



SAGA Working Paper
September 2006

**An Assessment of Changes in Infant and under-Five Mortality in
Demographic and Health Survey Data for Madagascar**

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Strategies and Analysis for Growth and Access (SAGA) is a project of Cornell and Clark Atlanta Universities, funded by cooperative agreement #HFM-A-00-01-00132-00 with the United States Agency for International Development.



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This research was supported by USAID/Madagascar through the Strategies and Analysis for Growth and Access (SAGA) program (Cooperative Agreement HFM-A-00-01-00132-00). Additional support was provided by UNICEF/Madagascar. We thank Wendy Benazerga, Lynne Gaffikin, and Noe Henri Rakotondrajaona (USAID/Madagascar), Diane Coury (UNICEF/Madagascar), and Mead Over and Jean Pierre Manshande (World Bank) for helpful discussions and the provision of documents. Additionally, we thank Keith Purvis and Soumaila Mariko of ORC Macro for providing the DHS data and documentation.

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1. Introduction

Repeated rounds of nationally representative surveys provide a vital source of information on changes in the welfare of a country's population. In particular, policymakers and donors in many developing countries rely heavily on the Demographic and Health Surveys (DHS) to provide information on levels and trends in indicators of the health status of the population, including child survival. In Madagascar, the most recent (2003/4) DHS (also known as EDSMD after its name in French) indicated a sharp drop in recent rates of infant and under-five mortality compared with the previous survey from 1997. However, this reduction is hard to reconcile with what is known about changes in incomes over the period, and some doubts have been raised about the quality of the most recent DHS.

The sources of concern about quality are two-fold. First, there is an apparent tendency of interviewers to mis-record the year of children's births to avoid having to ask additional questions about the children, and there is also evidence of misreporting or misrecording the age at death for young children. Second, a discrepancy is evident in the retrospective data on mortality from the 2003 and 1997 surveys. For the same calendar periods of time, retrospective information from two surveys should yield similar results for infant or under-five mortality, but in fact estimated mortality rates from the 2003 DHS are substantially below those in the 1997 data.

The purpose of this analysis is to (1) reassess the quality of the recent DHS data; (2) try to explain why discrepancies with earlier surveys arise, and (3) explore means of correcting for biases in the estimated rates of infant and under-five mortality. To do this we make use of the 1997 and 2003 DHS data as well as the 1992 DHS, and where possible resort to information from other sources. The plan of the analysis is as follows. In the next section we consider the presence (in each DHS) of birth year displacement problems and of age of death misreporting, and investigate the implications for estimates of infant and under-five mortality. This analysis also highlights the discrepancies between the 2003 DHS and earlier surveys in mortality rates calculated for similar periods of calendar time. In Section 3, we undertake a detailed comparison of sample characteristics in each survey, to see if problems in sampling might explain the discrepancies. In Section 4, we examine whether the problems instead may be attributable to the performance of specific interviewers. In Section 5 we consider two distinct approaches to correcting the mortality estimates for biases. For this we estimate hazard regression models of child survival. Section 6 considers whether the measured reductions in mortality may be explained, in whole or in part, by recent program activity that was centered in two of the six provinces of the country. A final section summarizes the findings of the analysis.

2. Birth displacement and misreporting of age at death: consequences for mortality rate estimates

The DHS typically requires interviewers to collect detailed information on maternal and child health for all births occurring in the previous five years. As the 2003/4 DHS report notes, this creates an incentive on the part of interviewers to misrecord or displace the time of birth from

under to over five years previous to the survey, or in the case of the 2003/4 DHS, from January or later to before January 1998. This in and of itself need not have a large effect on under-5 mortality rates, but as noted in the report, there is often a tendency to displace in particular the births of children who have died. This would tend to bias downward estimated under-5 mortality since deceased children are disproportionately removed from the sample of children under-5.

Table 1 shows, for each DHS round, the distribution of reported births by years prior to the survey date as well as the share of children in each birth year reported to have died. As the next to last column indicates, there was indeed a substantial displacement of births over the 5-year threshold for the last DHS. 912 births were recorded for 1998, 5 years before the survey, compared with 1321 in 1997, six years before. The problem does not seem to occur in the 1997 DHS, but it does in the 1992 DHS, if less severely than in 2003. On the other hand, the evidence for disproportionate transference of births of *deceased* children in the 2003 data is not clear. The share deceased for births 5 years and 6 years before the survey are similar—0.10 and 0.11--and both in fact are higher than for 4 years before the survey. Therefore the displacements of births from 1998 to 1997, while pronounced, did not occur disproportionately for deceased children. Note in contrast that this did seem to happen for the 1992 survey.

On the other hand, the share of deceased births does seem to increase sharply for births recorded at *seven and eight* years before the 2003 survey. Yet this probably is not due to a transference of deceased children, because this would also raise the total number of births in these earlier years and we do not see this in the data: the numbers born in 1995 and 1996 are quite in line with the other years from the 2003 survey excepting the special cases of 1998 and 1997.¹ It should be kept in mind that the share of children dying by the survey date should be higher for the older cohorts in any case, since their period of exposure to the risk of dying is longer. Indeed, this pattern is seen for all of the surveys. Further, the share deceased would also be higher for children born earlier if mortality is falling over time. Either of these factors could explain the larger numbers of deaths among the older cohorts by the time of the survey.² Note, finally, that it is somewhat difficult to make statements about the extent of transference of deceased births because of the large degree of sampling error given the relatively small number of death events per year of birth.

The second data problem noted by ORC Macro is an apparent tendency to misreport (or miscode) ages at death, leading to heaping of deaths at 12 months. For children dying before two years of age, interviewers are supposed to record the age of death in months. The clustering

¹ If *only* deceased births were transferred to these years, the effect on total number of births could be relatively small and thus hard to detect. However, as the data for 1997 and 1998 suggest, displacement only of deceased births is not likely; after all, there is an incentive to transfer living children as well to avoid the battery of additional questions for all children born after the cutoff date. Note also that if such a transference were occurring, it would mean that interviewers were committing quite egregious misbehavior by moving births occurring after 1997 to fully two or more years earlier.

² If one remains concerned that deceased births are potentially being transferred that far back in time, one could extend the period for calculating mortality rates that much further back from the survey date, to capture all such potentially displaced births and deaths. The problem with this is that an average calculated (say) over the last 10 years would also obscure any more recent trends in mortality. In our simulations based on statistical modeling in Section 5 we explore the sensitivity of estimates of recent under-1 and under-5 mortality to the length of interval chosen for the calculations.

at 12 months is likely due to a failure of interviewers to probe for the exact age when the respondent only gives the death as occurring at age one year. Since children recorded as dying at 12 months when they actually died earlier than that are not counted in the infant (under 12-month) mortality rates, these rates will be underestimated. Table 2 confirms this problem, though it also shows that it occurs in each of the DHSs to a greater or lesser degree. Heaping of deaths at 12 months seems particularly severe in the last survey but also is strongly evident in 1992. In each of the surveys, deaths tend to cluster at multiples of 6 months, though only the clustering at 12 months would lead to problems for calculating infant mortality.

Since both of these data problems involves improper transfer of events from below to above a threshold (births of deceased children from under to over 5 years previous to the survey, deaths from under 12 months to 12 months), an obvious solution is to extend the period of consideration to encompass the threshold point, thereby bringing the displaced events back into the mortality calculations. Alternatively, one could estimate mortality on a younger group that is well below the threshold age, such that age at birth or age at death would be relatively unlikely to be inflated to over the threshold point.

Considering infant mortality first, we calculated under-12, 15, and 9 month mortality rates. Most of the children whose deaths were improperly heaped at 12 months probably died before 15 months; if they were older than this and their deaths were misreported or misrecorded this would likely have placed them at 18 rather than 12 months (in any case there are relatively few deaths reported at over 12 and under 18 months). Similarly, the tendency to cluster deaths at 12 months would not lead to misrecording of deaths that occurred before 9 months; if anything, these would be put into the 6 month category.

Mortality was calculated using the ‘synthetic cohort’ method that is standard in analysis of the DHS. This procedure yields an estimate of the probability that a child in a given age range, say age 0 to 11 months, will die in some interval of calendar time, say 1985-1989.³ To compare trends in the data from each survey, we calculate mortality probabilities for the five 5-year intervals preceding each survey. Figures 1,2 and 3 show the estimates of infant, under 9-month, and under 15-month mortality rates, respectively. The points on the graph represent the midpoints of each five-year period.

The more serious the problem of transference of age at death, the more we should see the under-twelve mortality rates diverging from the under-nine and under-fifteen month patterns. As seen in Figures 1 and 2, however, the levels and trends over time are very similar for under-9 and under-12 mortality. Mortality per 1,000 is somewhat higher for the under-15 month case in Figure 3 but the trends are the same. Note that an exact equivalence of these measures would not be expected, even without any problems in the data: under-15 month mortality naturally should be higher than under-12, which should be higher than under-9, because the length of exposure to the risk of dying increases with age.⁴ Taking this into consideration, the figures suggest that

³ For details on the method, see Rutstein (1984). We thank Keith Purvis of ORC Macro for providing us with SAS code for the calculations.

⁴ However, if one uses a parametric model to estimate mortality probabilities as a function of age, it is possible to estimate the model for the appropriate interval (e.g., 15 months) to avoid or minimize bias from death or birth

biases in infant (under 12 months) mortality estimates due to age at death transference are not severe.

On the other hand, there is a striking and troublesome pattern in the figures: for the same calendar period of time (e.g., the five year interval centered on 1990), mortality is consistently lower in the 2003 survey than in the other two surveys. Put another way, under-12 (or 15, or 9) month mortality for a given birth cohort of children is lower in the 2003 data than in the earlier surveys with information on that cohort. Clearly, reported infant mortality rates for children born in the same year or over the same several year period should be similar for representative national surveys conducted at different points in time. Trying to explain these cross-survey discrepancies within cohorts, which is a different (and more serious) problem than displacement of births or deaths, will be the main focus of our analysis in this report.

A second observation is that all the estimates seem to have a downward bias in the period closest to the date of the survey, perhaps due to the method used to deal with censoring of children born less than 12 months (or 9 or 15 months) before the sample. But except for the fifteen-month estimates, this problem does not appear to be worse for the 2003 data than for 1992. 1997 appears to be the exception, with relatively smaller declines for the most recent period.

Next we consider the implications of birth year displacement by calculating under-5, under-6, and under-7 year mortality, again using the standard DHS approach. Note that the birth heaping issue involves only recent births, i.e., four or five years before the survey date. As Figures 4 and 5 demonstrate, mortality rates for this group are barely affected by using six rather than five years, even for the 2003 survey where the problem is more severe. The same is true for calculations including births reported up to seven years before the survey (not shown). As noted above, however, the displacement of births is not accompanied by a disproportionate transfer of deceased children, so this result is not surprising.

As with infant mortality, the bigger problem is that reported mortality is so much lower in the 2003 data than in either 1997 or 1992 for the same birth cohorts of children. To consider this point from a different angle, Figure 6 shows the cumulative share of reported deaths in each DHS survey by the reported age at death. With the exception of the jump at 12 months in the 2003 data (due to the relatively strong age at death transference noted above), the distributions of ages at death are quite similar for all three surveys. From this perspective, the data problems discussed above do not loom large. Figure 7, in contrast, shows the cumulative share of reported deaths as a share of all births rather than all deaths. Here the 2003 line is consistently below those of the other two surveys, across the entire distribution of ages, which of course is another way of saying that mortality rates are consistently lower. There is little indication that the 2003 exceptionality is concentrated at a particular age range.

Finally, this is shown as well in Table 3, which presents for each survey a range of mortality rate estimates in addition to infant and under-5 calculations, controlling as in the earlier figures for the cohort of the child. For similar cohorts, for each mortality measure, 1992 and 1997 are very close while 2003 is substantially lower.

displacement, and then calculate predicted mortality for the interval of interest (under 12 months). We do this below in Section 5.

Comparisons with results from the 2000 Multiple Indicators Cluster Survey

The MICS provides an additional source of recent information on mortality. It used the same sampling frame as the last two DHSs (see UNICEF 2000), that is, one based on the 1993 census, and the same sampling methodology (sampling procedures are discussed further in the next section). What is of particular interest for our purposes is the degree of consistency of the MICS mortality data with the DHS data. The summary report on the MICS data undertook this type of analysis with regard to the 1997 DHS. It shows that retrospective information on infant and child mortality in the 2000 MICS very closely tracks that collected in the 1997 DHS for the same calendar years (see in particular Figures 5 and 6 of the MICS report).

This makes the lack of consistency in retrospective mortality data for the 1997 and 2003 DHS all the more striking. The fact that the 1997 DHS and 2000 MICS are consistent, and the 1992 and 1997 DHSs are also broadly consistent (even though using different sampling frames), but the 2003 data are different from each of these, certainly casts some doubt on the accuracy of the 2003 DHS mortality data. It is true that while the 1997 and 2003 DHSs and the 2000 MICS all use the same sampling frame, the later DHS is three years further removed than the MICS from the original census on which the sampling is based. But this could hardly account for the level of discrepancies observed.⁵

Regionally disaggregated mortality estimates

As indicated by Figures 8 and 9 for under-five mortality, the inconsistencies across surveys are found in the data for both rural and urban areas. In both cases the 2003 line lies well below those for the earlier years. We also did the calculations by province (*Faritany*). For two of the smaller Faritany, some cohorts have zero deaths in a calendar year, preventing the calculation of the full trend line. Nevertheless a pattern is clear from the province level trends, graphed in Figures 10 through 15. The 2003 vs. 1997 (and 1992) gaps in estimated mortality for similar calendar periods are very large for Antananarivo and Fianarantoa, smaller for Toamasina and Mahajanga, and do not seem to exist at all for Toliary and (less conclusively) Antsiranana. Antananarivo and Fianarantoa are the two most populous provinces, together having a weighted share of about 53% of women; hence they determine to a large extent the aggregate picture seen above. This disaggregated analysis suggests that if there were problems in the 2003 survey, whether due to sampling procedure or interviewer performance, they were concentrated in several provinces. We return to this issue in the next section.

⁵ Another source of nationally representative data are the household surveys collected by INSTAT, known by their French acronym *EPM*. Two such surveys were carried out in the same years as the last two DHS surveys. However, these surveys do not contain a fertility module recording child births and deaths, so cannot be used as a check on the DHS mortality estimates.

3. Sampling issues

In this section we explore the possibilities of biases due to sampling procedure. We are not referring here to the usual survey sampling errors but to systematic biases arising from the use of a sample or samples that are non-representative. In other words, the large differences in mortality rates for given child cohorts could arise from the fact that the two surveys essentially cover different populations, rather than from interviewer underrecording or respondent underreporting of deaths. Strictly speaking, at best one can assess only whether the sampled populations in the two surveys are different, not whether one sample is more representative of the overall population than the other. However, in our case we do have data for a third survey year (1992) that can shed light on the 1997-2003 comparison.

The most likely source of ‘spurious’ differences across surveys (meaning, differences in the characteristics of the samples that do not reflect true changes over time) would be differences in the sampling frame. As noted, however, the 1997 and 2003 DHSs used the same sampling frame, based on the national census conducted in 1993 (see Annex A in the online DHS reports for 1997 and 2003). Both surveys were stratified on province (*Faritany*) and rural/urban location (the later survey was further stratified by large and small urban centers). For both surveys, 760 primary sampling units equivalent to *zones de denombrement* of the census were selected with probability proportional to size. Based on this, one would not expect there to be systematic differences across surveys other than those reflecting true changes over time.⁶ Still, it is worth considering the issue directly by looking at the data.

To do this, we distinguish two types of variables to compare across surveys. First and most useful are factors that essentially would not change over survey years for a given cohort of respondents. Women who were born in the period, say, 1963 to 1967, should on average report the same schooling, age at first marriage, and partner education, and be the same height, whether they were interviewed for the 1997 or 2003 survey. Thus for example if we have two nationally representative surveys of adult women, women age 30 to 34 in the 1997 survey should have same mean schooling as women age 36 to 40 in 2003. Hence comparing means for such ‘artificial’ cohorts would provide a clue as to the presence of systematic sampling errors in one or the other of the surveys (or in both but to different degrees). For these comparisons, one has to be careful to use cohorts that were old enough by the time of the earlier survey to have ‘permanent’ or fixed values of the variable in question. For schooling, for example, this presumably would be satisfied for women born in 1963-67—virtually all would have completed their schooling by 1997 when they were age 30 or older—and it would certainly be satisfied for older cohorts.

The other comparisons are of variables representing the standard of living (assets, housing), access to services (electricity, clean water), and household characteristics (marital status, household size, number of children). Here we do not want to compare cohort-specific means across surveys, because such means would in part capture true life-cycle effects and thus should change over time: women born in a given year would be six years older in 2003 than in 1997 and

⁶ An exception would be differential growth of the population across regions or rural/urban location, which would make the initial sampling weights increasingly inaccurate over time. However, Madagascar does not have very high internal migration, at least from rural to urban areas.

thus they and their households may have accumulated more assets, had additional children, or moved to a better house. Hence for these variables, we compare means for respondents of the *same age* (hence the same stage in the life cycle) in each survey, not the same birth cohort. Since even controlling for age there can be secular trends in these characteristics—for example, an individual in 2003 at the same stage of the lifecycle as someone in 1997 will have more assets if the economy has been growing—these comparisons are not as good at distinguishing spurious from actual changes over time. Still, we can gage the magnitude of any changes in terms of plausibility with reference to what we know about overall changes in incomes in Madagascar over the period. Further, changes in characteristics such as marital status and family size would likely occur only over the long term, in response to broad social or economic trends. Hence two representative surveys several years apart should not exhibit much or any difference in means for these variables.

Means by survey for the first set of variables are shown in Table 4. The means are calculated for five-year birth cohorts of women, beginning with 1953-57 and ending with 1968-72. Beneath the means are p-values for tests of the differences between 1992 and 1997 and between 1997 and 2003.⁷ Indicators for whether the respondent had any schooling (i.e., some primary or higher) and any secondary schooling are statistically the same across survey years for each cohort. On the other hand, the share of women with completed primary (shown in the third groups of columns) is sharply higher in 2003 relative to the previous two years. The reason for the difference with the any primary indicator is that in 2003 a much larger share of women with primary as the highest level were recorded as having completed grade five, which is equivalent to the completed primary cycle in Madagascar. There is no valid reason for this discrepancy: we are considering women born at the same time but merely interviewed at different dates, and all these women would have finished their primary schooling by even the earliest interview date. However, this may be a case of interviewer mis-recording rather than differences in the sample. Interviewers in the last survey may simply have assigned the maximum years to women reporting primary as their highest level. The same pattern is observed for completed secondary schooling.

For age at first marriage, there is a large and statistically significant increase between the 1997 and 2003 surveys. For each cohort, the mean is at least a full year higher in 2003. Between 1992 and 1997, in contrast, the differences are smaller and mostly statistically insignificant (these means of course are conditional on having been married).⁸ Women born in the same period are also taller in the 2003 sample than the 1997 one; the difference is statistically significant for three of the five cohorts.⁹ The difference is on the order of one centimeter. Finally, mean reported years of education of the woman's partner is about one year higher in

⁷ For all statistical tests reported in this report, variance calculations take into account the sample design.

⁸ There could be true increases over time in average age at first marriage for a cohort of women if some women marry later in life. However, in the DHS samples, less than 2 percent of ever married women reported an age at first marriage above 30, and less than 1 percent above age 32. Therefore late marriage should not affect the comparison of means across surveys, especially for the older cohorts in the table. Note as well that it would equally affect comparisons of the 1992 and 1997 means. Yet for the oldest two cohorts, mean age at first marriage is essentially the same in the first two surveys while it increases by a full year from 1997 to 2003.

⁹ Height data were not collected in the 1992 DHS, so only the 1997-2003 comparison could be made. For the next set of variables referring to education of the partner of the woman, there was a non-trivial number of 'don't know' or non-responses in the 1992 data, so again only 1997 and 2003 are shown.

2003 than in 1997. As shown in the last set of columns, the share with 5 years or more of schooling is more than a third higher in the later survey.

Significantly, where there are differences in these cohort specific means between the last two DHSs, they move (looking from 1997 to 2003) in directions that are usually associated with greater wealth or income. Higher income individuals typically have more education and more educated partners; are taller (reflecting better nutrition in childhood), and plausibly, are more likely to have delayed marriage for schooling or other reasons. It is possible that these differences are responsible for some of the differences in measured infant and under-five mortality between the last two surveys. Equally important, they suggest that other, unmeasured factors that are also associated with mortality may also differ across the surveys.

Table 5 shows the means for each survey year for a range of other characteristics, not conditioning on respondent cohort. Not all of these characteristics changed across surveys (e.g., household size and share married), but where they did change, with few exceptions the changes occurred between 1997 and 2003 rather than 1992 and 1997 (the exceptions are the marital status and household size indicators, which changed across both pairs of surveys by similar amounts). The mean number of children was lower in 2003 than 1997, while level of assets, access to electricity and piped water, and having a toilet or latrine in the domicile, were statistically higher. Sometimes these differences are substantial. Qualitatively speaking, these changes need not be spurious, as we have stressed: in a period when incomes were rising—if only slowly and mostly only from 1997 to the political crisis of 2002—one might expect the share of households with piped water or latrines to rise. Still, in view of the modest economic growth over the period (See Razafindravonona et. al. 2001), many of the increases seem very large. The share reporting having electricity rose from 14% to 22%; the share of households with piped water rose from 20% to 27%, a 35% proportional increase; the mean of the household wealth index was 17% percent higher in proportional terms in 2003 than in 1997 compared to a 4% difference between 1992 to 1997.

We conducted the same exercise for a series of variables related to maternal and infant health care and maternal health knowledge (results not shown). Rates of child immunizations for a range of diseases (collected for children under-5 at the time of each survey) were significantly higher in 2003 than in 1997, and in fact were often also significantly lower in 1997 than in 1992. Rates of professional prenatal and birthing care were much higher in 2003 than 1997, though rates of maternal tetanus toxoid injections during pregnancy fell. Knowledge of oral rehydration therapy rose both from 1992-1997 and from 1997-2003, while use of modern contraception did not change. Like many of the variables discussed above, the patterns for vaccinations and prenatal and birthing care strongly suggest that the 2003 sample was more advantaged in terms of factors that would improve child survival. For these variables, however, it is quite difficult to say whether the measured differences are spurious, reflecting sampling problems, or instead reflect actual changes, since coverage of maternal health and immunization services can improve significantly in a fairly short period of time as a result of policy.

Province-level sample comparisons

Given the analysis above showing the discrepancies in retrospective mortality estimates to be larger for some provinces than others, we consider whether differences in sample characteristics across surveys depend on location in a similar way. For analysis of ‘fixed’ characteristics by respondent cohort, sample size issues potentially become a problem when disaggregating by the six provinces and survey. Therefore we consider broader cohorts of women than those used earlier: born in 1953 to 1962, and 1963 to 1970. Table 6 shows the comparisons for the older cohort for age at first marriage, height, and partner education. For Tana and Fianarantsoa, for each variable the 2003 values are substantially and statistically higher than for 1997, as we would anticipate given the pattern in the mortality estimates in Figures 10 and 11. This is especially the case for Tana, where reported age at first marriage was 1.5 years higher in the later survey and height was fully 3 centimeters greater. But the discrepancies are also very large, in the same direction, for Toliary. For the younger (1963-70) cohort (Table 7), only Antananarivo and Fianarantsoa show consistently significant increases from 1997 to 2003, but the smaller sample sizes for the other provinces should be kept in mind. Toliary and Mahajanga have comparably sized increases in means for one or two of the variables, even though the differences are not significant.

We also examined changes across surveys in the second series of variables discussed above (results not shown). Few patterns stood out here, but for two of the variables the findings for Tana in particular are noteworthy. The share of households in Antananarivo with electricity was 0.27 in the 1997 survey (similar to 1992) but 0.45 percent in 2003, a 19 percentage point increase. For Fianarantsoa the increase was 6% and also significant, while for the other provinces the differences were smaller and not significant. Similarly, for the asset index, Antananarivo registered a 27 point proportional increase from 1997 to 2003 and Fianarantsoa registered a 16 percent increase, both much higher than for the other faritany. For Antananarivo at least, the magnitude of the changes seem to be too large to be explained by improvements in incomes over the period, even if, as argued by Razafindrakoto and Roubaud (1999), national statistics significantly underestimated economic growth in urban Antananarivo in the 1990s.¹⁰

The foregoing analyses at the aggregate and disaggregated levels at the very least raise concerns about the comparability of the 2003 and 1997 DHS samples. Certainly, given that the sampling frame was the same for 1997 and 2003, we would not have expected to find a number of differences in mean values of characteristics that should be the same once one controls for the cohort of the respondent. It bears noting that, as with the retrospective mortality information, we tend to find more correspondence in these variables between the 1992 and 1997 DHSs, despite their reliance on different sampling frames, than between 1997 and 2003.

The same applies to certain other characteristics such as the number of children and wealth-related indicators that would not be expected to change much over periods of just several years for comparable households, that is, households at the same stage of the lifecycle. These changes

¹⁰ See also Glick and Roubaud (forthcoming), who find real wage increases in the capital city in 1995-2001 that are much larger than implied by statistics on growth. Still, the increases in assets and share of household with electricity in Tana province between the 1997 and 2003 DHSs seem implausibly large, especially when seen together with the large discrepancies in the means of the ‘time-invariant’ indicators and in the retrospective mortality information.

are more substantial between the 1997 and 2003 DHSs than between the 1992 and 1997 surveys (and were more often not statistically different from zero between 1992 and 1997), despite the fact that the sampling frame changed between the first two surveys and did not change between the second and third. These welfare-related indicators by and large ‘improved’ in 2003 over 1997.¹¹ This in turn may explain part of the measured reduction in mortality between the 1997 and 2003 surveys.

As for why there should be systematic differences in the samples when the sampling frame did not change, it is difficult to do more than speculate. In that vein, however, one can point to several possibilities. One is that the sampling frame became out of date due to differential population growth or movement across areas. This does not seem too likely for a seven-year period characterized by modest economic growth and in a country with traditionally low rural-to-urban migration. Another possibility is that, while the overall sample design was consistent over the last two surveys, procedures were different in the field, for example, in the approach to the enumeration and selection of households within a PSU.¹² Finally, interviewers may not have posed questions or recorded answers the same way in the two surveys; the latter was seen to be the case with regard to the recording of the date of child births in 2003. However, it is difficult to see how this would lead to the systematic differences (meaning, by and large associated with greater affluence in 2003) in characteristics seen above.

The fact that the extent of the discrepancies in sample characteristics across surveys seem to vary by province is not surprising. In most nationwide surveys, once the survey is in the field, control over sampling activities as well as supervision of interviewers are decentralized to a significant degree. Hence if either of these factors are behind the discrepancies in sample characteristics across surveys, they could also lead to the differences observed across provinces. Note as well that these patterns by province also correspond to some degree with the variations in discrepancies in retrospective mortality observed in Section 2. In particular, Antananarivo, for which the data show the most consistent (and we think, implausible) ‘improvement’ in living standards between 1997 and 2003, is also one of the two Faritany with the largest ‘decrease’ in retrospective mortality across surveys.

If the differences in the samples are thought to be an artifact of the sampling procedure, one can attempt to control for this in a multivariate framework, ‘purging’ from the measured change in mortality that part which merely reflects spurious changes in sample characteristics. As discussed below, however, this procedure has shortcomings, in particular the inability to control for all relevant factors that differ between surveys.

Finally, we make note of another potential check of data consistency that is intuitively appealing but may lead to misleading conclusions. This would be to see if the relationship of mortality and its determinants is the same across surveys. The idea is that if this were found to be true (by

¹¹ The fact that the differences where they are found consistently point in one direction rules out an explanation based on simple sampling error. Such an explanation would not be very plausible anyway given the large sample sizes in each DHS.

¹² This could matter if, for example, household location within an enumeration area is correlated with wealth and health. Often, households that are more remote from village centers may be poorer. Therefore procedures in the field with respect to enumeration and household selection may affect average sample characteristics.

examining the stability across surveys of the coefficients in a regression to explain mortality), the observed differences in mortality would be more likely to be reliable; while if the relationships changed significantly, there must be some error, whether in sampling or from misreporting/misrecording. However, this reasoning is incorrect. If the relationship between mortality and other variables is approximately linear, one would get the same estimate of this relationship in samples drawn from two different populations that have different values of these characteristics. Therefore the differences in mortality could still be spurious, reflecting sample differences, even if the covariances are stable across surveys. Another way that a problem with sampling in the 2003 DHS could lead to a spurious change in mortality while leaving the covariances unchanged would be if it affected mortality primarily through differences in unobserved determinants that shift the mortality function down without affecting the slopes.

Nor would the opposite finding—a lack of stability in the relationship of mortality with other variables—be proof that the measured reductions in mortality in 2003 are spurious. This would only be assured if the potential sources of true declines in mortality are not mediated by these variables. This assumption can easily be violated. For example, if policies have been directed at improving maternal and child health service quality, the effect of utilization of these services on child mortality outcomes can change over time. Or, expansion of services to improve child survival may be directed at poor and less educated households. This will change the relationship of education and wealth to infant or under-five mortality over time. For this reason and the reasons given in the previous paragraph, it does not seem that much insight can be gained from examining whether the covariances of reported mortality and other factors have changed between surveys.

4. Interviewer Errors

The problems in the 2003 DHS with regard to heaping of birth years and age at death raise the possibility that interviewer practices are also responsible for other problems, including the much lower mortality rates in that survey compared with similar periods in the earlier DHSs. There are two possibilities that could lead to under recording of deaths¹³. One is that there is a systematic problem, perhaps originating in the training, leading to a general practice of under-recording. The second is that some share of interviewers are underperforming badly enough to account for the problems seen in the aggregate data. Even with the availability of interviewer identification codes in the data, the first hypothesis could not be tested because the errors would be present among all interviewers more or less equally. In contrast, the interviewer codes can be used to check for the second problem. Essentially we would be looking to see if the probability that a death is recorded is particularly low for some interviewers.

We do this in a regression framework where the dependent variable takes the value of 1 if a child is reported to have died and zero otherwise (the sample is all children whose births are recorded in the 2003 survey). The identification codes of the interviewers are entered as a series of dummy variables. In order to achieve better linguistic and ethnic matches with survey

¹³ We use the term ‘under recording’ to signal that we are talking about interviewer errors, not respondent reporting errors. Of course, respondent may ‘under report’ deaths in the sense that interviewers fail to properly ask for this information.

respondents and for logistical reasons, interviewers tend to be assigned to different areas of the country: typically, as confirmed for the DHS by a check of the data, they work within a single province. Hence it is necessary to control for region given that mortality varies independently along this dimension. Further, within a province, interviewers do not necessarily all visit the same communities, adding an additional source of potential confounding of ‘interviewer effects’ on mortality. Therefore in a series of models we added controls for smaller administrative units, namely *Firaisana*, of which there are 210 in the 2003 survey¹⁴, and the survey clusters (the PSUs of the survey), which number just over 300.

The last model is the best one for controlling for local area effects on mortality, but it will not be able to pick up interviewer problems that occur similarly for each member of a team of interviewers, an outcome that might arise from poor supervisor performance. This is because typically a survey cluster is surveyed by one team, and a team has one supervisor. Therefore the supervisor effects (or other team-level impacts) are differenced out of the cluster ‘fixed effects’ model. In the model with province controls only, in contrast, the interviewer coefficients may be picking up supervisor effects as well as any other aspect of sampling or performance that varies within a province.

The first two columns of Table 8 show the estimates for models with province controls only and with cluster controls added; the model with *Firaisana* controls did not add any additional insight. We present only the coefficients on the interviewer dummies.¹⁵ The estimates of this (linear) probability model indicate the effect of an interviewer on the probability that a child will be recorded as having died: in effect the model estimates interviewer-specific mortality rates, or more precisely, mortality rates relative to that of the interviewer which serves as the base category for the regression. In the model with only province controls a number of interviewer codes are statistically significant. In the model with cluster controls, none are. This suggests that individual interviewer performance is not the issue, though the magnitudes of the coefficients nevertheless suggest a wide range in interviewer-specific mean mortality rates.

This information is summarized below each model where we calculate percentiles of the estimated interviewer effects. The range in individual mortality rates between an interviewer at the 25th percentile and one at the 75th percentile is 0.08; for the 5th to 95th percentile the range is 0.10. Given that we have controlled for cluster mean effects, these differences are large in relation to the mean mortality rate itself for the sample, which is only about 0.11. On the other hand, the same exercise repeated on the 1997 data yields, for the model with cluster controls, results that are broadly similar. As summarized below the 2003 calculations in the table, the interquartile range in interviewer effects was actually somewhat larger in 1997.

We conclude therefore that individual interviewer underperformance is not the explanation for the large differences in reported mortality between the two surveys. Again, this does not rule out

¹⁴ These are relatively small administrative units (though larger than the cluster or primary sampling unit). There are 210 *Firaisana* represented in the DHS surveys. We thank Josee Randriamamonjy for providing us with code to match the survey clusters to the appropriate *Firaisana*.

¹⁵ Almost all interviewers conducted a hundred or more interviews each. Those with fewer than 50 interviews, accounting for less than 2 percent of all women interviewed and thus too rare to affect mean outcomes, were dropped from the estimation.

either general aspects of interviewer practice or variations in supervisor (or interviewer team) behavior. On the latter, it is noteworthy that the distribution of interviewer effects for the model with only province controls (in which the interviewer estimates may pick up variation in supervisor quality or other factors) is much larger in the later survey.

Finally, another check on interviewer quality in 2003 compared with 1997 is to check for systematic differences by interviewer code in the numbers of reported births per woman. This is done in the last two regressions in Table 8, which estimates interviewer effects on the number of children ever born. As the percentile calculations indicate, there was actually less variation across interviewers in this measure, controlling for either province or cluster, in 2003 compared with 1997.

5. Approaches to adjusting mortality estimates

Two approaches are possible to adjust mortality figures to correct for possible biases due to interviewer errors or sampling problems. One, just mentioned, is to use regression methods to control for differences in characteristics that seem to reflect errors. The other takes advantage of the fact that the DHSs collect complete fertility histories of women including all births as well as deaths. As already highlighted by the analysis in Section 2, this means that successive DHSs contain information on the same cohorts of children, i.e., children born in the same calendar year but who were of different ages (if alive) at the date of each survey. For a child born, say, in 1990, reported infant or under-five mortality should be the same whether this information was gathered in 1997 or 2003. If it is not, this is an indication of a problem with either the sampling or the practices of the interviewers, and the discrepancy can in principle be used to correct the mortality estimates, or more precisely, the estimated change in mortality. This is the approach proposed by ORC Macro in the Appendix to the report on the 2003/04 DHS. We discuss this next and present our refinement of the method. After that, we discuss the regression control approach.

Adjustment based on within-cohort comparisons

In their report, INSTAT and ORC Macro (2005) calculate under-five mortality rates for the ten-year period 1986-1995 for both the 1997 and 2003 DHSs. The ratio of the 2003 to the 1997 mortality rate is 1.32 and this is assumed to be a proportional measure of the error in the later survey. It is used as an inflation factor to adjust the 2003 under-5 mortality rate for recent cohorts (that is, for the eight year period prior to the survey). Thus the procedure accepts the trend in the 2003 estimates but adjusts the level.

This correction is attractive in that it has a logical basis for correcting the level of mortality while still making some use of the 2003 data (the trend in recent years). We offer a refinement of the approach below. However, we should point out that it relies on a number of assumptions, namely that:

1. The later survey estimates are incorrect and the 1997 estimates are the correct ones. As noted, the differences in samples does not tell us which one is representative, or more representative. The apparent errors with regard to recording the timing of births and deaths in the recent DHS does not explain why reported under-1 and under-5 mortality during 1986-1995 would be so much lower for the 2003 DHS than for 1997. This pattern remains no matter what age range is considered to deal with birth displacement or age of death transference problems. Therefore unless these problems somehow imply a general under recording (or under reporting) of deaths, they are not a basis to assume that 1997 mortality is 'correct' and 2003 is 'too low'. However, our considerations of the mortality data from a third DHS (the 1992 survey) as well as the MICS 2000 survey, as well as our analysis in the previous section of a range of other sample characteristics, does point to 2003 as the problematic survey. In any case, the choice of 'correct' year does not affect the validity of the approach as a way to correct the change in mortality between years.
2. There are no recall problems for births and deaths occurring some years before the survey, or that these problems do not increase with the time since the events. For the birth or death of a child that occurred in 1987, for example, a woman interviewed in 1997 must recall an event occurring 10 years earlier. For a woman interviewed in 2003, that event was 17 years in the past. Our priors would be that recall ability would be somewhat weaker in the latter case, potentially biasing the comparison within cohorts across surveys (depending on how recall difficulties affect reported births vs. deaths). However, it is not possible to confirm this or say whether it would be a serious problem.¹⁶
3. The trend in the 2003 data—that is, the change over time in infant or under-five mortality for respondents in the 2003 survey—is correct even though the level of mortality is underestimated. On its face, this seems questionable. Whether the problem is interviewer or respondent error or instead reflects the sampling of different populations, one might expect problems to occur in both dimensions. For example, if the sampled populations are not the same, it is possible that changes over time in access, utilization, or quality of health services differ, hence that trends in mortality differ also. However, we can examine the consistency of trends across surveys for similar periods of calendar time. This is done below.
4. Children within the same cohort but from different surveys are the same with respect to all possible determinants of mortality. This may not hold because while the children are of the same cohort, the cohorts of the *mothers* of these children will differ. For example, in the 1997 survey, the mothers of children born during 1992-96 will have given birth in the last five years; for the 2003 survey, the mothers of children in this cohort will have given birth between 7 and 11 years previously. The mean age of the two groups of women at the time they gave birth to these children will differ depending on the typical fertility-age profiles of women.¹⁷ Birth order and other age or lifecycle-related factors

¹⁶ One might compare reported infant or under-five mortality for recent and more distant years from the survey date, but any differences might reflect trends in mortality, not recall differences.

¹⁷ Consider the extreme case under which the probability of giving birth was the same for each age between 15 and 49 and essentially zero before age 15. Then the mothers of children in the 1992-96 birth cohort would have to be older in the later survey. Since they gave birth at least 7 years previously, this sample of mothers would have to be

such as household assets levels thus may also differ, and these differences are not spurious changes across surveys but real changes. Since these factors affect child health and mortality, this means that some of the observed difference in mortality within the same child cohort but across surveys may in fact be real. However, our expectation, based on examining survey differences in mean mother's age at birth for children in the same cohort, is that these effects will be small.

The foregoing considerations suggest that we need to be somewhat cautious when using the within-cohort comparisons to adjust the mortality estimates in the 2003 survey. Still, the overall within-cohort consistency of the 1992 and 1997 DHS mortality data (and the 2000 MICS data) seen above is noteworthy. The lack of similar consistency between 1997 and 2003 for common cohorts suggests a problem with the 2003 data.

A refinement of the approach

In this section we extend the approach used by ORC Macro. Their adjustment relied on calculating average mortality from each survey for children under five for a given interval of calendar time. Here we use a regression approach. Rather than using averages, we model the determinants of individual probabilities of dying before 12 months and before 5 years using standard hazard or survival modeling techniques, namely the proportional Cox model and the Weibull. The two gave similar results so we report on the results for the latter only. Hazard regressions model the time to death as a function of time (or age) itself and a set of covariates. They accommodate the fact that some observations—in the case of under-5 mortality, children born less than 5 years prior to the survey—are censored because the complete interval of 0 to 59 months is not observed.¹⁸

The most flexible version of the model includes dummies for year of birth of the child and for survey year, and interactions of survey year and birth year. The birth year dummies essentially captures the time trend in mortality; the survey year dummies capture 'spurious'¹⁹ survey effects, i.e., differences in mortality for the same cohorts in different survey years; and the interactions let the trend in mortality differ by survey year. This specification allows us to calculate separate estimates of child or infant mortality for each birth year cohort for each survey. We also estimated models with the trend expressed instead as a cubic function of year of birth, also interacting the trend with survey year. As a partial control for the potential effects noted in point (4) above, the models also include the age of the mother at the time of the birth of the child.

To deal with the problem of displacement of birth years for children in the 2003 survey, we estimate (for all the surveys) the determinants of survival to age six rather than five; hence we include information on children whose birth may have been misrecorded as being five rather than

at least 22 years old if there were negligible numbers of births for girls less than 15. In the 1997 survey, in contrast, the mothers of these children could include current 15-21 year olds. However, counteracting this tendency, the probability of a birth also falls off as a woman's age increases, and for this reason the data show a slight fall in the average age of mothers at time of birth for the same cohort when moving from earlier to later survey years.

¹⁸ ORC Macro's 'synthetic cohort' approach to estimating under-5 and under-1 mortality also deals with censoring, in a different manner. See Rutstein (1983).

¹⁹ 'Spurious' conditional on the assumptions above.

four years before the survey. It is straightforward to use the parameter estimates to calculate predicted survival (or its inverse, predicted mortality) to age five.²⁰ For infant mortality, to deal with age at death transference, we estimate a model of survival to 15 months and use the estimates to calculate mortality prior to 12 months.²¹

Figure 16 shows calculations of under-5 mortality rates based on hazard model estimates for survival to age 6 using the cubic time (birth-year) trend. Each point shows the estimated mortality for children born in the indicated year, for the indicated DHS survey. Immediately apparent is that, as with the descriptive calculations shown earlier, estimated mortality is much lower for 2003 than for 1997 (and also, 1992) for the same cohorts of children.²² Also as seen earlier, levels of mortality for 1992 and 1997 for cohorts observed in both surveys are quite close; 2003 seems very much the odd man out.

With respect to trends, there is a broad consistency across the surveys for the overlapping years from the late 70s to early 90s, with mortality peaking in the early to mid 80s and then falling. Note that these calculations of expected mortality tend to show greater trend consistency between 2003 and the earlier surveys than the earlier calculations using the ORC Macro method.²³ On the other hand, and significantly, the 1997 and 2003 survey trends diverge for the period 1994-1997, with the earlier survey indicating an uptick in under-5 mortality. Therefore with respect to the point raised in (3) above, we find partial but not complete consistency of the trends in mortality over the same years in the latest and earlier surveys.

Figure 17 shows under-5 mortality rates again, this time based on estimates from the model with year of birth dummies. This yields jagged trend lines but with similar overall patterns as in the previous model.²⁴

We next use the estimates from this model to calculate adjustments in under-5 mortality rates along the lines of the procedure suggested by ORC Macro. We assume as they did that the 2003

²⁰ Under-5 mortality is calculated as the inverse of the cumulative density function representing the probability of surviving to 59 months.

²¹ There may still be some bias in the estimates from the under 15 month survival model because deaths under 12 months are improperly bunched at month 12 rather than spread across earlier months. For the under-6 year survival models, on the other hand, the problem is not that the age at death is misrepresented but simply that the calendar year of birth may be displaced by a year.

²² In the hazard models of under 6 mortality, tests of significance of the survey year effect (2003 relative to 1997), incorporating the survey year dummies and their interactions, confirms at the 1% level that mortality was lower in 2003. For the under 15-month mortality models reported below, the difference is significant at least at the 5% level.

²³ To reiterate, our regression-based calculations show the probability of death by age 5 for a child born in the indicated year. The Orc Macro approach, in contrast, estimates mortality of children under 5 during a window of calendar time for which the indicated year is the midpoint. Relative to our method, their measure incorporates a lag since it includes children already born by the indicated year; or, relative to their method, ours shows a projection since it indicates mortality over the five-year period starting with the indicated year of birth.

²⁴ It is noteworthy that the patterns of year-by-year peaks and valleys are similar for 1997 and 2003, though sometimes for more recent years the peaks in the 2003 survey follow those in 1997 with a lag of a year or so. It is possible that this reflects a tendency of interviewers in each survey to record (or respondents to recall) death events around certain calendar years or certain (e.g., even-numbered) years prior to the interview. This would lead to sharp peaks even for under-5 mortality projections, for which death probabilities are spread over 5 years, because for any birth year cohort the largest share of deaths tends to occur before age 1. However, consideration of years for which there are large peaks or valleys does not suggest any particular pattern.

survey is the one in error so mortality rates for 2003 must be adjusted upward, but this seems reasonable based on the discussion above. The results are shown in Table 9. For the 1997 and 2003 DHSs, we calculated the mean of the estimated mortality per 1,000 for children born in the 10-year period 1986 to 1995 (the same used by ORC Macro), in the 15-year period 1981-1995, and in the 5-year period 1991 to 1995. The ratios of the 1997 to the 2003 averages are the proportional adjustment factors, shown in the first row of the table. These are very similar to each other—ranging from 1.33 to 1.37—and also very close to the correction factor calculated by ORC Macro (1.32).

We applied this adjustment to recent under-5 mortality in the 2003 DHS: first for average mortality in the eight years preceding the survey, then for the five years preceding the survey. The table shows the estimates for the eight or five year period preceding the 1997 DHS survey, followed by the unadjusted estimates for the same interval for the 2003 survey, followed by the adjusted 2003 estimates.²⁵ Note that the last calculation is not just an extension of the trend from the 1997 DHS: we have adjusted the *level* of the 2003 estimates only, so the measured change after the adjustment essentially captures the recent trend in mortality in the later survey.²⁶

Considering either the eight or five year interval before each survey (and using the 1986-1995 period to calculate the adjustment), we see that the correction sharply reduces the reduction in mortality between surveys, but there remains an improvement nonetheless. For the first case, the adjustment increases 2003 mortality from 106 to 143 per 1,000. The latter figure is similar to the 139 per 1,000 that ORC Macro arrives at for the same eight year period prior to the 2003 survey. Given under 5 mortality of 164 for 1997, the implied reduction in mortality between the 1997 and 2003 surveys after adjustment is about 22 deaths per 1,000. This is almost two thirds lower than the reduction implied by the unadjusted estimates (59 per 1,000).

For mortality in the 5 years previous to each survey (bottom three rows), the reduction is greater, from 167 to 127 per 1,000. Here the adjustment removes about half of the mortality reductions seen in the raw data. Finally, since the adjustment factors are very similar when calculated using 1981-1995 and 1991-1995, the implied reductions in mortality are also similar, as shown in the second and third columns.

Next we turn to infant mortality. Figures 18 and 19 calculate under-12 month mortality per thousand analogous to Figures 16 and 17 for under-5 mortality. Overall, trends over time and within-cohort differences by survey year have the same pattern as for under-5 mortality, though there is an improbably sharp fall in infant mortality in the last five years or so of the 1992 DHS. Although smoothed over by the polynomial function in Figure 18, the model with separate birth year dummies in Figure 19 indicates a sharp uptick in estimated infant mortality in 2003 over the previous year. As this is very recent, it should, if it is real, be of some concern to policymakers. It is possible that this is a function of the high degree of censoring among recent births (most

²⁵ Note that the adjusted mortality figures are not equal to the unadjusted 2003 figure over the unadjusted 1997 figure shown in the tables. These rates refer to recent period before each survey that hence do not overlap (much or at all) across the surveys; instead, the adjustment makes use of data from the overlapping years indicated in the column headings.

²⁶ One could easily adjust both level and trend to the 1997 data, the latter via an extrapolation of a polynomial trend to years after the 1997 survey. However, this essentially forsakes the 2003 data entirely.

children born in 2003 were not yet one year old at the time of the survey), which might make it harder for the model to accurately estimate survival probabilities. However, a similar pattern does not occur with recent births in the earlier two surveys, at least to the same extent. It could also be an artifact of the pronounced heaping of deaths at 12 months in the 2003 survey, even though the model setup attempts to deal with this. Therefore we also looked at deaths among children under six and nine months who were born in 2003, and did the equivalent for the previous two surveys. In the 2003 data, a pattern remains of a slight elevation of (under 6 or 9 month) mortality among children born in 2003 compared with the previous several years. In contrast, for the 1997 and 1992 surveys death rates were lower among infants born in the most recent year than in the year before. (This pattern is also evident in the ‘share died’ figures in Table 1).

Therefore censoring (as well as transference of deaths) does not explain the pattern in the last survey. The differences in these measures of mortality between the years 2003 on the one hand, and 2002 or 2001, on the other, are not statistically significant, which is not unexpected given the small number of deaths. Unfortunately, we are not able to say more about whether or not this reflects a real change in the recent past, though we can note that the rates of prenatal care and use of doctors or other health professionals to assist in birthing were no lower for 2003 births than for births in the previous several years.

Table 10 calculates adjustment factors for infant mortality using the same method as for under-5 mortality. Because of the greater variability in the 2003 data for recent infant mortality (see Figure 19), the adjustment factors and consequent adjusted 2003 rates are also more variable. However, using information from the 10-year period (1986-1995) preceding the 1997 DHS survey as the basis for the adjustment, estimated infant mortality over the eight year period preceding the 2003 survey is about 85 per 1,000 (second column, 3rd row), which is almost the same as ORC Macro’s adjusted infant mortality figure. This adjustment reduces the improvement in mortality between 1997 and 2003 from 43 per 1,000 (94.8 minus 51.7) in the unadjusted data to only 12 per 1,000 (94.8 minus 83). Hence these figures imply a smaller improvement in mortality for the eight-year period before each survey than was seen above for under-5 mortality. However, if we consider the five rather than eight year period preceding each survey, the adjusted 2003 infant mortality is lower (from 60 to 68 per 1,000—bottom row of table), and the implied reduction from 1997 is larger.

Adjustment based on regression controls for spurious differences in sample characteristics across surveys

The second approach for correcting for biases in mortality estimates assumes that sampling of different populations rather than enumerator error is causing the discrepancies between surveys in the retrospective information on deaths. To control at least partially for spurious differences between samples in factors that affect mortality, one can include these factors—or however many of them are available in the data sets—as controls in a mortality regression that also includes dummies for survey year. With an adequate set of controls, the survey coefficients isolate the ‘true’ change in mortality between surveys. The key requirement for this to be valid is that the controls in fact are able to capture the range of mortality determinants that differ

between the two sampled populations. Unfortunately, this condition is not likely to be met even with a comprehensive survey such as the DHS. In particular, the DHS do not collect information on the availability and quality local health services.²⁷

Further, one has to be very careful in determining which variables to include. As discussed earlier, some factors should not change, or not change very much, in the five to six year intervals between DHSs: certainly religion, but also province of residence, schooling of adults, average household size, marital status of adults, and age at first marriage. The assumption is necessary that that any measured changes in these characteristics between two surveys is spurious. Therefore, following the discussion above, we cannot include factors related to household wealth and availability of services such as clean water, even though they are expected to be important determinants of child survival. Including them as controls would insure that the coefficient on the survey year gives a misleading estimate of the change in mortality over time, since (true) changes in these variables will account for some of the changes in mortality and these effects would be netted out of the survey year estimates.

For this analysis, we estimate child survival to age 6 years and restrict the sample to children who were born in the eight-year period prior to each survey. The latter restriction means that the survey year dummies are largely if not totally capturing changes that occurred between surveys given that at least five years separates each survey.²⁸ The choice to estimate survival to age six, again, is determined by the desire to avoid problems caused by the heaping of births in the 2003 survey at five years prior to the survey.

Table 11 reports the survey year coefficients in Weibull models that successively add controls to the base model in column 1, which includes the survey year dummies only and a constant term. 1997 is the excluded year so the table shows the estimates for being in the 1992 and 2003 surveys relative to 1997. Below these coefficients are estimates of the predicted under-5 mortality rates for each year, calculated from the estimates and data. These predictions capture survey effects controlling for differences in other covariates in the models, since they are calculated holding other covariates at the mean values for the pooled (1992, 1997, and 2003) samples.

In all cases the survey effect for 2003 relative to 1997 is negative and highly significant, while there is usually no difference between 1992 and 1997 (keep in mind that since we are estimating hazard functions for dying at a given age, a negative coefficient indicates that the variable has a positive effect on survival). Note as well from the second and third columns that the apparent reduction in mortality between 1997 and 2003 occurred in both rural and urban areas; the

²⁷ There are many variables in the DHS relating to the individual's *utilization* of a range of maternal and child health services, and including them will bring up the R-squareds in the models. However, as is well recognized, these behavioral variables are likely to be jointly determined with health or mortality outcomes, i.e., are endogenous, and so do not belong in a reduced form model. While we are interested primarily in the estimated effects of survey year in our case, not the control variables, the former will also be biased from the endogeneity of the latter, depending on the pattern of covariance between the two.

²⁸ The model differs from the hazard regressions in the previous section in that it is restricted to this interval before each survey and also by the inclusion of location, individual, and household covariates, not just survey and cohort information. The current model could be nested in the earlier one if the latter included these covariates and they were interacted with cohort.

proportional reduction is larger in urban areas but the absolute decline (bottom row) is very similar. Strikingly, adding controls for potentially spurious changes between surveys has only small effects on the estimated change in mortality from 1997 to 2003. In columns 4 through 6 we add indicators for rural residence and province, controls for *Firaisana* administrative units, and a set of (more or less) fixed individual and household characteristics. Although there were many highly statistically significant coefficients on these controls, the net survey effect for 2003, which in the model without controls implied a reduction from 1997 of 58 deaths per 1,000, remains above 50 per 1,000 even in the last case.

A naive interpretation of these results would be that the estimated improvement in under-five mortality between the last two surveys is accurate and robust: even controlling for potentially spurious changes in household or individual characteristics across surveys does not matter much for this estimate. However, as noted, the controls may not adequately capture differences in mortality determinants across surveys. Or, the controls for sample characteristics may not have much effect because the problem is with interviewer practices rather than sampling. Further, it would be hard to reconcile this optimistic interpretation with the large discrepancies between the 2003 survey and the 1997 survey (and the 1992 survey) in mortality for the same cohorts of children.

To further explore the last point, in an additional exercise we ran the survival models of the previous section (for births since 1975) adding in controls for fixed individual and household characteristics (results not shown but available from the authors). This had almost no effect on the gap in predicted under-5 and under-1 mortality for similar periods in the 2003 and earlier surveys. If we accept the premise that this gap for is spurious--and most if not all of it certainly must be--then this result shows that the available controls are not able to adjust for the problems in the mortality data. Thus we prefer the approach to adjustment used in the previous section, which uses the discrepancy as the basis for adjustment. Even after that adjustment, there appears to have been improvements, if smaller ones, in the mortality indicators.

6. Does policy explain the changes in mortality?

To the extent that infant and under-five mortality did fall, it is important to understand why, and in particular, whether and which policies may have played a role. Unfortunately, when access to or quality of services related to child health and survival have been improving for the entire population, it is not possible to attribute changes in outcomes specifically to policies given that other factors such as household incomes may also have been changing at the same time. However, when programs have been implemented in some areas and not others, one may be able to say something about policy impacts, because the unaffected regions can act as controls.

The Basics/Linkages project, implemented extensively in Fianarantsoa and Antananarivo provinces starting in 1999, could be characterized this way. Supported by USAID, the program involved integrated interventions to improve child nutrition and survival, including vaccinations, nutrition services and education, and reproductive health services, with a strong community component. The initial Basics project, which began in 1995, was limited to two health districts in Fianarantsoa and Antananarivo, but after 1999 it expanded rapidly to cover 23 districts in

these provinces. A program review suggests that the expansion brought significant increases in services to the districts served (Basics II Project 2004). Given the wide coverage in Fianarantsoa and Antananarivo, if there were effects on survival outcomes of the program, they should show up in the 2003 DHS data for these areas.

We examine this issue first by interacting province with survey year in the survival models, shown in Table 12. As in the previous table, we model survival probabilities to age 6 for children who were born in the eight-year period prior to each survey. Because of the presence of the interaction terms, most significance tests of interest, namely of the effects by province of the change in survival probabilities between surveys and of differences in these effects across provinces, cannot be read off the table. However, the appropriate calculations indicate that the effect of survey year 2003 relative to 1997 was significantly different from zero at at least the 10% level, and usually at 5%, in each of the six provinces except for Antsirabe ($p=0.12$). With respect to comparisons across provinces, the negative and significant coefficient on the 2003*Fianarantsoa interaction in column 1 of the table indicates that the reduction in reported mortality in Fianarantsoa from 1997 to 2003 was greater than in Taomasina, the base category. Further statistical tests reveal that the change from 1997 was greater in Fianarantsoa than in each of the other provinces except for Antananarivo. On the other hand, Tana province, where the Linkages project was also implemented on a wide scale, did not experience any larger reductions in mortality than elsewhere.

The next two columns show estimates from separate models for rural and urban areas. The rural results are qualitatively very similar to the countrywide results, which reflects the fact that about 80% of the population of the country is rural. Statistical tests indicate improvement in child survival in four of the six provinces (Toliari and Antsirabe are the exceptions). Again, changes were greater in Fianarantsoa than elsewhere. In urban areas, changes since 1997 also occurred in four of six provinces. In this case, however, the changes were no greater in Fianarantsoa (or Antananarivo) than elsewhere.

In view of the analysis in the previous sections of this report, the problem with these comparisons across surveys should be obvious: we have serious reservations about the comparability of the 1997 and 2003 DHSs. This is all the more problematic since Fianarantsoa and Antananarivo are the two provinces with the greatest apparent discrepancies across surveys in retrospective mortality information, and for Tana in particular, with respect to other sample characteristics. Therefore one should view the foregoing results very cautiously. However, we can also investigate province level changes over time using just the 2003 data by relying on retrospective data on mortality. This gets around the survey comparability problem, though it requires the same assumption discussed in Section 5 with reference to adjusting the mortality estimates, namely that the trends in the 2003 DHS are accurate. We estimate a survival model on children born since 1990, including a quadratic time trend that is interacted with province (higher order polynomials were also tried but did change the picture).

Predicted under-5 mortality by province is graphed in Figure 20. Fianarantsoa stands out again as having a sharp reduction in mortality, though starting somewhat earlier (the early to mid 90s)

than one might expect based on the timing of the expansion of the Linkages program.²⁹ Weaker reductions were seen in Mahajanga and Toamasina, while trend in Antananarivo actually suggests an increase in mortality in the last decade.

Subject again to the concerns about the accuracy of the mortality data, these results provide partial support for the view that the Basics/Linkages intervention has been successful at improving child survival. Reductions in mortality appear to have been larger in Fianarantsoa, one of the two provinces where the program was implemented, than elsewhere, with the exception (in the regressions measuring changes between surveys) of Tana province, which also received the program. On the other hand, changes in Tana were not larger than elsewhere and if one looks at trends derived from retrospective information in the 2003 data, there is an indication of an increase in under-five mortality. The analysis, it should be noted, does not control for changes in other determinants of child survival that may have occurred differentially across regions of the country.

Finally, one might pose the question: can the large drops in recent under-five and infant mortality reflected in the 2003 DHS (compared with estimates for a similar interval before the 1997 survey) be validated by the extensive program activity occurring between surveys? The answer to this would be no, for two reasons. First, measured reductions in mortality between the 1997 and 2003 surveys also occurred outside the areas covered by the Basics/Linkages program. Second, the large discrepancy in retrospective mortality data in the two surveys remains, and suggests an upward bias in (the absolute value of) the change in mortality between surveys.³⁰

7. Summary and Conclusions

We now summarize the main findings of this analysis.

As first reported by ORC Macro, the 2003 DHS data exhibit problems of displacement of births of children born after January 1998 to before January 1998, and problems of transference of infant deaths resulting in clumping of reported deaths at 12 months. With regard to the former, there is no evidence that the birth displacement occurred disproportionately for deceased children, something that would have led to underestimation of recent under-five mortality. In any case, the effects of these problems on estimates of infant and under-five mortality are not large, and can be dealt with by extending the age interval used in the mortality calculations.

The more serious problem is the large discrepancy in mortality rates estimated from different DHS surveys for the same cohorts of children. These rates should be the same or very similar. However, for 2003 they are well below those for 1997 and 1992 and are also below the

²⁹ Note though that the years on the x-axis represents the birth year, so children born several years before the program reached their areas would still have been exposed to it for some period while under 5 years of age.

³⁰ The fact that these discrepancies are largest in Antananarivo and Fianarantsoa, the two Faritany receiving the Basic/Linkages program, raises the interesting possibility that reporting of past mortality events was somehow influenced by the presence of the intervention. For example, in the light of positive child survival trends and the presence of the program, women in 2003 may have underestimated or been less inclined to report earlier deaths (or for that matter, interviewers may have been less inclined to ask about or record them). However, we do not think this effect is very likely.

equivalent calculations from the 2000 MICS data. This problem is not directly related to birth displacement or age at death transference and corrections to those problems do not address the discrepancy issue. The extent of the discrepancy varies markedly across the six provinces of the country, occurring with the greatest severity in Antananarivo and Fianarantsoa provinces and not occurring much or at all in Toliary and Antsiranana.

Further, broader checks of the data for all three DHSs indicate that a number of basic sample characteristics differ substantively and statistically between the 1997 and 2003 surveys. For a given cohort of women, means of characteristics such as height, age at first marriage, and partner's level of schooling should be the same in each survey, but they nevertheless appear to change between 1997 and 2003. Other indicators, such as the number of children, household assets, and access to electricity and piped water, should change at most only slowly over time. In many cases, however, there are large changes between the two surveys. There were far fewer statistically significant differences between the 1992 and 1997 surveys. There was regional variation here as well, with Antananarivo province in particular showing large differences in several key indicators between 1997 and 2003.

The differences in sample characteristics between the 1997 and 2003 surveys are systematic: they imply that the later survey sampled a somewhat more wealthy population than the earlier one. Given that the sampling frame used for the last two DHSs was the same, it is puzzling to find such systematic differences in respondent characteristics between the two surveys. It is possible, however, that sampling practices in the field (enumeration and selection of households) in 2003 resulted in the drawing of a sample of households that was better off than in the previous survey. Such households would also have experienced fewer child deaths. Hence differences in sampling may explain why, for the same cohorts of children, reported under-five and infant mortality is so much lower in the 2003 data than in the previous two surveys.

The fact that these discrepancies vary by province is not inconsistent with a sampling-related explanation. Once the survey is in the field, the organization of activities, including those related to sampling, becomes more decentralized. It should be noted that the differences in sample characteristics and mortality do not mean that it is the 2003 mortality estimates are too low rather than the 1997 being too high. As noted, however, there is an overall consistency of the 1997 data with the 1992 DHS as well as the 2000 MICS data.

An alternative explanation for the discrepancies in mortality across surveys is that interviewers in tended to under-record (or fail to probe sufficiently for information on) deaths in the later survey. This hypothesis gains credibility from the finding that interviewers in 2003 made a disproportionate number of a different kind of error in the fertility questionnaire, the displacement of births of children under five. We cannot discount this hypothesis with the information at hand. However, while poor interviewer performance could explain low numbers of recorded child deaths, it is hard to see how it would lead to systematic biases in characteristics associated with household wealth. And the two taken together, as just noted, are consistent with sampling of different populations, one less wealthy with higher mortality and the other more wealthy with lower mortality. Consequently, and with appropriate caution given the limits of what the data can tell us, we tend to favor this explanation.

Further with respect to interviewer behavior, we were able to reject the hypothesis that there was a problem in the 2003 DHS with certain *individual* interviewers substantially under recording deaths. Controlling for location (survey cluster), interviewer effects on the probability of a child being reported as deceased were not statistically significant, and the variation in interviewer-specific mortality rates was no greater in 2003 than in 1997. This does not rule out problems with the behavior of teams of interviewers, which could be caused by inadequate supervisor performance. It also does not rule out general problems in interview practices that could arise from the way the training was conducted, if all interviewers received the same training: this would affect the behavior of all interviewers, hence not show up as problems with specific individuals. However, if there was a problem affecting all interviewers, it would not lead to the observed differences across provinces in the discrepancies in cohort-specific mortality. In contrast, problems related to the performance of specific supervisors would be consistent with these differences, since supervisors and the teams they work with tend to be assigned to specific regions of the country.

Diagnosing the source of the problem is one thing; correcting for it is another. To adjust the mortality estimates for 2003 (or at least, to correct the estimate of the change in mortality from 1997 to 2003), two approaches were considered. The first is a regression-based elaboration of the correction proposed by ORC Macro. This approach assumes that the gap between estimates from the 1997 and 2003 surveys in mortality for the same cohort of children represents the proportional underestimate in 2003, and applies the differences to the 2003 data to get a corrected estimate of under-5 and infant mortality. The validity of this approach depends on several assumptions, a key one being that while the level in 2003 is wrong, the recent trend in mortality in the 2003 data is accurate. Examination of overlapping years in the 1997 and 2003 DHSs provides only partial support for this assumption.

The other approach assumes that the problem is with the sampling in one of the years. Variables that should be fixed or almost unchanging over time can serve as controls in regression for spurious differences across the surveys. The estimate of survey year effects conditioning on these controls would then give accurate measures of the change in survival probabilities over time. Even more than with the other method, validity depends on strong assumptions. Most importantly, the included covariates must capture the effects of all spurious changes in factors across surveys that affect mortality, an assumption that is hard to meet. In fact, adding the controls in hazard models had very little effect on the estimated survey year impacts—which we suspect is for precisely this reason. However, an alternative explanation for why the controls do not change the apparent impact of survey year is that rather than problems in sampling, the issue is one of underreporting or under recording of child deaths by interviewers.

Based on these considerations, we believe that the basic approach outlined by ORC Macro is the better of the two means of adjusting the 2003 mortality estimates. Our elaboration based on calculations from parametric survival models yields corrections that are very similar to those of ORC Macro. For under-five mortality, a range of estimates centers closely on a proportional adjustment factor of 1.33, meaning that for the specific cohorts considered (for which estimated mortality should be the same or very similar in the two surveys), the 1997 mortality rate is about 33 percent higher than that calculated from the 2003 data. Depending on the period for which ‘current’, i.e., recent, under 5 mortality is considered, the adjusted deaths per 1,000 births in

2003 ranges from 125 to 143; either figure still represents an improvement over comparable rates for the 1997 survey, if a substantially smaller one than implied by the unadjusted 2003 data. For example, for the second case, the adjusted reduction in under 5 mortality is slightly more than one third the unadjusted reduction.

For infant mortality the estimates are more variable, but the middle of our range of adjustment factors is 1.28, leading to adjusted recent infant mortality estimates of 83 per 1,000 for the eight years preceding the 2003 survey and 66 per 1,000 for the five years preceding the 2003 survey. The rates for the same intervals prior to the 1997 survey are 95 and 92 per 1,000. It should be stressed, however, that these and the under 5 estimates still rely on the accuracy of recent trends in mortality in the last survey; the only alternative to this assumption is to extrapolate the trend from the 1997 data, which essentially means ignoring the 2003 data completely.

Finally, we considered the possibility that even if incomes did not rise very much over the period between the last two surveys, the large measured reductions in mortality were real and reflect the expansion of programs to enhance child survival. In particular, the Basics/Linkages project was applied on a wide scale in Fianarantsoa and Antananarivo provinces starting in 1999. Relying on trends calculated solely from the 2003 survey to avoid comparability problems with the previous survey, the evidence suggests that mortality fell more sharply in Fianarantsoa than elsewhere, but did not fall in Antananarivo. The findings for Fianarantsoa provide (partial) support for the effectiveness of the Basics/Linkages interventions. However, in light of reductions in other provinces as well as the data discrepancies explored in this report, stepped up program activity does not explain all of the measured drop in mortality between the surveys, and it does not relieve concerns about data comparability.

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Table 1 - Distribution of recorded births and mortality by year, zero to 8 years before each survey

years before survey	<i>DHS survey round</i>					
	1992		1997		2003	
	births	share died	births	share died	births	share died
0	752	0.041	1,122	0.052	1,207	0.061
1	1,121	0.088	1,270	0.082	1,138	0.059
2	1,071	0.120	1,151	0.139	979	0.080
3	1,010	0.150	1,099	0.138	1,167	0.092
4	1,032	0.155	1,343	0.176	1,020	0.061
5	899	0.172	1,206	0.159	912	0.103
6	1,046	0.220	1,169	0.159	1,321	0.113
7	933	0.184	1,098	0.182	1,103	0.147
8	932	0.194	1,011	0.191	1,027	0.150

Note: Share died is the number of deaths by the date of the survey over the number of births in the indicated year

Table 2 - Distribution of recorded deaths, for deaths occurring at 24 months or younger

age at death (months)	<i>DHS survey round</i>		
	1992	1997	2003
0	589	663	468
1	111	112	59
2	82	118	54
3	91	112	52
4	55	78	33
5	47	48	32
6	107	124	62
7	55	66	31
8	85	108	44
9	58	72	50
10	47	40	27
11	30	50	22
12	136	100	173
13	38	50	6
14	44	40	3
15	17	17	4
16	15	25	2
17	14	13	3
18	107	115	27
19	6	9	3
20	19	18	4
21	10	4	1
22	6	3	1
23	9	13	0
24	227	234	135

Note: for children born 15 years or less before each survey

Table 3 - Estimated mortality rates by age group, cohort, and DHS survey round

<i>Birth Cohort</i>	Mortality per 1,000 births:								
	Neonatal	Post-neonatal	Under 9 mos.	Infant	Under 15 mos.	Child	Under 5	Under 6	Under 7
	2003								
1999-2003	32	26	53	57	65	37	92	93	95
1994-1998	37	46	75	82	101	53	131	137	143
1989-1993	40	37	67	76	91	55	127	137	142
1984-1988	34	41	70	76	97	70	141	148	155
1979-1983	27	58	77	86	104	59	140	160	171
	1997								
1993-1997	40	55	86	96	110	68	157	163	167
1988-1992	41	62	93	103	114	75	171	178	181
1983-1987	46	71	105	117	134	99	204	212	218
1978-1982	45	56	88	102	119	85	178	185	189
1973-1977	34	61	83	95	113	90	177	185	196
	1992								
1988-1992	39	53	83	92	107	76	161	166	170
1983-1987	48	66	103	114	134	92	195	204	209
1978-1982	42	62	92	104	122	87	182	187	194
1973-1977	34	58	82	91	108	86	170	173	177
1968-1972	40	64	90	104	116	74	171	176	181

Table 4 - mean respondent characteristics by birth cohort and survey

Survey year	Any schooling <i>cohort:</i>				Some secondary school or higher <i>cohort:</i>				Completed primary <i>cohort:</i>				Completed secondary <i>cohort:</i>			
	1953-57	1958-62	1963-67	1968-72	1953-57	1958-62	1963-67	1968-72	1953-57	1958-62	1963-67	1968-72	1953-57	1958-62	1963-67	1968-72
1992	0.764	0.787	0.829	0.872	0.191	0.242	0.357	0.338	0.027	0.027	0.025	0.030	0.038	0.050	0.050	0.026
1997	0.766	0.750	0.784	0.827	0.185	0.239	0.359	0.328	0.039	0.041	0.027	0.041	0.038	0.035	0.048	0.037
2003	0.740	0.729	0.767	0.833	0.227	0.258	0.345	0.368	0.157	0.144	0.128	0.167	0.074	0.073	0.120	0.091
Observations	1,894	2,567	2,962	3,552	1,894	2,567	2,962	3,552	1,894	2,567	2,962	3,552	1,894	2,567	2,962	3,552
Test of differences in means across surveys (p-values)																
1992, 1997	0.95	0.23	0.10	0.05	0.84	0.93	0.97	0.77	0.29	0.08	0.85	0.21	0.99	0.26	0.90	0.23
1997, 2003	0.55	0.57	0.59	0.83	0.24	0.59	0.73	0.22	0.00	0.00	0.00	0.00	0.04	0.02	0.00	0.00

Table 4 continued - mean respondent characteristics by birth cohort and survey

Survey year	Age at first marriage <i>cohort:</i>				Height (cm) <i>cohort:</i>				Partner years of education <i>cohort:</i>				Partner completed primary school <i>cohort:</i>			
	1953-57	1958-62	1963-67	1968-72	1953-57	1958-62	1963-67	1968-72	1953-57	1958-62	1963-67	1968-72	1953-57	1958-62	1963-67	1968-72
1992	18.49	18.26	18.16	17.23												
1997	18.58	18.30	18.55	18.07	152.40	153.37	153.80	152.79	3.84	4.10	4.55	4.34	0.297	0.321	0.377	0.383
2003	19.53	19.38	19.71	19.11	153.44	155.01	154.08	154.05	5.04	5.10	5.15	5.39	0.546	0.491	0.515	0.522
Observations	1,825	2,444	2,680	2,866	625	1,128	1,439	1,807	1,528	2,084	2,370	2,575	1,528	2,084	2,370	2,575
Test of differences in means across surveys (p-values)																
1992, 1997	0.77	0.87	0.11	0.00	--	--	--	--	--	--	--	--	--	--	--	--
1997, 2003	0.02	0.00	0.00	0.00	0.30	0.00	0.50	0.00	0.00	0.01	0.11	0.00	0.00	0.00	0.00	0.00

Table 5 - Mean respondent and household characteristics by survey year

<i>Survey year</i>	married	household size	number of children	log asset index ^a	electricity	pipel water	well water	latrine/ flush toilet	Province = tana	fian	toam	maha	toli	ants	catholic	protestant
1992	0.60	6.55	1.27	0.39	0.126	0.222	0.153	0.426	0.32	0.23	0.13	0.13	0.12	0.08	0.38	0.40
1997	0.63	5.98	1.25	0.42	0.140	0.204	0.224	0.432	0.34	0.20	0.14	0.12	0.12	0.07	0.35	0.40
2003	0.65	5.58	1.07	0.59	0.224	0.269	0.215	0.567	0.34	0.20	0.15	0.12	0.12	0.07	0.39	0.38
Test of differences in means across surveys (p-values):																
1992, 1997	0.01	0.00	0.48	0.17	0.57	0.59	0.04	0.87	0.80	0.69	0.70	0.86	0.86	0.80	0.35	0.94
1997, 2003	0.20	0.00	0.00	0.00	0.01	0.08	0.81	0.00	0.97	0.97	0.84	0.91	0.91	0.85	0.12	0.34

^a asset index is calculated from information on durable goods using factor analysis. See Stifel and Sahn (2003) for details.

Table 6 - Mean respondent characteristics by province and survey, women born in 1953-62

		Age at first marriage					Height (cm)						Partner years of education						Partner completed primary							
Survey year		Tana	Fian	Toam	Maha	Toli	Ants	Tana	Fian	Toam	Maha	Toli	Ants	Tana	Fian	Toam	Maha	Toli	Ants	Tana	Fian	Toam	Maha	Toli	Ants	
1992		19.22	17.98	19.82	17.19	16.61	18.18																			
1997		19.19	18.10	19.31	17.60	16.48	18.00	151.97	153.56	152.22	154.36	153.61	155.56	5.24	3.42	3.49	3.74	2.13	3.87	0.41	0.25	0.27	0.31	0.18	0.30	
2003		20.71	19.29	19.47	18.49	17.63	18.38	154.54	152.90	153.41	155.06	156.38	156.16	6.80	4.64	4.26	3.73	4.01	3.93	0.67	0.50	0.42	0.34	0.44	0.38	
Observations		1520	775	573	500	445	455	603	308	271	192	203	177	1304	649	498	429	371	360	1304	649	498	429	371	360	
Test of differences in means across surveys (p-values)																										
1992, 1997		0.93	0.82	0.25	0.45	0.85	0.77	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	
1997, 2003		0.00	0.08	0.77	0.33	0.15	0.51	0.00	0.51	0.24	0.53	0.28	0.53	0.01	0.07	0.36	0.98	0.08	0.90	0.00	0.00	0.10	0.73	0.01	0.21	
Differences in means across surveys																										
1997-1992		-0.03	0.11	-0.51	0.41	-0.13	-0.18	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	
2003-1997		1.52	1.19	0.16	0.89	1.14	0.38	2.57	-0.66	1.19	0.70	2.77	0.60	1.56	1.22	0.77	-0.02	1.88	0.07	0.27	0.25	0.14	0.03	0.26	0.08	

Table 7 - Mean respondent characteristics by province and survey, women born in 1963-70

		Age at first marriage					Height (cm)						Partner years of education						Partner completed primary							
Survey year		Tana	Fian	Toam	Maha	Toli	Ants	Tana	Fian	Toam	Maha	Toli	Ants	Tana	Fian	Toam	Maha	Toli	Ants	Tana	Fian	Toam	Maha	Toli	Ants	
1992		18.72	17.80	18.56	16.82	17.21	17.83																			
1997		19.72	17.85	19.56	17.19	16.36	18.43	152.67	153.24	153.07	153.29	154.47	154.34	6.02	3.75	4.59	3.99	3.00	4.01	0.53	0.28	0.41	0.33	0.29	0.33	
2003		20.84	18.92	19.38	18.44	18.16	19.14	154.11	154.60	153.19	154.43	155.62	153.40	6.73	4.78	4.21	4.67	3.94	4.98	0.66	0.45	0.42	0.43	0.39	0.48	
Observations		1,528	784	559	578	517	426	883	422	326	336	279	224	1,352	707	495	514	468	360	1,352	707	495	514	468	360	
Test of differences in means across surveys (p-values)																										
1992, 1997		0.00	0.92	0.01	0.39	0.19	0.17	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	
1997, 2003		0.03	0.04	0.75	0.06	0.03	0.33	0.00	0.04	0.91	0.20	0.20	0.50	0.27	0.12	0.59	0.39	0.30	0.09	0.03	0.03	0.86	0.23	0.28	0.05	
Differences in means across surveys																										
1997-1992		1.01	0.05	1.00	0.37	-0.84	0.61	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	
2003-1997		1.12	1.07	-0.18	1.26	1.80	0.71	1.44	1.37	0.12	1.14	1.16	-0.94	0.71	1.04	-0.38	0.68	0.95	0.97	0.14	0.17	0.02	0.10	0.10	0.15	

Table 8 - 2003 DHS: interviewer impacts on the probability of a reported death and on reported number of children ever born

interviewer id codes	Dependent variable: =1 if child died, 0 otherwise ^a		Dependent variable: children ever born ^b	
	including province controls	including cluster controls	including province controls	including cluster controls
intridcd== 3013	-0.038 [2.17]**	-0.035 [0.57]	-0.946 [3.74]***	-0.709 [0.52]
intridcd== 3014	-0.026 [1.45]	-0.017 [0.23]	-1.203 [4.79]***	-1.002 [0.74]
intridcd== 3015	-0.031 [1.86]*	-0.034 [0.62]	-1.317 [5.54]***	-1.022 [0.74]
intridcd== 3018	-0.007 [0.41]	0.01 [0.20]	-0.48 [1.96]**	-0.243 [0.20]
intridcd== 3019	-0.007 [0.41]	0.000 [0.01]	-0.735 [2.94]***	-0.582 [0.48]
intridcd== 3020	-0.008 [0.46]	-0.002 [0.05]	-0.84 [3.35]***	-0.689 [0.58]
intridcd== 3023	-0.111 [5.35]***	-0.077 [1.52]	-0.387 [1.22]	-0.274 [0.21]
intridcd== 3024	-0.012 [0.59]	0.017 [0.36]	-0.736 [2.42]**	-0.493 [0.38]
intridcd== 3025	0.007 [0.35]	0.032 [0.67]	-0.366 [1.19]	-0.208 [0.16]
intridcd== 3028	-0.011 [0.60]	0.039 [0.76]	-0.895 [3.43]***	-0.285 [0.31]
intridcd== 3029	-0.042 [1.75]*	-0.03 [0.50]	-0.831 [2.31]**	-0.761 [0.78]
intridcd== 3030	-0.033 [1.80]*	0.008 [0.16]	-0.961 [3.54]***	-0.359 [0.38]
intridcd== 3033	-0.078 [4.76]***	-0.046 [1.01]	-0.784 [3.32]***	-0.165 [0.15]
intridcd== 3034	-0.05 [3.31]***	-0.01 [0.22]	-0.578 [2.57]**	0.17 [0.16]
intridcd== 3035	-0.043 [2.75]***	-0.004 [0.09]	-0.787 [3.47]***	-0.076 [0.07]
intridcd== 3038	0.006 [0.31]	0.031 [0.57]	-0.503 [1.79]*	0.149 [0.11]
intridcd== 3039	-0.059 [3.21]***	-0.032 [0.60]	-0.753 [2.80]***	-0.122 [0.09]
intridcd== 3040	0.008 [0.46]	0.034 [0.64]	-0.281 [1.06]	0.39 [0.29]
intridcd== 3043	-0.118 [6.30]***	-0.032 [0.42]	-1.497 [5.46]***	-0.44 [0.30]
intridcd== 3044	-0.128 [7.09]***	-0.047 [0.67]	-1.381 [5.12]***	-0.248 [0.17]
intridcd== 3045	-0.093 [5.01]***	-0.01 [0.13]	-1.347 [4.95]***	-0.32 [0.22]
intridcd== 3048	0.014 [0.79]	0.001 [0.03]	-0.766 [2.96]***	-0.225 [0.21]
intridcd== 3049	-0.026 [1.39]	-0.034 [0.70]	-0.894 [3.31]***	-0.32 [0.29]
intridcd== 3050	-0.054 [3.00]***	-0.064 [1.29]	-0.996 [3.77]***	-0.376 [0.35]
intridcd== 3053	-0.011 [0.82]	-0.006 [0.44]	-0.273 [1.27]	-0.106 [0.52]

Continued

Table 8 - continued

interviewer id codes	Dependent variable: =1 if child died, 0 otherwise ^a		Dependent variable: children ever born ^b	
	including province controls	including cluster controls	including province controls	including cluster controls
intridcd== 3054	0.001 [0.05]	0.006 [0.36]	-0.185 [0.82]	-0.134 [0.61]
intridcd== 3058	0.007 [0.43]	-0.014 [0.24]	0.111 [0.43]	-0.408 [0.39]
intridcd== 3059	-0.001 [0.08]	-0.018 [0.34]	-0.116 [0.47]	-0.661 [0.65]
intridcd== 3060	-0.06 [3.61]***	-0.074 [1.45]	0.025 [0.10]	-0.505 [0.50]
intridcd== 3063	-0.03 [1.83]*	0.003 [0.06]	-0.651 [2.70]***	0.828 [0.75]
intridcd== 3064	-0.024 [1.48]	0.011 [0.22]	-0.725 [3.02]***	0.752 [0.68]
intridcd== 3065	-0.04 [2.41]**	-0.006 [0.13]	-0.411 [1.67]*	1.055 [0.95]
Observations	22116	22116	7723	7723

t statistics in brackets * significant at 10%; ** significant at 5%; *** significant at 1%

^a sample is all children ever born

^b sample is all women

percentiles of interviewer
effects

0.05	-0.114	-0.069	-1.362	-0.869
0.10	-0.092	-0.047	-1.306	-0.707
0.25	-0.051	-0.033	-0.908	-0.496
0.50	-0.028	-0.008	-0.745	-0.280
0.75	-0.007	0.007	-0.405	-0.118
0.90	0.007	0.030	-0.194	0.368
0.95	0.007	0.033	-0.053	0.786
range .05-.95:	0.122	0.101	1.310	1.656
range .25-.75:	0.098	0.077	1.112	1.075

percentiles of interviewer
effects - 1997 DHS

0.05	-0.036	-0.022	-1.953	-1.440
0.10	-0.034	-0.015	-1.765	-0.831
0.25	-0.020	0.000	-1.571	-0.213
0.50	-0.002	0.026	-1.377	0.051
0.75	0.010	0.053	-0.862	0.263
0.90	0.024	0.084	-0.361	1.205
0.95	0.031	0.101	-0.188	1.380
range .05-.95:	0.067	0.124	1.764	2.820
range .25-.75:	0.058	0.099	1.403	2.036

Table 9 - Adjusted under-5 mortality rates for 2003 DHS

	(1)	(2)	(3)
	Period for calculating adjustment factor		
	1986-1995	1981-1995	1991-1995
proportional adjustment factor	1.350	1.371	1.334
<i>mortality (per 1000) for 8 years preceding survey:</i>			
1997 DHS	164.1	164.1	164.1
2003 DHS	105.6	105.6	105.6
2003 DHS adjusted	142.6	144.7	140.9
<i>mortality (per 1000) for 5 years preceding survey:</i>			
1997 DHS	167.1	167.1	167.1
2003 DHS	94.2	94.2	94.2
2003 DHS adjusted	127.2	129.1	125.7

Notes:

Based on hazard model estimates

Table 10 - Adjusted infant mortality rates for 2003 DHS

	(1)	(2)	(3)
	Period for calculating adjustment factor		
	1986-1995	1981-1995	1991-1995
proportional adjustment factor	1.279	1.304	1.160
<i>mortality (per 1000) for 8 years preceding survey:</i>			
1997 DHS	94.8	94.8	94.8
2003 DHS	51.7	51.7	51.7
2003 DHS adjusted	83.0	84.6	75.3
<i>mortality (per 1000) for 5 years preceding survey:</i>			
1997 DHS	92.2	92.2	92.2
2003 DHS	51.7	51.7	51.7
2003 DHS adjusted	66.2	67.5	60.0

Notes:

Based on hazard model estimates

Table 11 - Weibull model estimates for under 6 mortality in the eight years preceding each survey

	(1)	(2)	(3)	(4)	(5)	(6)
	survey year only	rural: survey year only	urban: survey year only	add rural and province dummies	add Firaiana dummies	Add other 'permanent' covariates ^a
Survey year coefficients (base year is 1997):						
1992	0.08 [1.24]	0.057 [0.81]	0.059 [0.51]	0.058 [0.95]	0.213 [4.38]***	0.071 [1.22]
2003	-0.467 [4.97]***	-0.453 [4.30]***	-0.624 [5.45]***	-0.485 [5.32]***	-0.506 [8.43]***	-0.492 [4.80]***
No. of observations	26,408	17,417	8,991	26,408	25,602	26,379
predicted under-5 mortality probabilities:						
1992	0.176	0.183	0.135	0.172	0.177	0.149
1997	0.164	0.173	0.128	0.163	0.146	0.139
2003	0.106	0.114	0.071	0.104	0.091	0.088
2003 - 1997	-0.058	-0.059	-0.057	-0.059	-0.055	-0.051

Note: calculations of under 5 mortality probabilities are at pooled (all survey) sample means of the indicated covariates, so show the survey effect after removing the effects of differences in the covariates

Robust z statistics in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

^a marital status, age at 1st marriage, household size, respondent education, partner education, religion

Table 12 - Weibull model estimates for under 6 mortality, including survey year*province interactions

	(1)	(2)	(3)
	Including survey*province interactions	rural: including survey*province interactions	urban: including survey*province interactions
survey_92	0.214 [1.74]*	0.178 [1.38]	0.227 [0.67]
survey_03	-0.37 [2.06]**	-0.371 [1.93]*	-0.479 [1.55]
tana	-0.312 [2.11]**	-0.355 [2.13]**	-0.078 [0.25]
fian	0.174 [1.29]	0.186 [1.34]	0.039 [0.11]
maha	0.026 [0.20]	0.015 [0.10]	0.032 [0.11]
toli	-0.033 [0.22]	-0.066 [0.39]	0.168 [0.59]
ants	-0.293 [2.01]**	-0.301 [1.98]**	-0.335 [0.91]
survey92*tana	-0.075 [0.39]	-0.054 [0.25]	-0.053 [0.13]
survey92*fian	-0.28 [1.59]	-0.301 [1.64]	-0.116 [0.27]
survey92*maha	-0.106 [0.56]	-0.086 [0.43]	-0.772 [1.97]**
survey92*toli	-0.061 [0.32]	-0.042 [0.19]	-0.116 [0.29]
survey92*ants	-0.191 [0.83]	-0.124 [0.49]	-0.144 [0.31]
survey03*tana	-0.144	-0.058 [0.15]	-0.387 [1.00]
survey03*fian	-0.645 [2.42]**	-0.713 [2.43]**	-0.146 [0.35]
survey03*maha	0.016 [0.07]	0.052 [0.21]	-0.382 [1.04]
survey03*toli	0.237 [0.94]	0.269 [0.94]	0.17 [0.48]
survey03*ants	0.367 [1.38]	0.358 [1.28]	0.233 [0.53]
Constant	-3.614 [35.27]***	-3.553 [32.82]***	-3.892 [15.21]***
Observations	26408	17417	8991

Robust z statistics in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

Excluded province is Toamasina. Excluded survey year is 1997.

Figure 1 - Under-twelve-month (infant) mortality estimates

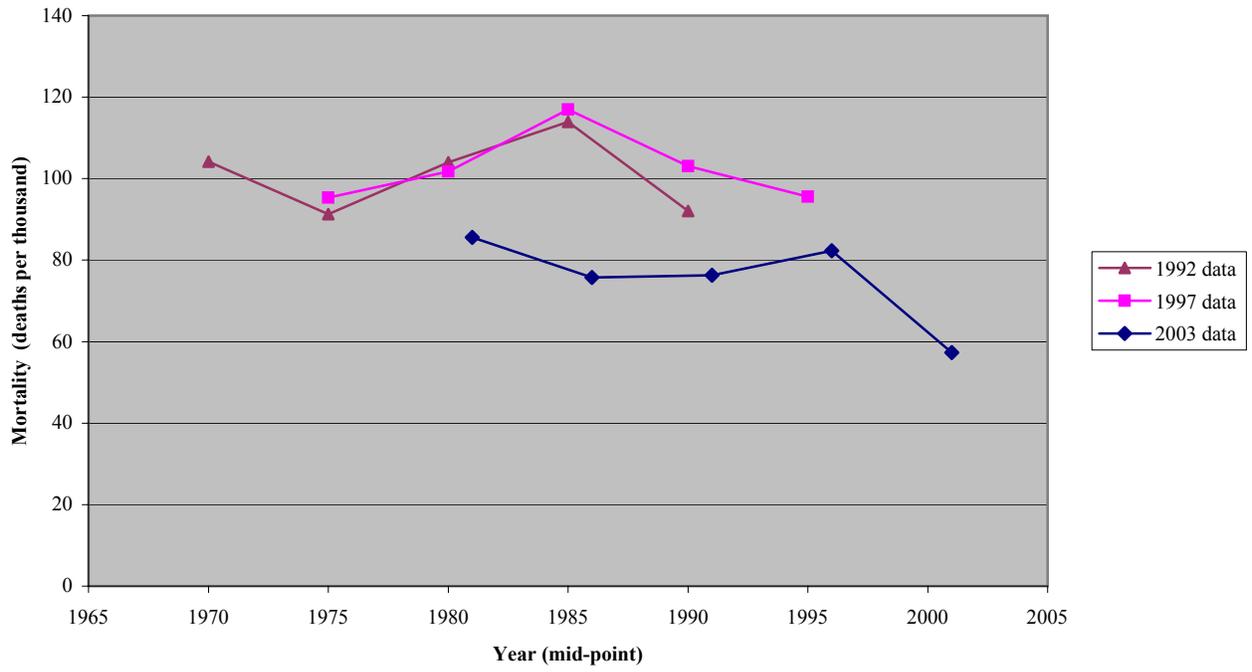


Figure 2 - Under 9 month mortality estimates

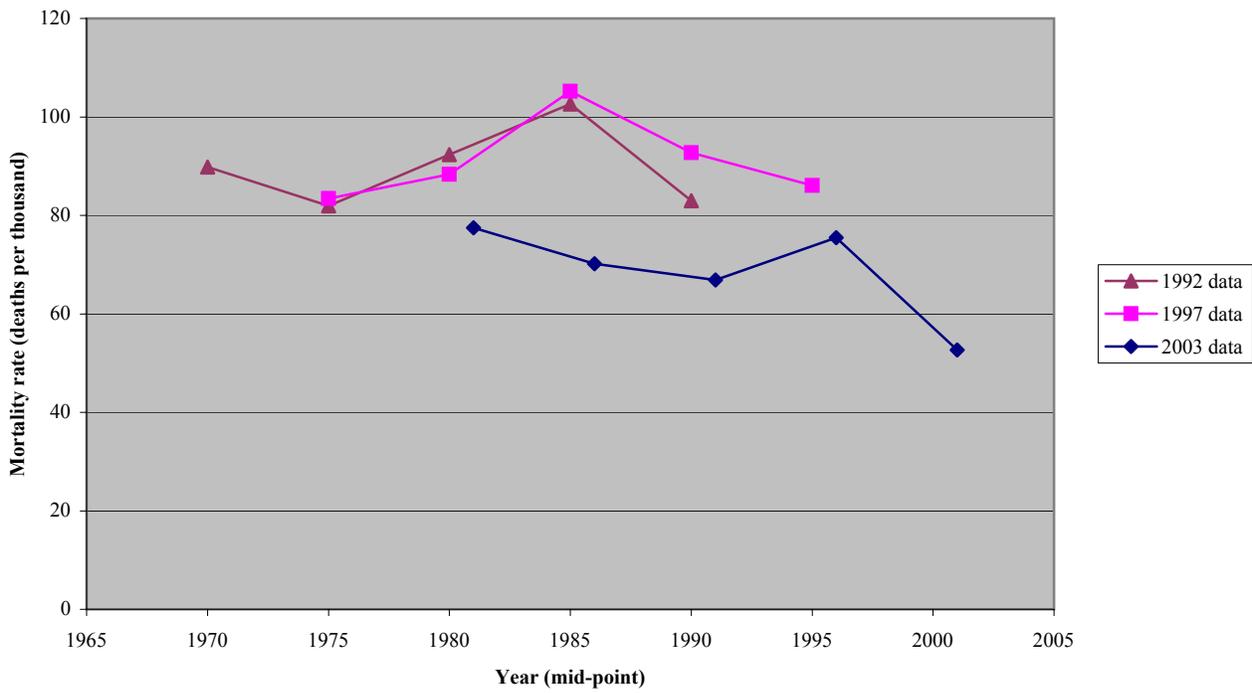


Figure 3 - Under 15 month mortality estimates

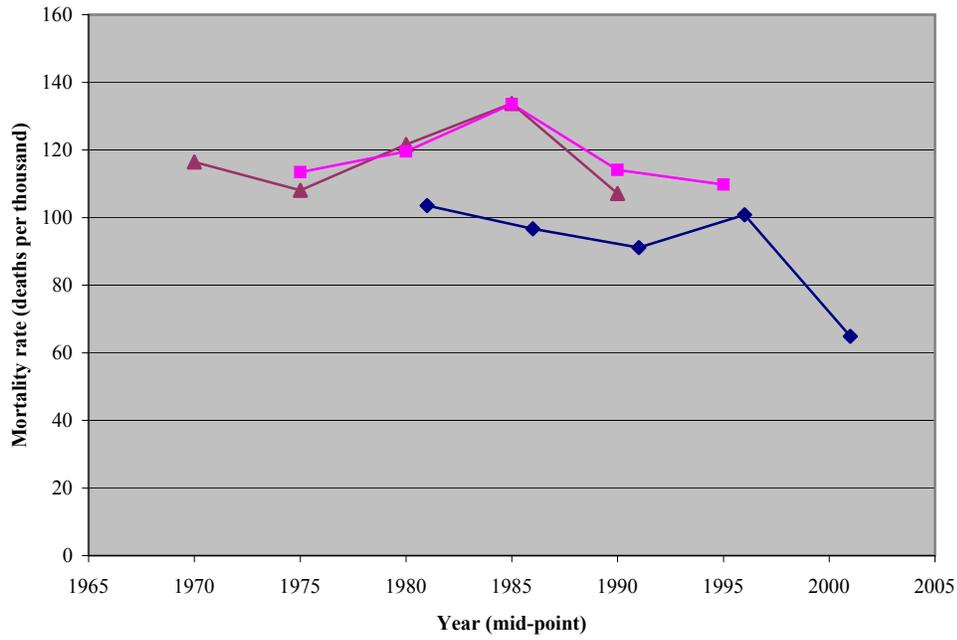


Figure 4 - Under-5 mortality estimates

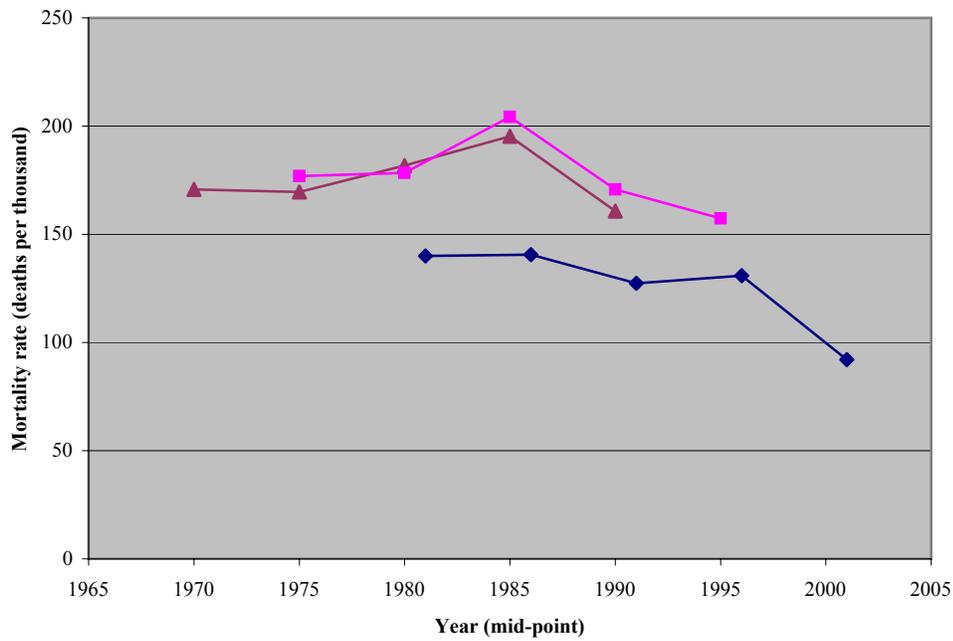


Figure 5 - Under 6 mortality estimates

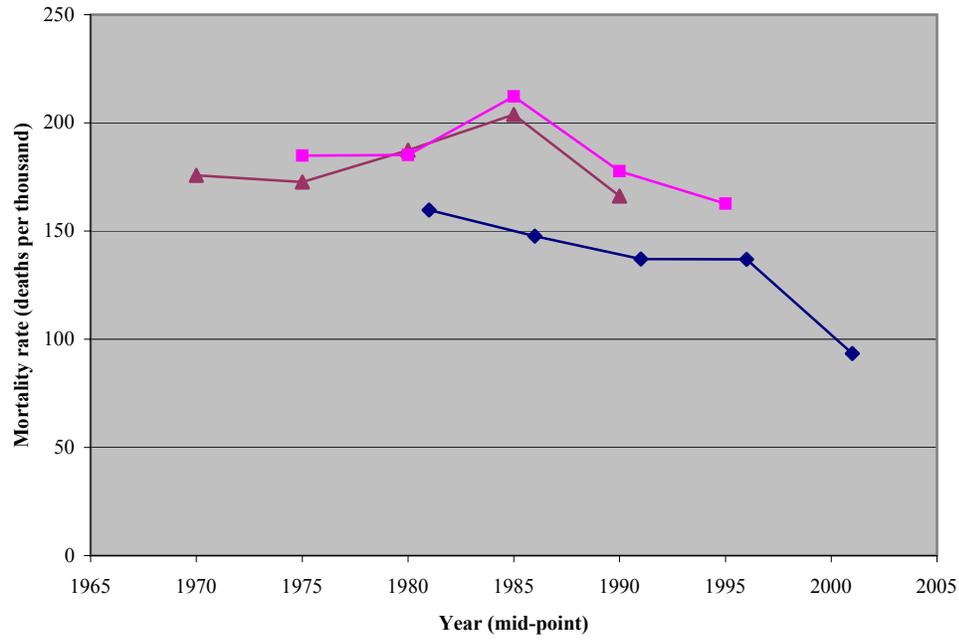


Figure 6 -- Cumulate share of reported deaths by age of death

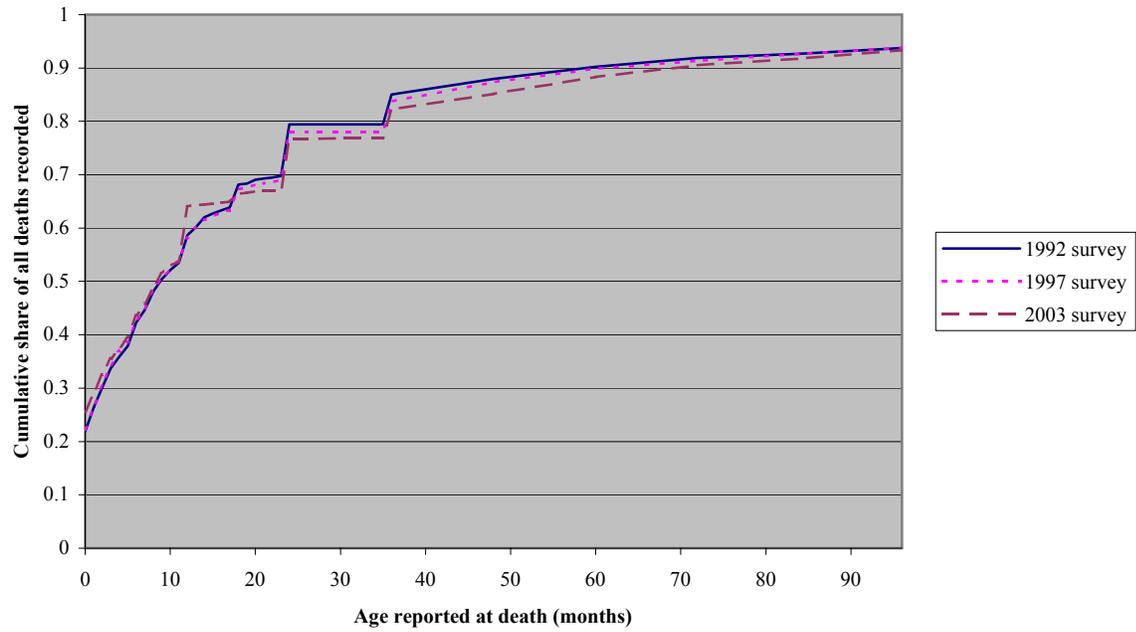


Figure 7 - Cumulative share of deaths over cumulative births by age at death

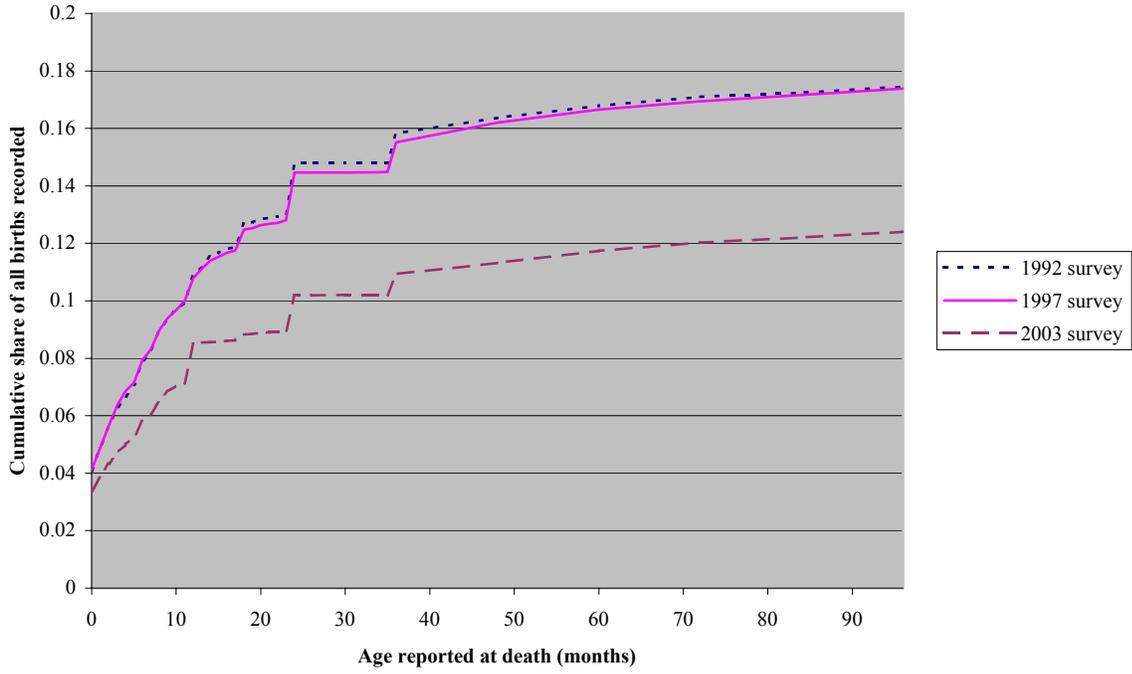


Figure 8 - Under-5 mortality estimates: rural areas

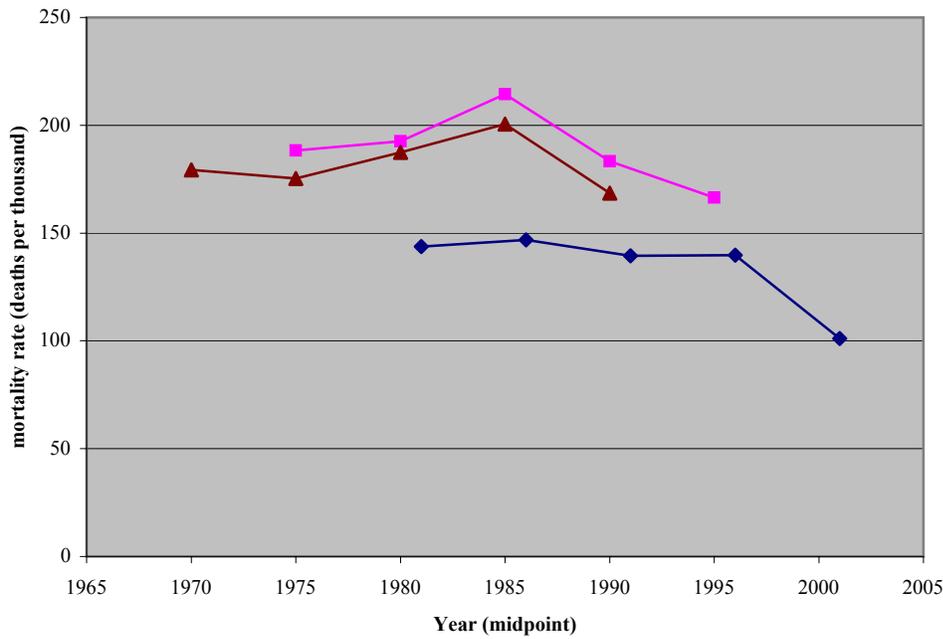


Figure 9 - Under-5 mortality estimates: urban areas

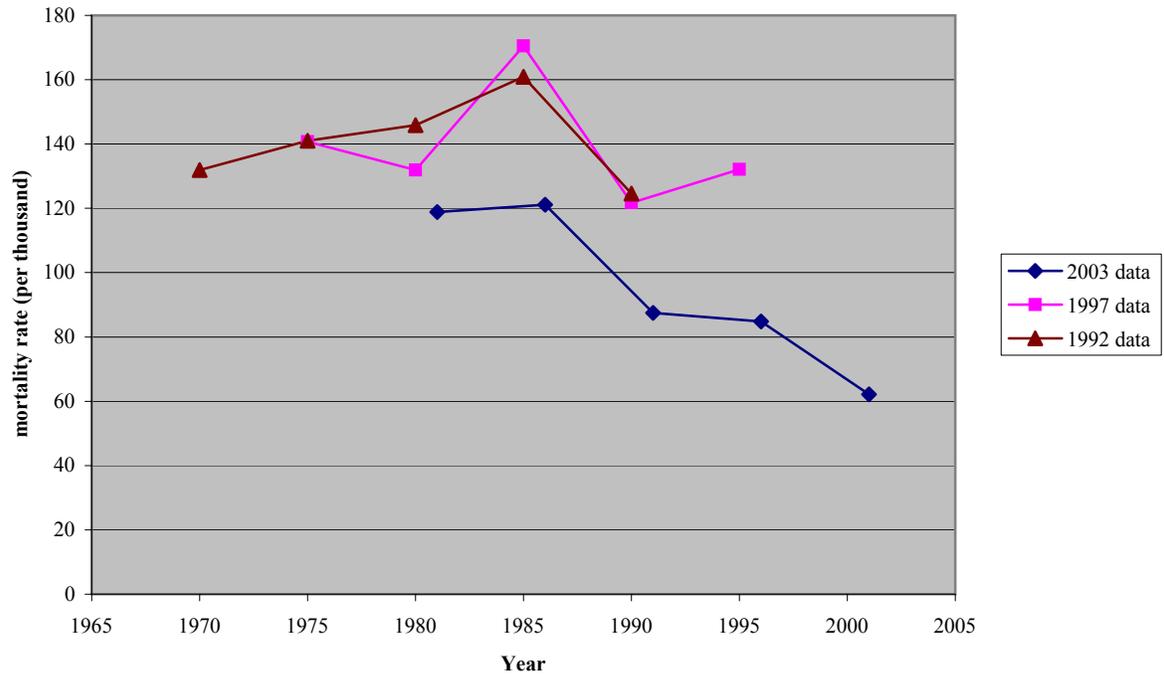


Figure 10 - Under 5 mortality estimates: Antananarivo province

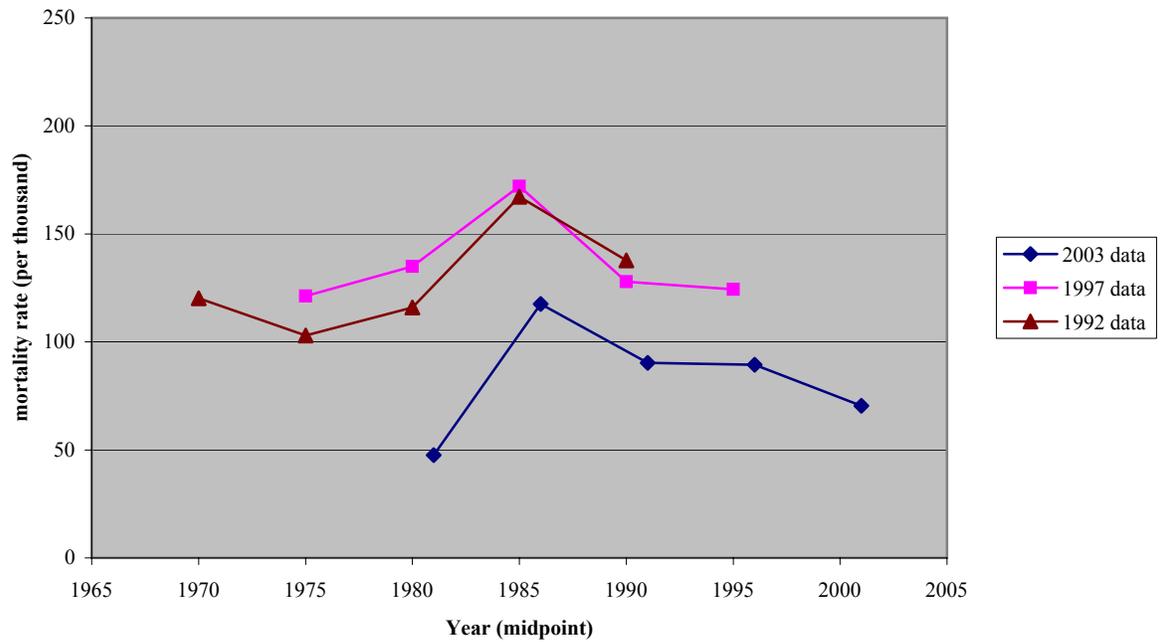


Figure 11 - Under 5 mortality estimates: Fianarantsoa Province

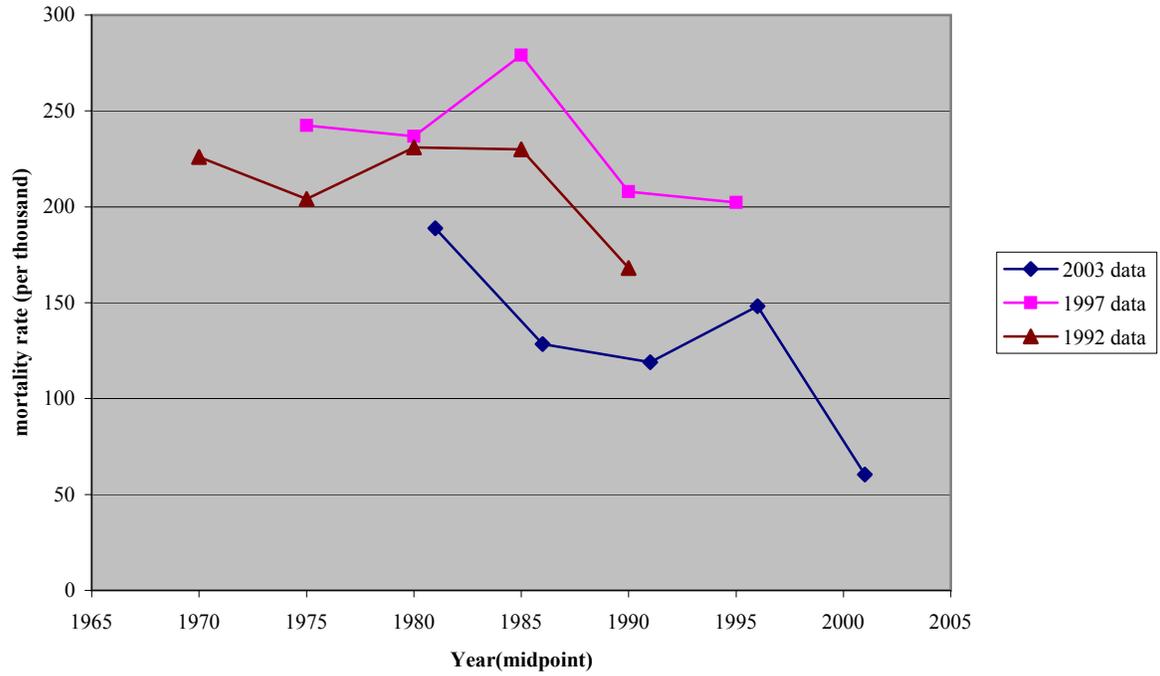


Figure 12 - Under 5 mortality estimates: Toamasina province

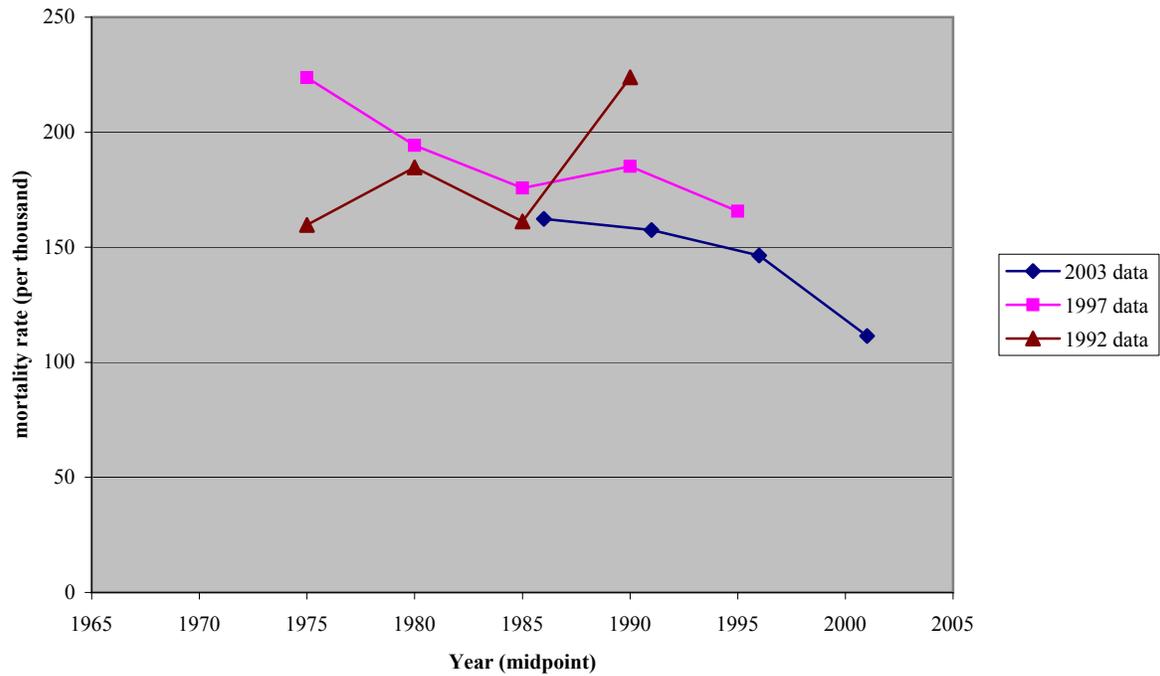


Figure 13 - Under 5 mortality estimates: Mahajanga province

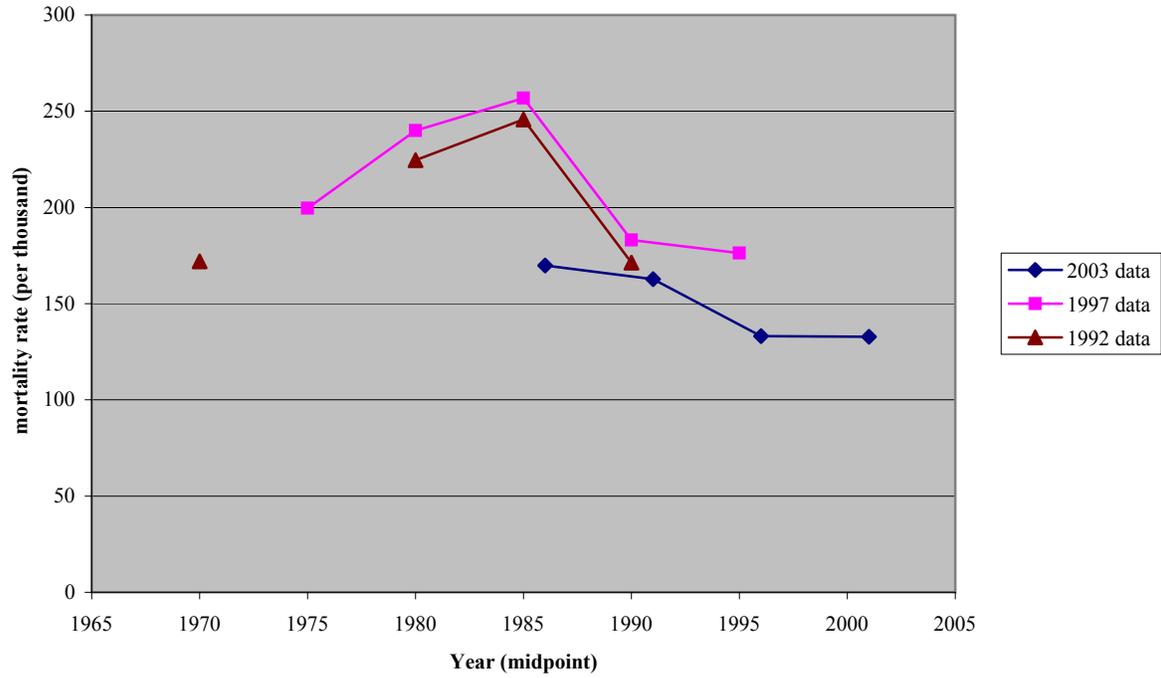


Figure 14 - Under 5 mortality estimates: Toliary province

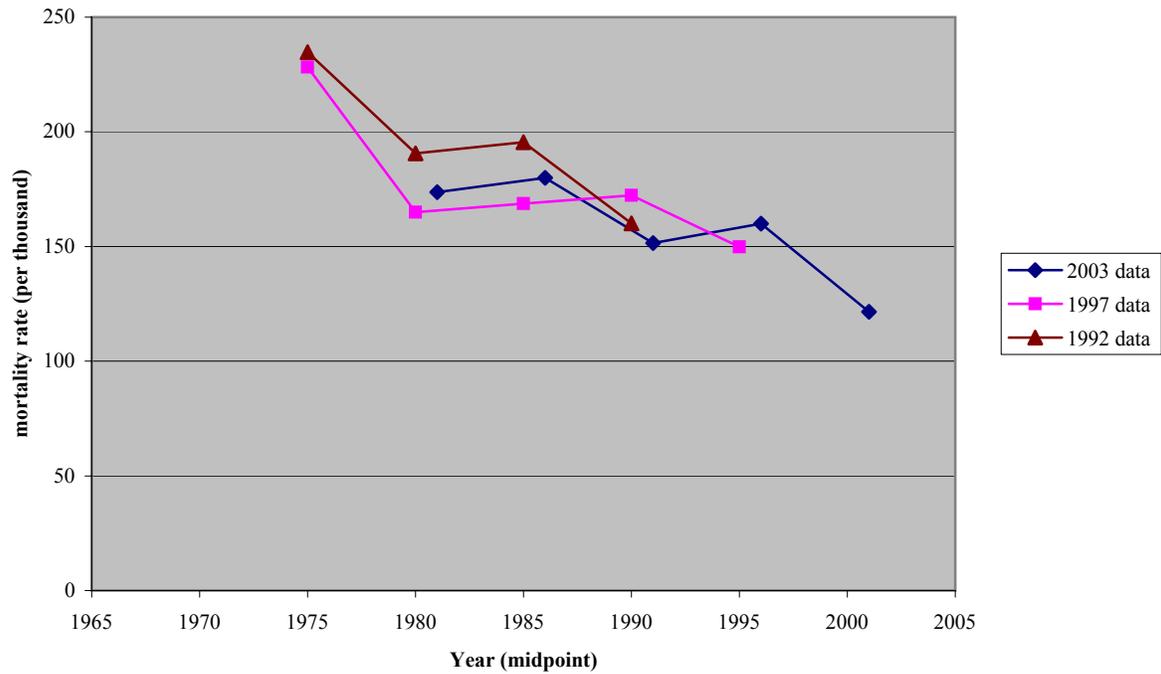


Figure 15- Under 5 mortality estimates: Antsiranana province

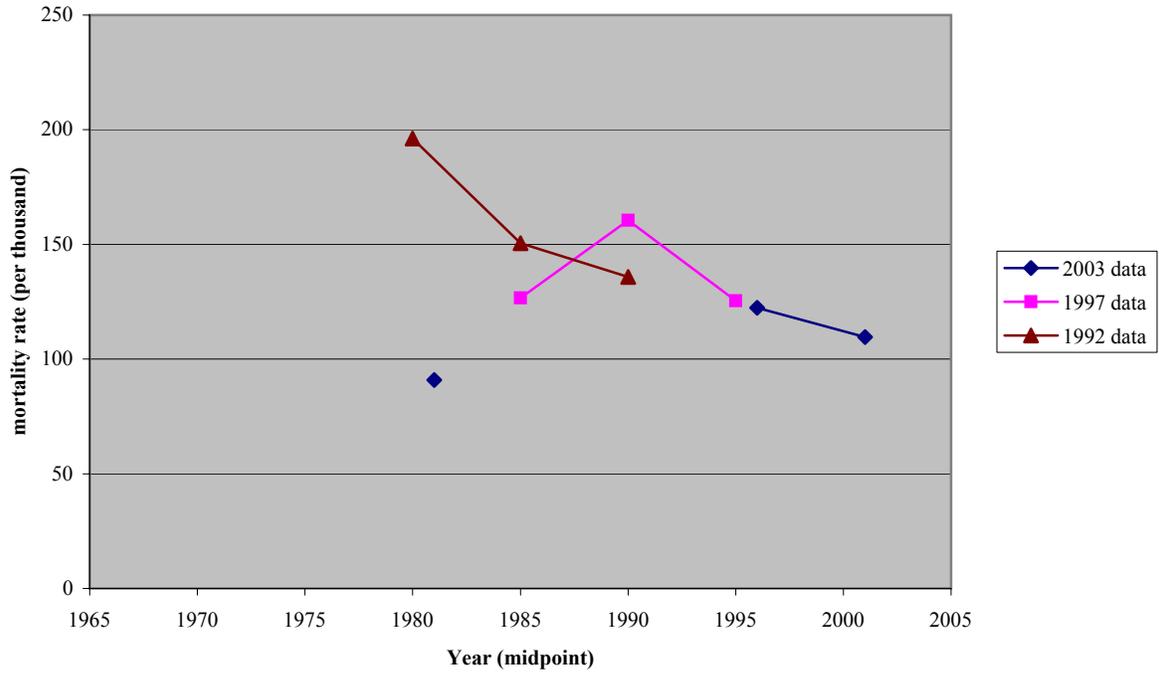


Figure 16 - Under-5 mortality rates based on hazard model estimates (cubic time trend)

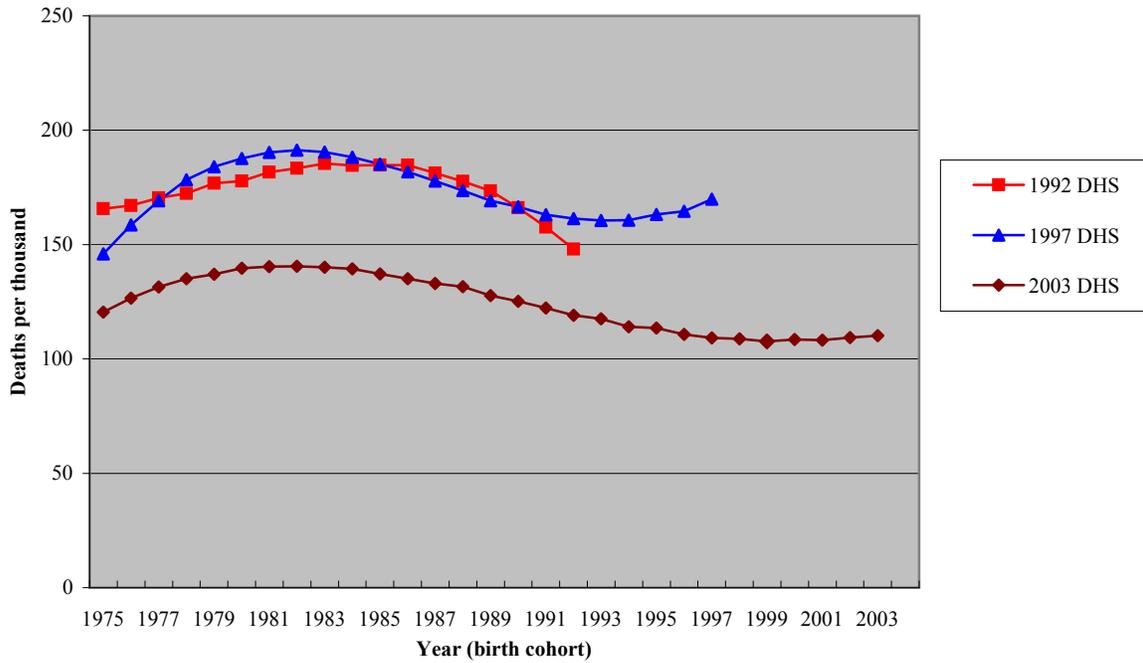


Figure 17 - Under-five mortality rates based on hazard model estimates (birth year cohort dummies)

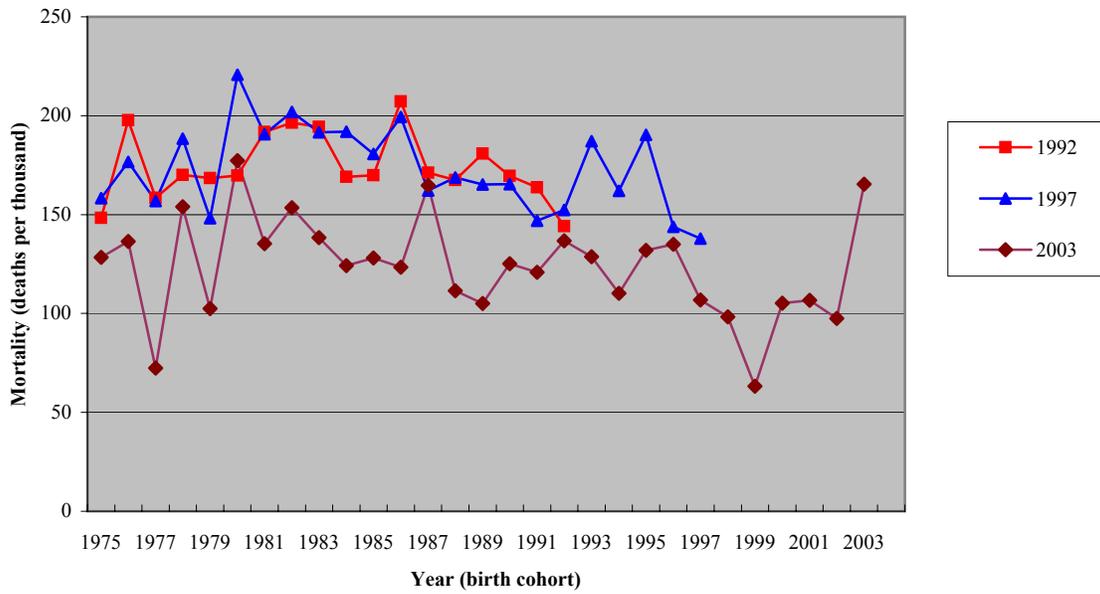


Figure 18 - Infant mortality rates based on hazard model estimates (cubic time trend)

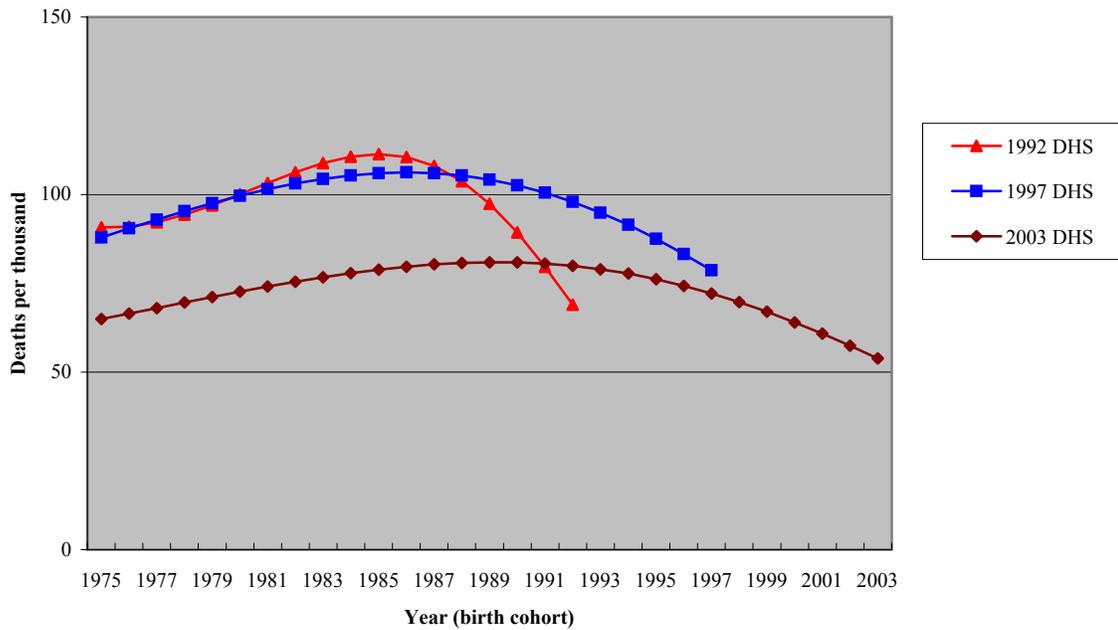


Figure 19 - Infant mortality rates based on hazard model estimates (birth year cohort dummies)

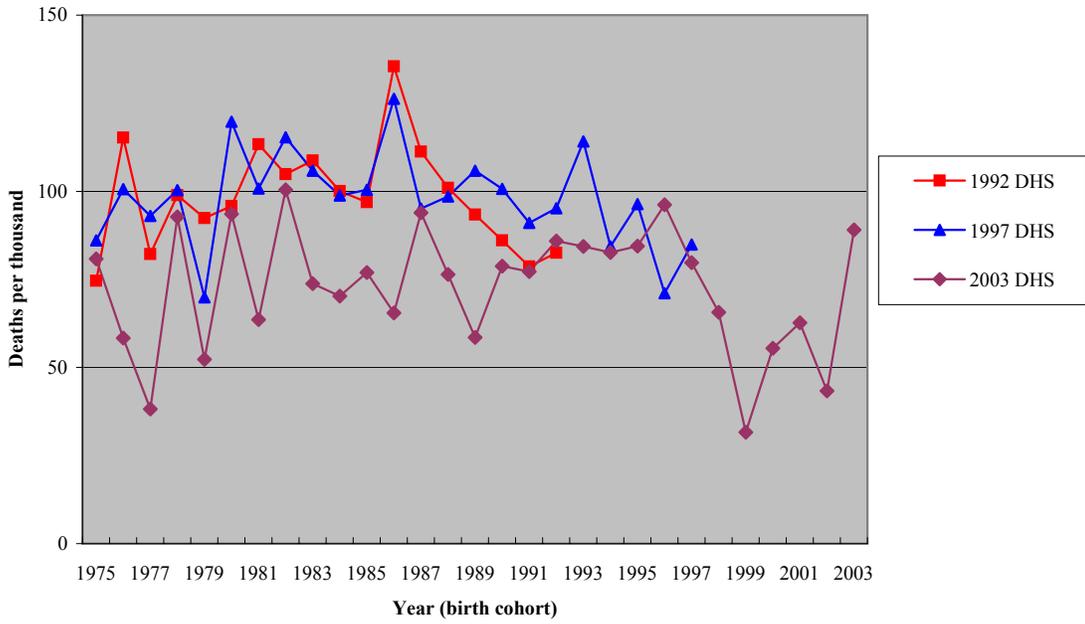


Fig 20- Under-5 mortality rates by province based on hazard model estimates (quadratic time trend, using 2003 DHS)

