed to speeding up the compilation and processing of records are the exception. In Egypt, for example, registration of vital events was reasonably complete up to the 1980s (possibly excepting infant deaths and the relevant births). In the mid-1980s, the system for compiling and processing registration data was completely overhauled, largely in order to provide a basis for evaluating the National Control of Diarrhoeal Diseases Project; and the delay was considerably reduced.

A second positive experience has been sample registration. In this process, a vital registration system is introduced in a representative sample of areas. Unlike national systems, registration in the sample areas is active rather than passive. That is, a person in the area is responsible for identifying vital events and making sure they are registered, in contrast to the more usual administrative system in which a registrar waits to be advised of the event. A successful example is the Indian Sample Registration System, introduced in some states of India in the 1960s and currently operating, apparently satisfactorily, throughout the country.

In summary, vital registration provides adequate estimates of child mortality in only a small proportion of developing countries. Of the 82 countries included in this volume, vital registration is the main foundation of such estimates in only 15, all but one of them in Asia or Latin America. For all developing countries, however, estimates derived from vital registration should be evaluated before they are used. Time trends in child mortality derived from vital registration data also need to be interpreted with caution. It is sometimes argued that trends may be correct even if levels are not, on the grounds that coverage is unchanging. This argument may be acceptable for short periods of time, but long series of data should be validated from time to time, say at five-year intervals, and recalibrated for coverage changes as necessary.

Non-traditional approaches: household surveys

In countries where the vital registration system as a basis for child mortality estimates is of uncertain or unacceptable quality, information from some type of

household survey is required to validate, calibrate or substitute for the vital registration estimates. Different types of surveys have been used for this purpose, and within similar types, different data relevant to child mortality have been collected. Here, a distinction is drawn between prospective and retrospective surveys. The various types of data that can be collected within these two forms are discussed below.

Prospective surveys

The essential characteristic of a prospective survey is that a defined population (or the population of a defined area or areas) is followed over time, with the vital events occurring to the population being recorded. Two types of prospective survey can be distinguished: the prospective sample survey; and the surveillance system.

Prospective sample surveys

In a sample (typically nationally representative) of areas, the initial population is recorded by a household survey, which can also include retrospective questions about demographic conditions in the past. The population is then resurveyed at regular intervals (usually six months or a year); the resurveys include questions on vital events occurring since the last round. The events reported are checked against the changes in household composition between rounds in order to minimize omission. With survey intervals of six months or less, omission of births and early infant deaths can be further controlled by the inclusion of a question on pregnancy status for all women at risk of child-bearing. Each pregnancy should correspond to a reported birth at a subsequent round, and the survival of the birth can then also be probed. This survey design can also be combined with active vital registration in sample areas, a combination commonly called a "dual-record survey". The Population Growth Estimation Experiment of Pakistan, 1964-1967 (Farooqui and Farooq, 1971) is an example of this type. With careful fieldwork, prospective surveys can provide relatively accurate estimates of child mortality and of differentials by age, sex and socio-economic conditions. Such surveys, however, require careful fieldwork over an extended period of time to estimate trends; and because of sample size limitations, they may have to be extended over several years to provide stable estimates of child mortality levels. This continuity of effort may be difficult to achieve in some developing country settings.

Surveillance systems

The surveillance system defines a population, typically of a single geographical area. The population is then followed intensively, with active registration, frequent house-to-house visits and occasional censuses. The best-known example is the Matlab area of Bangladesh (Cholera Research Laboratory, 1978). Surveillance systems provide a wealth of information about child mortality (among other things), but they are usually not nationally representative and indeed may become increasingly unrepresentative as various interventions are introduced in the surveillance area.

Retrospective questions in censuses and other household surveys

Questions that obtain information about child mortality can be included in censuses and surveys. Most of them, in one way or another, obtain information from mothers about the survival of their children, and methods used in such data are reviewed in considerable detail because they provide the backbone of the estimates presented in this volume. These techniques are all affected to a greater or lesser extent by potential selection bias, because in order for a child to be reported the mother must be a member of the study population at the time of the survey. Thus, either death or emigration of the mother can affect the reporting coverage. Before reviewing such techniques, however, a retrospective substitute for death registration is discussed.

Questions on recent household deaths by age

A population census or household survey can include a set of questions on deaths to household members using some reference period, typically 12 months, prior to the interview. For each death, the age and usually the sex of the deceased person is recorded. The survey itself provides the age distribution of the population and thus exposure to risk, so with a modest adjustment of the population from the end to the middle of the retrospective exposure period, age-specific mortality rates and a full life-table can easily be calculated.

Questions of this type have been included in a number of censuses and sample surveys, including some of the surveys included in the World Fertility Survey (WFS) programme. Experience with these

questions has been mixed. The reporting of adult deaths can be evaluated by comparing the age distribution of adult deaths with the age distribution of the population, but no similar simple evaluation exists for child deaths. Child mortality estimates based on recent deaths need to be carefully examined in the light of other existing sources before being accepted, given the mixed experience with the approach. Since this evaluation has to be made in comparison with other indicators of child mortality, the technique usually adds little to the body of evidence from existing data sources. If its estimate agrees with other sources, it can be accepted, but if it does not agree, it is dismissed.

Brass questions to women on aggregate numbers of children born and dead

Censuses or other types of household surveys can include questions concerning the number of children a woman has ever borne and the number among those children that died. These questions are often referred to as the "Brass" questions after the person who began the development of indirect methods for analysing the data (Brass, 1964). Such questions have been very widely used in all the less developed regions. Estimates derived from such data are presented for 70 of the 82 countries included in the database. By region, 82 per cent of the included countries of Africa, 85 per cent of those in Asia and Oceania and 91 per cent of those in Latin America provide estimates based on Brass questions from censuses or general household surveys. In view of the importance of this type of data, the estimation process is discussed in some detail.

The methodology can best be explained by a simple example. Consider a group of women of exact age 25, all of whom have just one child born exactly five years earlier when the women were exact age 20. The children have all been exposed to the risk of dying for exactly five years; thus, the proportion of children dead at the time of observation is the probability of dying by age 5, ${}_5q_0$, for the cohort of births. This probability will be some type of average of the period-specific mortality risks in operation at each time-point over the five years. In a second group of women, all aged exactly 25 and all with one child born at exact age 22, the proportion of children dead would be the probability of dying by age 3, ${}_3q_0$, and would be some form of average of mortality

risks in operation at each time-point over the past three years. If both groups of women were later observed at exact age 30, having had no further children, the proportions dead would be equal to probabilities of dying by age 10 for the first group and by age 8 for the second group. Thus, the current age of the women, in combination with the age of the women at the births, determines the length of the period of exposure to the risk of the children dying, as well as the time period over which the risks were experienced.

It can therefore be seen that the proportion dead of children ever born to women of a particular age will be equal to the probability of dying between birth and some age n , ${}_nq_0$, where n will depend upon the age of the women and the distribution of their births over the recent past. Brass used models of fertility and mortality to establish the relationship between the proportion dead of children ever born to women in a specified age group and the probability of dying by an exact age of childhood (under the assumption of unchanging fertility and mortality) through the use of simple indicators of the age pattern of fertility. A number of authors propose useful developments of the original method. Sullivan (1972) and Trussell (1975) expand the range of mortality and fertility models for which relationships are estimated. Of particular importance is the development of methods for estimating mortality trends under conditions of changing mortality. Feeney (1976, 1980) and Coale and Trussell (1978) propose methods for estimating reference dates for the child mortality estimates obtained if the assumption of constant mortality is replaced by an assumption of steadily changing mortality over time. The method can also be extended to allow for changing fertility. Coale and Trussell (United Nations, 1983) propose a method using indicators of cohort fertility patterns instead of cross-sectional parity ratios. Palloni and Heligman (1986) provide modifications suited for use with the United Nations model life-tables for developing countries. The Trussell and the Palloni and Heligman methods are used here to estimate child mortality levels and their reference dates prior to the survey. Both of these methods are explained in detail in the manual on estimation of child mortality (United Nations, 1990).

The relationship between the proportion dead and the life-table probability of dying is sensitive to the

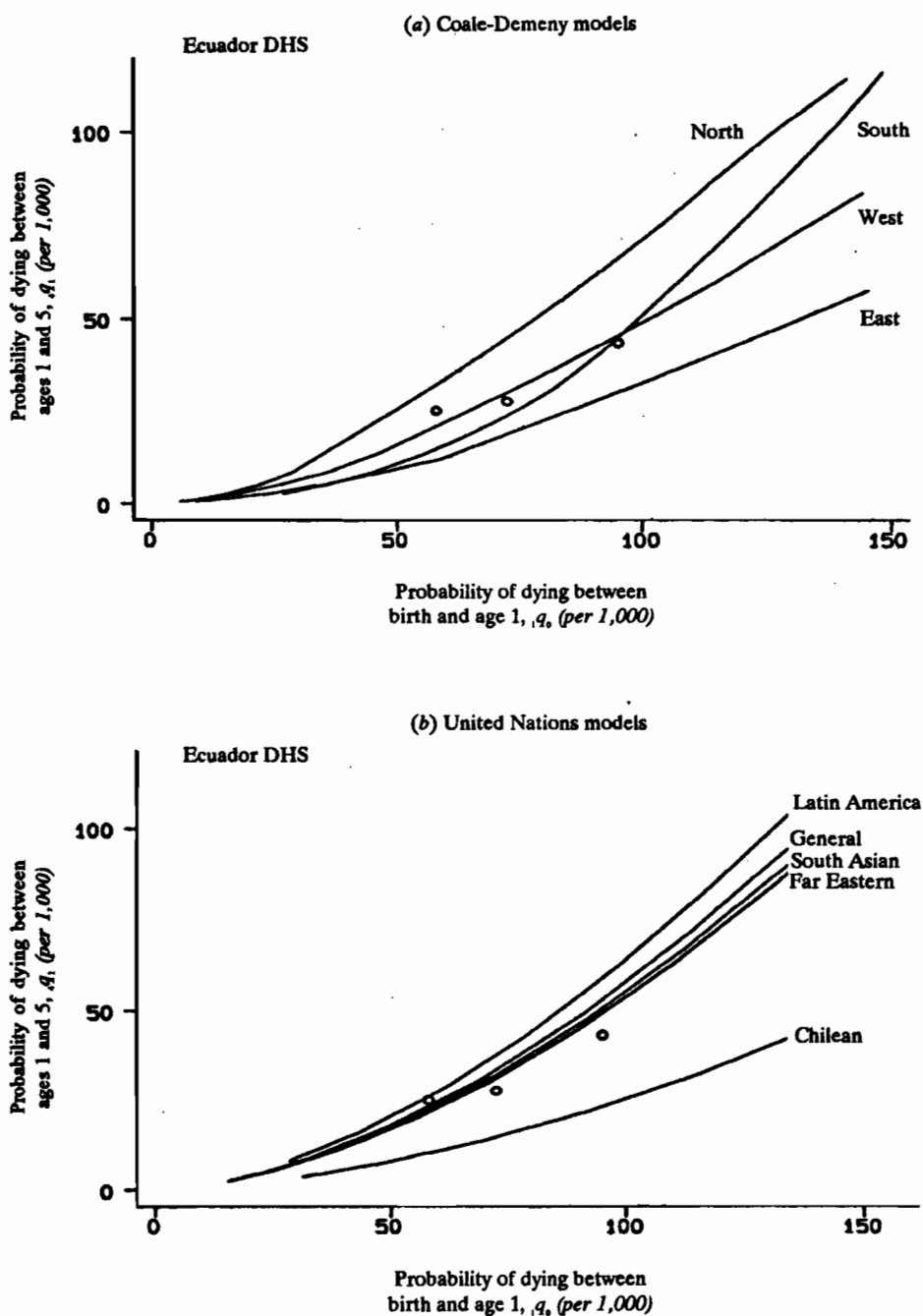
age pattern of mortality in childhood, at least for estimates of mortality in the first year or two of life. It is therefore necessary to select a mortality pattern from one of the model systems described above. There are three ways in which this selection can be made, depending upon the amount of relevant information available. First, independent information on the age pattern of child mortality, typically from a maternity history, can be used. A simple way to choose is to plot ${}_1q_0$ against ${}_4q_1$, preferably for a series of time-points, on a graph on which are also plotted the lines showing the relationship within a particular life-table family at different mortality levels. Figure II shows the points for three time periods from the Ecuador Demographic and Health Survey (DHS) using the Coale-Demeny and the United Nations model life-table systems, respectively. The points fall close to the Coale-Demeny West relationship or the United Nations general pattern for all three time-points, suggesting the use of one of these families. Secondly, if no direct data from a maternity history or from a multi-round survey are available for the country under study, it may be possible to select a model on the basis of consistency between two successive data sets. Figure III shows estimates of ${}_4q_0$ plotted against their reference time-points from two successive censuses of Kenya using different model life-table families. In figure III(a), the Coale-Demeny North model was used as the basis for estimation, whereas in figure III(b) the South model was used. The two sequences fit together very neatly when the North family is used but appear as two parallel lines with a hiatus between them when South is used. The North model appears to be more consistent with the available data than the South model. Although data errors will affect consistency, in the absence of more direct indications the model that gives the most consistent pattern over time is generally to be preferred. The choice of the North model for Kenya, however, is supported by direct indications from maternity histories. Thirdly, in the absence of direct indications or inter-survey consistency, a model life-table can be chosen on the grounds of geographical proximity. For example, the United Nations Latin American pattern could be used for a country in Latin America or a direct estimate from a neighbouring country could be taken as an adequate indication.

Despite the flexibility introduced into indirect estimation of child mortality over the past two decades,

assumptions remain. Three such assumptions—those concerning the nature of mortality change over time, the homogeneity of mortality risks by age group of mother and unchanging fertility—deserve further discussion. The estimation of reference dates for indirect estimates assumes that child mortality rates have been changing at a fixed rate for a considerable period in the past. This assumption is very often unjustified, for example, in a country that has undergone a period of civil upheaval. Even if mortality change has not been steady, an indirect estimate will still represent a weighted average of the period mortality conditions from the first child-bearing by women of the cohort being analysed to the present, but the weighted average may not fall on the actual trend line followed by child mortality. For example, a sharp spike in mortality during one year will raise the proportion dead for all women with children but will have the greatest impact on infants, where mortality rates are highest. The spike will therefore be spread over reports of women in a range of age groups, following the distribution of recent births. Thus, indirect estimation procedures cannot capture the full magnitude or exact timing of short-term fluctuations in child mortality but should be able, other things being equal, to capture longer term trends.

The indirect estimation procedures also assume that all children, regardless of the mother's age, are exposed to the same, homogenous mortality risks. This assumption is incorrect in at least one important respect. The women who have children at the youngest ages, particularly under age 20, tend to come from the less privileged strata of society; and a higher proportion of those children than of the children of older women will be first births. Both belonging to a lower socio-economic class and being a first birth are risk factors; thus, the proportions dead of the children of younger mothers often imply much higher mortality risks than do the proportions dead for older women. Therefore, the indirect estimate of child mortality derived from women aged 15-19 is excluded from the graphs presented in this publication, on the grounds that it is frequently seriously biased, as well as unreliable, because of the relatively small numbers of events. In some cases, the same selection bias also applies to information for women aged 20-24 and can be seen in the graphs as an upward deviation for the time-point closest to the survey date. The problem is

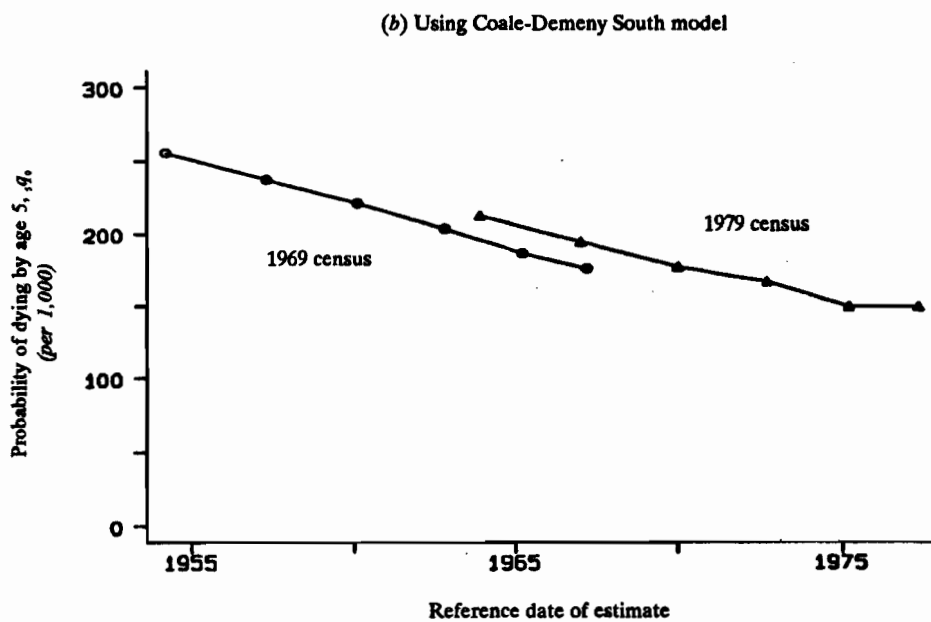
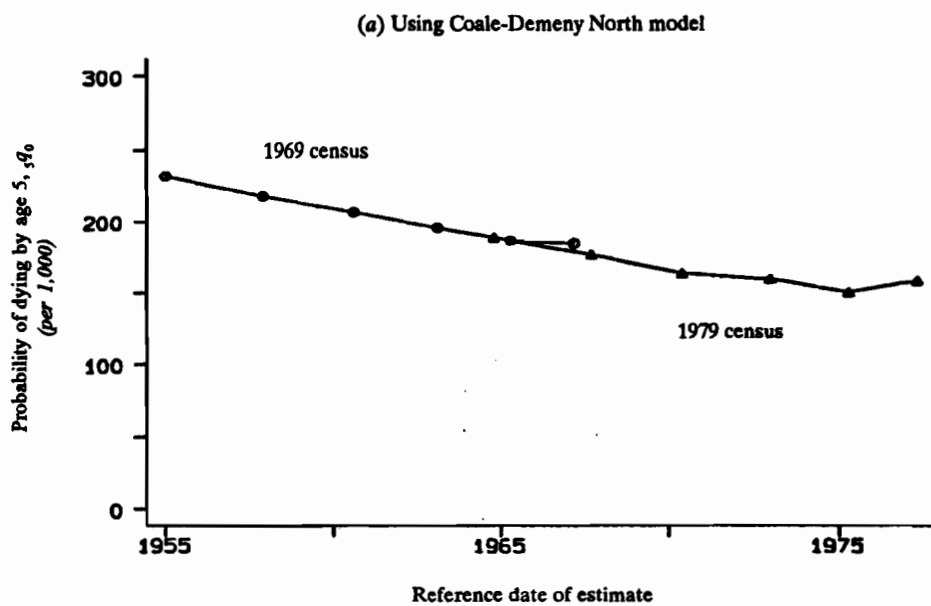
Figure II. Age patterns of child mortality in the Coale-Demeny and United Nations model life-table families, with observations from Ecuador



Sources: Ansley J. Coale and Paul Demeny, with Barbara Vaughan, *Regional Model Life Tables and Stable Populations*, 2nd ed. (New York, Academic Press, 1983); *Model Life Tables for Developing Countries*, Population Studies, No. 77 (United Nations publication, Sales No. E.81.XIII.7); and chapter IV of the present publication.

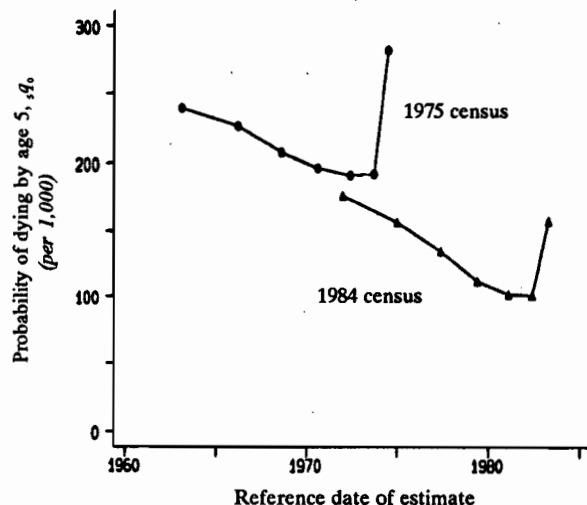
NOTE: DHS = Demographic and Health Survey.

Figure III. Indirect estimates of child mortality using different models, Kenya, 1969 and 1979 censuses



Sources: Chapter IV of the present publication and additional calculations.

Figure IV. Indirect estimates of child mortality from 1975 and 1984 censuses of Tunisia, all age groups



Source: Chapter IV of the present publication.

very pronounced in the case of Tunisia, shown in figure IV using data from the 1975 and 1984 censuses. It is unfortunate that this bias applies to the youngest women, because it is these women whose children have been exposed only to the most recent child mortality risks and whose reports thus provide estimates for period mortality closest to the time of the survey. The proportion dead of children born to women aged 15-19 should in theory provide an estimate of child mortality little more than a year before the survey; that for women aged 20-24 should give an estimate for some two or three years before the survey. In practice, neither point is generally reliable, and the most recent estimate available is that based on reports of women aged 25-29, with a reference date some four or five years prior to the survey.

It may be noted in passing that this problem of non-homogeneity of mortality risks is much reduced if the information on children ever born and children dead can be tabulated by time since first marriage or time since first birth of the mother. The shortest exposure group, time since first marriage or first birth group 0-4 years, clearly still includes a disproportionate number of first births, but it includes births from all social groups rather than primarily from the disadvantaged. The reference date for an

estimate based on this shortest exposure group is also only a year or two prior to the survey date, improving the timeliness of indirect estimates. Thus, consideration should be given to the inclusion in general household surveys and possibly even in censuses of a question on date of first marriage (for countries where almost all child-bearing occurs within marriage) or date of first birth (for countries with substantial proportions of premarital or extramarital births). Data have not been analysed by duration of marriage in this data set because of the almost perfect overlap between the data sets that have information on duration of marriage and those which include complete maternity histories and thus provide direct estimates that limit the need for indirect estimates.

The third assumption is that fertility has been constant, or rather, that cross-sectional parity ratios for age groups 15-19, 20-24 and 25-29 can be used to represent the distribution of births in the past for different cohorts of women. Fertility changes after age 30 have no effect on these ratios and will not affect the estimation procedure. If fertility under age 30 has been falling, however, perhaps as a result of rising age at marriage, parity ratios for age groups will not represent the birth distribution in the past for any cohort. The ratios will tend to be too small, indicating too recent a pattern of births and thus tending to overestimate child mortality levels and assign to them time-points that are too recent. The problem of changing fertility may also be reduced by classifying women by duration of marriage or duration of motherhood rather than by age, or if data from successive surveys are available, by using the Coale-Trussell procedure based on cohort, rather than period, parity ratios.

In summary, the indirect estimation of child mortality from aggregate information on children ever born and children dead for women classified by age group has worked well in a wide variety of data-collection vehicles (censuses, large surveys, small surveys etc.) and socio-economic contexts. However, although in most applications the data appear to be of reasonable quality, there are undoubtedly some applications in which the data are suspect. The most common data flaws are the omission of dead children, particularly by older respondents, leading to underestimates of mortality for the most distant time periods and thus to underestimates of the rates of child mortality decline. Stillbirths and miscarriages

could be included in the number of children ever born and dead children, increasing the proportion dead and leading to overestimates of child mortality. Age-misreporting by the mothers will affect most data sets, although the likely impact of such errors is not clear-cut. In addition to data errors, deviations from the assumptions of the method may also bias estimates. Heterogeneity of risk will distort (usually upward) estimates based on reports of women aged 15-19 and to a lesser extent those aged 20-24; sharp or other non-linear changes in mortality with time will be reduced in amplitude and spread over adjoining time periods; and declining fertility will distort the allowance for fertility patterns, tending to overestimate recent child mortality. These potential biases need to be borne in mind when interpreting the indirect estimates in comparison with estimates from other sources.

Questions to women on their most recent birth and its survival

Censuses and surveys in the developing countries often ask women of reproductive age about recent child-bearing, through a question either on the date of the most recent birth or on whether the woman had a birth in a defined period, such as the 12 months prior to the survey. The primary objective of such a question is to measure fertility, but an additional question on the survival of the child also provides a basis for measuring early child mortality. It is not possible to use information on births more than 12 months prior to the survey because of selection bias; the birth interval following the birth of a child that dies is, on average, substantially shorter than that following the birth of a child that survives, partially because of the interruption of breast-feeding that occurs when a child dies. As a result, the births of children that died in infancy are more likely to have been followed by subsequent births and thus to have been superseded as most recent births. Therefore, data on survival of recent births can only provide an estimate of infant mortality; no usable information is obtained about later child mortality. In its simplest form, this method has tended to give rise to clear underestimates of infant mortality, probably largely because of a tendency to fail to report the birth of a child that subsequently died as being a most recent birth.

Maternity histories

In a maternity history, each woman (in some cultures, only those ever married) is asked for the date of birth and, if applicable, the age at death of each of her live-born children. Data of this type permit the calculation of life-table measures of child mortality for time periods, as well as extensive analysis of the relationships between child mortality risks and characteristics of the child, the mother and her environment. The collection of such data has expanded enormously since the mid-1970s, as a result of the World Fertility Survey and the Demographic and Health Surveys programmes. Maternity histories are available for 51 of the 82 countries included in this volume. By region, such histories are available for the following proportions of the countries included in this publication: Africa, 61 per cent; Asia and Oceania, 52 per cent; Latin America, 78 per cent.

Maternity history data do have certain limitations. As a rule, the questions are only put to women of reproductive age, resulting in progressive age truncation for time periods further in the past. At the survey, only women under age 50 are interviewed; for the period five years earlier, only births to women under age 45 are recorded; for 10 years earlier, only births to women under age 40; and for 15 years earlier, only those to women under age 35. Strong differentials in child mortality by age of mother could distort trends in the more distant past, unless only children born to women under some specified age are considered. Because of this potential truncation bias, the database only presents estimates of child mortality based on maternity histories for the 15 years preceding the survey. The data may also be distorted to some extent by incorrect dating of births, particularly if the errors vary with the survival status of the child. Misreporting of age at death is also a common problem and has a particularly large impact on the infant mortality rate; deaths that occur around age 1 are often reported as occurring at age 1, with the result that deaths that should contribute to the infant mortality rate are moved into the age range from 1 to 2. Reporting of children that have died may also be incomplete, although it appears that careful training can reduce this error to low levels even in largely illiterate societies. This need for

careful training, however, has an important consequence in that it severely limits the maximum sample size (often to about 10,000 women), and this limitation restricts the temporal and spatial detail that can be obtained. In order to accumulate sufficient events to provide stable estimates, child mortality measures are usually calculated for five-year time periods, and even for such wide periods the sample variability on the estimates may be substantial.

A complete maternity history collects the numbers of children ever born and children dead for each woman and thus provides the information needed for the application of indirect estimation procedures. These indirect estimates can be compared with direct measures. Because the numbers of births and deaths are identical for both methods, differences in the resulting estimates arise from different locations (explicit in the case of direct estimates, implicit in the case of indirect estimates) of events in the past. Mislocation of births in the past and heaping of age at death on preferred digits may substantially affect direct estimates (particularly of the infant mortality rate), whereas changing fertility and inability to capture short-term fluctuations adequately may affect indirect estimates.

Truncated maternity histories that capture only events in the five years or so preceding the survey have been used as an alternative to complete maternity histories. Although truncated histories lessen the amount of fieldwork somewhat, they greatly limit the amount of information obtained about mortality. No information is obtained about trends, and there are only very limited observations of mortality for ages over 3. Moreover, the truncated history format has proved difficult to implement in the field.

In summary, maternity history data, such as those collected under the auspices of the World Fertility Survey and the Demographic and Health Surveys programmes, have provided a wealth of information on child mortality in developing countries. The main contribution of such data has been to the exploration of differentials and associations in child mortality rather than to the determination of levels and trends, because of the small sample sizes typically involved in surveys of this type.

E. EVALUATION OF CHILD MORTALITY ESTIMATES

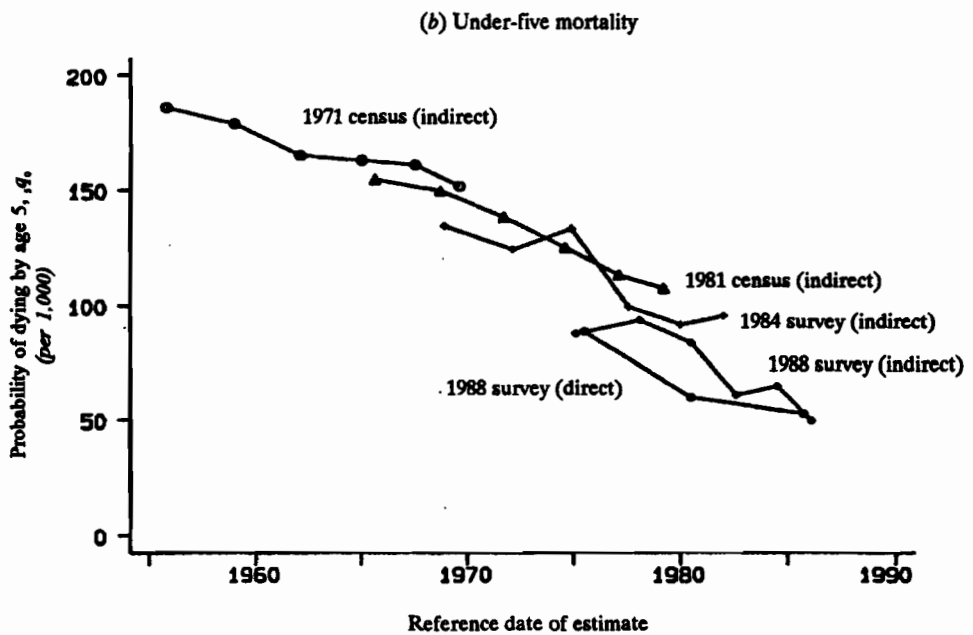
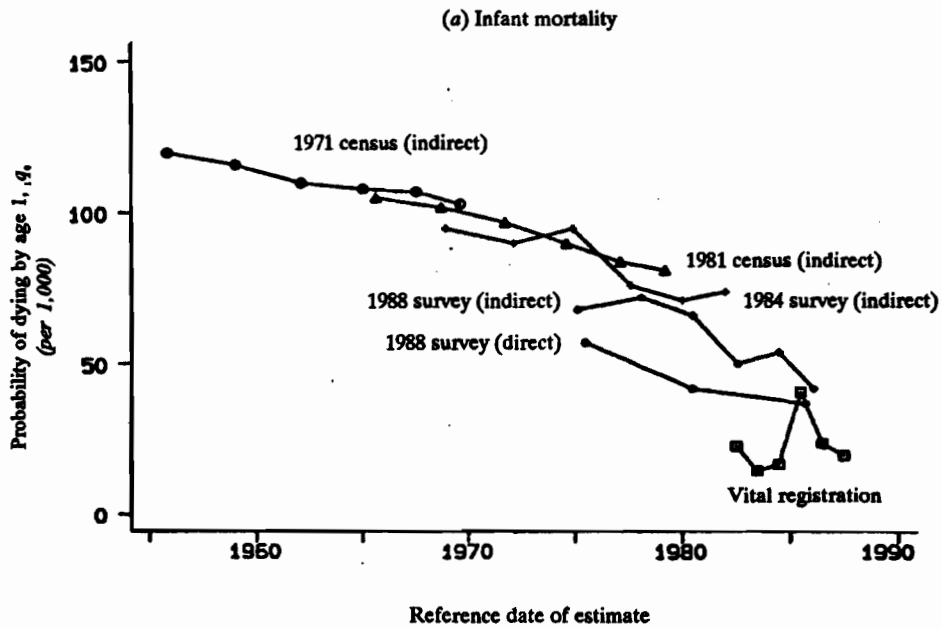
The foregoing discussion was included to provide a basis for understanding how to evaluate estimates of child mortality from different sources. Two approaches to evaluation can be used: the "common index" approach, in which estimates from different sources are converted into an equivalent standard indicator for graphical comparison; and the inter-survey change approach, whereby consistency of reporting at successive surveys is assessed.

"Common index" approach

Particular types of data suffer from certain types of error, and the patterns of these errors can often be seen in a comparison of estimates from different sources. To make such comparisons, estimates from different sources must be converted into comparable indices with comparable reference times. The conversion into a common index (the probability of dying by age 5, ${}_5q_0$, is a good choice because of its robustness to choice of mortality pattern) is made through a system of model life-tables. Comparable reference times are approximated by locating indirect estimates at their reference time-points and locating other measures at the approximate midpoint of the exposure to risk period underlying them. The different estimates, in comparable forms, are displayed graphically in order to show the pattern and extent of agreement and disagreement. An attempt can then be made to explain the inconsistencies in terms of the known strengths and weaknesses of the various methods.

Data for Botswana can be used to illustrate the common index approach. Brass questions were included in the 1971 and 1981 censuses and in a household survey (Family Health Survey I) in 1984, providing three series of indirect estimates. In 1988, Family Health Survey II (DHS) collected maternity histories, providing both direct and indirect estimates of child mortality. Vital registration provides numbers of births and infant deaths for the mid-1980s. Figure V(a) shows estimates of the infant mortality rate obtained either directly from the 1988 DHS and vital registration or indirectly by converting ${}_5q_0$ estimates into ${}_1q_0$ equivalents using the Coale-Demeny South model life-tables. The two censuses show a very consistent downward trend in the infant

Figure V. Estimates of infant and child mortality, Botswana, various sources



Source: Chapter IV of the present publication.

mortality rate, falling from about 120 in 1955 to about 80 in 1980. The indirect estimates from the 1984 survey are a bit more erratic than the census estimates and appear to fluctuate around a somewhat lower level, although the difference is not large. The indirect estimates from the 1988 DHS are still more erratic and again appear to fluctuate around an even lower level. The direct estimates from DHS show a smoother decline but around a substantially lower level than the DHS indirect estimates. The vital registration estimates are very erratic and are below any other estimates except that for 1985, which shows a sharp spike. The comparison suggests that the vital registration estimates are too low. The substantial difference between the direct and indirect estimates from DHS suggests that the direct estimates may have been reduced by overreporting age at death in the maternity history; deaths around age 1 were probably reported as occurring at 12 months, thus taking them out of the infant mortality rate. The general comparison of the DHS estimates with those from other sources suggests that the former figures are systematically too low. This apparent error could be due either to sampling problems or to omission of dead children from the DHS histories. The greater variability of the survey-based estimates in comparison with the census-based estimates can almost certainly be ascribed to the sampling errors affecting the surveys; similar examples are given throughout this volume.

Figure V(b) shows a comparison of estimates of q_0 for Botswana. In this case, the indirect estimates have been converted into equivalent q_0 values, again using the Coale-Demeny South family of model life-tables, whereas the maternity history estimates provide direct measures. No estimates are available from vital registration. The basic pattern is much the same as in figure V(a) except that the difference between the direct and indirect estimates from DHS has narrowed considerably. Otherwise, the census estimates are smooth and consistent, the 1984 survey estimates are more erratic and somewhat lower, and the 1988 DHS estimates are even more erratic and even lower. The improved consistency between the DHS direct and indirect estimates offers support for the view that the differences in the infant mortality rates were largely due to heaping of age at death on 12 months.

Inter-survey change approach

When working with surveys collecting data on aggregate numbers of children ever born and dead that are conducted about five years or about 10 years apart, it is possible to compare the average number of children ever born and the average number of children that died at two time-points for the same cohort of mothers. For example, with a five-year interval between surveys, it is possible to compare the average parity and average number of children dead for women aged 25-29 at the first survey with those for women aged 30-34 at the second survey. Since the women aged 30-34 at the second survey are the survivors of the women aged 25-29 at the first, the change in average parity represents inter-survey child-bearing by the cohort (and cannot be negative), whereas the change in average numbers of dead children represents inter-survey child deaths for the cohort of mothers (and again cannot be negative). Implausible changes, whether large or small, indicate changes in sampling or reporting quality and provide a warning against naïve acceptance of the estimates. These comparisons cannot by themselves indicate which, if either, survey is wrong, although comparisons with other estimates available for the population may offer some clues.

TABLE 1. COMPARISONS OF CHILDREN EVER BORN AND CHILDREN DEAD BY AGE GROUP OF MOTHER, BOTSWANA, 1984 AND 1988

Age group	Average parity		Average number of children dead	
	1984	1988	1984	1988
15-19	0.25	0.26	0.010	0.009
20-24	1.44	1.17	0.120	0.053
25-29	2.87	2.55	0.250	0.155
30-34	4.16	3.70	0.412	0.215
35-39	5.36	5.09	0.740	0.418
40-44	6.27	5.43	0.853	0.527
45-49	6.84	5.75	1.067	0.556

Data for Botswana from Family Health Survey I (1984) and from Family Health Survey II (1988, DHS) are used here as an example. Table 1 compares the average numbers of children ever born

and the numbers dead by age group from the two surveys. The two surveys were not conducted exactly five years apart—the interval was close to 4.5 years—so the correspondence between age groups and cohorts is not exact. It is, however, close enough to provide a valuable consistency check. The women aged 20-24 at the time of the 1984 survey would have been aged about 24.5-29.5 at the time of the 1988 survey; therefore, some 90 per cent of women from the first cohort would be identified in the 25-29 age group at the second survey. Comparing the average numbers of children ever born in columns (2) and (3) of table 1, each cohort adds to its average parity between 1984 and 1988 except for the 1984 cohort aged 40-44, whose parity declines from 6.27 in 1984 to 5.75 for age group 45-49 in 1988; the increment for the cohort aged 35-39 in 1984, from 5.36 to 5.44, is also suspiciously small. Thus, the numbers of children ever born reported in 1988 are not consistent with the numbers reported in 1984, at least for older women. The omission of children ever born to older women in the second survey appears to be the likely explanation, although sampling error cannot be ruled out.

Comparison of the average numbers of children that died reported in the two surveys and shown in table 1, columns (4) and (5), reveals more serious

inconsistencies. From the cohort aged 25-29 in 1984 upward, the average number of children dead is lower (or the same, for the cohort aged 30-34) in 1988 than in 1984. For the cohorts aged 35-39 and 40-44 in 1984, the differences are very large: 0.74 dead child per mother in 1984 for women aged 35-39 falls to 0.53 for women aged 40-44 in 1988; and 0.85 for women aged 40-44 falls to 0.56 for women aged 45-49. The numbers of dead children reported in 1988 are clearly well below the numbers reported in 1984, explaining the lower direct and indirect estimates of mortality derived from the 1988 survey. The lower reporting may be due either to greater omission of dead children in 1988 or to sampling problems in 1988; the decline in both children ever born and children dead and the very early age (25-29) at which the differences become evident suggest sampling problems rather than omission levels as the source of the problem. The consistency of the two censuses and the 1984 survey strongly suggests that it is the 1988 survey which is incorrect.

NOTE

¹ Including the countries of Africa, Asia (excluding Japan), Latin America and Oceania (excluding Australia and New Zealand).