Have infant and child mortality increased in West Africa? Evaluation of evidence from Demographic and Health Surveys

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Acknowledgments

This research was supported by a grant from Macro International. We would like to thank Shea Rutstein and Jerry Sullivan for helpful comments.

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Abstract

Recent Demographic and Health Surveys (DHS) have shown increases in infant and/or child mortality in Burkina Faso, Cameroun, Ivory Coast, Mali and Senegal. This article examines data from eight countries in West Africa and Cameroun which had multiple DHS surveys during the past 15 years. Data quality checks are done and adjustments are made where deficiencies are found. After adjustments, a significant downward trend is documented for Mali and Niger. In four other countries infant and/or child mortality is higher for the last five-year time period than for the five-year period before the previous survey, but not significantly so. Three of these four countries have HIV prevalence among pregnant women of 5%-12% which could explain the increases.

Africa has the highest levels of infant and child mortality in the world and within Africa, four of the eight countries with infant mortality above 120 per 1000 are in West Africa -- Guinea Bissau, Sierra Leone, Niger and Liberia (Population Reference Bureau, 2003). Mortality was much higher in previous decades[1]. Hill (1993) documented the decline of mortality up to 1985 for about 20 African countries.

Local studies have established the correlates of the mortality decline. For example, Pison et al. (1993) showed a very rapid decline of under-five mortality in the Mlomp area of Senegal and attributed the decline mainly to improved health services. Effects of health interventions–-immunization, ORT, malaria control, and antibiotic treatment of respiratory disease--have been reviewed in the Sub-Saharan Africa (SSA) context (Ewbank and Gribble, 1993); declines due to measles and tetanus immunizations and ARI treatment in particular, are documented.

Since 1987 the Demographic and Health Surveys project (DHS) has conducted nationally representative surveys in most African nations and these provide estimates of infant and child mortality (See for example, Bicego and Ahmed, 1996). In the DHS, women age 15-49 in sampled households are interviewed. In addition to standard modules or sets of questions on background characteristics, contraceptive knowledge and practice, marriage, and AIDS knowledge, each woman is asked a complete birth history and then breastfeeding, nutrition, immunization and health information about each child under a certain age (usually five but sometimes three years). In the birth history, starting with the first birth and for each consecutive one, questions are asked about date of birth, name, sex, survival status, and age at death if deceased.

Recent DHS surveys in a number of African countries have shown an increase in infant and/or child mortality. Of the sixteen countries in SSA which have had a second or third DHS survey since 1995, ten showed an increase between surveys in infant or child mortality or both. In West Africa, of the eight countries that have had two or more surveys (Burkina Faso, Ivory Coast, Ghana, Mali, Niger, Nigeria, Senegal and Togo), four have shown increases (all more than 10%) in infant and/or child mortality, comparing estimates for the five-year period before the respective surveys. Table 1 gives these estimates.

In these analyses we will also include Cameroun, which is actually in Central Africa but borders West Africa and has had an increase in mortality.

TABLE 1 ABOUT HERE

The first thing which comes to mind for many upon hearing of increasing mortality in Africa, is HIV/AIDS. Indeed, AIDS is a major epidemic in Sub-Saharan Africa with approximately 2 million AIDS deaths in 1999 and 70% of the world's HIV-infected persons residing there (UNAIDS, 2003). However, only Ivory Coast, among all the West African nations has an HIV-prevalence level among pregnant women above 10%[2]. This is in marked contrast to some nations of East and Southern Africa where as high as 40% of pregnant women are HIV-positive, and increases in child mortality there are linked to the epidemic (U.S. Census Bureau, 2003). Since data are lacking on HIV prevalence in these DHS surveys (except in the Mali survey of 2001), this will be considered again in the discussion section.

As further potential explanations, the DHS surveys in Burkina Faso, Mali, and Cameroun which showed increases in mortality also showed that child malnutrition rose and/or child immunization levels declined (Barrere, Mboup and Ayad, 1999; Mali Republique, 1996). Economic structural adjustment and currency devaluation in the last decade have made conditions difficult in the region in general and in Cameroun in particular. In other countries with devaluation, salaries of civil servants were eventually raised to partially make up for the lost purchasing power; however in Cameroun these salaries were actually reduced (Israr et al, 2000).

However, before attempting explanations of these reported mortality increases, it is important to assess the validity of the reported changes. Two concerns must be addressed in this regard--data quality and sampling variability.

DATA QUALITY

Four types of reporting errors in birth history data can affect estimates of period infant and child mortality rates: omission of dead children, incomplete information on date of birth and/or age at death, displacement of events in time, and errors in reported age at death. There are calculations available which can sometimes detect these response errors and adjustments that can be made[3]. Each type is now considered.

Omission of Births and Deaths. Events omitted from birth histories are disproportionately children who have died. These omitted deaths usually have distinct characteristics: they tend to be babies who died soon after birth and/or many years before the survey. Levels of omission can be measured with validation or reliability studies. In a validation study of pregnancy history data in Bangladesh in 1980 (in the Matlab vital registration area), surviving children were omitted at a rate of 1.4 percent (n=1497) and dead children at a rate of 2.6 percent (n=266) among births within 14 years of the survey (Becker and Mahmud, 1984). A more recent validation comparing birth and pregnancy histories in the same area yielded an omission rate for deaths of 7.0% (n=327) for the birth history and 4.2%(n=380) for the pregnancy history (Espeut, 2002).

In reliability studies in which data are compared from interviews with the same respondent twice, it is presumed that women do not invent deaths, so deaths reported in one survey but not in the other are defined as omitted from the latter. A reliability study using a pregnancy history questionnaire in Zaire (Chahnazarian, 1988) found an extremely high rate of omission of dead children in a previous birth history questionnaire (61%, n=790 total deaths) which was five times that for living children (12%; n=2499 total births). A parallel study in Liberia but with two 7-year truncated pregnancy histories found that 5% of surviving children (n=908) and 28% of dead children (n=343) were missed in the original survey (Becker, Thornton and Holder, 1993).

One of the few reliability studies of DHS was done in Pakistan after the 1991 survey (Curtis and Arnold, 1994). From matching of events of the same women in the two surveys, it was found that women reported 37% more births in the reinterview than in the main survey for the three-year period before the survey (n=459 births in the reinterview); the corresponding figure for deaths was 49% (n=281 deaths in the reinterview). The vastly better reporting in all these reinterviews could be due to one or a combination of several factors: typically the best interviewers (or supervisors) do the reinterviews, and the questionnaires for the reinterviews were all much shorter than the original ones so more focus could be given to the pregnancy or birth history. Clearly omission of vital events can be a major problem in demographic surveys.

In the absence of reliability studies[4], indirect approaches are available to detect omission. Because the risk of death is highest in the first hours and days of life and declines very rapidly thereafter in all populations, it is possible to detect differential omission of early deaths by examining the ratio of early neonatal deaths (those in the first week of life) to all neonatal deaths (those in the first four weeks of life). The expectation is that approximately 70% of neonatal deaths occur in the first six days of life (Sullivan, Bicego and Rutstein, 1990). As overall mortality falls, it is also known that this ratio tends to rise.

Omission of births can also sometimes be discovered by calculation of the sex ratio at birth for different years. It is known that the sex ratio at birth in human populations normally varies only between 103 and 107 (James, 1987).[5] Of course to detect omission, one must consider sampling variability.   Birth date displacement that is differential by sex could cause such distortions also, but in the absence of omission this displacement would be seen by an excess of the opposite sex in another period.

The sex ratio of deaths can also be calculated and, for biological reasons, there should be more male than female deaths in the neonatal period. However, the sex ratio of deaths cannot reliably be used to detect omission at later ages as it can vary quite widely due to differences across societies in the treatment of girls *vis á vis* boys. It is also true that there can be significant rates of omission and at the same time both the sex ratio of births and of deaths are within normal range, i.e. if omission rates do not vary by sex.

Incomplete Data. Women may remember their births and child deaths but not the dates of the events. For births with missing information, a hot-deck procedure is used by DHS staff to allocate a month and/or year of birth (Croft, 1991); the procedure considers birth interval and other factors in imputing a date. Presumably this imputation should not lead to any systematic error. In previous DHS surveys the proportion of birth events with missing information has been low–-an earlier analysis of nineteen DHS surveys from 1986-1989 showed a median of only 0.5% of cases that required imputation of both month and year (Arnold, 1990).

Displacement of Events in Time. When women have difficulty recalling exact dates of their birth events, the interviewer is instructed to probe using stated age of the child, historical events, and so on, and in most cases eventually a date is recorded. Validation studies have shown that women are slightly more likely to pre-date the birthdays (over-report ages) of their children than the opposite (Becker and Mahmud, 1984; Bairagi, Edmonston and Khan, 1987; Espeut, 2002). In the DHS if a woman was not sure of the date of birth, the interviewers could avoid work by placing the estimated date beyond a certain cutoff. Details regarding the DHS questionnaire will clarify this. After the entire birth history is collected, the mother is asked for detailed information about prenatal and delivery care, breastfeeding, immunization, and health for children born within a fixed time period (three or five years depending on the survey) of the interview. To abbreviate, we call these pregnancy and health modules. There was variation between rounds of DHS surveys in this regard. In the early rounds in almost all countries where immunization information was asked, it was asked for both surviving and dead children. However, in the third round of surveys (1993 to 1998) the questionnaire asked the immunization questions only for surviving children. Thus if the birth date for a child was not given by the mother (which is quite often the case in countries with low literacy) interviewers could avoid work by assigning a date of birth of both surviving and dead children beyond the five year cutoff in the earlier survey round, and the incentive would be greater for surviving (than for dead) births in the later survey round.

The displacement of events beyond the reference or cutoff date of the pregnancy and health modules can be seen by tabulating birth events by year of birth for surviving and dead children and calculating ratios $100*(2B_x/(B_{x-1} + B_{x+1})$ where B_x refers to the number of births reported in year x. Normally these ratios should be close to 100. In data quality tabulations for nine SSA surveys between 1986 and 1989, Arnold (1990) found ratios between 71 and 101 (median = 89) for year five before the survey, but values between 106 and 144 (median = 112) for the 6^{th} year before the survey. For dead children the ratios of births at year five were sometimes lower and sometimes higher than those for living children, so the effects of the displacement on mortality estimates would be variable. In surveys between 1990 and 1993 in Sub-Saharan

African nations, the birth ratios for year five ranged from 74 to 104 (median=89) for living children and 62 to 112 (median=74) for dead children (Curtis, 1995, Table 5.3), indicating a probable greater displacement of deaths than surviving children. This implies that there is very likely underestimation of under-five mortality for the period immediately before the survey. Model results suggest that the order of magnitude of the underestimation is 4-5% (Sullivan, Bicego, and Rutstein, 1990).

In some of the third round of DHS surveys the reference period for births for the pregnancy and health modules was switched to three years so displacement would have less effect on mortality (and fertility) calculated for five-year periods. The effects on mortality estimated for five-year intervals before the survey can be expected to vary between surveys where the reference period was switched from five to three years, i.e. there would be little or no effect when a three-year reference period for the pregnancy and health modules was used, but a noticeable impact may occur with a five-year reference period.

Mis-reported Age at Death. In the birth history women are asked to specify the age at death of each deceased child in days if the child was less than one month old, in months up to two years of age, and then in years. Heaping typically occurs at 7 days, 30 days, 12 months and 24 months. A classic problem is heaping at 12 months of age. This affects the calculation of infant mortality, i.e. if some children reported as dying at 12 months of age were really only 11 months old, then the infant mortality rate will be underestimated because 12 months is classified in the 1 to 4-year age group. The magnitude of this problem can be assessed by calculation of heaping ratios [e.g. $3*D_{12}/(D_{11}+D_{12}+D_{13})$] where D_x is the number of deaths reported at age x in months; the ratio should be close to 1.0 if there is no heaping.

For eleven SSA countries with DHS surveys in the period 1990-93, Curtis (1995), using a related index [$4*D_{12}/(D_{10}+D_{11}+D_{13}+D_{14})$] found that it ranged from 1.9 to 7.7 (median =4.4) for deaths 0-24 years before the survey. This represents a major problem of heaping, i.e. an average of four times more values on 12 months than expected. The heaping was only slightly less for the period 0-4 years before the survey. By taking 25% of the heaped deaths out of 12 months and putting them into the infant period, Curtis found that infant mortality estimates would increase by 1% to 7% (median=3%) across countries. But the logic of the 25% figure is elusive, particularly since validation studies have found ages of children in general to be over-stated. Furthermore, DHS does not adjust infant mortality estimates for this heaping at all, which seems problematic. One reason for not adjusting these data is that it is unclear what percentage should be allocated to the infant period. Below we present results from a validation study which provide an estimate of this percentage.

In summary, there are known problems of quality with survey data used for estimates of infant and child mortality. These problems need full exploration to determine whether they can account for recent reported increases in mortality in West Africa. In particular, it is crucial to address potential changes in data quality between survey rounds that could lead to biased estimates of changes.

Sampling variability is also important to account for in comparing mortality estimates from DHS surveys. DHS gives sampling errors in the appendix of the First reports but does not use them in the presentation of mortality trends in the same report. Changes in the estimates over time could be due merely to sampling variability and not represent true changes. We will estimate the sample variance for each mortality estimate.

METHODS

For these analyses we utilize the birth history data for the twenty DHS surveys in the eight West African countries (Burkina Faso, Cameroun, Ivory Coast, Ghana, Mali, Niger, Nigeria, Senegal and Togo) and Cameroun which had two or more surveys since 1986.[6] A brief description of the surveys is found in Table 2 and details are found in the First country reports (See references). These nationally representative sample surveys included between 3044 (Ivory Coast 1998) and 9704 (Mali, 1995/96) women of reproductive age and all had response rates above 90%.

The purpose of the analyses is to determine if infant and/or child mortality have either decreased or increased significantly over the last 10-15 years in each of the nine nations, after adjustment for data quality problems and accounting for sampling variability. Period mortality is estimated by the synthetic cohort approach used by Macro

International (Rutstein, 1984) so our unadjusted estimates match published estimates. All calculations of statistical modeling and of mortality estimation utilize individual sampling weights. However, for the data quality checks, tabulations of unweighted data were used. For statistical significance we utilize the standard p-value cut-off of 0.05.

TABLE 2 ABOUT HERE

Detection and adjustment for omission of events. The top three panels of Table 3 list the ratios that are calculated with data from each survey to check for possible omitted events. Also given are the criterion for each ratio and how the data are adjusted if a significant departure from the criterion value is found. The adjustments consist of imputing events that are presumed missing. Each of these is considered in turn.

TABLE 3 ABOUT HERE

The ratio of early neonatal to neonatal deaths is examined to detect omission of births that died early. It is expected to have a value of approximately 0.7. (See Sullivan, Bicego and Rutstein, 1990, for a justification.) For five-year time periods with ratios significantly below 0.7, the data are adjusted by imputing early neonatal deaths. To determine the number to impute, we solve the equation $[(E+k)/(D+k)] = 0.7$ for k, where E and D are the numbers of observed early neonatal and total neonatal deaths respectively. (Note: We let r=E/D in Table 3.) For imputation, deaths in the given period were randomly selected for duplication.

We calculate the proportion of births that are male for all births for each time period before the survey of three to five years. A proportion is considered an outlier if the presumed true value of 0.51 is not in the 95% confidence interval. (A z-test was used for confirmation.) In that case, births of a given sex are imputed until the proportion male reaches 0.51. Survival status is derived from the randomly selected birth (from the indicated time period) that is duplicated; therefore there will presumably not be much, if any, effect on mortality estimates. This procedure is obviously conservative since it is very likely that births which are missing are disproportionately those which died soon after birth.

The sex ratio of neonatal deaths is also examined. It is known that most deaths in this age group are due to genetic, biological or delivery-associated causes and for these, male deaths predominate. As a reference level we take 120 male deaths per 100 female deaths or a proportion male of 0.55[7] and if ratios are significantly different from this value, deaths are imputed.

Incomplete data. In the 20 surveys, the median percentage of births with incomplete information was 0.2%--the maximum was 9.6% in the Mali 1987 survey but the next higher value was 1.9%. The value was less than or equal to 1% in all the other surveys. (Month alone was missing more often, but if the year is correct then imputation of month makes virtually no difference in mortality estimates for multi-year periods.) Similarly, age at death is missing for less than 1% of deaths in all twenty surveys.[8] Therefore, we do not consider this further.

Detection and Adjustment for Displacement of Events in Time.

For each survey, we test the hypothesis that births were pushed back in time beyond the three or five-year cut-point for the pregnancy and health modules.[9] The birth ratios for the years around the cutoff date for all 20 surveys are shown in the last two columns of Table 2. All the values for the year before the reference date are above 105 (median = 117) and all except one for the subsequent year are 93 or below (median = 85.5). Clearly there is a problem of birth displacement in these surveys. Yearly fluctuations in numbers of births are common, so to test the hypothesis we utilize a statistical model rather than simply adjusting the data until the yearly birth ratios before and after the cutpoint reach 100. We compare monthly data for three years before and after the cutpoint date as defined by the screen questions for entry or not of a birth into the pregnancy and child health modules. The cutoff was almost always the January between three and six years before the interview (Table 2).

We statistically fit the monthly data of counts of births for surviving children and dead children separately. Events in the 15-year period are considered, starting the month before the survey fieldwork began. We fit the 15-year monthly series of data with a time series model (Bloomfield, 1976), including seasonal and trend components as well as a set of indicator variables with coefficients that estimate a step function for each of the three years on both sides of the cutoff viz:

$$y_t = a + b_0 t + b_1 t^2 + \sum_{j=i}^{5} [g_j \cos\left(\frac{2pjt}{12}\right) + t_j \sin\left(\frac{2pjt}{12}\right)] + \mathrm{S}_{k=1}^{6} d_k \; I(t,T_k) \qquad [1]$$

where t refers to time in months before the survey, y_t is the number of surviving (deceased) births occurring in month t, and we define T_k for use with the indicator function as follows:

$T_1 = (t_c - 36, t_c - 25)$

$T_2 = (t_c - 24, t_c - 13)$

$T_3 = (t_c - 12, t_c - 1)$

$T_4 = (t_c, t_c + 11)$

$T_5 = (t_c + 12, t_c + 23)$

$T_6 = (t_c + 24, t_c + 35)$

with t_c defined as the date of cutoff, so for example, $I(t,T_1) = 1$ if t is in the interval $(t_c - 36, t_c - 25)$ and 0 otherwise. Then a, ß$_0$, ß$_1$, ?$_j$, t_j and d_k are parameters to be estimated. From this statistical fitting, estimates are obtained of the number of events to redistribute from before the cutoff point as follows.

First, we fit the equation and then performed a series of tests on the coefficients of the six indicator variables representing the years immediately after and before the cut-off date. Starting with each of the three years after and separately the three years before the cut-off, we tested whether the coefficients of the indicators for the three years were significantly different from each other or not. If all three coefficients were not significantly different then we grouped all the births in the three years and used one indicator variable. If any of the three coefficients was significantly different from the other two, then we tested again for a difference between the coefficients of the indicator variables for the two years adjacent to the cut-off date. If these coefficients were not significantly different, then we grouped the births in these two years (i.e. one single indicator variable was made) and the model of equation 1 was rerun. If the two coefficients were significantly different then we chose only the year adjacent to the cut-off date to represent with an indicator variable in equation 1. Finally, we tested the difference in the two coefficients for indicators that were so derived, for before and after the cut-off date. If the difference was significant, this was taken to indicate event displacement.

To adjust the data, the average of the two coefficients for before and after the cutoff is calculated and differences between the estimates and the average were used to estimate the number of events in deficit. The deficit so defined on one side of the cutoff is exactly matched by an excess on the other side, though the number of years on the two sides might vary due to the results from the testing just described. Births were moved from before into the period after the cut-off date by revising the reported date of birth. Births to be shifted were randomly chosen from those in the respective years before the cut-off. Date of birth was modified depending on how many years it was moved. For example, if births were moved from the year immediately before the cutoff to each of the three years after the cutoff, we randomly divided the births to be moved into three equal groups and changed their dates of birth by subtracting 12, 24, and 36 months, respectively.

Detection and Adjustment for Misreporting of Age at Death at 12 Months. The heaping ratios of deaths ($3*D_{12}/(D_{11}+D_{12}+D_{13})$) were calculated for each survey for deaths in three-year periods before the survey.[10] Values above 1.5 are taken to indicate a level of heaping that needs adjustment. From algebra, a level of the heaping index of 1.5 indicates that there are 50% more deaths reported at 12 months than expected.

Data that document where the heaped events might actually belong are available from a validation study in the Matlab vital registration area of Bangladesh. Of the deaths that women reported as 12 months of age (n=19) in the demographic and health survey there, 42% were actually 11 months or younger according to the vital registration data. Since the numbers were small, the 95% confidence interval of this estimate is (0.20, 0.64). Nevertheless, these data lend credence to the *a priori* assumption that 50% of deaths heaped at 12 months belong to the period of infancy and 50% to the group 1-4 years of age. However, rather than merely use a 50-50 allocation of the excess deaths at 12 months, we decided to compare this and three other methods of allocation:

1. Move 0% before 12 months and keep 100% in the 1-4 age group This corresponds to the current DHS procedure of no adjustment.

2. Move 100% before 12 months and keep 0% in the 1-4 age group.

3. Use a statistical model to determine the appropriate

proportion to allocate to each period.

The negative exponential model is a natural choice; it models the rapid decline of mortality by time after birth and is simple to fit. It has density function f(x) = ?*exp(-?x). Thus 1-F(x) = exp[-?x] so ln(1-F(x)) = -?x. Therefore the parameter ? can be estimated by fitting a straight line to values of the logarithm of the observed survivorship function. Data for deaths by month in months 0 to 23 were used in a least squares fit. With the estimated ?, fitted values for months 10,11,12,13 and 14 were derived and from differences and after normalization, the observed and expected monthly distributions within that time interval were calculated. Then observed deaths at 12 months of age were randomly chosen and redistributed to months 10,11,13 and 14 until the distribution in those months matched the expected distribution.

The various methods for allocation of deaths heaped at 12 months were evaluated by an empirical test. For the nine countries and twenty surveys we compared estimates for the twenty five-year time periods in which there were estimates of mortality available from two surveys.[11] To evaluate each method we compute the absolute difference of the two estimates for the same period and sum these. Allocation methods which give lower sums are judged superior.

Sampling variability

Standard errors for each mortality estimate for each five-year period were estimated using the jackknife procedure (Le and Verma, 1997). It is implemented by the ISSA program (adapted for this research) of DHS (Macro International, 1998). These allow testing of differences between estimates at two time periods. Also 95% confidence intervals are displayed in the graphs of mortality.

RESULTS

Omission of Deaths.

Values of the ratio of early neonatal to all neonatal deaths (for five-year periods) were significantly below the reference level of 0.70 in Burkina Faso, Niger, Nigeria and Senegal (not shown). Therefore neonatal deaths were imputed as described above; the time periods and numbers of events so imputed are given in the first column of results of Table 4. The number of imputed events in a survey ranges from 88 in the Nigeria 1990 survey to 340 in the Niger 1992 survey.

TABLE 4 ABOUT HERE

Regarding the sex ratio at birth, values of the proportion of births that were male were significantly below the expected value of 0.51 in all countries (for at least one survey) except Burkina Faso and Togo (not shown). The number of male births that were therefore imputed for given time periods are shown in the second column of results of Table 4; these ranged from 89 in the Ghana 1993 survey to 234 in the Mali 1996 survey.

The proportions of neonatal deaths that are male by five-year time period were significantly above the expected value of 0.55 in Burkina Faso, Cameroun, Togo, and Ghana, but we did not impute any data from this test because the low values were partially corrected by imputation for the low sex ratio at birth (not shown).

Displacement of Events in Time.

The time series model fits for monthly counts of births (surviving and deceased separately) had r-squared values above

0.50 in all of the 20 surveys for surviving children and in 12 of the 20 for dead children. In contradistinction to the checks described above, from these analyses events are not imputed but merely moved in time. Significance of the coefficients of the indicator variables demonstrated that there was significant pushing of events backward in time in all nine countries and nearly all surveys. (The only exception for both living and dead children is the 1988 Ghana survey.) The estimated numbers of events to move for living children varied from a low of 70 in Togo (1988) to a high of 434 in Mali (1987) and for dead children the corresponding numbers were 19 in Ghana (1998) to 183 in Mali (1987). (See Table 4.)

Misreporting of age at death

Figure 1 shows the heaping ratios at 12 months of age ($3*D_{12}/(D_{11}+D_{12}+D_{13})$) for three-year intervals of the death event for the surveys in each country. (Country results are presented in the figures in order of geographical location from west to east.) The patterns are quite varied between countries. For example, in Burkina Faso the ratio is nearly constant at 2.0 in both surveys and across time while in Ivory Coast it is close to 1.0 (no heaping) in the first survey for deaths before 1985, but there was a peak near 2.0 around 1987. A parallel peak at that time was seen in the 1998 survey data for that country. Heaping was uniformly greater in one survey than the other in nearly all countries, a clear indication of differences in data quality between surveys in the same country. In Senegal, Ghana, Mali and Togo, the first surveys had ratios near 3.0; this was also the case for the second survey in Niger. A value of 3.0 is the maximum possible since it means that there were no reported deaths at 11 or 13 months, indicating very poor quality of data.

FIGURE 1 ABOUT HERE

Table 5 shows the results of the different options for adjusting for the heaping of deaths at 12 months. What are shown are differences between ${}_1q_0$ (and ${}_4q_1$ as well) across all 20 surveys for time periods where the two or three surveys overlap, according to which method is used to distribute excess deaths reported at 12 months. As can be seen, for both age groups, the statistical modeling technique gives the smallest difference in estimated mortality levels across all the surveys.[12]

TABLE 5 ABOUT HERE

Next best are the method of assigning 50% of excess deaths to the infant period and 50% to the child period as well as the method which assigns 100% to the infant period. The next to largest discrepancies are present when 100% of excess deaths at 12 months are allotted to the child age group. Note that this is the current DHS procedure, i.e. it accepts deaths reported at 12 months at ‘face value’ as child deaths. The unadjusted data give the largest differences, i.e. the worst option. This is one indication of the value of the other adjustments that were done.

From these analyses, we tentatively conclude that the best estimates of mortality are had when heaped deaths at 12 months are allotted to the infant or child ages according to the given statistical model. Therefore we used the model to re-allocate age at death for excess cases at 12 months to before or after that age. The last column of Table 4 shows the numbers of deaths with ages at death that were changed for each survey. These are relatively large numbers of events, though they are distributed over 20 years.[13]

Adjusted mortality

Figures 2,3 and 4 show infant, child and under-five mortality estimates respectively after all the adjustments described above were made. The 95% confidence intervals are also shown. Significant declines are seen in Mali and Niger for ${}_1q_0$ and ${}_5q_0$. In the seven other countries, though declines in mortality over the earliest decade are obvious, there is neither a significant decline nor a significant increase for the recent 15-year period, i.e. mortality levels have stabilized and there is some indication, albeit non-significant, that there may be increases in infant mortality for three countries and in child mortality for six countries. Of course a flat level can also be interpreted as a stagnation of mortality decline.

FIGURES 2,3 AND 4 ABOUT HERE

But the data should not be interpreted too extensively because there remains a major problem: In Mali, Ivory Coast, Burkina Faso, Togo, Niger and Cameroun, as with the unadjusted estimates (not shown), a pronounced discrepancy in infant mortality estimates between the two surveys is seen for the most recent time interval covered in the earlier survey. In all six of these countries, the estimates for the first survey are 10 to 20 points (per 1000) lower than those in the second survey for the same time period.[14]

This leads to a new hypothesis: that in some surveys there is a tendency to omit infant deaths that occurred in the period just before the survey, at least in the earlier surveys. Could this be due to a reticence on the part of the mother to talk about recent infant deaths or perhaps women with such deaths are differentially not at home? The low non-response rates (Table 2) argue against the latter though if the child mortality experience of non-responders were much higher than that of responders, there could still be an effect on the overall result.

If either one of the explanations is correct, then we would hypothesize that the effect would be even greater for the most recent period when shorter time intervals than five years are used. To examine this, mortality was estimated for two-year time periods (for the first survey) using the adjusted data for the six identified countries with large differences.

The results are shown in Figure 5. In five of the six countries, the latest two-year estimates are the lowest (consistent with the hypothesis) and are therefore even more discrepant (than the five-year estimate) *vis á vis* the estimate for the same time period from the later survey.

FIGURE 5 ABOUT HERE

As a further way to confirm or refute the hypothesis of this pattern, we utilize recently published data from a subsequent DHS in Mali fielded in 2001 (Mali, Republique, 2002). In that survey the estimate of infant mortality for the period 5-9 years before the survey (corresponding roughly to the period 0-4 years before the 1996 survey) was 139.0, a full 16 points per 1000 above the estimate (122.5) for about the same period, i.e. 0-4 years before the 1995-96 survey. Thus, the same pattern persists.[15]

To portray the effects of all the adjustments, Figure 6 shows the unadjusted and adjusted infant mortality levels.[16] The adjustments reduced the discontinuity in trend estimates for infant mortality in Mali, Senegal and Togo. They also reduced the discontinuity for child mortality in Togo and for under-five mortality in Mali and Cameroun (not shown). In Mali there was a complete reversal; what appeared to be an increase in infant mortality between the surveys with unadjusted data, after adjustment, shows a decline in mortality between surveys.

However, the correspondence of the trend lines of both unadjusted and adjusted estimates was poor in five of the nine countries for infant mortality. On the other hand, for child mortality and under-five mortality only Burkina Faso and Nigeria have major discrepancies in trends in both the unadjusted and adjusted data (not shown).

FIGURE 6 ABOUT HERE

CONCLUSIONS/DISCUSSION

In countries with low levels of female education such as these in Sub Saharan Africa, a significant level of response errors for demographic events in the past in survey questionnaires can be expected. Thus it is not surprising that problems of data quality were found for most of the 20 DHS surveys in West Africa and Cameroun employing checks based on knowledge of demographic patterns in human populations and statistical modeling. From these checks we were also able to adjust the data via imputation. Sampling errors were also calculated for the mortality estimates. After applying these procedures to the surveys in the nine countries, we only detected a significant downward trend in mortality within the 15-year period before the last survey in Mali and Niger (for both infant and under-five mortality). In three of the seven other countries for infant mortality and four of seven for child mortality, the adjusted estimates for the most recent period before the latest survey are higher than those for the corresponding period before the previous survey, but not significantly so.

Given the recent increase in HIV prevalence (albeit at relatively low levels) in the region, and known levels of maternal-to-child transmission in the absence of therapy, AIDS mortality of infants could be expected to counteract the momentum of mortality decline that had been occurring in previous decades.

Several recent studies in areas with population-based data have estimated the impact of HIV on infant and child mortality. We shall focus on infant mortality here. The relative risks of death for children of mother’s who were HIV-positive were reported as 3.1 and 2.1 in two studies in Uganda (Nakiyingi, et al. 2003; Sewankambo, et al. 2000), 2.0 in Tanzania (Ng'weshemi et al. 2003), and 2.9 in Malawi (Crampin et al. 2003), giving an average relative risk of 2.5. Observed mortality is a weighted average of mortality for HIV-positive and HIV-negative mothers, i.e. $M = p*M_+ + (1-p)*M_-$ where p is the proportion of women who are HIV-positive and M_+ and M_- indicate mortality for infants of HIV-negative and HIV-positive mothers respectively. To estimate the impact in these nine countries we use values of p from sentinel surveillance sites in each country (UNAIDS, 2003) and then solve the equation for M_-. Then the difference $M - M_-$ gives the increase in mortality expected due to HIV. Comparing these results to the observed increases (between surveys in this case, one when HIV-prevalence was relatively low and the second recent survey) in the third column of Table 6, we see that in the three countries with highest prevalence, infant mortality increased and the increases are quite consistent with the predicted value in Burkina Faso and Ivory Coast. (The observed increase in Cameroun is double that expected due to HIV.) For countries with lower HIV prevalence, no pattern is seen. Thus where HIV prevalence is above 5%, a measurable impact on infant mortality is expected and observed despite relatively small sample sizes in these national surveys.

TABLE 6 ABOUT HERE

Nevertheless, even the adjusted survey data are of limited value for studying short-term trends in mortality because of the two factors evidenced in Figures 2 to 4: a) quite wide confidence intervals relative to the size of differences that one would want to detect; and b) as yet unidentified sources of response errors which lead to quite different estimates of mortality for the same time period in the two surveys. Further research is needed to try to identify these sources. In particular it would be good to have reliability studies conducted in some of these countries in conjunction with future survey rounds. Due to these inconsistencies in the data, we question the usefulness of performing survival analyses with individual data to determine causes for changes that are within sampling error and could be due to non-sampling error.

While we believe our adjusted estimates are closer to the actual levels of mortality in these countries, given the disjuncture between the time trend of mortality for some pairs of surveys even in the adjusted series, it is clear that some of the estimates still have systematic error. This is expected because the data quality tests can only detect and adjust for certain patterns of error. For example omissions which are not differential by sex, age group or time period would not be detected.

Among the adjustments we did, one is more open to critique than the others. We assumed that the ratio of early neonatal to all neonatal mortality is fixed at 0.7, but it is known that as mortality declines the ratio should increase. Thus, using a fixed value is too crude. However, choosing a value for the data quality checks which depends on the level of mortality would lead to a circularity problem because it is mortality that is being adjusted! But it is true that one of the countries with the highest mortality--Niger--is also one with a ratio consistently below 0.7. As a measure of sensitivity, if the ratio were set at 0.6 for Niger, then all of the confidence intervals for the rates from the observed data would have included that value and no deaths would be imputed. In that case the adjusted estimates of infant mortality in Niger would be 7-9% lower than those shown in Figure 2 but the pattern in time would remain the same (not shown). Similar corrections downward would apply for the other three countries where we adjusted using the ratio of 0.7 (Burkina Faso, Senegal and Nigeria--see Table 4).

Some scientists may disapprove of imputing events and changing reported dates of events as we have done in this research. However, imputation is standard procedure in censuses and surveys. Indeed, using other criteria, data are already imputed in the DHS as described earlier. For example, besides imputation for unknown dates (e.g. month of birth not given), if the difference in birth dates of two children is less than a certain minimum based on biology, then one of the dates is changed. Conceptually this is only slightly different than changing the date of death for some of the deaths heaped at 12 months though admittedly there is a difference in kind between these when it comes to imputation--for short birth intervals, data for a specific case is altered while for age at death the error is only detected in the aggregate and individual cases to change are selected at random. It is an admirable goal to preserve the original data as much as possible but when it is clearly known to be in error (e.g. distorted sex ratio at birth or major heaping of age at death at 12 months) then perhaps a middle course would be to make both the original and imputed data available and researchers could choose which to use. (For example, the U.S. Census Bureau in the decennial census indicates with a flag what data have been imputed.) We do not necessarily recommend that such files include all of the imputation procedures we have used. However, they all deserve consideration before analyses of mortality trends are undertaken. We do recommend that consideration be given to altering one current procedure--allocation of all deaths reported at 12 months to the 1-4 year period. From all indications, this clearly leads to underestimation of infant mortality.[17]

Table 1: Estimates of infant and child mortality (per 1000) for eight countries of West Africa and Cameroun with two (or three) DHS surveys and percentage change[a]

Country	Year of :		Age group and survey: Infants			1-4 years			0-5 years		
	First survey	Second survey	First survey	Second survey	% change	First survey	Second survey	% change[a]	First survey	Second survey	% change[a]
Burkina Faso	1992/3	1998/9	94	105	+12	103	127	+23	187	219	+17
Cameroun	1991	1998	65	77	+18	66	80	+21	125	151	+20
Cote d'Ivoire	1994	1998/9	89	112	+26	67	78	+16	150	181	+21
Ghana[b]	1988	1998/9	77(66)	57	-14	84 (57)	54	-5	155(119)	108	-23(-10)
Mali	1987	1995/6	108	123	+14	159	131	-18	247	238	-4
Niger	1992	1998	123	123	0	223	172	-22	318	274	-14
Nigeria	1990	1999	87	75	-14	115	70	-39	193	140	-27
Senegal[b]	1986	1997	86(68)	68	0	114(68)	77	+13	195(132)	139	-32(+6)
Togo	1988	1998	81	80	-1	85	72	-15	155	146	-5

[a] Percent change between two (most recent) surveys, i.e. 100*(M2-M1)/M1
[b] Ghana and Senegal have had 3 DHS surveys; estimates from the middle survey are shown in parentheses.

Table 2: Selected details of 20 DHS surveys in eight West African nations and Cameroun

Country	Survey field work		Number of births in 15 years. before the survey	Pregnancy & health module		Birth ratios[a] for:	
	Date of beginning	Percent complete		Reference date for births	Vaccine data for dead children?	Year before cutoff	Year after cutoff
Burkina Faso	1992/12	93	16,151	1/1987	yes	138	72
	1998/12	96	16,117	1/1993	no	120	82
Cameroun	1991/04	94	8,922	1/1986	yes	113	85
	1998/02	96	11,334	1/1995	no	116	86
Ghana	1988/02	98	10,093	1/1983	no	106	101
	1993/09	97	9,431	1/1990	yes	150	72
	1998/11	97	8,932	1/1993	no	116	89
Ivory Coast	1994/06	98	18,618	1/1991	yes	119	80
	1998/11	96	5,485	1/1993	no	126	86
Mali	1987/03	100	9,268	3/1982	no	117	79
	1995/11	96	28,512	1/1992	no	111	86
Niger	1992/04	96	18,672	1/1987	yes	117	84
	1998/03	96	21,362	1/1995	no	117	89
Nigeria	1990/04	95	21,847	1/1985	yes	140	77
	1999/03	92	16,524	1/1996	yes	115	87
Senegal	1986/04	97	10,881	4/1981	no	109	89
	1992/11	95	14,485	1/1987	yes	108	82

	1997/01	94	18,687	none	—[b]	107	93
Togo	1988/06	99	8,150	6/1983	__[b]	112	81
	1998/02	96	18.088	1/1995	no	112	90

[a] $100*B_x /(B_{x-1} + B_{x+1})$

[b] Senegal and Togo did not collect the immunization information in the respective surveys.

Source: Various DHS Survey First Reports.

Table 3: Data quality problems in birth history data by means of detention and appropriate adjustment

Data problem	Tabulation/calculation to detect	Criterion to detect problem	Adjustment made	Determination of number of events for adjustment	Details of adjustment
Omission of early neonatal deaths	Ratio of early neonatal to neonatal deaths	Expected ratio=0.7	Births and neonatal deaths imputed	K=D*(0.7 - r) / (1-0.7), where r is the observed ratio and D is the observed number of neonatal deaths	Selected randomly from all neonatal deaths and duplicate record added to the original data file
Omission of male births	Proportion of male births out of total births	Expected proportion male=0.51	Births and deaths imputed	K=M*(0.51 - r) / 0.49r, where r is the observed proportion of male births and M is the number of male births	Selected randomly from all male births in the time period showing problem and duplicate record added to the original data file
Differential omission of neonatal deaths by sex	Ratio of male to total neonatal deaths	Expected ratio of male neonatal deaths to total neonatal deaths=0.55	Male births and deaths imputed	K=D*(0.55 - r) / 0.45, where r is the observed proportion of male neonatal deaths and D is the total number of neonatal deaths	Selected randomly from all male neonatal deaths in the time period showing problem and both birth and death added to the original data file
Heaping of deaths at age 12	Age ratio of deaths and comparison of	Large differences between expected and observed	Age of death changed	See text	Randomly flagged from among cases with reported age

months	distribution with negative exponential model	distributions			at death of 12 months, then randomly reassigned age at death of 10, 11, 12 or 13*
Displacement of births	Comparison of monthly events to trigonometric model fit	Significantly different coefficients for indicator variables on two sides of cutoff	Shift of date of birth	See text	See text

* In some cases deaths reported at ages other than 12 months were also reassigned according to the statistical model (see text).

Table 4: Number of events imputed after finding significant omission or moved due to significant misplacement, by survey and type of problem detected

Country	Survey Year	Events imputed due to omission of:		Number of events moved across cutpoint according to survival status		No. deaths heaped at 12 months moved
		Early neonatal deaths	Male births	Surviving children	Deceased children	
Burkina Faso	1993	279	--	174	73	308
	1999	212	--	139	54	334
Cameroun	1991	--	--	--	28	48
	1998	--	101	–	80	108
Ghana	1988	--	--	--	--	221
	1993	--	89	176	39	108
	1999	--	125	--	19	86
Ivory Coast	1994	--	161	186	160	36
	1998	--	128	91	--	26
Mali	1987	--	--	143	183	407
	1996	--	234	434	139	341
Niger	1992	340	--	181	91	312
	1998	334	192	265	173	120
Nigeria	1990	113	--	90	163	346
	1999	88	206	113	--	273
Senegal	1986	196	--	167	--	356

	1993	183	189	150	66	185
	1997	183	--	183	--	349
Togo	1988	--	--	70	51	167
	1998	--	--	--	65	160

Figure 1: DHS estimates of probability of death between birth and age 1 year (1q0), for 5-year time periods before each survey for West African countries, by year

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1. In this report we use the word 'mortality' which has ambiguous meaning in that it can refer to both a central death rate (M_x) and a probability of death (q_x). Unless otherwise specified, we use it in the second sense, i.e. infant mortality refers to ${}_1q_0$. Also there are three measures of interest, ${}_1q_0$, ${}_4q_1$ and ${}_5q_0$. The last is a composite of the first two [${}_5q_0 = 1 - (1-{}_1q_0)(1-{}_4q_1)$].

2. Data at the population level are only available in many places for pregnant women because they come to health facilities for antenatal care and blood specimens can be tested for HIV.

3. The DHS is concerned about data quality and regularly considers this in the section of the First Country reports (of its surveys) where mortality estimates are presented. Also, appendices to the reports routinely provide data quality tables, many of which are now initiated during the field work to potentially catch problems early in specific teams and even individual interviewers.

4. A reliability study was done after the Nigeria 1990 survey. However, the data were never individually matched so appropriate analyses are not available.

5. An exception is found in places where ultrasound is used during pregnancy to determine the sex

so that female fetuses can be selectively aborted, but this is not the case in West Africa.

6. Birth history data were from the standard recode files provided by Macro International.

7. Michelle Garenne has provided us with data from the Niakhar demographic surveillance project in Senegal, which gives a ratio of male neonatal deaths to female neonatal deaths of 1.32 or a proportion male of 0.57. (The value of 0.55 is derived from vital registration data of Matlab, Bangladesh for the years 1979-1997). We chose the value of 0.55 as it is more conservative in the sense of requiring less imputation and also it is based on a large population.

8. Results were abstracted from data quality tables, DHS First Survey Reports or, where not available, separate tabulations were done.

9. In the nine countries, two kept the five-year interval in the second survey, six switched to three years and Ivory Coast had three years in the first and five in the second survey.

10. This ratio is preferred to the index used by Curtis (described above) because the latter is sometimes undefined (i.e. if there is a denominator of 0).

11. In some cases the estimate for one survey was derived by interpolation between two five-year estimates so it would coincide with the reference period for the second survey.

12. As a check, the same modeling procedure with data from 0-23 months was used with normalization for months 7-17 instead of months 10-14. Similar results were obtained.

13. In 3 of the 20 surveys, deaths at months other than 12 had to be distributed in order for the

observed distribution to match the negative exponential.

14. Estimates of mortality from the 1999 Nigeria survey are consistently below those for the same period from the 1990 Nigeria survey. The Nigeria DHS of 1999 had very serious problems of data quality. In the First Report, the summary of data quality checks included the following: "the neonatal mortality rate for the Northeast Region is unrealistically low and inconsistent with the postneonatal mortality rate. Both the mortality and fertility data for the Central Region appear particularly flawed.... the weight of evidence indicates that the mortality rates based on the data are most probably underestimates. Moreover, the nature and scope of the data defects leading to this conclusion suggest that the possibility of repairing these data so that they would form the basis for reliable mortality estimates for Nigeria is not good."

15. Results of a subsequent survey in Nigeria (2003) have also just been published with estimates of infant, child and under-five mortality for the five-year period before the survey of 100, 115 and 203 per 1000 respectively. (These are much higher than the levels given in the 1999 survey (Table 1). However, it was known (footnote 14) that the data quality was unusually poor in the 1999 survey.

16. Results for the other age groups are available from the authors upon request.

17. Given that the procedure without adjustment has been used in nearly 100 surveys over 20 years of DHS experience, it is understandable that adopting a new methodology at this point would be very difficult.