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AN ASSESSMENT OF AGE AND DATE REPORTING IN THE DHS SURVEYS, 1985-2003

DHS METHODOLOGICAL REPORTS 5

DECEMBER 2006

This publication was produced for review by the United States Agency for International Development. Thomas W. Pullum of The University of Texas at Austin.

The MEASURE DHS project assists countries worldwide in the collection and use of data to monitor and evaluate population, health, nutrition, and HIV/AIDS programs. Funded by the United States Agency for International Development (USAID) under Contract No. GPO-C-00-03-00002-00, MEASURE DHS is implemented by Macro International Inc. in Calverton, Maryland.

The main objectives of the MEASURE DHS project are:

- To provide decisionmakers in survey countries with information useful for informed policy choices;
- To expand the international population and health database;
- To advance survey methodology; and
- To develop in participating countries the skills and resources necessary to conduct high-quality demographic and health surveys.

Additional information about the MEASURE DHS project is available on the Internet at http://www.measuredhs.com or by contacting Macro International Inc., MEASURE DHS, 11785 Beltsville Drive, Suite 300, Calverton, MD 20705 USA; Telephone: 301-572-0200, Fax: 301-572-0999, E-mail: reports@orcmacro.com.

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The author's views expressed in this publication do not necessarily reflect the views of the United States Agency for International Development or the United States Government.

Editor: Sidney Moore
Document Production: Kaye Mitchell

This publication was made possible through support provided by the U.S. Agency for International Development under the terms of Contract No. GPO-C-00-03-00002-00.

Recommended citation:

Pullum, Thomas W. 2006. *An Assessment of Age and Date Reporting in the DHS Surveys, 1985-2003.* Methodological Reports No. 5. Calverton, Maryland: Macro International Inc.

Contents

Preface

The Demographic and Health Surveys (DHS) program has become one of the principal sources of international data on fertility, family planning, maternal and child health, nutrition, mortality, and HIV/AIDS. The quality of this data is of utmost importance to researchers worldwide.

Because survey methodology has a major impact on data quality, one of the objectives of the MEASURE DHS project is to advance the methodology and procedures used to carry out national-level surveys. This will improve the accuracy and depth of information relied on by policymakers and program managers in developing countries.

The topics in the *DHS Methodological Reports* series are selected by MEASURE DHS staff in consultation with the U.S. Agency for International Development. While data quality is a main topic of the reports, they also examine issues of sampling, questionnaire comparability, survey procedures, and methodological approaches. Some reports are updates of previously published reports.

This report deals with the quality of age and date reporting in DHS surveys. Most of the indicators produced by DHS surveys depend on accurate reporting of ages of women and children, as well as dates when events occurred. From 1985 to 2003, 141 DHS surveys of households and women were carried out in 66 countries. These data are examined here.

It is hoped that the *DHS Methodological Reports* series will be useful to researchers and survey specialists, particularly those engaged in work in developing countries.

Martin Vaessen
Project Director

Acknowledgments

Several DHS staff members provided valuable assistance during the preparation of this report. Special thanks go to Fred Arnold, Bridgette James, Sidney Moore, Shea Rutstein, and Jerry Sullivan. Khatuna Doliashvili helped during the early stages of data preparation.

Executive Summary

Complete and accurate reports of dates and ages are critically important for DHS surveys. Eligibility for inclusion in the survey of women age 15-49, as well as most surveys of men and special surveys, depends on the age given in the household survey. Eligibility of children for questions on health depends on the birth dates given in the birth histories. Both the numerators and the denominators of age-specific fertility rates, infant mortality rates, and other rates depend on reported age. In addition, the quality of the reports of ages and dates reflects on the quality of other information in the surveys.

This assessment is based on virtually all of the surveys of households and surveys of women conducted between 1985 and 2003 that were available in standard recode format: 102 household surveys and 128 surveys of women, including complete child files from the surveys of women. Incompleteness of reporting (such as giving age but not a birth year or month), heaping of ages or birth years (stating final digit 0 or 5, or age at death 12 months, for example), and displacement (transfers below age 15, above age 49, or outside the interval for the child health questions) are examined. Results are presented mainly in the form of histograms showing the distribution of indexes of these kinds of problems and lists of surveys that exceed specific thresholds. The effect of displacement on estimates of fertility and infant mortality is explored, as well as evidence that education of the household respondent, household head, and the mother tend to affect misreporting. Trends over time and differences between major regions are presented.

In general, surveys in Latin America have the highest quality of age and date reporting. The South Asian surveys show the strongest evidence of incompleteness and heaping, although these problems are also prevalent in Africa. Surveys in Sub-Saharan Africa have the highest levels of age displacement. Over time, DHS surveys show a marked reduction in the incompleteness of children's birthdates and in the upward displacement of older women, two of the most important types of misreporting. No indexes have deteriorated, but several have fluctuated or remained much the same.

1 Introduction

The Demographic and Health Surveys (DHS) project has become one of the principal sources of international data on fertility, family planning, child health, child mortality, and reproductive health. From 1985 through 2003, DHS conducted 141 surveys of households and women in 66 different countries. In addition, it has conducted many surveys on the availability of health and family planning services, surveys of men, and other special surveys.

Most of the measures produced from DHS surveys depend to a critical degree on reports of ages and dates. At the most basic level, inclusion of women age 15-49 in the surveys depends on accurate reports of the ages of women near the boundaries of that age interval in the household survey. The inclusion of children under five (or another specified age) for the questions about child health, immunizations, and nutrition also depends on accurate reports of their birth dates.

Many measures are age-specific, such as estimates of age-specific fertility rates and infant and child mortality rates. Estimates of levels and trends in such rates may be affected by misreporting of ages and dates of birth for a woman and her children, or dates of death for her children. Misreporting of adult ages often takes the form of a preference for numbers ending in the digits 0 or 5. Since standard age intervals begin with those digits, misreporting can shift women into the next higher age interval. Age displacement of children can seriously distort estimates of current levels and recent trends in fertility and mortality.

1.1 Objectives

This report uses household and individual data from nearly all of the DHS surveys to identify evidence of misreporting of ages and dates. It focuses on the levels of incompleteness, digit preference, and transfers across specific boundaries such as ages 5, 15, and 50. Omission and misreporting of variables other than ages and dates, while important, are not included in this report.

The data examined are drawn from surveys conducted in four phases of DHS data collection: DHS-I (1985-89), DHS-II (1990-92), DHS-III (1993-98), and MEASURE DHS+ (1999-2003)—a total of 141 surveys. The countries and survey dates are listed in Appendix A. This analysis includes 128 of those surveys; 13 are omitted. Eleven surveys are excluded because of restrictions imposed by the host countries (Botswana 1988; Cambodia 2000; Egypt 1988 and 1998; Eritrea 1995 and 2002; Jordan 1990, 1997, and 2002; Turkmenistan 2000; and Yemen 1997). One is excluded (Peru 2003) because it had a different design, and one (Senegal 1999) is excluded because no standard recode file was ever prepared for it.

Each reference in Appendix A is to a pair of surveys, one for households and one for women age 15-49. The principal purposes of the household survey are to identify eligible respondents for the survey of women and to collect information on housing characteristics and household assets. Household surveys conducted between 1985 and 1989 are not included in the data analysis because standard recode files were not prepared for them. This analysis is thus based on 102 household surveys conducted between 1990 and 2003 and 128 surveys of women age 15-49 conducted between 1985 and 2003. Sixty-one countries are represented, with one to five surveys from each country. Table 1.1 describes the data files used in the report. Tables in Section 6.1, where the results are summarized, describes the distribution across regions and time periods.

Table 1.1 Number of data files used in the report and number of cases in those files					
Types of files	Number of files	Smallest	Median	Mean	Largest
Household	102	5,764	37,235	54,267	488,839
Woman	128	1,286	7,198	9,973	90,303
Child	128	3,256	24,562	31,079	285,599

Note: The household files consist of one record for each person in each household. The smallest files are all from the Dominican Republic 1999 survey. The largest household and woman files are from the India 1998-99 survey. The largest child file is from the India 1992-93 survey.

DHS has carried out several earlier studies of age and date reporting, specifically examining data collected during DHS-I and DHS-II. A brief description of those earlier studies can be found in Appendix B. The quality of DHS data is also monitored during the data collection process, and each country report includes appendix tables with some discussion of this topic. Those tables are described in Appendix C.

It is important to conduct overall assessments of this type from time to time. However, the kinds of problems that appear in DHS surveys are largely endemic in settings where ages and dates are not important in the daily lives of most people and do not necessarily indicate faulty data collection procedures. For example, heaping is repeatedly observed in all censuses and surveys in some countries, and cannot be eliminated even with special training of interviewers. One function of this analysis is to identify the countries, surveys, and topics that show the strongest evidence of reporting problems, so that efforts can be made to reduce—if not eliminate—such problems in the future. Another function is to determine whether, as would be expected because of cumulative experience and increasing average educational attainment of respondents, there has been a general improvement in data quality over time.

There is a large demographic literature on age and date reporting in surveys, censuses, and vital statistics data. This report adapts and advances those methods by fitting them into the more general framework of statistical models. The procedures used are described in Section 1.2, with some details deferred to Appendix D.

1.2 Methods

This report focuses on measuring the levels and patterns of four kinds of potential errors: incompleteness, heaping or digit preference, transfers across boundaries, and inconsistencies between successive surveys.

1.2.1 Missing or Incomplete Data

For the most important kinds of dates, DHS includes a code to indicate whether imputation was necessary. Each such item is recoded to the value 0 if the original response was valid and complete or 1 if the response was incomplete in any way. For each dataset, the output measure will be the proportion of cases with code 1.

1.2.2 Heaping or Digit Preference

One of the most frequently employed indicators of data quality is the degree of digit preference in stated age. If age is not known, and is estimated by either the respondent or the interviewer, there is often a tendency to heap on a number ending in 0 or, to a lesser extent, ending in 5. Heaping tends to increase as age increases.

The standard summary measure of digit preference is produced by Myers' blended method. The blending procedure adjusts for the natural tendency of a final 0 to occur more often than a final 1, and a final 1 more often than a final 2, and so on, simply because population growth and mortality tend to result in more people age *x* than age *x*+1. The blending procedure for the index requires that the age range be a multiple of ten years, so that each final digit (0 through 9) occurs the same number of times.

Myers' Index is equivalent to the index of dissimilarity for a comparison between the blended percentage observed at each digit and the expected percentages (uniformly 10 percent). It is half the sum of the absolute deviations of the observed percentages from the expected percentages, and can be interpreted as the minimum percentage of the cases that would have to be shifted from one digit to another to achieve a uniform distribution across ages.

A large value of the index can result from large deviations at any digit, not necessarily 0 or 5. For example, age distributions of children will often suggest a preference for even numbers. In any case, the preferred digits will be those with the largest positive deviations. In addition to Myers' Index, this report gives the excess percentages at final digits 0 and 5, calculated as the blended percentages at those two digits minus 20. This measure does not use absolute deviations and can be negative. If Myers' Index is large, but the excess at 0 and 5 is not, then the heaping is on other digits.

If age is not known, it is possible that an estimate is made of year of birth, and age is calculated by subtracting year of birth from year of interview. Digit preference could then be traced to the year of birth. For example, if the survey was done in 2002, a preference for birth years ending in 0 will produce an excess of ages ending in 2. Similarly, if birth year is calculated by subtracting age from survey year, heaping on ages ending in 0 will produce an excess of birth years ending in 2. To check on this possibility, a modification of Myers' blended method will be applied to the reported or implied year of birth, as well as to reported age.

Myers' blended method is illustrated in Siegel and Swanson (2004: 139) using the single-year age distribution from the 1990 census of the Philippines. This report uses an equivalent formulation as a statistical model, which has been verified to agree with the usual approach. A routine written in Stata is applied to a file of individual data, using multinomial logit regression on the final digit (as a category of a 10-category categorical dependent variable) with no predictor variables and with the Myers' weights, extracting the coefficients from that regression, converting the coefficients to proportions at each integer, and calculating the summary index.

Another context in which heaping can occur is in the reported age of a child at death. In DHS surveys, if a child died before the second birthday, age at death is reported in months. Not surprisingly, the data often show some heaping at 12 months (and, to a lesser degree, at 6 and 18 months). If the excess at 12 months is partly due to misreporting of deaths that actually occurred at 10 or 11 months, this can lead to an underestimate of the probability of dying in the first year of life (months 0-11), ${}_1q_0$. It can also cause an overestimate of the probability of dying at ages 1-4 (months 12-59), ${}_4q_1$. Earlier assessments have measured this phenomenon by dividing the reported number of deaths in month 12 by the average of the numbers in months 10, 11, 13, and 14. That is, if the numbers reported in months 10, 11, 12, 13, and 14 are labeled *a, b, c, d,* and *e*, respectively, this heaping measure is calculated as

$$\frac{c}{(a+b+d+e)/4} = 4c/(a+b+d+e).$$

It is the ratio of the observed number of deaths in month 12 to the expected number, under a model of linear change in the true monthly frequencies from month 10 to month 14. Heaping will produce a ratio greater than one.

In this report, this measure of heaping of age at death at 12 months is calculated with a logit regression (limited to births in the ten years before the survey). In the file of births, a binary variable is constructed, coded 1 if a death was reported at 12 months, 0 if a death was reported at 10, 11, 13, or 14 months, and missing otherwise (that is, if the death occurred outside these months or the child did not die at all). In a logit regression with this constructed variable as the outcome and no predictor variables, the coefficient for the intercept is exponentiated and multiplied by four to find the measure of heaping. This approach allows for easy incorporation of weights and clusters and also yields a statistical test of the null hypothesis that there is no heaping in the population.

All measures in this report must be accompanied by a warning that evidence of misreporting may be a spurious consequence of genuine variation. Such a misdiagnosis is probably unlikely for this measure because it uses only a five-month range of age within a ten-year interval of time.

1.2.3 Transfers Across Boundaries

The household survey contains a considerable amount of information that can be analyzed in its own right, but its main purpose is to identify women age 15-49 who are eligible for the individual survey (and sometimes to identify eligible men or other respondents). The interviewers have some incentive, in order to reduce their workload, to misstate the ages of some women by entering them as too young or too old to participate in the main survey. Such an effect could cause a woman who is actually age 15-19 to be reported as 10-14, or a woman who is actually age 45-49 to be reported as 50-54. It is virtually impossible to know which specific women have been transferred across a boundary, but evidence of the effect will appear as an excess of women reported at ages 10-14 and 50-54. Two main approaches to identifying such displacement have been used in the literature: sex ratios and age ratios. This report does not use the method of sex ratios, because it requires an assumption that displacement is restricted to women and that reporting of age is correct for men. Many DHS surveys have included male samples as well as female samples, so that assumption would not be valid.

Age ratios: original use

The age ratio for an age interval is defined as the ratio of the reported number of cases in that interval, divided by the reported number of cases in the preceding age interval (sometimes multiplied by 100). Because of a history of population growth in almost all countries, and the increase in mortality with age, we expect age ratios to be a little less than one (or 100) at most ages and to decline fairly regularly as age increases. Looking for possible transfers outside the age 15-49 interval, Rutstein and Bicego (1990:8) reported the following relevant age ratios from the 1988 household survey of Botswana:

Age interval	Age ratio
10-14	126
15-19	76
....	
45-49	73
50-54	152

The irregularity of these age ratios clearly indicates displacement from age 15-19 to 10-14 and from 45-49 to 50-54. To summarize this displacement, Rutstein and Bicego calculate AR_i-AR_o, which is the age ratio inside (i) the boundary minus the age ratio outside (o) the boundary. For Botswana 1988, this indicator is 76-126 = -50 for the lower age boundary and 73-152 = -79 for the upper age boundary, or a total of (-50) + (-79) = -129. This report builds on the logic of this approach, first with aggregated data, and then adapting it to a statistical model that can be used with individual-level data.

Age ratios: reformulation

Focusing on the lower boundary for eligibility, age 15, we identify two age intervals below the boundary and two above it:

Age interval	Observed frequency	Fitted frequency
5- 9	a	a
10-14	b	$\hat{b}$
15-19	c	$\hat{c}$
20-24	d	d

Thus, for Botswana 1988, b/a=1.26 and c/b=.76; frequency d was not used by Rutstein and Bicego. We propose a model to estimate the second and third frequencies (b and c) using the first and fourth frequencies (a and d), which are assumed to be reported correctly. This will be done with two requirements. First, we assume that the total of the second and third frequencies is correct: $\hat{b} + \hat{c} = b + c$. Second, we assume that the three successive fitted age ratios, $d/\hat{c}$, $\hat{c}/\hat{b}$, and $\hat{b}/a$, are linear on a log scale. The second assumption seems plausible because changes in both cohort size and the force of mortality tend to be linear on a log scale.

As shown in Appendix D, the second requirement, after some algebra, leads to $\ln(\hat{c}/\hat{b}) = [\ln(d/a)]/3$. Defining $u = (d/a)^{1/3}$, we obtain

$$\hat{b} = (b+c)[1/(1+u)]$$

and $$\hat{c} = (b+c)[u/(1+u)] \ .$$

The proportion age 15-19 who were misreported downwards as 10-14 is then estimated as

$$(\hat{c} - c)/\hat{c} = 1 - c/\hat{c} \, .$$

To identify transfers upwards across age 50, the four intervals would be 40-44 through 55-59, and the proportion of 45-49 year olds who were misreported as 50-54 would be estimated as

$$(\hat{b} - b)/\hat{b} = 1 - b/\hat{b} \, .$$

This approach can be illustrated by applying it to the females in the Zambia 2001-02 household survey (unweighted). The frequencies in age intervals 5-9 through 20-24 are a=3047, b=2684, c=2039, and d=1789, respectively. The age ratio for age 10-14 is 2684/3047=.88 and for age 15-19 is 2039/2684=.76, leading to AR_i-AR_o,=-12, suggesting some, but not a large amount of, displacement. Our method gives

$$\ln(\hat{c}/\hat{b}) = [\ln(d/a)]/3 = -.1775 \, ,$$

leading to

$$u = \exp(-.1775) = .8374$$

and then

$$\hat{b} = (2684 + 2039)/1.8374 = 2570.5$$

and

$$\hat{c} = (2684 + 2039)(.8374/1.8374) = 2152.5.$$

This suggests that a proportion $1 - c/\hat{c} = 1 - 2039/2152.5 = .053$ of women age 15-19 were misreported as 10-14. We suggest that this modest extension of the traditional age ratio approach gives a more interpretable parameter estimate. The application of this model to individual-level data is described in Appendix D.

In this report, the four-category method for identifying age transfers is applied in two contexts other than possible transfers outside the age range of eligibility for the survey of women. First, in a male survey, we identify transfers outside the age range of eligibility. Second, possible transfers of children outside the interval of eligibility for the extra questions about child health are investigated. In these contexts, the four intervals are single calendar years, two prior to the boundary and two after the boundary. For example, in the 2001-02 survey of Zambia, the extra questions applied to all children born in January 1996 or later. We look at the births reported for calendar years 1994, 1995, 1996, and 1997. Distortion would appear as an excess of births in 1995 and a deficit of births in 1996 because some children actually born in 1996 were misreported as being born in 1995. We assume that such transfers affect only the single calendar year on each side of the boundary (e.g., 1995 and 1996), and that the other two years (e.g., 1994 and 1997) are reported correctly. It is possible, of course, that more years are involved in the misreporting. It is also possible that in some contexts there are patterns of transfers of children's ages that have nothing to do with the questionnaire. If either of these conditions applies, our method will be deficient.

1.2.4 Inconsistencies Between Successive Surveys

The report checks for agreement between two successive surveys in the estimates of fertility prior to the first survey. Such disagreements can be due to misreporting of the ages of women—and possibly to mortality of women between the surveys—but are more likely due to misreporting of dates of childbirth. We focus on age-specific fertility rates and the total fertility rate (TFR) for the three calendar years prior to the first month of interview in the first survey. The rates for this reference period are estimated from the birth histories in the first survey and then from the birth histories in the second survey.

The rates are estimated with a Poisson regression procedure that is equivalent to the usual DHS method and allows easy adjustment for sampling weights and clusters. The difference between the logarithms of two estimated age-specific rates will have an approximately normal distribution with a standard error that is the square root of the sum of the squares of the two log rates' standard errors. It is thus relatively easy to perform tests of statistical significance.

Most successive surveys are five years apart. The second survey will include few or no women who had exposure to the risk of childbearing during age 45-49 during the reference period. Therefore the comparisons will be restricted to ages 15-44.

As an example, Zambia has had three DHS surveys, in 1992, 1996, and 2001-02. For the comparison between the 1992 and 1996 surveys, the reference period was 1989-91. The TFR (age 15-44, rather than 15-49) estimate for this period from the first survey was 6.25, and from the second survey was 6.40. These estimates are quite close; there are no statistically significant differences within any pairs of age-specific rates or between the two Tars.

In the comparison between the 1996 and 2001-02 surveys, the reference period was the calendar years 1993-95. The estimated TFR (age 15-44) for this period from the 1996 survey was 6.05; from the 2001-02 survey it was 7.08. The difference is statistically significant for four of the six age-specific fertility rates as well as for the TFR.

When this type of comparison between surveys is carried out, discrepancies will be measured in terms of the difference in TFR, the relative difference in TFR, and a chi-square statistic that synthesizes the z-scores for the six age-specific rates.

A similar comparison between successive surveys is made for the infant mortality rate (IMR). This comparison uses the files of children rather than the files of women. The reference time period is the five, rather than three, calendar years prior to the first month of interview in the first survey. For example, in the comparison between the 1996 and 2001-02 Zambia surveys, the reference period for infant mortality is the calendar years 1991-95. DHS normally uses three-year intervals for recent fertility and five-year intervals for recent mortality, but their intervals extend back from an individual woman's date of interview, which would not give a fixed reference period.

The IMR for the reference period is calculated for both surveys using a log probability procedure (the IMR is actually an estimated probability) that is equivalent to the standard DHS procedure and allows for sampling weights and clustering. The difference between the logarithms of two estimated probabilities will have an approximately normal distribution with a standard error that is the square root of the sum of the squares of the two log probabilities' standard errors. Therefore the difference between the two estimates of the IMR can be evaluated for both magnitude and statistical significance.

It is important to recognize the limitations on our ability to identify misreporting of ages and dates, which can lead to two complementary types of misinterpretations. First, there may be instances in which the reporting is actually better, or more accurate, than we infer. For example, the true age distribution is the consequence of an irregular history of births and deaths, and any assumption about its shape, across either twenty years or four years in our applications, is probably false to some degree. In some of the datasets, the frequencies in successive age groups appear very erratic, but any kind of smoothing procedure runs the risk of removing true variation. Second, there may be other instances in which the reporting is actually accurate than we infer.

Test statistics can be calculated for all of the indices in this report, but they are only presented for the comparison of fertility rates and infant mortality rates from successive surveys, the heaping of age at death at twelve months, and estimates of an effect of education on misreporting. In other contexts there is too great a risk of over-interpreting a test statistic by confounding sampling errors with systematic nonsampling errors and giving too much credence to a particular method for smoothing or adjusting data. Instead, we list the surveys that have the highest levels of each index, above an admittedly arbitrary cutoff such as 10 percent or 20 percent. Before reaching the conclusion that a survey is indeed flawed, we would advise a thorough country-specific analysis that would go into more detail than can be applied to every country in this kind of report.

2 Household Survey

This chapter analyzes age reporting in 102 household surveys. Each survey was processed as a file of the individual household members, defined by de facto residence and omitting persons above age 79. Topics are age heaping; differences between the quality of age data for the household respondent and other household members; evidence of transfers outside the age range of eligibility for women age 15-49; and evidence of such transfers for men when there was a male survey.

2.1 Digit Preference or Age Heaping

Figures 2.1 and 2.2 give the distribution of the Myers' Index and the percentage excess at 0 and 5 for reported ages 0-79. These figures are followed by Table 2.1, which lists the countries for which either index is above 10 percent. This cutoff is at least twice the median values (5.0 percent for Myers' Index and 3.7 percent for the heaping at 0 and 5). In Table 2.1 and other tables, if the survey's fieldwork extended across two successive calendar years, the calendar year listed is the year in which a majority of the interviews occurred, referred to as the median year of interview.

The two indexes have similar distributions and are almost perfectly correlated with each other (r=.98), implying that most digit preference for age is indeed at final digits 0 and 5. With the exception of the 1991-92 survey of Yemen, the countries listed are either in South Asia (India, Bangladesh, and Pakistan) or Western/Middle Africa (Benin, Niger, Nigeria, and Chad, which are adjacent to one another). In successive surveys in Niger and Nigeria, the level of heaping appears to have declined. In Bangladesh there was a decline between the 1993-94 survey and the 1996-97 survey, but the level was higher in 1999-2000 than in either of the two earlier surveys. In India, Myers' Index was the same for both the 1993 and 1999 surveys.

The household surveys do not include year of birth, but it can be estimated approximately as year of interview minus reported years of age. We calculated Myers' Index and the excess at final digits 0 and 5 for this implied year of birth. Myers' Index was about the same as in Figure 2.1, with no evidence of heaping at calendar years of birth ending in 0 or 5, so the results need not be presented here.

Figure 2.1 Myers' Index for household members (0-79 years)

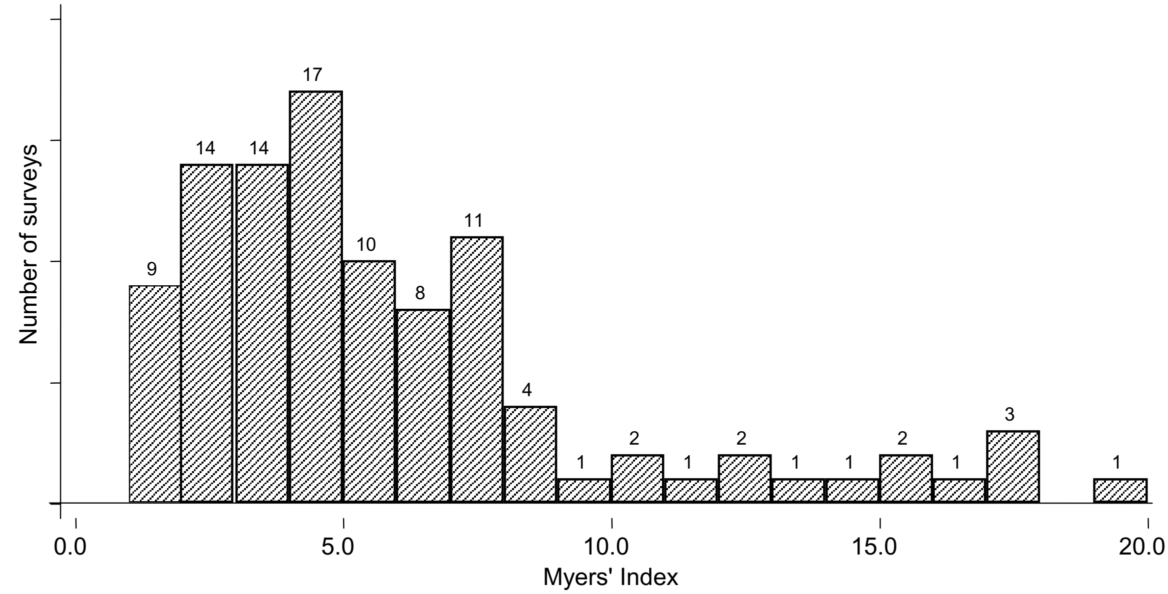


Figure 2.2 Heaping at digits 0 or 5 for household members (0-79 years)

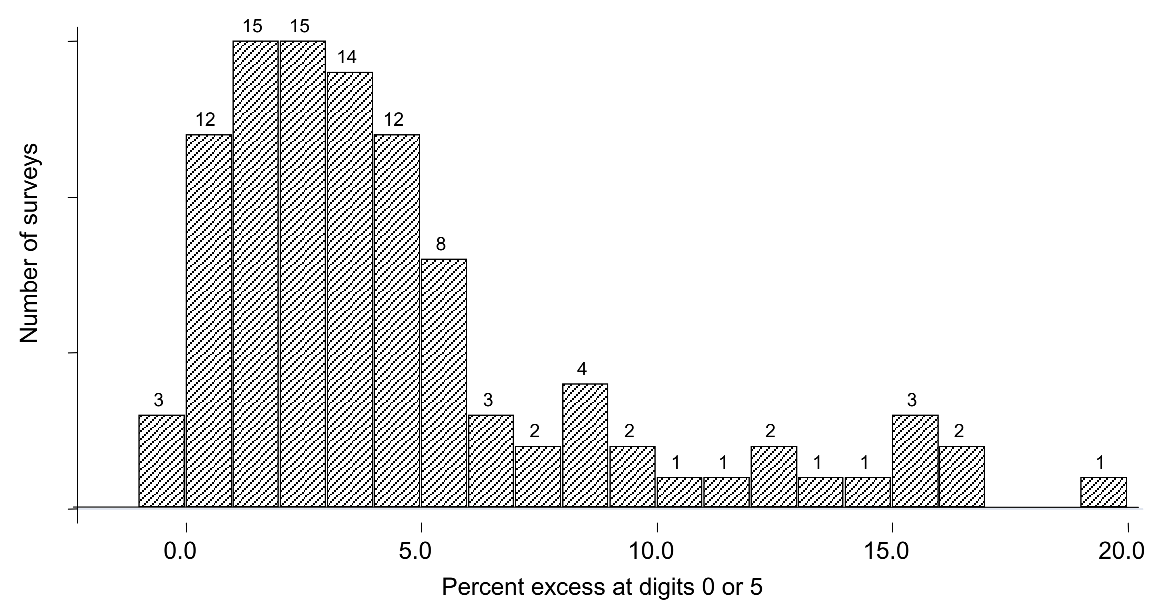


Table 2.1 Household surveys with strongest evidence of heaping by age

Country	Median year of survey	Myers' Index	Percent excess at final digit 0 or 5
Bangladesh	1994	11.6	10.4
Bangladesh	1996	10.4	9.7
Bangladesh	2000	12.4	12.1
Benin	2001	10.6	8.4
Chad	1997	12.4	11.0
India	1993	17.1	15.0
India	1999	17.1	15.3
Niger	1992	15.3	14.3
Niger	1998	13.2	12.8
Nigeria	1990	19.7	19.6
Nigeria	1999	15.6	15.2
Nigeria	2003	14.7	13.4
Pakistan	1991	17.9	16.9
Yemen	1991	16.6	16.6

Note: The table lists surveys for which either the Myers' Index or the percent excess at final digit 0 or 5 is greater than 10.

2.2 Digit Preference or Age Heaping: Household Respondent Compared with Other Household Members

The information about household members is obtained from a specific person, usually the household head or spouse. It is reasonable to expect that this person will report information more accurately about himself or herself than about other household members.

Figures 2.3 and 2.4 give the reduction, or improvement, in Myers' Index and the percentage excess at final digits 0 and 5 for the household respondent compared with other members of the household. The comparisons are limited to ages 20-59, because the household respondent is almost always in this age range. Table 2.2 lists the surveys with the largest difference.

The differences are almost always positive, but they also are generally small, only occasionally above 5 percent. The only large differences, listed in Table 2.2, are for the three surveys in Bangladesh. They imply a very low level of heaping, less than 5 percent, when the respondent is giving his or her own age, but a very high level, 20 percent to 30 percent, when the same person reports the age of other household members. The net effect, as given in Table 2.1, is a high overall level of heaping in all the Bangladesh surveys, but the overall level disguises the relatively accurate self-reports of the household respondent. No other country listed in Table 2.1 shows this kind of bifurcation.

Figure 2.3 Reduction in Myers' Index for respondent (20-59 years)

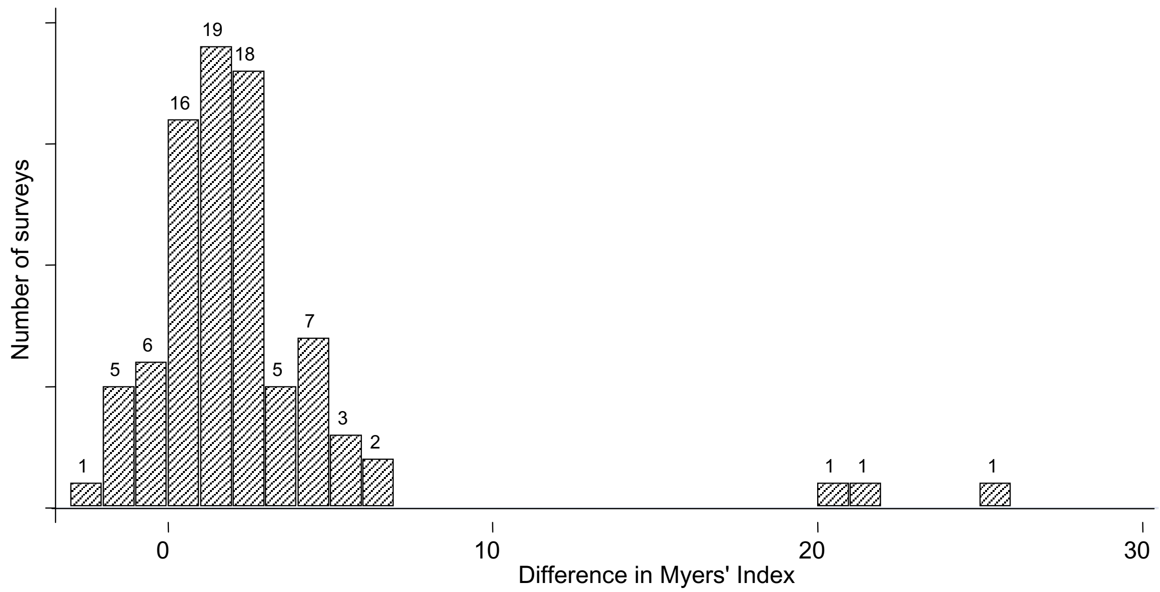


Figure 2.4 Reduction in heaping at digits 0 or 5 for respondent (20-59 years)

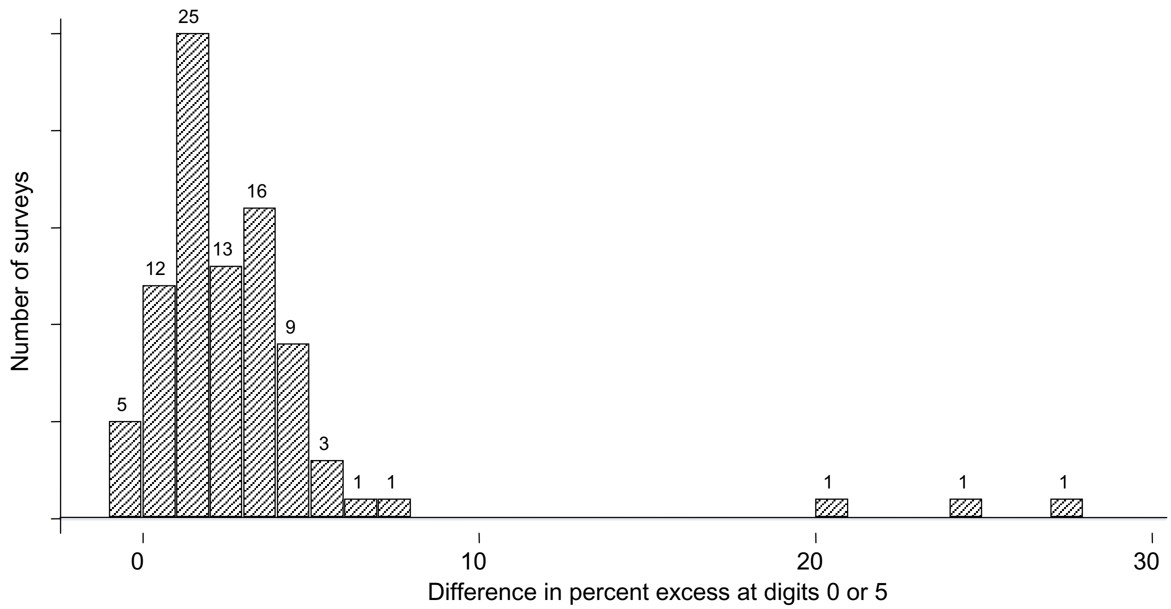


Table 2.2 Household surveys with largest difference in heaping by age between other household members and the household respondent

		Myers' Index			Excess at 0 or 5		
Country	Median year of survey	Nonhousehold respondents age 20-59	Household respondents age 20-59	Difference	Nonhousehold respondents age 20-59	Household respondents age 20-59	Difference
Bangladesh	1994	25.0	4.7	20.29	23.8	3.0	20.75
Bangladesh	1996	25.5	3.5	21.99	25.3	1.0	24.26
Bangladesh	2000	29.7	3.7	25.99	29.7	1.3	28.36

Note: The table lists surveys for which the reduction in either the Myers' Index or the percent excess at digits 0 or 5 is greater than 10.

2.3 Age Displacement of Women

The principal function of the household survey is to identify eligible respondents for the survey of women age 15-49. Errors of inclusion have potential ramifications for several measures produced by the surveys of women. The errors are most likely due to intentional efforts by interviewers to reduce their workload, because the respondents themselves are not aware of the screening function and have no incentive to shift ages across the boundaries of 15 and 50. This section examines internal evidence of downward and upward transfers.

Evidence of net transfers from ages 15-19 into 10-14 is given in Figure 2.5 and Table 2.3. The figure gives the distribution of the index, expressed as the estimated percentage of women with true age 15-19 who were misreported at 10-14. The table lists the countries for which the percentage exceeds 10 percent.

There is one data set, the 1999 survey of Nigeria, for which the method indicates a 17.4 percent *increase* in the number of women 15-19, compared to the estimated true number. A transfer in this direction is not expected. As Table 2.3 shows, the previous Nigeria survey (for 1990) appears to have had a high level of transfers in the expected direction—indeed, the highest level of any survey.

An effort was apparently made in the 1999 survey of Nigeria to reduce the displacement out of age 15-19 that had been observed in 1990. According to the report on the 1999 survey, household screening for the survey of women age 15-49 included an eligibility code for ages 10-49, with ages 10-14 screened out later. However, as the report notes, this change induced substantial transfers from ages 10-14 into 5-9. The weighted age distribution of females in the household survey included 2762 at ages 0-4, 3445 age 5-9, 1817 age 10-14, 1918 age 15-19, and 1622 age 20-24. The obvious transfers from ages 10-14 into 5-9 would cause our method to give a false diagnosis of transfers from 10-14 into 15-19, so we do not conclude that there was a such a shift. Indeed, it appears likely that the two-step screening procedure for the 1999 survey did give a more plausible count for age 15-19.

In any case, 12 different countries are represented in the list of 18 surveys given in Table 2.3, with estimated transfer rates above 10 percent. Six countries appear twice. With the exception of Kyrgyzstan, all of the countries are in sub-Saharan Africa: Burkina Faso, Benin, Ghana, Kenya, Madagascar, Mali, Malawi, Nigeria, Uganda, and South Africa.

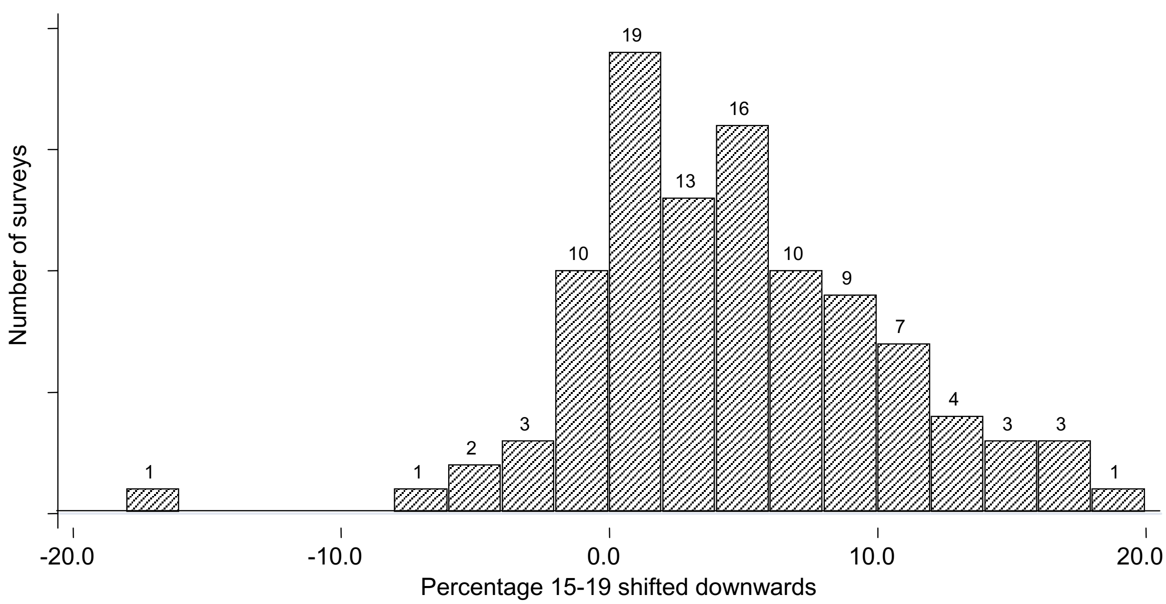


Table 2.3 Household surveys with strongest evidence of downward age transfers for younger women

Country	Median year of survey	Estimated percentage of women 15-19 misreported at 10-14	Observed number				Estimated "true" number	
			Age 5-9	Age 10-14	Age 15-19	Age 20-24	Age 10-14	Age 15-19
Benin	1996	11.2	2240.8	1787.3	1139.8	1067.0	1643.6	1283.5
Burkina Faso	1993	12.1	2879.8	2394.2	1465.9	1265.0	2193.0	1667.1
Burkina Faso	1999	10.1	2610.2	2350.3	1522.6	1228.9	2178.3	1694.6
Ghana	1993	17.8	1742.0	1479.0	842.0	861.0	1296.2	1024.8
Ghana	1998	14.1	1602.5	1482.5	949.1	924.9	1326.8	1104.7
Kenya	1993	16.1	3409.5	3110.4	1838.7	1709.5	2758.0	2191.1
Kenya	1998	13.3	2754.0	2996.9	1965.0	1638.0	2695.3	2266.7
Kyrgyzstan	1997	11.1	971.0	1050.4	740.5	640.1	957.6	833.3
Madagascar	1992	10.7	2105.3	2060.1	1431.9	1286.7	1889.0	1603.0
Malawi	1992	14.8	1909.9	1899.4	1160.8	987.9	1697.5	1362.6
Malawi	2000	11.7	4859.1	4355.4	2991.6	3043.9	3959.3	3387.7
Mali	1996	11.6	4234.7	3353.3	1996.4	1649.7	3091.7	2258.0
Mali	2001	15.4	5476.2	4754.2	2738.2	2403.4	4257.2	3235.2
Mozambique	1997	12.5	3297.6	3068.1	2018.8	1883.4	2780.2	2306.7
Nigeria	1990	19.7	3973.9	3258.6	1733.0	1760.4	2832.4	2159.2
Uganda	1995	10.3	3142.4	2519.9	1672.8	1613.9	2328.2	1864.5
Uganda	2001	12.0	3121.3	2723.9	1734.2	1555.6	2486.6	1971.5
Zambia	1998	16.1	3442.8	3825.7	2462.7	2303.7	3354.5	2934.0

Note: The table lists surveys for which the estimated percentage of women 15-19 misreported at 10-14 is greater than 10.

To interpret the other numbers provided in Table 2.3, consider the age distribution given for the 1993 survey of Ghana. The observed ratio of women age 15-19 to 10-14 is 842.0 / 1479.0 = .57, an implausible decline from one age group to the next. After adjustment, retaining the total for these two age groups, the ratio becomes 1024.8 / 1296.2 = .79. The adjusted decline is more plausible, in a context of high fertility and nonnegligible mortality across these years of age, and gains credibility by comparison with the pattern in Ghana's 1998 age distribution, in which each cohort was five years older. However, there are also some age distributions in Table 2.3 that would be a challenge to any method. The age distribution for the 1998 survey of Kenya reported 2754.0 women age 5-9, then an *increase* to 2996.9 age 10-14, a precipitous drop to 1965.0 women age 15-19, and then a more modest decline to 1638.0 women age 20-24. In order to preserve the observed numbers age 5-9 and 20-24, and the total for the interval 10-19, our method produces estimates for 10-14 and 15-19 that are probably more plausible than the observed numbers, but they are certainly themselves in error. It is likely that there is a broader pattern of misstatement of age than the method assumes. Nevertheless, although the percentage transferred from age 15-19 into age 10-14 may not be estimated very well, there is clearly something wrong with the Kenya distribution, and it belongs on this list.

We now look for evidence of upward transfers, across age 50. Using a similar methodology, Figure 2.6 shows the distribution of estimated transfers of women from age 45-49 into 50-54, and Table 2.4 lists the data sets believed to have shifts greater than 20 percent. This is a higher threshold than used for Table 2.4, but so many surveys are between 10 percent and 20 percent, or between -10 percent and -20 percent, that it would be hard to see a pattern with a 10 percent threshold (no surveys are below -20 percent). Evidence of widespread transfers across age 50 is very strong, much stronger than across age 15.

Figure 2.6 Estimated upward transfers of women age 45-49

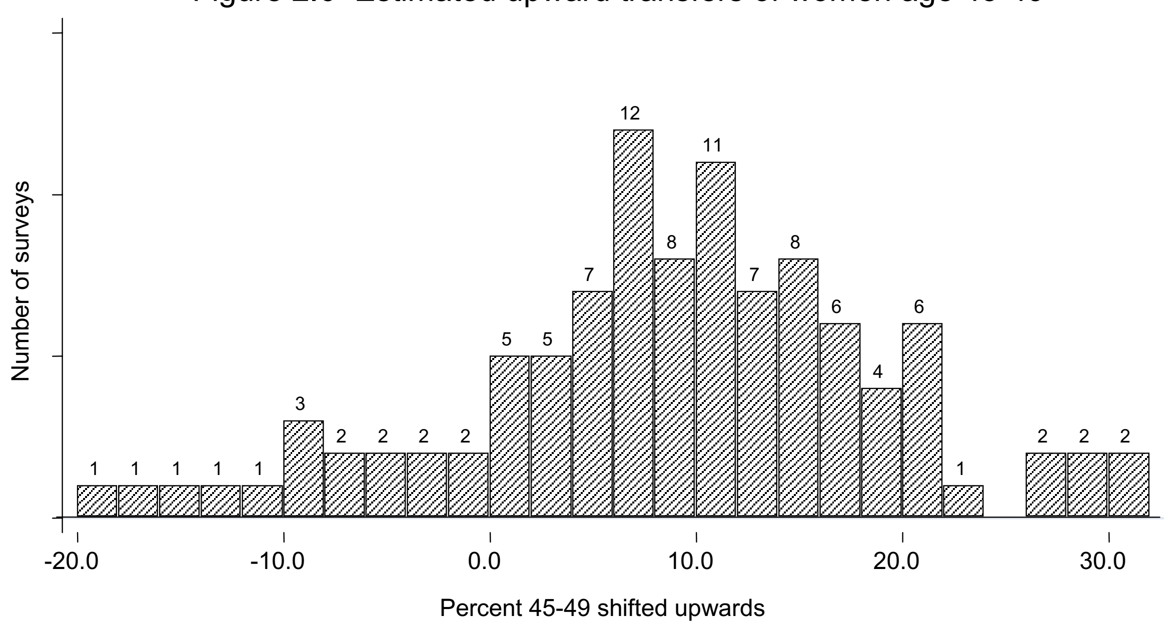


Table 2.4 Household surveys with strongest evidence of upward age transfers for older women								
		Estimated percentage of women 45-49	Observed number				Estimated "true" number	
Country	Median year of survey	misreported at 50-54	Age 40-44	Age 45-49	Age 50-54	Age 55-59	Age 45-49	Age 50-54
Burkina Faso	1993	32.0	521.1	394.1	730.9	435.9	579.2	545.8
Cameroon	1991	27.3	323.0	221.7	365.6	255.7	305.1	282.2
Comoros	1996	20.4	212.0	219.0	313.0	173.0	275.0	257.0
Ghana	1993	20.6	433.0	337.0	444.0	257.0	424.4	356.6
Kenya	1993	28.5	684.7	452.9	722.7	428.7	633.6	542.0
Madagascar	1992	21.8	550.9	355.7	474.6	309.8	454.8	375.4
Namibia	2000	22.5	697.4	465.8	621.6	369.2	601.1	486.3
Niger	1992	27.6	544.9	354.2	551.3	334.7	489.4	416.0
Nigeria	1990	28.8	887.8	642.0	1051.7	603.5	901.2	792.4
Senegal	1993	21.5	619.0	398.0	542.0	387.0	506.7	433.3
Togo	1998	21.4	693.7	583.9	854.4	569.0	742.9	695.4
Uganda	1995	31.3	430.4	290.6	510.7	307.2	423.2	378.2
Zimbabwe	1999	21.3	512.2	399.5	537.3	309.0	507.8	429.1

Note: The table lists surveys for which the estimated percentage of women 45-49 misreported at 50-54 is greater than 20.

There are several reasons to expect a higher probability of transfers across age 50 than across age 15. First, because of improvements in education, birth registration, and greater use of age in everyday life, age is better documented and more accurately known for younger cohorts. Second, it is generally harder to estimate the age of someone who is older than someone who is younger. Third, any motivation for interviewers to reduce their workload by displacing potential respondents will be greater for older women, because much more of the questionnaire, in particular a longer birth history, will apply to them.

Each of the 13 countries in Table 2.4 appears in only one survey. All are in sub-Saharan Africa: Burkina Faso, Cameroon, Ghana, Kenya, Comoros, Madagascar, Nigeria, Niger, Namibia, Senegal, Togo, Uganda, and Zimbabwe. Half of the countries in Tables 2.3 and 2.4 appear on both lists. Indeed, the duplicates on the list consist of six surveys conducted about the same time: Burkina Faso (1992-93), Ghana (1993), Kenya (1993), Madagascar (1992), Nigeria (1992), and Uganda (1995). The most stunning example is the 1992-93 survey in Burkina Faso, which reported 394.1 women age 45-49 and 730.9 women 50-54. Transfers of this magnitude should have been (and probably were) detected during the fieldwork for the household survey. However, this entire part of the age distribution is problematic for that survey. The total number of women reported in the middle two age groups, 45-49 and 50-54, was 18 percent more than the total number reported at 40-44 and 55-59. The true subtotals would probably be much closer to each other.

The rates of boundary transfer tend to be higher around age 50 than around age 15, but the actual number of women affected is smaller at 50 than at 15. For example, we estimate that in the 1992-93 Burkina Faso survey, the downward transfer rate was 12.1 percent and involved the loss of 1667 - 1466 = 201 women age 15-19. The upward transfer rate was much larger, at 32.0 percent, but involved the loss of slightly fewer women age 45-49, namely 579 - 394 = 185.

The total loss of cases due to age displacement in this survey is estimated to be 201 + 185 = 386 women. The number of women age 15-49 in the main survey was 6354. If the 386 women had been included, the sample size would have been 6354 + 386 = 6740. Compared with this potential sample size, the 386 displaced women would represent a 5.7 percent loss to the sample. We have not done this calculation for all surveys, but few if any surveys had a combined loss in excess of this level.

2.4 Age Displacement of Men

Many DHS surveys include a survey of males, who are screened for eligibility with the household data. This section checks for internal evidence of age transfers of men who were eligible for such a survey.

The beginning age was 15 or 20 for 55 male surveys, and the ending age was 54, 59, or 64 for 51 male surveys. In this section we check for evidence of displacement around the beginning or ending ages of those surveys. Surveys are not included here if the starting or ending age was not specific or if the last digit of the ending age was not 4 or 9. As with the women, the threshold for the younger age transfers is 10 percent and for the older ages is 20 percent.

Four of the 55 surveys with a clear indication of the starting age of eligibility indicate displacement of at least 10 percent of males out of the five-year age group just above the lowest boundary (15-19 or 20-24): Armenia 2000, Benin 2001, Côte d'Ivoire 1998-99, and Ghana 2003. Two countries, Benin and Ghana, also showed serious transfers of women across the lower boundary of eligibility in Table 2.3, but for different surveys.

The age distribution of young men in the 2000 male survey of Armenia is strikingly irregular. This was a survey in which about a third of men in the household survey who were age 15-54 were selected. The weighted age distribution, unadjusted, gives 1087 males age 5-9, 1325 at age 10-14, 920 at age 15-19, and 814 at age 20-24. Our method adjusts the two middle age groups to estimated "true" frequencies of 1176 at age 10-14 and 1068 at age 15-19. The "bump" for age 10-14 is substantially reduced, but cannot be eliminated. The distribution for females at these ages in Armenia does not show the same pattern as the males, and gives very irregular sex ratios. Even after adjustment, the sex ratios in these ages are implausible, but they bolster the evidence that the Armenia survey had substantial transfers across age 15 for males but not (to nearly as great a degree) for females.

Figure 2.7 Estimated downward transfers of younger men

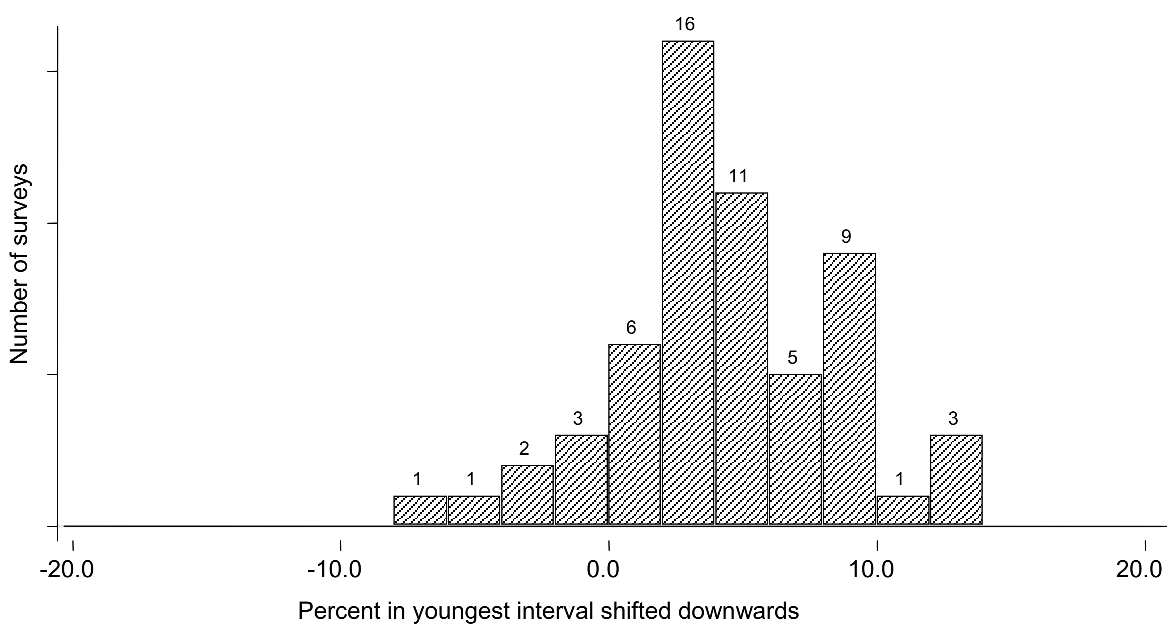


Table 2.5 Household surveys with strongest evidence of downward age transfers for younger men								
		Estimated percentage of men						
	Median	age x	Observed number				Estimated "true" number	
	year of	misreported	Age	Age	Age	Age	Age	Age
Country	survey	at x-1	x-2	x-1	x	x+1	x-1	x
Armenia	2000	13.9	1087.4	1324.8	919.6	814.2	1176.3	1068.1
Benin	2001	14.4	2472.9	2167.6	1214.3	934.2	1962.9	1419.0
Côte d'Ivoire	1999	13.3	887.3	874.4	568.1	510.2	787.6	654.9
Ghana	2003	10.6	1952.3	2014.0	1212.7	747.0	1869.5	1357.2

Note: Age x is the first five-year interval in a male survey; x-1 is the immediately preceding interval, etc. The table lists surveys for which the estimated percentage of men age x misreported at x-1 is greater than 10.

As with the surveys of women, there is considerably stronger evidence of transfers across the upper age boundary than across the lower boundary, so the threshold will again be 20 percent rather than the 10 percent used for downward transfers. Figure 2.8 shows the distribution of the estimated transfer rate in the 51 male surveys with a clear upper age boundary, and Table 2.6 lists the four surveys that were above the 20 percent cutoff. The four surveys are Bolivia 1998, Dominican Republic 2002, Ghana 1993, and Rwanda 2000. All of them show far more men in the age group just above the upper age boundary than in the age group just below it. The Ghana 1993 survey appeared earlier in Table 2.3 and Table 2.4 for its high levels of both downwards and upwards transfers out of the survey of women age 15-44. The other three surveys on this list do not give other evidence of high transfer rates.

Figure 2.8 Estimated upward transfers of older men

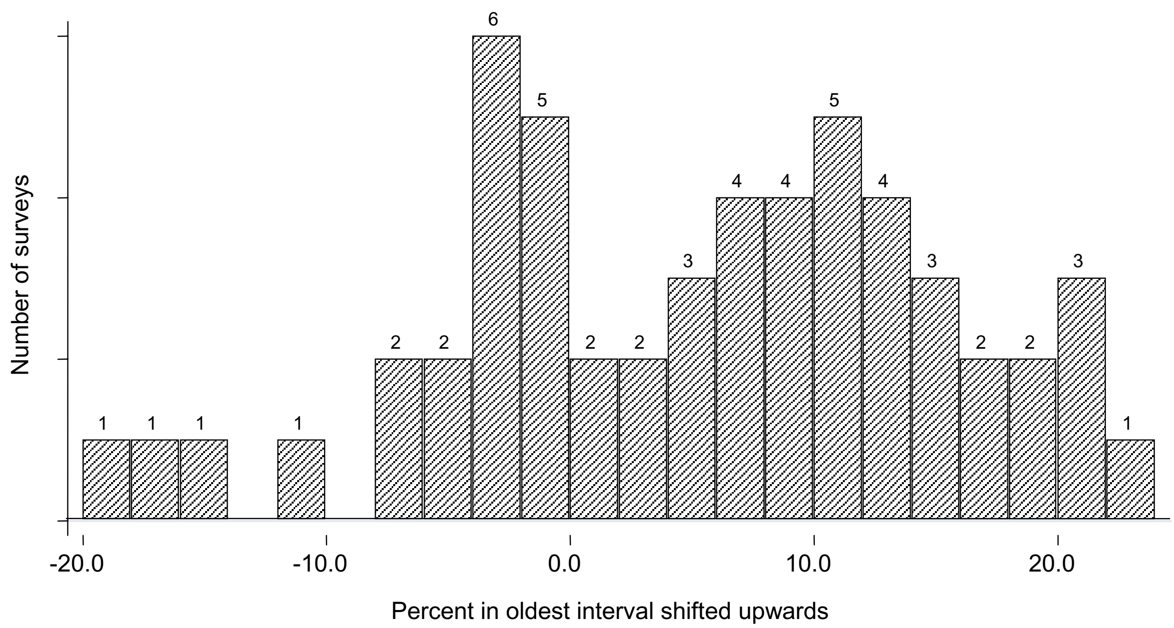


Table 2.6 Household surveys with strongest evidence of upward age transfers for older men

Country	Median year of survey	Estimated percentage of men age x misreported at x+1	Observed number				Estimated "true" number	
			Age x-1	Age x	Age x+1	Age x+2	Age x	Age x+1
Bolivia	1998	24.1	704.3	480.4	638.0	318.3	632.8	485.6
Dominican Republic	2002	21.3	2170.7	1280.9	1632.0	1071.4	1627.0	1285.8
Ghana	1993	20.5	290.0	196.0	444.0	136.0	246.5	191.5
Rwanda	2000	20.4	408.0	239.4	600.5	224.7	300.8	246.5

Note: Age x is the last five-year interval in a male survey; x+1 is the next higher interval, etc. The table lists surveys for which the estimated percentage of men age x misreported at x+1 is greater than 20.

3 Survey of Women Age 15-49

This chapter examines the main surveys of women age 15-49. The data file consists of the information in 128 such surveys. This file contains the DHS-I surveys, conducted before 1991, that could not be included in Chapter 2. Topics concern the completeness of reporting, age and birth date, age at marriage, date of marriage, years since marriage, and a comparison between two successive surveys' estimates of fertility in a three-year reference period prior to the first survey.

3.1 Incompleteness

Three variables to describe the quality of age and date reporting are included in all the surveys of women. The first one refers to the woman's age. Ideally, each woman will provide three items: her age in completed years, a year of birth, and a month of birth. At a minimum, there should be an age or a birth year. However, some women do not provide all three items, and even if all information is provided, there may be inconsistencies that require the imputation of one or even two of the items.

The first index is the proportion of women in a survey who did not provide all three items or for whom there was at least one inconsistency requiring imputation. The distribution of this proportion is given in Figure 3.1.

The second index refers to the completeness (or incompleteness) of data about age and date at first marriage. Ideally a woman provides both her age and the year and month at first marriage. The index is the proportion of women who did not provide all three items or for whom there was at least one inconsistency requiring imputation. The distribution of this proportion is given in Figure 3.2.

The third index summarizes the completeness of date information in the birth history. Each woman is asked about the month and year of each of her births. In some settings it is fairly common to omit the month or to be able to provide only the current age or years since the birth. The data files include a variable for each birth, describing whether the month and year were provided and were acceptable or had to be imputed (for example, if one birth date was too close to another birth date, then one or both would be adjusted to achieve compatibility with an imputation procedure). These variables were summed to obtain the third index. This index can be greater than 1, because there is a potential contribution from each birth, and it will tend to be larger for women who have had more children. (Section 4.1 will look again at the incompleteness of birth dates, using births rather than women as the units of analysis.)

Table 3.1 identifies the surveys with the highest levels of incompleteness, defined by the age index being greater than 0.6, or the marriage index being greater than 0.6, or the birth index being greater than 1.0. There were 29 such surveys, representing 19 different countries. About half of these surveys are above the threshold on two or three of the indices, not just one, because there is a moderate level of positive association among the indices.

The affected surveys include all of the South Asia surveys in this study: Bangladesh (both 1996-97 and 1999-2000), India (both 1993 and 1999), Nepal (both 1996 and 2001), and the only survey conducted in Pakistan (1990-91). They include three North Africa/West Asia surveys: Morocco 1992, Egypt (1995 only), and Yemen 1991. All the remaining countries are in Western, Middle, and Eastern Africa, with the exception of the small country of Comoros, in Southern Africa.

The imputation procedures used by DHS have been carefully developed and tested, and it is likely that incomplete data on ages and dates has a relatively minor impact on rates, means, or other calculations. Imputed values are almost always within a year of any plausible alternatives, given the information that is available. Nevertheless, this problem serves as a warning about other possible deficiencies. Many of the countries and surveys in Table 3.1 appear in other tables of this report.

Figure 3.1 Incompleteness of women's age data

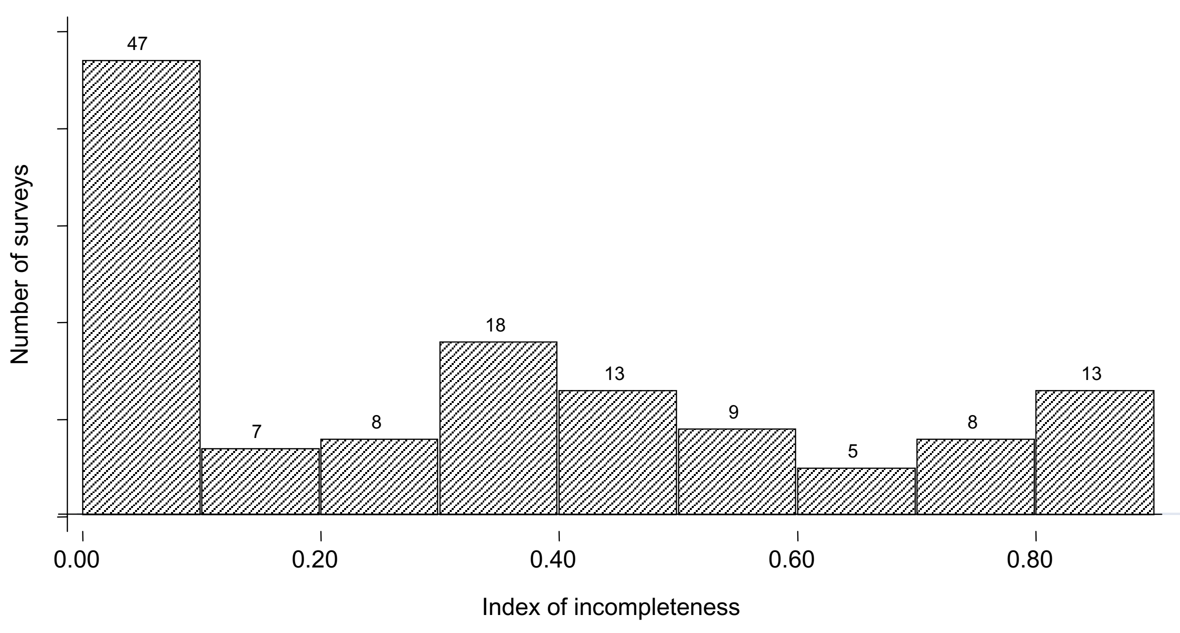


Figure 3.2 Incompleteness of women's marriage data

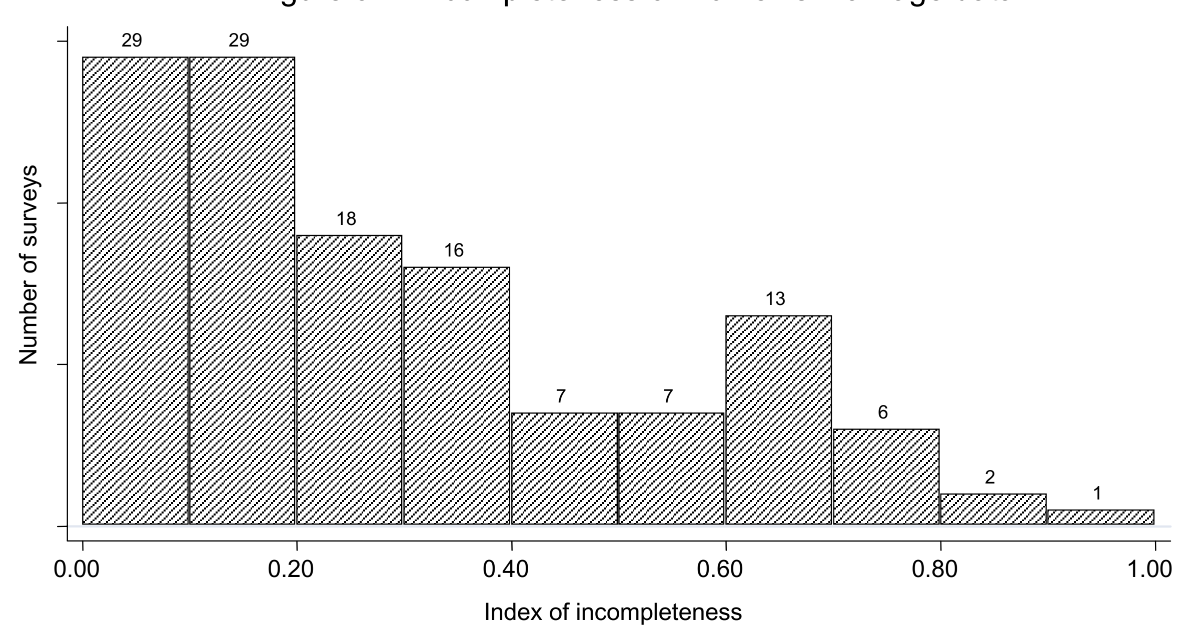


Figure 3.3 Incompleteness for women's birth history data

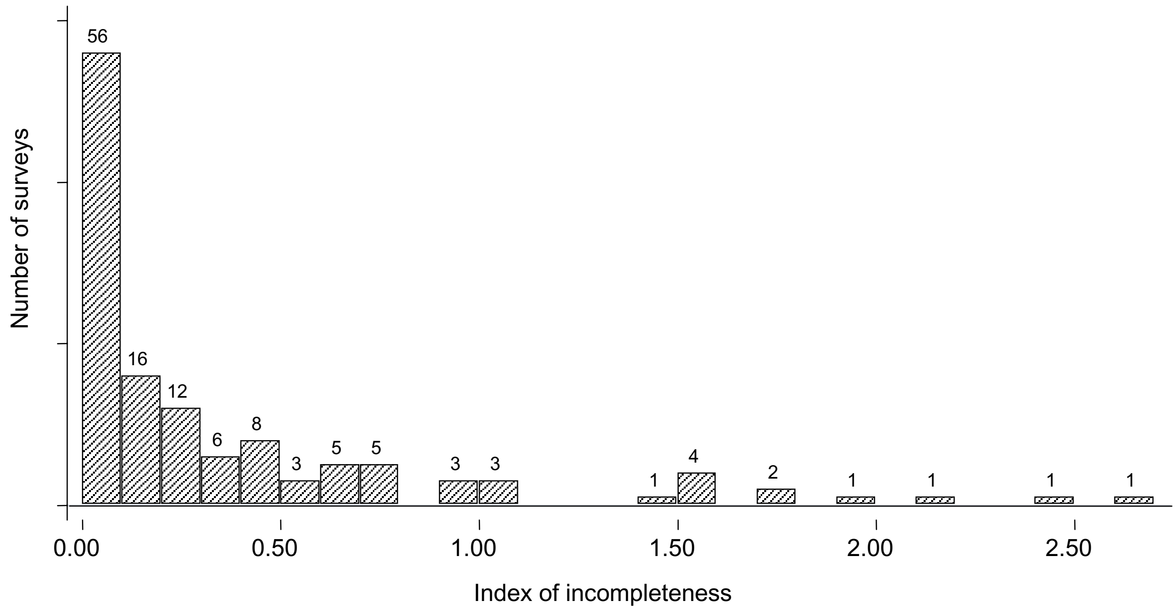


Table 3.1 Surveys of women age 15-49 with highest levels of incompleteness of age, marriage, or birth data

		Proportion of women missing any information		Sum, across births, of proportions missing any birth history
Country	Median year of survey	Age or birth date	Marriage age or date	
Bangladesh	1997	0.78	0.10	0.03
Bangladesh	2000	0.92	0.66	0.11
Benin	1996	0.81	0.69	1.74
Benin	2001	0.74	0.67	1.42
Burkina Faso	1993	0.72	0.57	0.94
Burkina Faso	1999	0.82	0.70	1.58
Burkina Faso	2003	0.81	0.71	0.28
Burundi	1987	0.62	0.30	0.60
Comoros	1996	0.66	0.45	1.00
Egypt	1995	0.51	0.38	1.01
Ethiopia[1]	1992	0.79	0.51	0.30
Guinea	1999	0.87	0.73	2.12
India	1993	0.79	na	0.10
India	1999	0.72	na	0.13
Mali	1987	0.91	0.89	2.46
Mali	1996	0.84	0.62	0.17
Mali	2001	0.81	0.62	0.26
Morocco	1987	0.87	0.77	1.77
Nepal[1]	2952	0.57	0.70	0.01
Nepal[1]	2057	0.56	0.70	0.00
Niger	1992	0.87	0.69	1.54
Niger	1998	0.86	0.71	0.22
Pakistan	1991	0.70	0.44	0.42
Rwanda	1992	0.60	0.17	0.26
Senegal	1986	0.66	0.68	0.75
Senegal	1993	0.50	0.65	1.53
Senegal	1997	0.48	0.60	0.75
Sudan	1990	0.84	0.64	1.99
Togo	1988	0.73	0.63	1.58
Togo	1998	0.69	0.64	1.02
Yemen	1991	0.88	0.85	2.71

Note: The table lists surveys for which the index of incompleteness of women's age is greater than 0.6, or the index of incompleteness of women's marriage is greater than 0.6, or the index of incompleteness of the birth history is greater than 1.0.

[1] The Ethiopian survey was conducted in 1992 in the Ethiopian calendar, 2000 in the Western calendar. The Nepalese surveys were conducted in 2052 and 2057 in the Nepalese calendar, 1996 and 2001 in the Western calendar.

na = Not applicable

To clarify the pattern of missing information, Table 3.2 gives the distributions of types of responses for age and for first union in a pooling all 128 surveys in this study. Across all those surveys, 452,052 women required some imputation of their age data. Because of the large size of the two surveys of India and endemic problems with reports of ages and dates in that country, 30 percent (135,061) of these imputations can be attributed to those two surveys. A total of 292,054 women required some imputation of their marriage or first union data. The 1992-93 survey of India did not ask about age or date at first union. The 1998-99 survey of India asked only about age at first cohabitation, not the date, and a satisfactory response was given by all but 155 women.

Table 3.2 includes a brief description of each type of incompleteness and its resolution, but some further explanation may be helpful. The data are stated in terms of age and birth date, but with rephrasing would also apply to marriage. Ideally, the woman provides an age, year of birth, and month of birth, and that information will be internally consistent and consistent with the date of interview. If this happens, DHS assigns code 1. If the responses are inconsistent, consistency is imposed, with priority given first to stated age, then to year of birth, and then to month of birth. Similarly, if an item is missing, a value is imputed with the same priorities. That is, imputation will be done if an item is given but is inconsistent with a higher priority item, as well as if it was omitted entirely. This is probably the best strategy for handling missing data, and our analysis does not lead to any suggestions for modifying the imputation procedures.

There are several possible combinations of acceptable or unacceptable responses to these three items:

DHS code 1: Current age and month and year of birth are given and are compatible.

DHS code 2: Month of birth and age are given. Year of birth is not given, or is incompatible with age and must be imputed.

DHS code 3: Year of birth and age are given. Month is not given, or is incompatible with age and must be imputed.

DHS code 4: Year of birth and age are given, but year is inconsistent with age. Year of birth, and if necessary, month of birth, are imputed.

DHS code 5: Year of birth is given. Both age and month of birth must be imputed.

DHS code 6: Age is given. Both year of birth and month of birth must be imputed.

DHS code 7: Month of birth is given. Both age and year of birth must be imputed.

DHS code 8: None given, all imputed.

DHS code 9: Not defined (found only in the 1992 survey of Namibia).

For both types of data—age and birth date—the most common type of incompleteness is that age (current age or age at first union) is given but year and month (of birth or first union) must be imputed. These cases receive DHS code 6. 14 percent and 17 percent, respectively, of all responses are of this type (this includes the cases for the 1998-99 survey of India for which code 6 applied to all responses on first unions other than the 155 cases with code 8). Another common type of incompleteness is when year and age are given, and only month must be imputed (DHS code 3), accounting for 12 percent and 8 percent, respectively, of all responses. For age, another 6 percent of cases include year and age but year is incompatible with age (DHS code 4). For marriage, another 11 percent of cases include only the year, so both age and month must be imputed (DHS code 5).

The data files do not contain information about the magnitudes of discrepancies. If, say, year of birth and current age are incompatible with the date of the survey, in which case year of birth will be revised, we expect that it usually needs only to be shifted by one or two years, but this could only be checked by comparing the files before and after imputation.

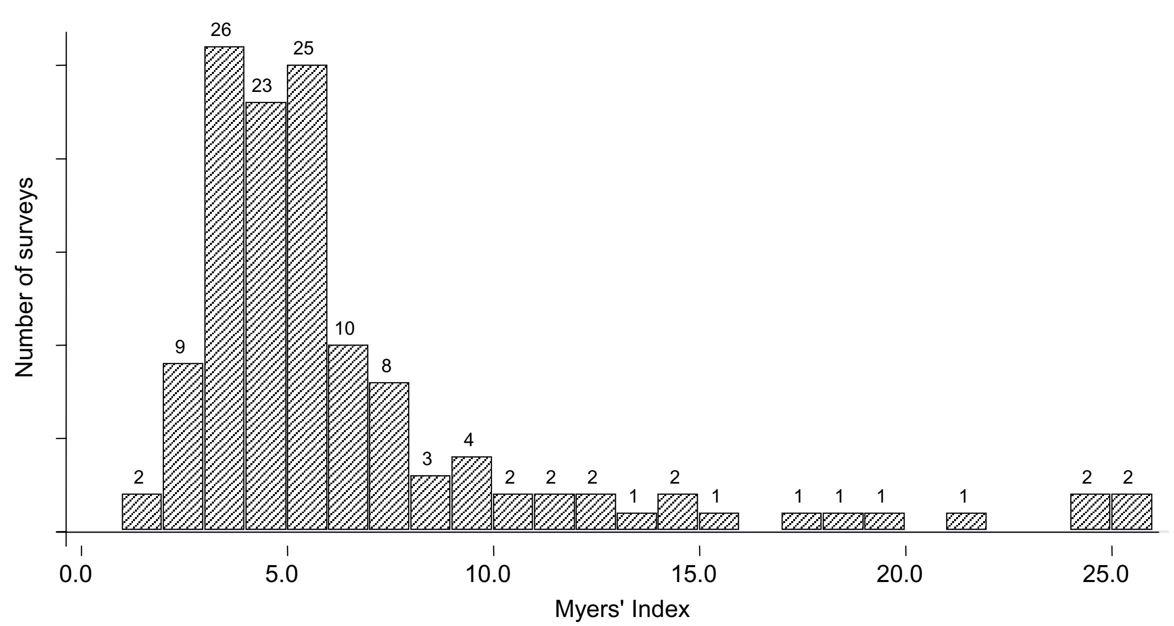
Table 3.2 Percent distribution of types of imputation of age or marriage data, across all 128 surveys of women age 15-49

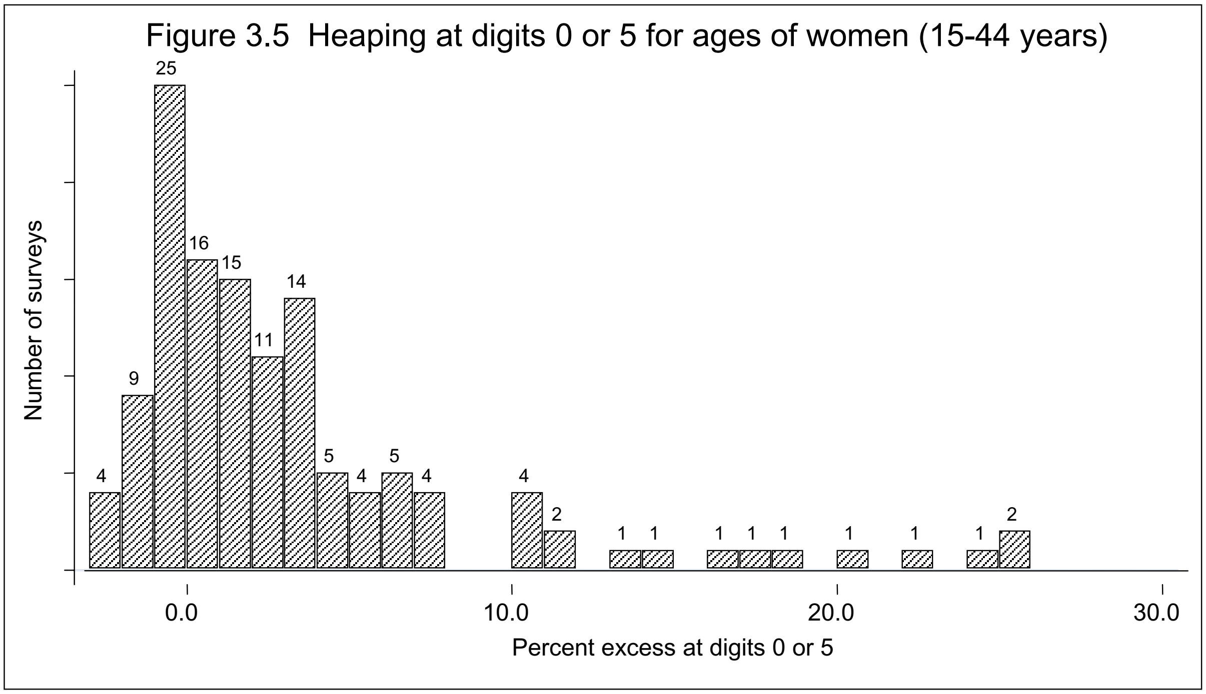
DHS code	Information	Age	Marriage
1	Age, month and year given and okay	63.6	58.8
2	Month and age given, year imputed	3.5	2.6
3	Year and age given, month imputed	12.4	7.8
4	Year and age given, year ignored	6.4	1.8
5	Year given, age and month imputed	0.0	10.6
6	Age given, year and month imputed	13.9	17.4
7	Month given, age and year imputed	0.2	0.1
8	None given, all imputed	0.1	0.8
9	Not defined	0.0	0.1
Total		100.0	100.0
Number of women		1,276,546	960,586

3.2 Age and Birth Year

All women in a main survey also appeared in the corresponding household survey, and their age distributions were analyzed in that context. We now look just at the women who were included in the main surveys, looking for evidence of heaping. These women would have provided their age information themselves, whereas in the household survey it could have come from someone else (although the eligible respondent for the main survey is often the household informant for the household survey). In order to have equal representation of all final digits, we look only at the thirty-year age interval 15-44. This age distribution is examined for overall evenness using Myers' Index (Figure 3.4) and for heaping on final digits 0 and 5 in (Table 3.4). The strong similarity between Figures 3.4 and 3.5 is due to the fact that digit preference is almost entirely in favor of 0 and 5.

Figure 3.4 Myers' Index for ages of women (15-44 years)





Surveys with measures in excess of 10 percent are given in Table 3.3. The same countries listed earlier for their conspicuous age heaping in the household schedule and incompleteness in age and date reports tend to reappear here. The list includes Pakistan 1990-91 and Yemen 1991-92, relatively early surveys and the only surveys in those two countries. Apart from Egypt 1995, all other surveys are in Western, Middle, and Eastern Africa.

Table 3.3 Surveys of women age 15-49 with highest levels of incompleteness of age, marriage, or birth data

Country	Median year of survey	Myers' Index	Percent excess at final digit 0 or 5
Benin	2001	14.2	11.4
Chad	1997	18.0	16.5
Egypt	1995	11.6	10.3
Ethiopia[1]	1992	10.1	7.8
Ghana	1988	13.1	10.4
Ghana	1993	10.2	7.7
Guinea	1999	12.0	11.6
Mali	1996	11.9	10.6
Mali	2001	12.3	10.3
Niger	1992	18.3	17.8
Niger	1998	14.0	13.9
Nigeria	1990	26.1	26.1
Nigeria	1999	19.3	18.8
Nigeria	2003	15.7	14.3
Ondo State[2]	1986	24.3	22.2
Pakistan	1991	21.8	20.6
Sudan	1990	24.6	24.6
Yemen	1991	26.0	26.0

Note: The table lists surveys for which either the Myers' Index or the percent excess at final digit or 5 is greater than 10.
[1] The Ethiopian survey was conducted in 1992 in the Ethiopian calendar, 2000 in the Western calendar.
[2] Ondo State is in Nigeria.

As mentioned earlier, it is possible that some age heaping can be traced indirectly to a preference for calendar years ending in 0 and 5, rather than ages ending in 0 and 5. Figure 3.6 gives the distribution of Myers' Index calculated for birth year. As would be expected, it is similar in shape to Figure 3.5. Figure 3.7 gives the distribution of the excess percentage at calendar years ending in 0 and 5. This figure is fairly symmetric, by comparison with Figure 3.5, which showed the preference for ages ending in 0 and 5, and has only five values above 10 percent.

Table 3.4 lists the surveys that are above 10 percent on either index. Surveys with a survey year ending in 0 or 5 are omitted because age and birth year will be confounded for them (this includes surveys conducted across two years, either of which ended in 0 or 5). If a survey shows heaping on birth years ending in 0 and 5, the country will show a high percentage in the last column of Table 3.4. The only surveys with this pattern are Guinea (1999), Ondo State (Nigeria 1986), and Yemen (1991). However, these surveys occurred in calendar years adjacent to years ending in 0 and 5, and there is enough ambiguity about the timing of the respondents' birthdays that heaping on age could be confounded with heaping on birth year.

Thus there is little evidence to support the hypothesis of a preference for birth years ending in 0 and 5. The source of heaping appears to be a preference for ages ending in 0 or 5.

Figure 3.6 Myers' Index for birth years of women (15-44 years ago)

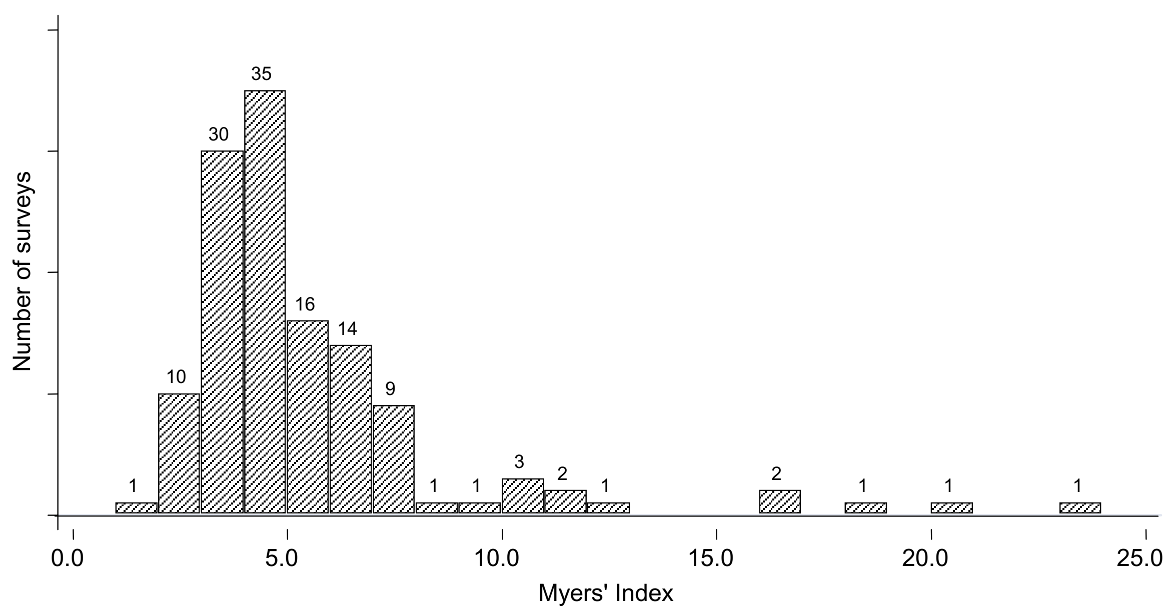


Figure 3.7 Heaping at digits 0 or 5 for birth years of women (15-44 years ago)

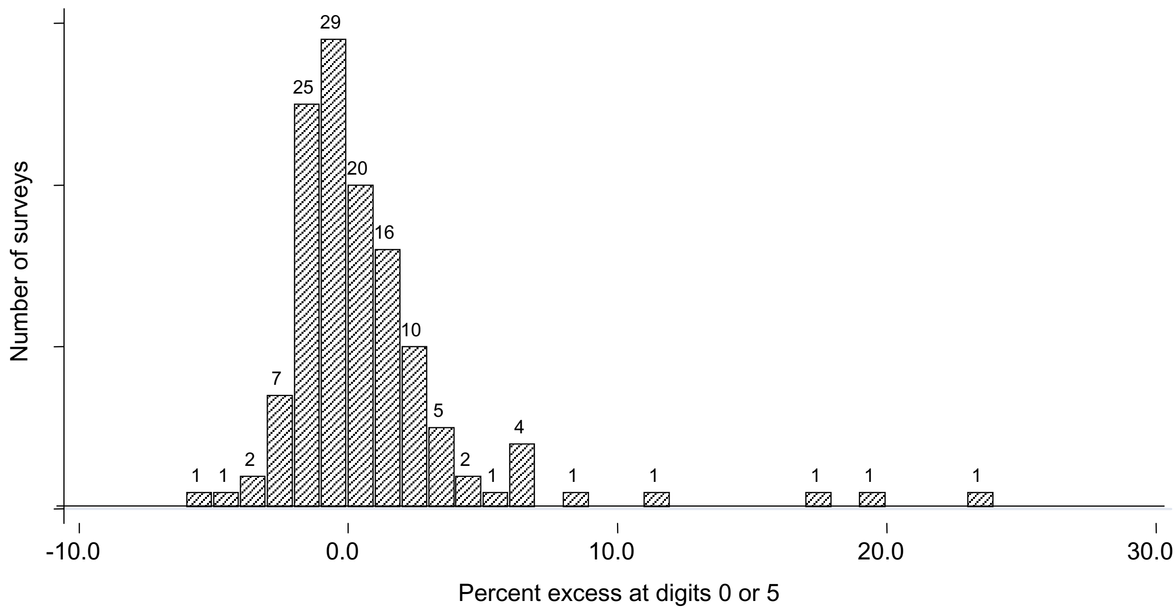


Table 3.4 Surveys of women age 15-49 with strongest evidence of heaping by birth year			
Country	Median year of survey	Myers' Index	Percent excess at final digit 0 or 5
Guinea	1999	11.7	11.7
Mali	2001	11.6	-5.2
Niger	1992	10.0	-0.0
Niger	1998	10.0	0.2
Nigeria	1999	12.9	6.6
Ondo State[1]	1986	20.1	19.1
Pakistan	1991	16.5	-3.5
Yemen	1991	24.3	24.3

Note: The table lists surveys for which either the Myers' Index or the percent excess at final digit 0 or 5 is greater than 10.
[1] Ondo State is in Nigeria.

3.3 Year of First Marriage and Years Since First Marriage

The most important date provided by a woman, other than date of birth and dates of childbirth, is the date when she was first married or lived together with a man as married (following standard practice, the term "marriage" is used regardless of whether there was a marriage ceremony). Typically, the woman is asked for the month and year when she married or started living with her first partner. If she gives at least the year, then her age at that time is imputed, and she is not asked for it. If she does not give the year, she is asked her age at that time. Date and age at first marriage were not included in the two India surveys because of the prevalence of early arranged marriages there and the frequent gap between marriage and cohabitation in these cases. (Age at first cohabitation was asked in the second India survey.)

Regardless of whether the year or age response is given, it is possible that the response is based on some calculations (by the woman or by the interviewer) involving some combination of year of marriage, years ago, and/or age at marriage. We now look for evidence of heaping and digit preference involving these data.

First, Figures 3.8 and 3.9 give the distribution of Myers' Index and the excess at final digits 0 and 5 for year of marriage, which is the preferred response. Only one survey exceeds a level of 10 percent on either measure. This survey, Nigeria 1990, is listed in Table 3.5. Most of the heaping is indeed on 0 and 5.

Second, Figures 3.10 and 3.11 give the distribution of Myers' Index and the excess percentage on final digits 0 and 5 for implied years since marriage (calculated as the year of interview minus the stated year of marriage, ignoring months). No survey exceeds the 10 percent threshold; the maximum is about 7 percent.

Third, there may be heaping or digit preference of age at marriage, but our methods are unable to evaluate it. In every survey, the reported age at first marriage is compressed into a very narrow age range, usually less than ten years, such as 15-24.

This section thus finds no noteworthy evidence of heaping or digit preference that directly or indirectly involves age at marriage.

Table 3.5 Survey of women age 15-49 with strongest evidence of heaping by year of marriage

Country	Median year of survey	Myers' Index	Percent excess at final digit 0 or 5
Nigeria	1990	10.8	9.3

Note: The table lists the only survey for which either the Myers' Index or the percent excess at final digit 0 or 5 is greater than 10.

Figure 3.8 Myers' Index for year of marriage (15-44 years ago)

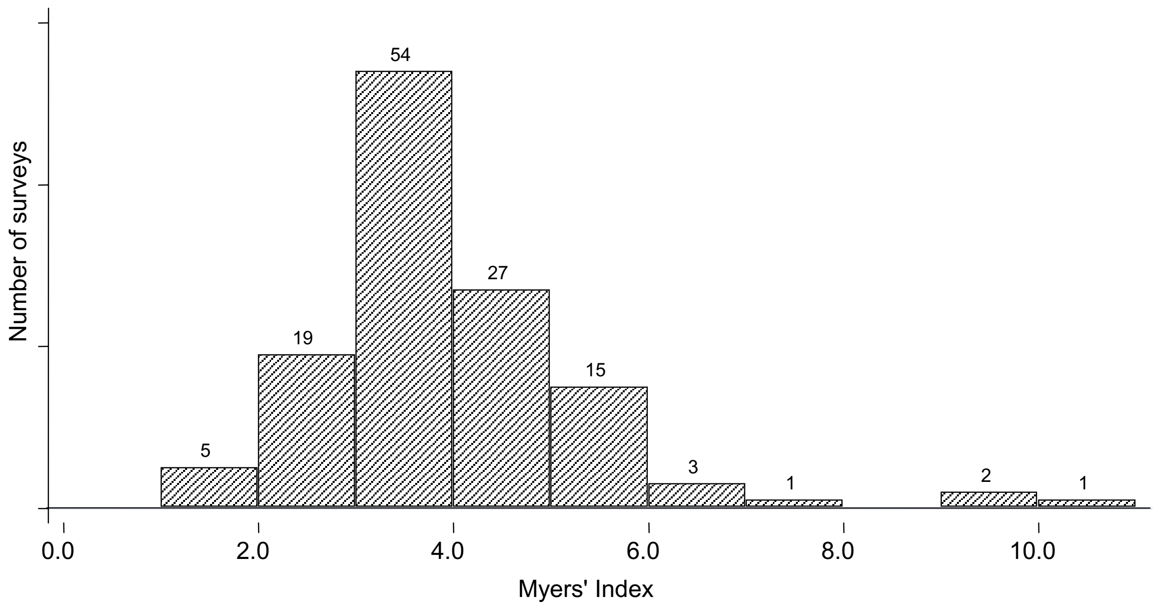
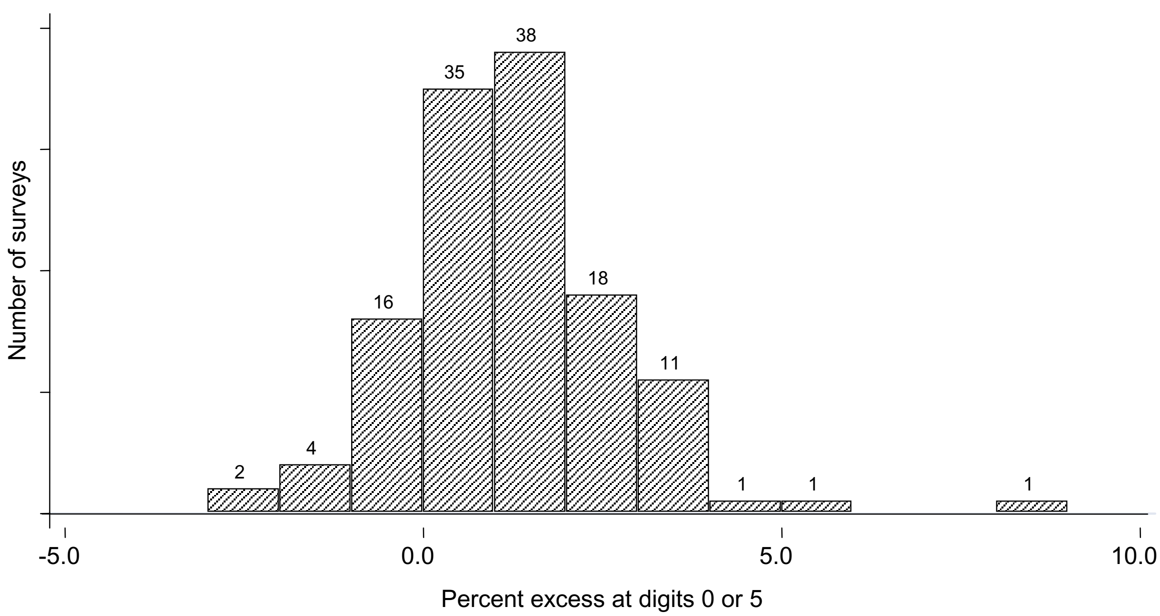


Figure 3.9 Heaping at digits 0 or 5 for year of marriage (15-44 years ago)



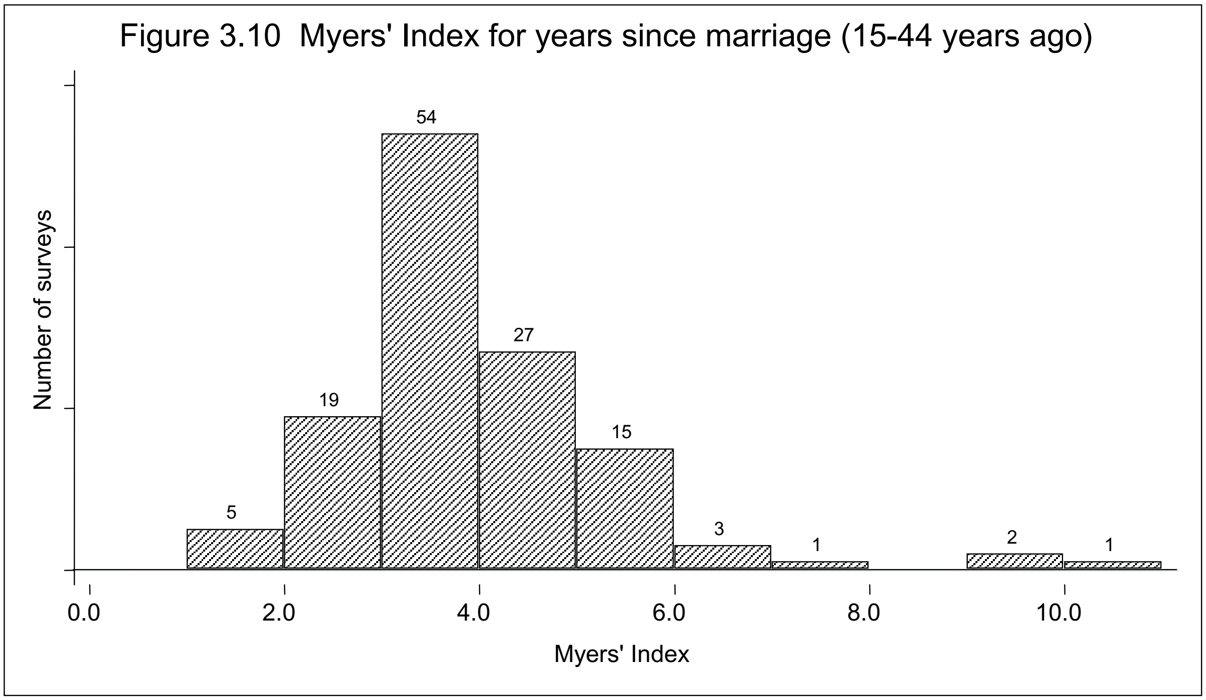


Figure 3.10 Myers' Index for years since marriage (15-44 years ago)
Number of surveys
5
19
54
27
15
3
1
2
1
0.0
2.0
4.0
6.0
8.0
10.0
Myers' Index

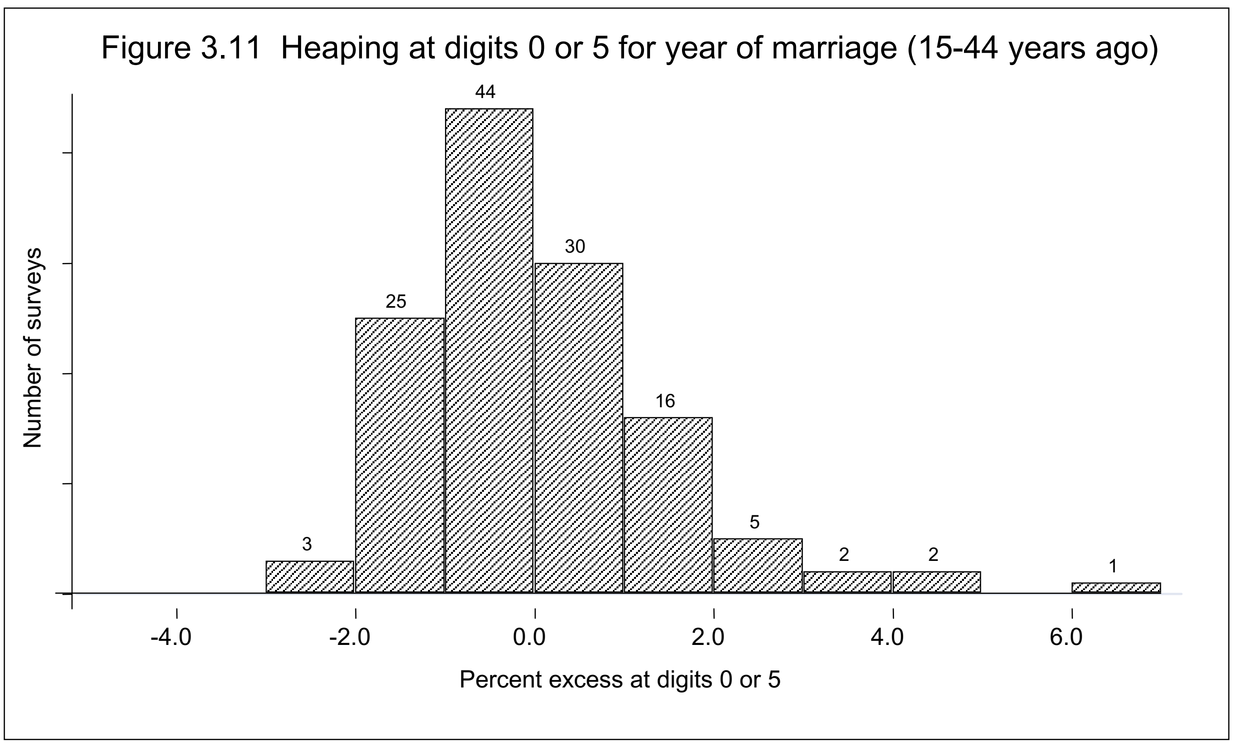


Figure 3.11 Heaping at digits 0 or 5 for year of marriage (15-44 years ago)
Number of surveys
3
25
44
30
16
5
2
2
1
-4.0
-2.0
0.0
2.0
4.0
6.0
Percent excess at digits 0 or 5

3.4 Fertility Rates from Successive Surveys in the Same Country

An important function of DHS surveys is to monitor levels and trends in fertility. All of the main reports give the age-specific rates and Total Fertility Rate (TFR, five times the sum of the age-specific rates) for a period before the survey, generally the three years prior to the date of interview. These estimates are subject to error if women's ages are misreported, if some children are omitted, or if the dates of recent childbirths tend to be shifted systematically. One of the best ways to assess such misreporting is to reestimate those rates using a subsequent survey in the same country.

This section uses 67 pairs of successive DHS surveys in the same country, usually about five years apart, to identify such discrepancies. The definition of "recent fertility" is only slightly different from that used by DHS: the three calendar years prior to the median year of interview in the first survey, rather than the three years (36 months) prior to each respondent's date of interview. Age-specific rates and the TFR for ages 15-44 are calculated for this reference period from the birth histories in the first survey, and are then reestimated from the birth histories in the second survey, using statistical methods described in Section 1.2.

Figure 3.12 describes the difference as the TFR estimate from the second survey minus that from the first survey. Thus, if the second survey gave a higher estimate than the first, the difference in this figure will be a positive number. Figure 3.13 is similar but expresses the difference as a relative difference: the difference in Figure 3.13 is divided by the TFR estimate from the first survey and multiplied by 100. Figure 3.14 gives a chi-square test statistic for the differences between the two sets of age-specific rates. This chi-square has six degrees of freedom, and the .05 critical value for the null hypothesis that the estimates agree is 16.8. The intervals in Figure 3.13 are multiples of 16.8, so this null hypothesis would be rejected for all surveys outside the first interval. (The figure omits three surveys with chi-square values above 400, but they are included in Table 3.6.) Note that this test statistic does not test whether the TFR estimates differ. Frequently the differences in the age-specific rates have different signs in different age groups that tend to cancel one another out in the summation. Some pairs have very large chi-square statistics but only modest differences in TFR estimates.

A little more than half of the pairs have a difference in TFRs that is less than half a child in absolute value, relative differences that are less than 10 percent in absolute value, and acceptable chi-square test statistics (less than 16.8). Most analysts would probably be satisfied with this level of agreement.

Table 3.6 lists those pairs of surveys for which the difference in Figure 3.12 was one of the "worst" six (about 10 percent of all pairs) by any of the three measures that appear in the figures. This table omits Brazil because its first survey, conducted in 1991, was not national and the differences do not necessarily imply measurement errors. Some changes in coverage may account for other apparent differences.

Some discrepancies are very large and reflect poorly on the quality of the data. Table 3.6 shows that the arithmetic difference between the two TFR estimates is as large as 2.08 children (for Burkina Faso 1992-93 and 1998-99) and 1.80 children (for Mali 1995-96 and 2001). Other countries in this table (ignoring Brazil, for which the comparison is questionable) are Benin (1996 and 2001), Egypt (1995 and 2000), Indonesia (1991 and 1994; 1994 and 1997; 1997 and 2002), Morocco (1987 and 1992), Mozambique (1997 and 2003), Nigeria (1999 and 2003), Niger (1992 and 1998), Peru (1992 and 1996), Senegal (1986 and 1992-93), and Turkey (1993 and 1998). African countries predominate, but the presence of Indonesia and Turkey make this a truly international list.

There is a tendency for the differences to be positive, so that the second survey produces a larger TFR for the reference period than the first survey does. In some instances (Egypt, Indonesia, Morocco, and Turkey) the difference is negative. When a negative difference occurs, it is small but highly significant, implying that the age-specific differences are large but inconsistent in sign. Indonesia appears in three pairings of surveys. Each time, the second survey gave a lower estimate than the first survey, by an amount .39, .42, and .20 of a child, respectively. Pairs of surveys tend to agree in the amount of *change* in the TFR over time.

Discrepancies between surveys probably stem from systematic errors in the statements of children's birth dates in one or both surveys. For example, upward age transfers can arise from the use of a calendar or window for detailed questions about child health, or other indicators. The interviewer may be motivated to reduce her workload by placing some children above the upper age boundary for those questions. This type of transfer has already been mentioned and will be discussed further in Section 4.2.

Upward age transfers are also endemic in some cultures, regardless of whether any extra information is to be gathered for young children. This phenomenon has long been observed for Indonesian surveys and censuses. It has also been observed in Pakistan, although Pakistan had only one survey and cannot be included in this particular comparison.

Tables 3.7 and 3.8 give more detail on the two pairs of surveys that show the largest discrepancies in TFR estimates: Burkina Faso (1992-93 and 1998-99) and Mali (1995-96 and 2001). For both of these pairs, all age-specific estimates are higher in the second survey than in the first one. The distortion is almost certainly due to a shifting of birth dates that is fairly uniform across different ages of women.

A more thorough analysis of all pairs of surveys in Table 3.6 is recommended, looking at age groups, cohorts of women, and all overlapping calendar years in more detail. Without such checks it is impossible to tell whether discrepancies are due to age displacement in the first survey, the second survey, or both surveys.

Figure 3.12 TFR for second survey minus TFR for first survey, for ages 15-44

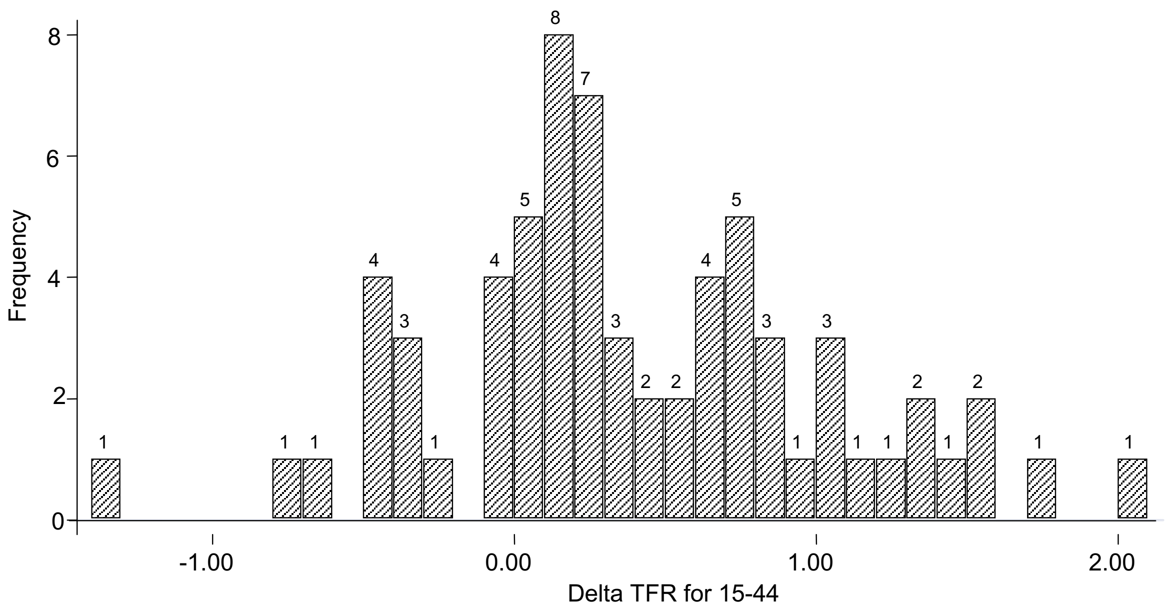


Figure 3.13 Percentage difference between TFR estimates, for ages 15-44

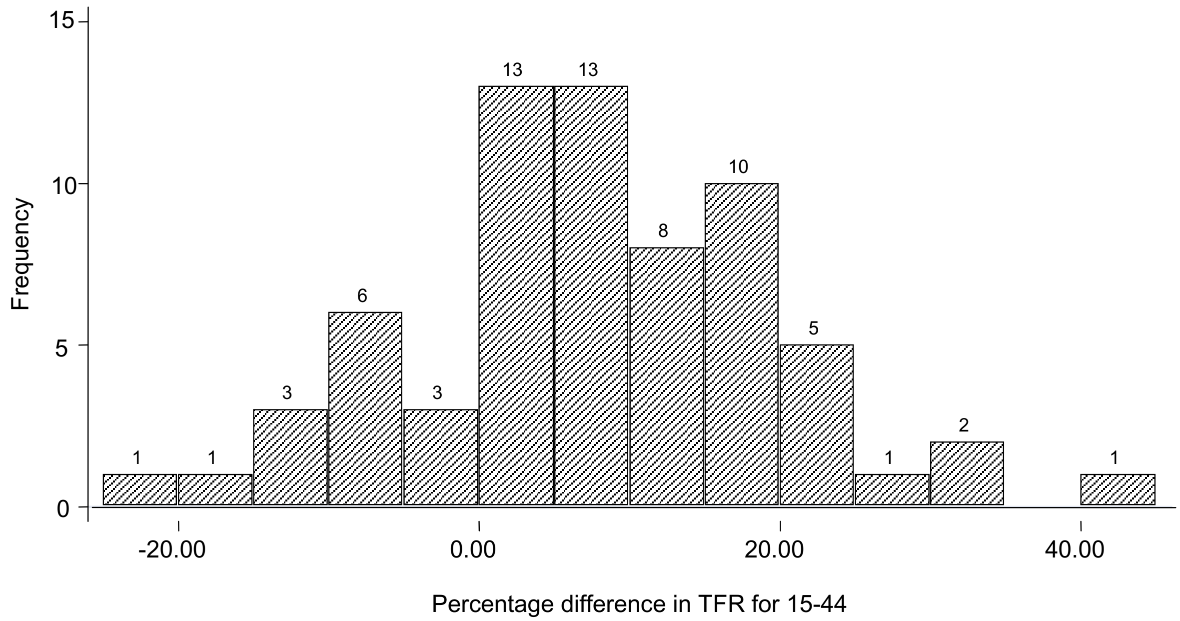
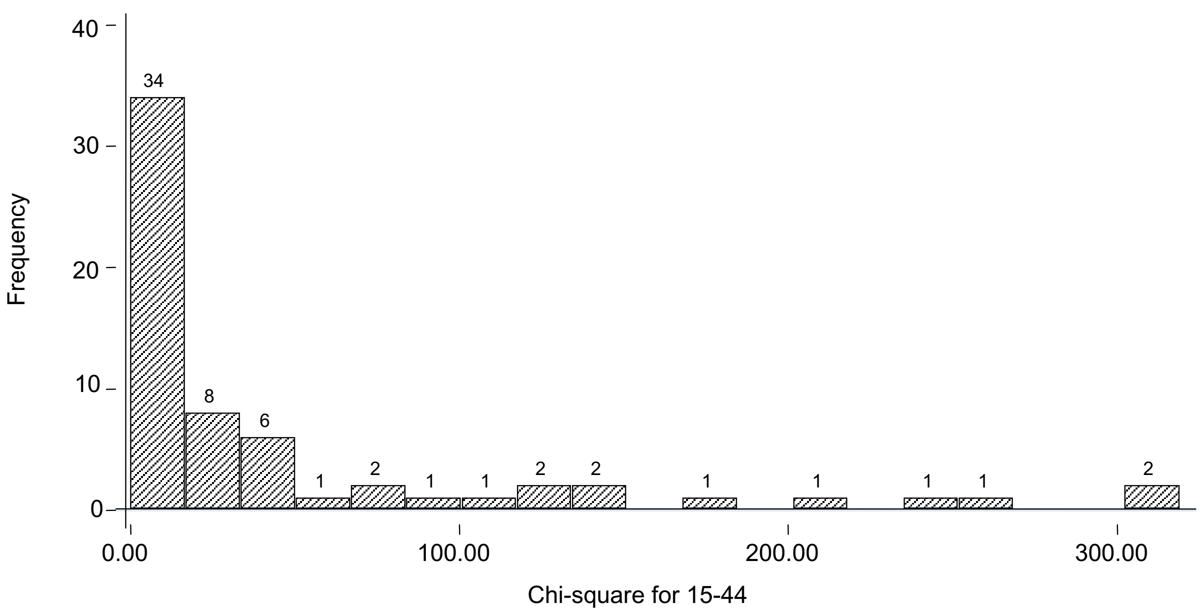


Figure 3.14 Chi-square test of differences between ASFRs for ages 15-44



Note: Three surveys with chi-square values over 400 are omitted.

Table 3.6 Pairs of surveys with greatest discrepancies between fertility rates in the reference period

Country	Century month code: Beginning month of 3-year window	Century month code: Final month of 3-year window	Median year: First survey	Median year: Second survey	TFR (15-44): Window estimated from first survey	TFR (15-44): Window estimated from second survey	Difference	Relative difference as a percentage	Chi-square for difference between age-specific fertility rates[1]
Benin	1117	1152	1996	2001	5.76	7.15	1.39	24.10	65.22
Burkina Faso	1081	1116	1993	1999	6.23	8.32	2.08	33.40	117.99
Egypt	1105	1140	1995	2000	4.71	4.31	-0.40	-8.50	1058.29
Indonesia	1057	1092	1991	1994	3.93	3.54	-0.39	-9.90	310.14
Indonesia	1093	1128	1994	1997	3.76	3.35	-0.42	-11.20	314.04
Indonesia	1129	1164	1997	2002	3.55	3.35	-0.20	-5.60	425.49
Mali	1117	1152	1996	2001	6.54	8.34	1.80	27.50	123.73
Morocco	1009	1044	1987	1992	6.21	4.90	-1.31	-21.10	2547.75
Mozambique	1129	1164	1997	2003	4.97	6.51	1.54	31.00	13.28
Niger	1069	1104	1992	1998	6.85	8.24	1.40	20.40	10.11
Nigeria	1153	1188	1999	2003	4.75	5.92	1.16	24.40	74.88
Senegal	997	1032	1986	1993	6.40	7.86	1.46	22.80	30.31
Turkey	1081	1116	1993	1998	3.51	3.20	-0.30	-8.50	428.61

Note: The table lists pairs of surveys in the highest decile of values of the absolute difference, the absolute relative difference, or chi-square.
[1] Six degrees of freedom; .01 critical value is 16.8

Table 3.7 Fertility rates for the period 1990-1992 in Burkina Faso estimated from surveys conducted in 1992-93 and 1998-99

Five-year age interval	Estimated age-specific fertility rates: 1993 survey	Estimated age-specific fertility rates: 1999 survey	Difference 1999-1993	z statistic for (4), adjusted for sample weights and clustering
15-19	146.6	181.8	35.2	3.21
20-24	281.7	354.0	72.3	5.80
25-29	282.4	353.5	71.1	6.13
30-34	233.9	332.5	98.6	4.80
35-39	196.9	266.0	69.1	3.22
40-44	105.4	175.9	70.5	1.75
15-44	6.23[a]	8.32[a]	2.08	117.99[b]

[a] Total fertility rate (15-44)
[b] Sum of squares of z scores, which has a chi-square distribution

Table 3.8 Fertility rates for the period 1993-1995 in Mali estimated from surveys conducted in 1995-96 and 2001

Five-year age interval	Estimated age-specific fertility rates: 1996 survey	Estimated age-specific fertility rates: 2001 survey	Difference 2001-1996	z statistic for (4), adjusted for sample weights and clustering
15-19	185.5	225.3	39.8	3.94
20-24	294.9	337.1	42.2	6.10
25-29	293.2	350.5	57.3	5.88
30-34	241.7	308.9	67.2	4.50
35-39	200.2	260.0	59.8	3.50
40-44	92.1	185.7	93.6	1.98
15-44	6.54[a]	8.34[a]	1.80	123.73[b]

[a] Total fertility rate (15-44)
[b] Sum of squares of z scores, which has a chi-square distribution

4 Children in the Birth Histories

This chapter examines the ages, dates, and intervals that are given within the birth histories collected from the 128 surveys of women age 15-49. The data file contains all births in these birth histories. Topics include the completeness of the responses; heaping on birth dates and ages, and transfers outside the calendar interval for the additional information; heaping of the length of preceding birth intervals; heaping of the age at child death; and a comparison between two successive surveys' estimates of infant mortality in a five-year reference period prior to the first survey.

4.1 Incompleteness

Each birth was given a code by DHS to indicate the completeness of information about the birth date of the child and the amount of imputation that was required. For this report, the information is considered incomplete if anything less than the month and year of birth was provided. The distribution of the proportion of births that were incomplete is described in Figure 4.1.

Much the same information was given in the summary measure on birth dates in Figure 3.1 and Table 3.1. However, in that context a country with high fertility—i.e., more births per woman—would tend to get a higher score. Here, with births as the unit, there is no confounding with the level of fertility. A lower threshold is used here. Surveys with a woman's birth-date incompleteness index of 1.0 or more were included in Table 3.1, but if the child's incompleteness index exceeded .2—that is, if 20 percent or more of the births were incomplete—the survey is listed in Table 4.1.

Twenty-four surveys, representing 17 countries, exceeded this threshold. There is considerable overlap with the surveys listed in Table 3.1, except that no South Asia surveys are above the .2 threshold. Except for Egypt, Morocco, Yemen, and the earliest of the Indonesian surveys, all countries are in Western and Middle Africa.

Figure 4.1 Incompleteness of birth data

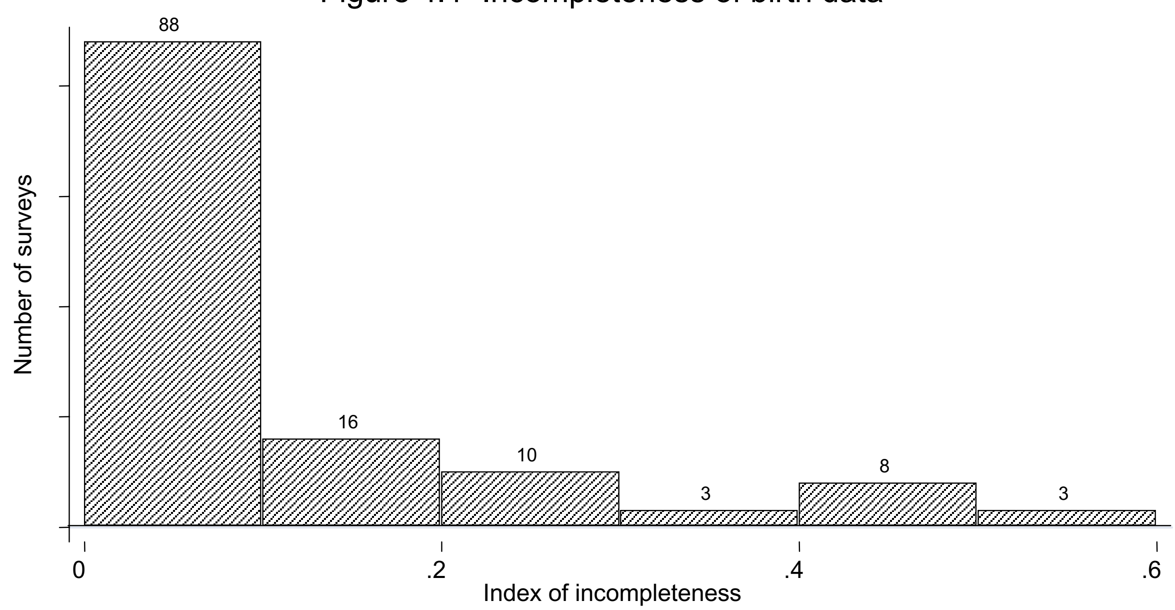


Table 4.1 Surveys with highest levels of missing data in birth histories

Country	Median year of survey	Proportion of births with missing data
Benin	1996	.470
Benin	2001	.426
Burkina Faso	1993	.272
Burkina Faso	1999	.435
Cameroon	1991	.283
Comoros	1996	.343
Egypt	1995	.264
Ghana	1988	.230
Ghana	1993	.203
Guinea	1999	.592
Indonesia	1987	.234
Madagascar	1997	.206
Mali	1987	.625
Morocco	1987	.418
Mozambique	1997	.313
Niger	1992	.403
Sudan	1990	.452
Senegal	1986	.218
Senegal	1993	.435
Senegal	1997	.218
Tanzania	1991	.218
Togo	1988	.465
Togo	1998	.307
Yemen	1991	.558

Note: The table lists surveys for which the proportion missing exceeds .2.

On average, about 11 percent of the births in a survey had incomplete information about date or age. Across the full set of 128 surveys, 392,450 births (out of 3,688,462) received some kind of imputation. The distribution of imputation types is given in Table 4.2. (See Chapter 3, Section 3.1 for the meaning of the DHS codes.)

The most common type of imputation, affecting about 4 percent of births (or 38 percent of the births that required any imputation at all) involved just the imputation of the calendar month of birth (DHS code 3). This is a relatively innocuous type of imputation that should have only a trivial effect on most kinds of calculations from the birth histories because the calendar year of birth is accepted. Year of birth is also unaffected for DHS code 5, which applies to 3 percent of all births (27 percent of imputed births). The next largest category is DHS code 4, in which both year and month are modified to become consistent with the stated age of the child. There is more potential impact on various rates from this kind of adjustment, but it only affects 2 percent of all births (19 percent of imputations). These three categories together account for 83 percent of all imputations. About 0.3 percent of all births have code 8, which is most serious, because both age and year of birth must be imputed. All types of incompleteness become more common as the elapsed time between the birth and the interview increases.

Table 4.2 Percent distribution of types of imputation of children's birth dates across all 128 surveys of women age 15-49, by years since birth (unweighted)

DHS code	Information	Years since birth 0-9	10-19	20+	Total
1	Month and year given and okay	93.2	86.9	82.9	89.4
2	Month and age given, year imputed	0.4	0.6	0.8	0.5
3	Year and age given, month imputed	2.4	5.1	6.7	4.1
4	Year and age given, year ignored	1.4	2.5	2.6	2.0
5	Year given, age and month imputed	1.6	3.4	5.2	2.8
6	Age given, year and month imputed	0.3	0.4	0.5	0.4
7	Month given, age and year imputed	0.5	0.7	0.8	0.6
8	None given, all imputed	0.2	0.3	0.5	0.3
Total		100.0	100.0	100.0	100.0

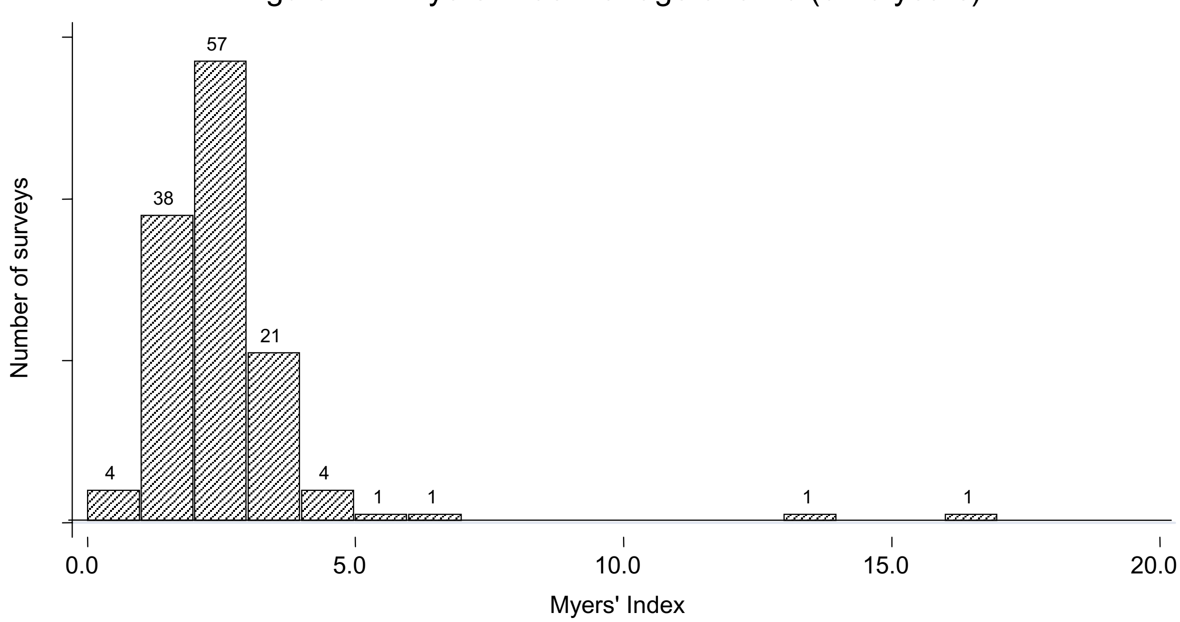
Number of births = 3,688,462

4.2 Birth Dates and Ages of Children

The birth histories include the ages of children who were still alive at the date of interview. The distribution of Myers' Index and the percentage excess at final digits of age 0 and 5 are shown in Figure 4.2 and Figure 4.3, respectively. Surveys exceeding a threshold of 10 percent on either measure are listed in Table 4.3.

There is virtually no evidence of heaping on age of child. Only two surveys exceed the threshold of 10 percent on either measure. The relatively symmetric distribution of the second measure, shown in Figure 4.3, is due to the fact that the small deviations from a uniform distribution tend *not* to be at final digits 0 and 5. The two surveys in Table 4.3 are El Salvador 1985 and Ondo State, Nigeria 1986—two of the very earliest DHS surveys.

Figure 4.2 Myers' Index for age of child (0-29 years)



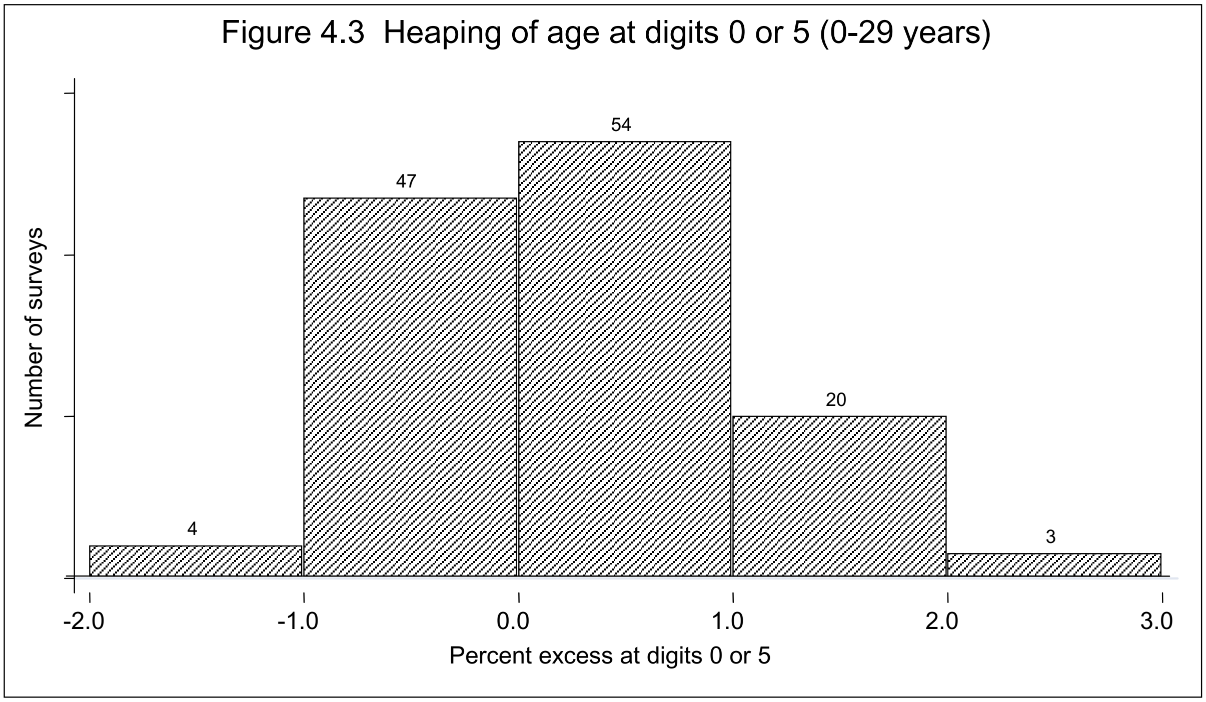


Table 4.3 Surveys with highest levels of heaping on children's ages

Country	Median year of survey	Myers' Index	Percent excess at final digit 0 or 5
El Salvador	1985	13.1	1.5
Ondo State[1]	1986	16.5	-1.0

Note: The table lists surveys for which either the Myers' Index or the percent excess at final digit 0 or 5 is greater than 10.
[1] Ondo State is in Nigeria.

Although there is virtually no evidence of heaping on children's age, year of birth does not fare so well. Figure 4.4 and Figure 4.5 give the distribution of Myers' Index and the percentage excess at final digits 0 and 5 for year of birth. No surveys have a conspicuous excess at final digits 0 and 5, but 27 surveys have Myers' Index above 10 percent and are listed in Table 4.4. This list includes surveys from 21 countries. Apart from early surveys in Brazil (1991) and El Salvador (1985) and a more recent one in Guatemala (1998-99), all these surveys are from sub-Saharan Africa. All three of the Uganda surveys, all three of the Zambia surveys, and both of the Madagascar surveys show strong heaping of children's year of birth.

Figure 4.4 Myers' Index for year of birth (0-29 years ago)

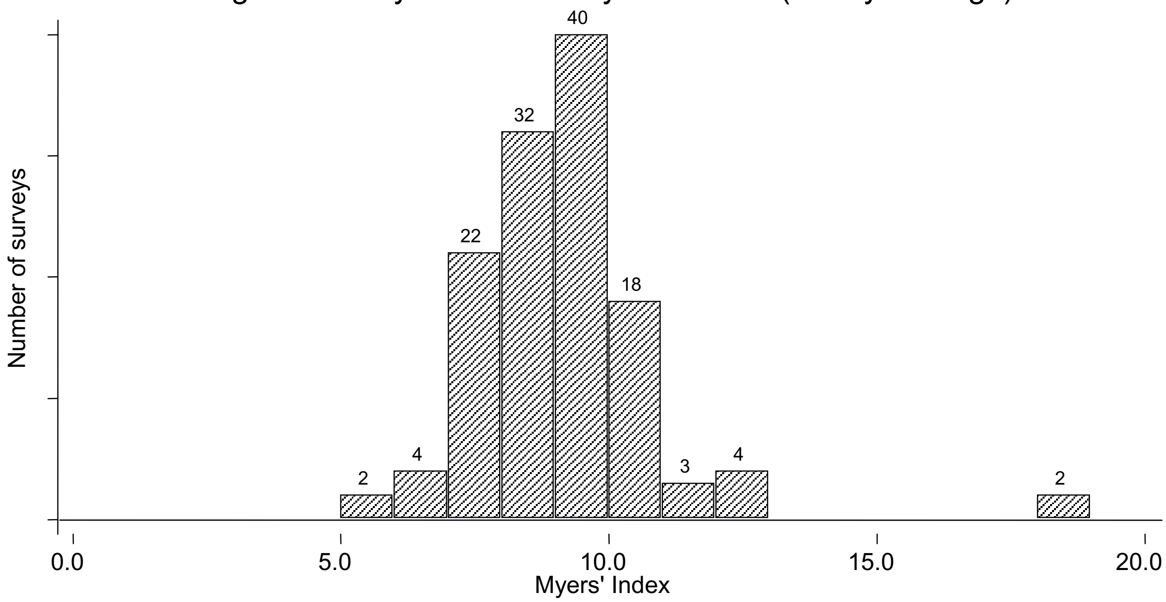


Figure 4.5 Heaping of birth years at digits 0 or 5 (0-29 years ago)

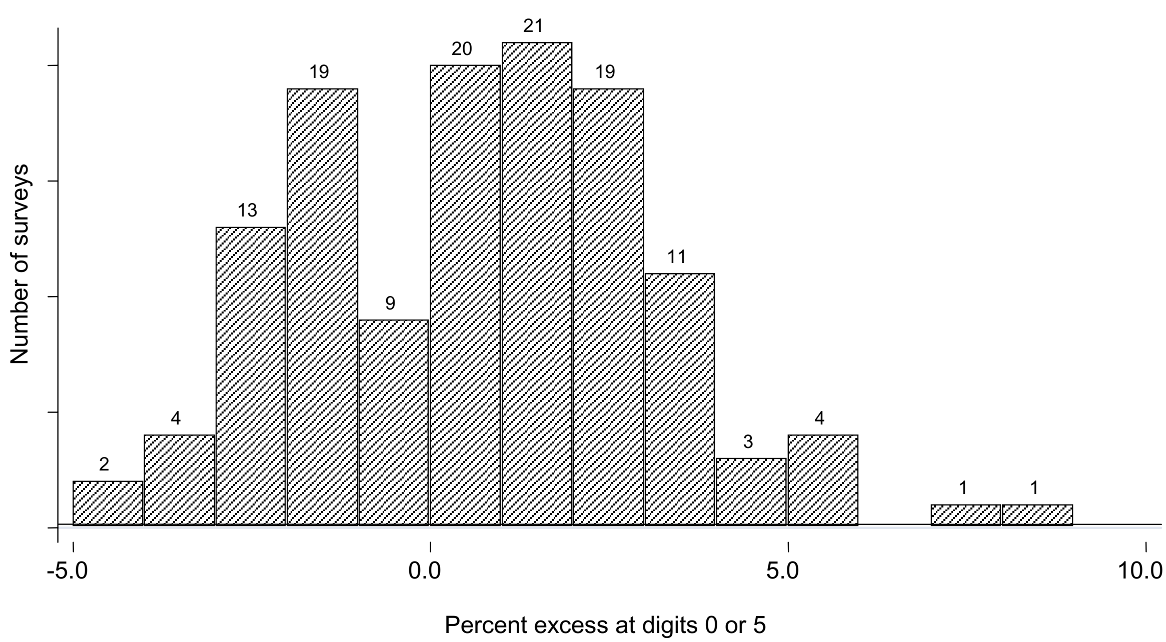


Table 4.4 Surveys with highest levels of heaping on birth year in the birth histories			
Country	Median year of survey	Myers' Index	Percent excess at final digit 0 or 5
Brazil[1]	1986	10.1	3.3
Burkina Faso	1993	10.3	1.9
Burundi	1987	10.9	2.1
Chad	1997	10.5	3.5
El Salvador	1985	18.5	3.7
Ghana	1988	10.5	0.2
Guatemala	1999	10.0	0.6
Kenya	1998	10.0	0.2
Kenya	2003	11.5	0.2
Liberia	1986	12.0	7.8
Madagascar	1992	10.3	2.2
Madagascar	1997	10.3	1.4
Malawi	2000	12.1	4.9
Namibia	1992	10.5	2.3
Ondo State[2]	1986	19.4	8.7
Rwanda	2000	10.5	3.2
Senegal	1986	10.1	4.4
Tanzania	1991	10.5	3.0
Tanzania	1996	10.1	4.7
Togo	1988	10.1	0.1
Uganda	1988	11.1	0.4
Uganda	1995	12.1	1.6
Uganda	2001	11.5	4.4
Zambia	1992	10.1	1.7
Zambia	1996	12.2	5.1
Zambia	2002	10.0	2.8
Zimbabwe	1988	10.3	0.3

Note: The table lists surveys for which either the Myers' Index or the percent excess at final digit 0 or 5 is greater than 10.
[1] The 1986 Brazil survey was restricted to the northeastern part of the country.
[2] Ondo State is in Nigeria.

The next type of potential problem with children's ages to be considered is transfers outside a window or calendar of eligibility for the special questions on child health. This has long been known to be a risk because interviewers may be motivated to reduce their workload by shifting children to a higher age. The issue and methods to estimate its prevalence were introduced in Section 1.2 of this report.

Except for the earliest surveys, the standard recode file includes a variable in the survey of women 15-49 (v017) that gives the century month code for the start of the calendar for contraception and the proximate determinants. It is generally January of the fifth full calendar year preceding the year when interviewing began.

The standard recode file does not include a specific code for the start of the child health questions—that is, the earliest month of birth for which a child would become eligible for the child health questions. It is recommended that such a code be added to the standard recode file. It is also generally January of the fifth full calendar year preceding the year when interviewing began, but sometimes (for six surveys) was in the fourth preceding calendar year and sometimes (in twenty surveys) was in the third preceding calendar year. There were four other exceptions (Institute for Resource Development, 1990:90): for the 1987 survey in Mali, the calendar started in March 1982; for the 1986 survey in Senegal, the calendar started in April 1981; for the 1988 survey in Tunisia, the calendar started in May 1983; and for the 1988 survey in Togo, the calendar started in June 1983.

Data on child health are collected for all children born in or after this beginning month, but such data are discarded if the birth date is more than 59 months before the month of interview. For example, if an interview occurred in April 1998, the data on child health (for a typical survey) would have been collected for births occurring in January 1993 and later. After the fieldwork, data on births in January through April of 1993 would have been discarded. Months 0 to 59 before the interview would be May 1993 through April 1998.

In terms of the interviewer's workload, the first month of the child health questions is pivotal. Displacement of births refers to any tendency to transfer births which actually occurred in that month, or later, backwards into an earlier month. This tendency was assessed for 125 surveys—all surveys except Ecuador 1987 and Indonesia 1987, the only surveys that did not include child health questions, and India 1998-99, because it used different start dates in different states.

In 34 surveys it appears that at least 10 percent of the children actually born in the earliest year of the calendar were transferred to the preceding year. For no survey is there a suggestion of more than a 10 percent shift in the other direction (that is, into the window). The distribution of the transfer rate is given in Figure 4.6, and the surveys over the 10 percent threshold are listed in Table 4.5.

The majority of the listed surveys are in sub-Saharan Africa. The exceptions are Egypt 2000, Guatemala 1987, Haiti 1994-95, Indonesia 1997 and 2002, Pakistan 1990-91, Turkey 1993, and Vietnam 1997. The Pakistan survey has the highest transfer rate of all, 28 percent, but it is also one of the earliest surveys on the list. For all the surveys in Table 4.5, substantially more births were reported for the year just before the window (column 5 of the table) than for the preceding year (column 4) or the following year (column 6), which is the earliest year in the window.

Several of the countries on this list also appeared in Table 3.6, because of discrepancies between successive surveys in their estimates of fertility rates in a window before the first survey. Several also appeared on lists of countries with other kinds of age transfers.

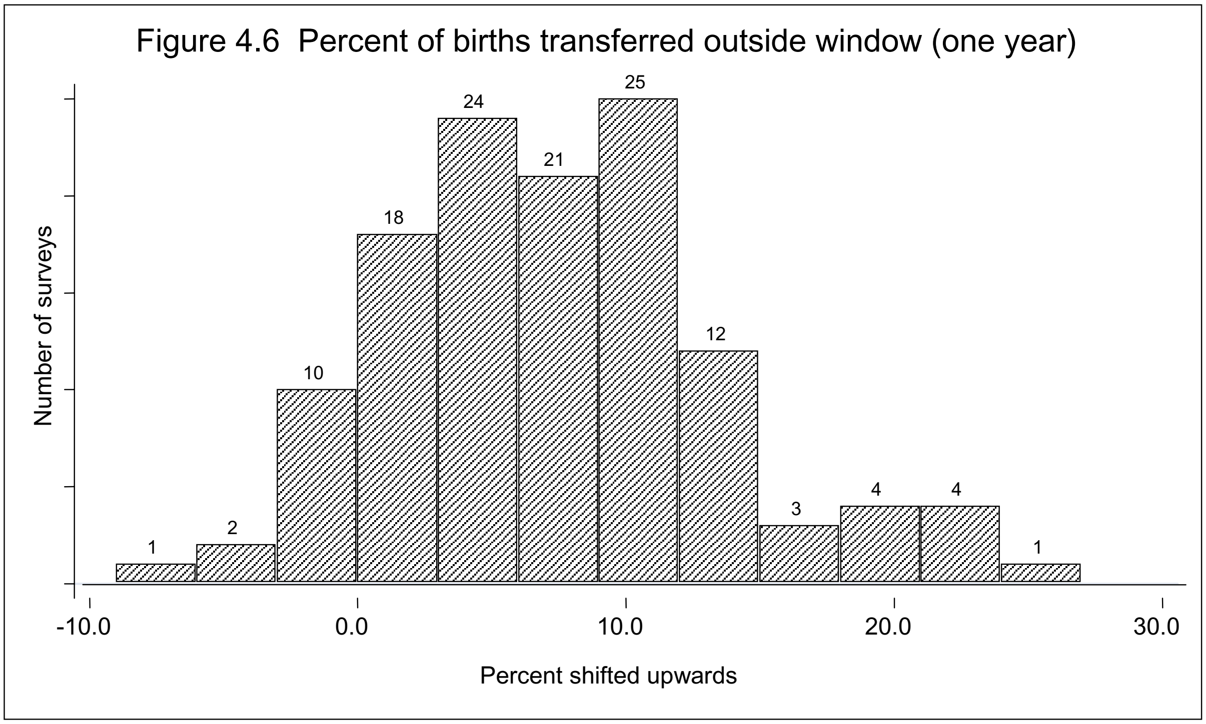


Figure 4.6 Percent of births transferred outside window (one year)
Number of surveys
1
2
10
18
24
21
25
12
3
4
4
1
-10.0
0.0
10.0
20.0
30.0
Percent shifted upwards

Table 4.5 Surveys with strongest evidence of transfer outside the calendar

Country	Median year of survey	Estimated percentage of births in year x misreported at x-1	Observed number of births				Estimated "true" number of births	
			Year x-2	Year x-1	Year x	Year x+1	Year x-1	Year x
Burkina Faso	1993	21.3	1126.5	1417.9	922.2	1134.3	1168.7	1171.4
Burkina Faso	1999	12.7	1193.5	1356.2	1059.1	1225.2	1202.4	1212.9
Burkina Faso	2003	13.2	2222.2	2425.6	1864.3	2241.9	2141.8	2148.1
Burundi	1987	13.5	720.8	861.3	655.4	718.5	758.7	757.9
Chad	1997	18.2	1303.0	1669.3	1189.4	1444.3	1404.8	1453.9
Comoros	1996	11.6	395.0	464.0	368.0	396.0	415.8	416.2
Côte d'Ivoire	1994	13.2	1487.4	1614.3	1218.2	1411.2	1428.6	1403.8
Côte d'Ivoire	1999	13.7	381.7	456.1	334.8	340.4	403.0	387.9
Egypt	2000	12.0	2197.4	2553.8	2028.2	2275.6	2277.6	2304.4
Ghana	1993	23.4	685.0	965.0	605.0	711.0	780.1	789.9
Ghana	2003	14.1	753.9	824.7	627.2	781.2	721.7	730.2
Guatemala	1987	11.8	889.0	1041.0	835.0	937.0	929.8	946.2
Haiti	1994	12.8	658.8	740.4	584.8	707.1	654.8	670.4
Indonesia	1997	11.0	3906.8	4083.9	3159.2	3457.1	3695.4	3547.7
Indonesia	2002	14.2	3417.7	3784.7	2734.7	2981.9	3333.8	3185.6
Kenya	1989	18.9	1068.5	1617.5	1189.2	1393.8	1341.2	1465.5
Liberia	1986	22.9	943.3	1251.6	800.8	1013.3	1014.0	1038.4
Malawi	2000	23.0	2018.0	2684.5	1702.3	2111.3	2176.9	2209.9
Mozambique	1997	19.6	1384.7	1827.7	1198.7	1263.9	1536.2	1490.2
Mozambique	2003	13.5	1954.7	2233.7	1700.7	1947.9	1940.5	1993.9
Namibia	2000	11.0	742.6	839.3	694.4	821.6	754.0	779.8
Niger	1992	11.1	1399.3	1526.8	1219.4	1391.5	1374.4	1371.8
Nigeria	1990	19.3	1480.7	1997.3	1381.6	1607.4	1666.3	1712.6
Ondo State[1]	1986	17.9	653.0	825.0	576.0	661.0	699.1	701.9
Pakistan	1991	27.8	1790.0	2032.9	1052.4	1284.7	1627.8	1457.4
Rwanda	2000	15.7	1372.9	1737.8	1342.3	1682.0	1487.9	1592.1
Senegal	1986	11.9	838.0	966.0	771.0	879.0	861.6	875.4
Tanzania	1996	11.8	1218.9	1482.9	1192.8	1304.5	1322.7	1353.0
Togo	1988	10.9	656.0	665.0	534.0	655.0	599.7	599.3
Turkey	1993	12.0	883.1	949.0	710.8	754.1	851.7	808.1
Uganda	1995	17.3	1251.9	1587.1	1150.5	1379.5	1330.4	1407.2
Uganda	2001	14.1	1218.3	1458.5	1139.1	1377.2	1272.2	1325.3
Vietnam	1997	11.1	763.3	741.6	570.1	666.0	673.2	638.5
Zambia	1996	13.1	1181.6	1434.9	1162.0	1412.5	1259.8	1337.1

Note: Year x is the first year within the calendar; year x-1 is the first year before the calendar, etc. Example: Survey begins in 2001; calendar begins January 1996; x=1996, x-1=1995. The table lists surveys for which the estimated percentage of births in year x misreported in x-1 is greater than 10.

[1] Ondo State is in Nigeria.

4.3 Birth Intervals

Another perspective on the accuracy of birth dates comes from examining the intervals between births, expressed in months. For each birth, DHS data files include the months since the immediately preceding birth (for second and higher order births) and the months until the immediately following birth (when applicable). The subsequent interval for one birth will equal the preceding interval for the next birth, except that it will be censored for the most recent birth. The focus, therefore, is just on the preceding birth interval. As described in Section 1.2, a modification of Myers' Index will identify heaping on multiples of six months. Most birth intervals are five years or less in length, and few are less than a year because of the nine months of pregnancy and postpartum amenorrhea. This method requires the use of a maximum interval that is a multiple of 12 months. The index, therefore, is restricted to intervals 12 to 59 months in length.

Figure 4.7 gives the distribution of surveys by the value of Myers' Index, and Figure 4.8 gives the percent excess at multiples of six months. Those surveys that exceed a 10 percent threshold on either measure are listed in Table 4.6. There is no evidence of heaping on multiples of six months, but five surveys exceed the 10 percent threshold of Myers' Index for any irregularities: Guinea 1999, Comoros 1996, Nigeria 1999, Pakistan 1990-91, and Yemen 1991-92. All of these surveys have appeared on earlier lists of possible misreporting.

Although these five surveys exceed the threshold, the results are generally quite favorable, probably because the imputation procedures are effective in avoiding heaping at intervals of 24, 36, and 48 months. When birth year is given but month is not, month is imputed with a random component to avoid heaping at such intervals. The results of Figure 4.8 show clearly that this kind of heaping has been completely averted. There is still some heaping at other values, but probably not enough to distort the analysis of birth intervals.

Figure 4.7 Myers' Index for months in previous birth interval (12-59 months)

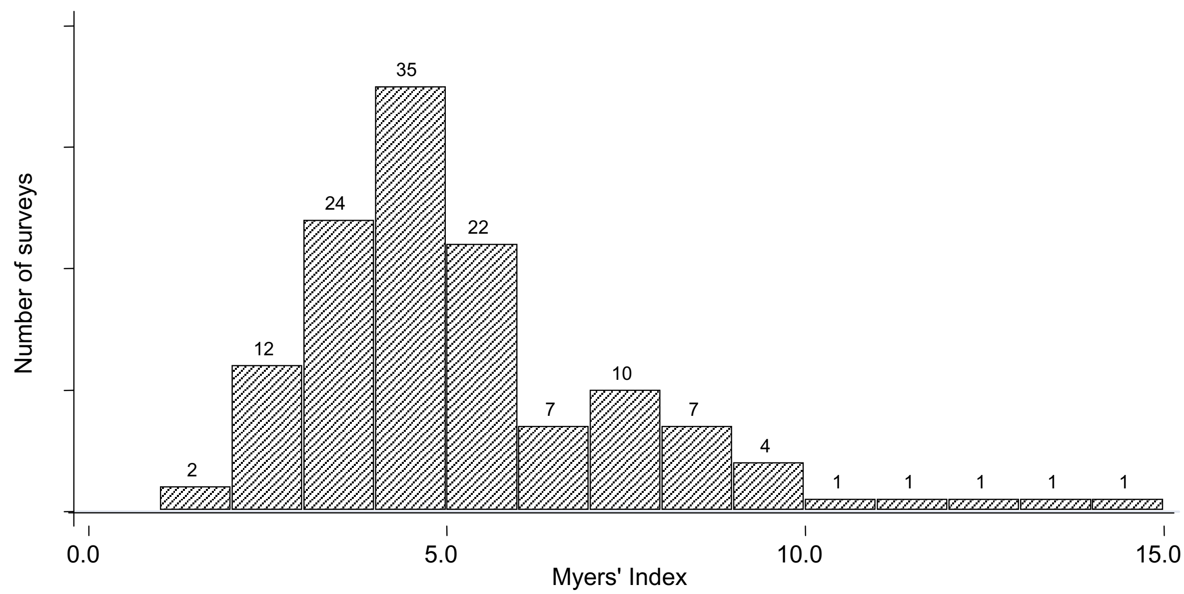
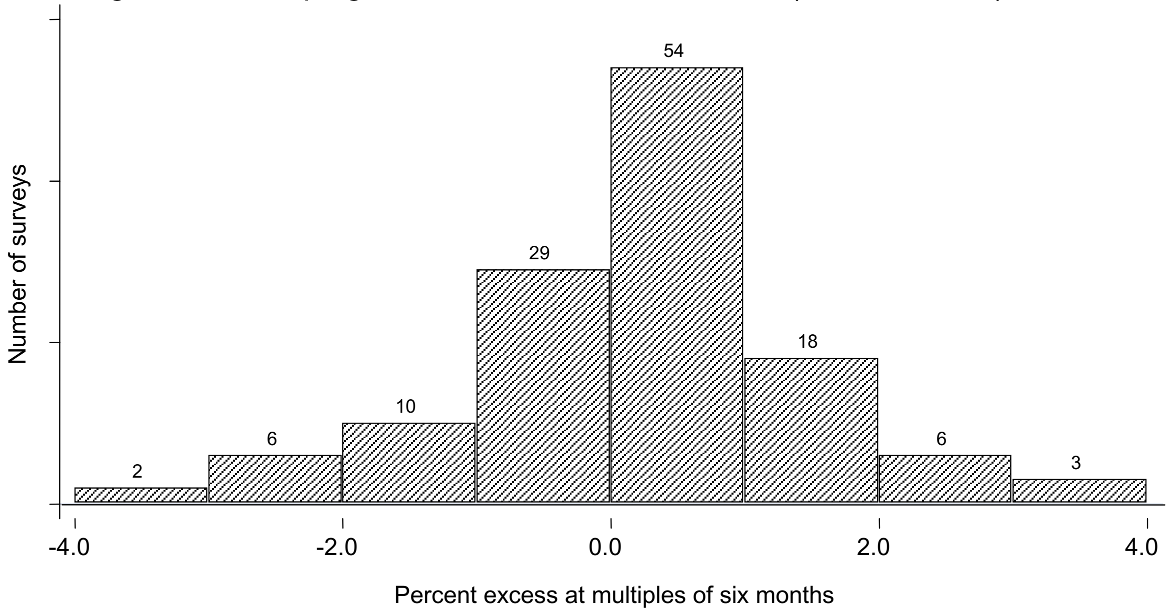


Table 4.6 Surveys with highest levels of heaping on length of previous birth interval (12-59 months)			
Country	Median year of survey	Myers' Index	Percent excess at a multiple of 6 months
Comoros	1996	15.5	2.6
Guinea	1999	12.0	-1.5
Nigeria	1999	10.5	-1.6
Pakistan	1991	11.6	-1.6
Yemen	1991	13.7	-0.5

Note: The table lists surveys for which either the Myers' Index or the percent excess at a multiple of 6 months is greater than 10.

Figure 4.8 Heaping of birth interval at 6X months (12-59 months)



4.4 Age at Death

For each child who died, DHS provides a code for age at death, which may involve some imputation. If the child died before the second birthday, age at death is given in months. There is a tendency for these reports to be heaped at multiples of six months, and particularly at twelve months. As was mentioned in Section 1.2, this type of heaping, immediately above the boundary for infant deaths, can be a source of underestimation of the infant (age 0) mortality rate and overestimation of the child (ages 1-4) mortality rate.

Our measure of heaping at twelve months is the ratio of the observed cases to the expected cases, where the expected number is the average for months 10, 11, 13, and 14. This is calculated for all deaths under age 2 that were reported for the ten years before the survey. The distribution of this ratio of observed to expected numbers of cases is given in Figure 4.9. Almost all surveys show some heaping of this sort. For 22 surveys, the ratio of observed to expected is 10 or more, which most analysts would consider to be very high. The distribution of the *z* statistic for testing the significance of this heaping is given in Figure 4.9. The *z* statistic is above 2, approximately the critical value for a two-tailed .05 test, for 42 surveys.

For many surveys the ratio is well below 1.00, and *z* is below -2 for 17 surveys. The reason for this is that some surveys—El Salvador 1985 is a good example—have very substantial heaping at "months ago" that are multiples of 10 rather than 12. It is likely that this pattern resulted from an imputation procedure that is no longer in use.

Table 4.7 lists the surveys with heaping ratios at 12 months that exceed 10.0 or *z* statistics that exceed 5.0. This is a very high and rather arbitrary threshold for *z,* but because of considerable variation from one survey to another in the frequencies of deaths reported at months 10-14, there are a few surveys with high heaping ratios but small *z* statistics, as well as the other way around. The 24 surveys on this list come from 21 different countries. Again, a majority are from sub-Saharan Africa. The others are Bangladesh 1993-94, Bolivia 1989, Dominican Republic 1999, Ecuador 1987, Egypt 2000, India 1998-99, Liberia 1986, Thailand 1987, Tunisia 1988, and Yemen 1991, representing all regions. The highest ratio by far, 36.3, is from the 1988 Ghana survey.

Figure 4.9 Heaping of age at death at 12 months

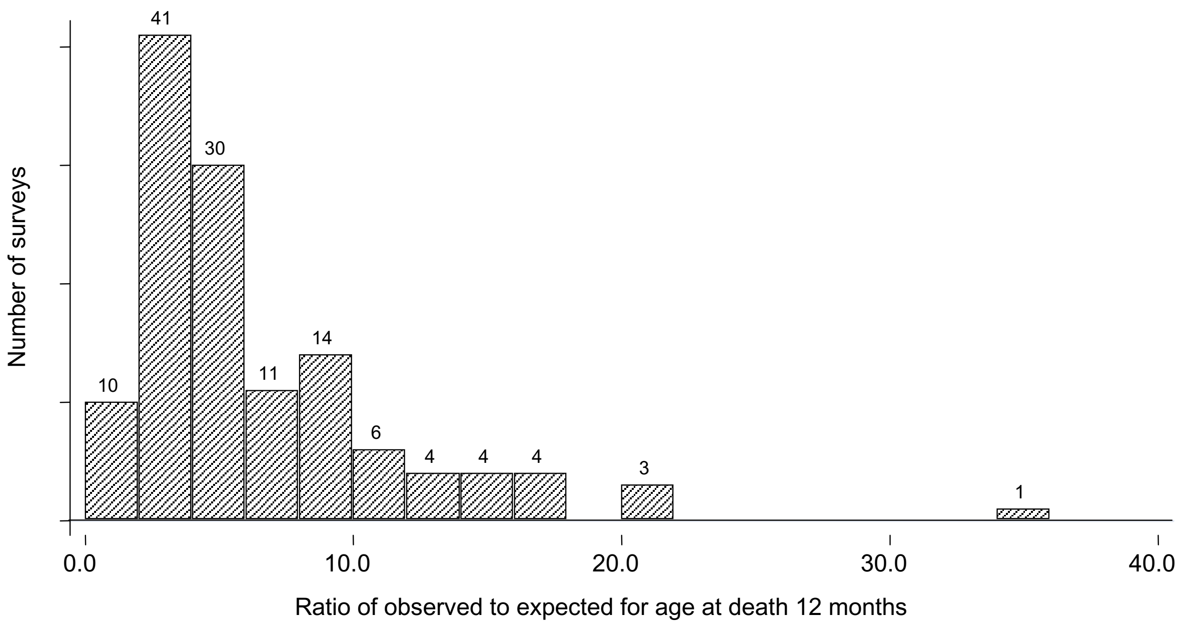


Table 4.7 Surveys with highest levels of heaping on months of age at child death (0-59 months)

Country	Median year of survey	Ratio of observed to expected at 12 months	z test statistic for significance of (3)
Bangladesh	1994	12.2	5.6
Bolivia	1989	11.8	5.9
Burundi	1987	21.2	6.3
Dominican Republic	1999	17.1	1.3
Ecuador	1987	13.3	3.6
Egypt	2000	10.1	4.3
Ghana	1988	36.3	7.8
Ghana	2003	16.3	4.5
Guinea	1999	20.3	9.3
India	1999	6.8	6.6
Liberia	1986	14.6	6.6
Mali	1987	21.5	8.6
Mali	2001	8.4	7.2
Morocco	1987	10.1	4.9
Nigeria	1999	11.9	6.3
Senegal	1986	16.8	7.8
Senegal	1997	10.4	6.1
Sudan	1990	14.2	7.5
Togo	1988	17.9	5.6
Thailand	1987	15.9	2.1
Tunisia	1988	13.1	4.1
Uganda	1988	13.4	6.8
Yemen	1991	14.8	6.8
Zambia	1998	10.1	3.4

Note: The table lists surveys for which either the ratio is greater than 10 or z is greater than 5.

4.5 Infant Mortality Rates from Successive Surveys in the Same Country

Infant mortality rates from successive surveys in the same country can be compared for a fixed reference period before the first survey. This comparison will be described for the same pairs of countries that were used in Section 3.4 to check the consistency of the fertility data. Section 3.4 used the files of women, and the window or reference period was the three calendar years prior to the year in which the fieldwork began. This section uses the files of births with the reference period extended backward two more years (for a total of five years) to yield a maximum number of events. As with the fertility comparison, we take the first survey as the reference because the estimates are commonly believed to be accurate.

In the case of fertility, the estimate for the reference period tended to be higher for the second survey than the first. When this discrepancy occurred, it was interpreted as the likely result of distortion in one or both surveys that pushed some children with true ages 0-4 into reported ages 5-9—these age intervals are approximate; the actual ages depend on the calendar-related questions for children—combined with the typical five-year interval between surveys.

Figures 4.11 and 4.12. show that the second survey also tends to produce a higher estimate of infant mortality than the first survey. Figure 4.11 expresses the difference arithmetically: second estimate minus first estimate, with a factor of 1,000 in the infant mortality rate. Figure 4.12 expresses it in relative terms as the percentage by which the second estimate exceeds the first estimate. For about three-quarters of the comparisons, the second estimate is higher than the first estimate. The greatest arithmetic differences are 49 points for Nigeria (1999 and 2003 surveys) and 47 points for Mali (1987 and 1995 surveys). The greatest relative difference, by far, is for Colombia, where the 1995 survey estimate for 1985-89 was 102 percent greater than the 1990 survey estimate for the same period.

Figure 4.13 gives the distribution of z test statistics to test the null hypothesis that the two estimates are compatible or consistent. Of the 63 pairs of surveys for which this statistic was computed, 49 are in a range from -2 to +2 and would be judged to be consistent. In several cases, the difference is fairly large, arithmetically or relatively, but is not statistically significant. For example, the Dominican Republic 2002 survey gives an estimate for 1994-98 that is 40 percent higher than the 1999 survey. However, both of the estimates were fairly low (an IMR of 25 using the 1999 survey and 35 using the 2002 survey), so there were few deaths, and the z statistic was only 0.98.

Thirteen pairs of surveys with significantly different estimates (at the .05 two-tailed level) are listed in Table 4.8. (The pair of surveys from Brazil are omitted because the first survey was not national.) One pair of surveys had a significant difference in a negative direction: the Namibia 2000 survey gave a lower estimate for 1987-91 than the 1992 survey. For the other twelve pairs of surveys in Table 4.8, the second estimate was higher than the first. These include surveys from Bolivia (1989 and 1993), Comoros (1991 and 1998), Colombia (1990 and 1995), Egypt (1992 and 1995), Haiti (1994 and 2000), Indonesia (1997 and 2000), Mali (1987 and 1995), Nigeria (1999 and 2003), Niger (1992 and 1998), Nepal (1996 and 2001), and Tanzania (1996 and 1999). All geographical regions are represented on this list. Although many countries had three or more surveys, and therefore appear in two or more pairs of successive surveys, no country had more than one pair of surveys with a high level of this kind of inconsistency.

A comparison between the discrepancies in the two fertility estimates and the discrepancies in the two infant mortality estimates shows considerable similarity. Combining the summary data file for this section with the one in Section 3.4, and calculating the correlation (r) between the difference in TFRs with the difference in IMRs gives r=.44 for all pairs, increasing to r=.83 when limited to the twelve pairs of surveys that have statistically significant discrepancies for both fertility and mortality.

There is no obvious reason why an inconsistency in fertility estimates should be associated with an inconsistency in mortality estimates, apart from the fact that both kinds of inconsistencies are more likely if knowledge of dates is poor. If some children who died early (for example, in month 0) were omitted from a birth history, then there would be an underestimate of both fertility and infant mortality. However, such omissions would be less likely if the event was recent, and we tend to observe lower estimates for the first survey, a pattern that is inconsistent with this mechanism.

The association between fertility and mortality estimates may depend on whether there is a consistent pattern of misreporting in both surveys. Distortion in the first survey or in the second survey, or in both surveys, may account for inconsistencies. Further research into the underlying sources of error, with a view toward anticipating such error during data collection, would be worthwhile.

Regardless of the mechanism, it is not unusual in DHS surveys to show an inconsistency between earlier and later estimates of infant mortality in the same reference period. It is general practice in DHS reports to describe trends in infant mortality, not just levels. This practice should be continued by piecing together successive surveys and, additionally, by going backward within each survey to attempt to identify and reconcile discrepancies for the periods of overlap.

Figure 4.11 $_1q_0$ from second survey minus $_1q_0$ from first survey

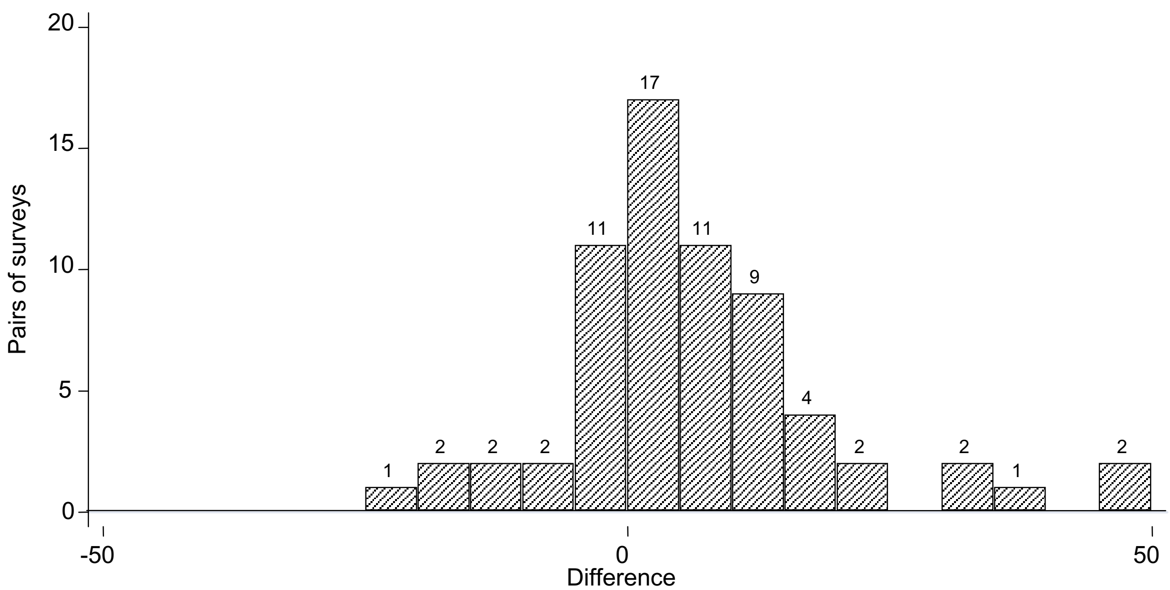


Figure 4.12 Percentage difference between $_1q_0$ estimates.

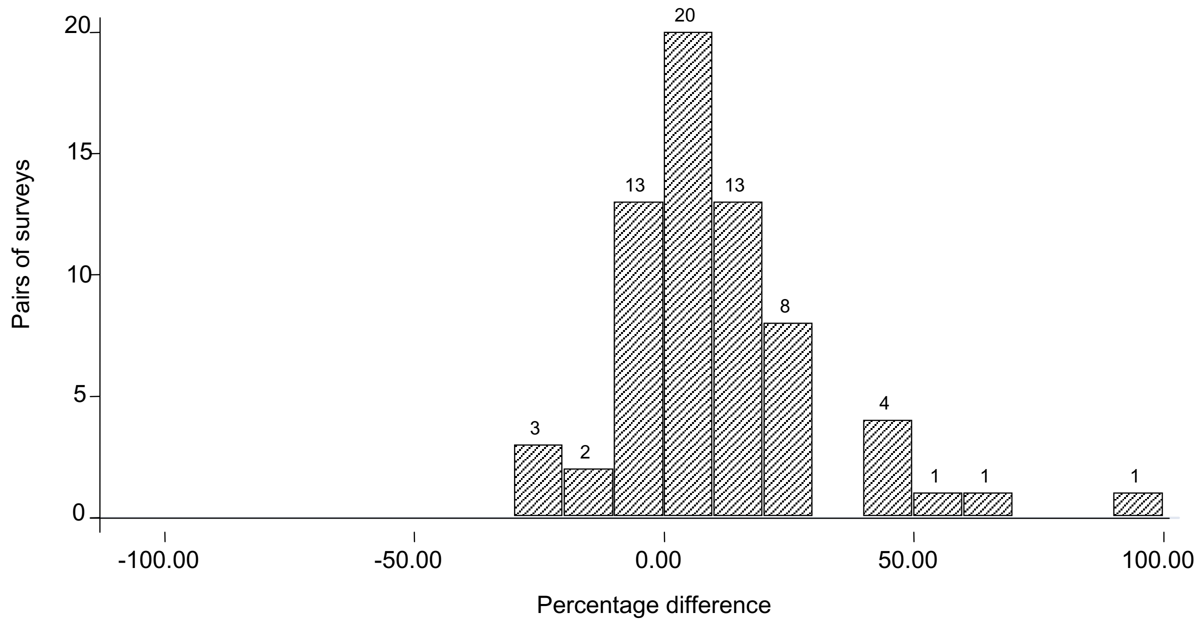


Figure 4.13 z test of difference between $_1q_0$ estimates

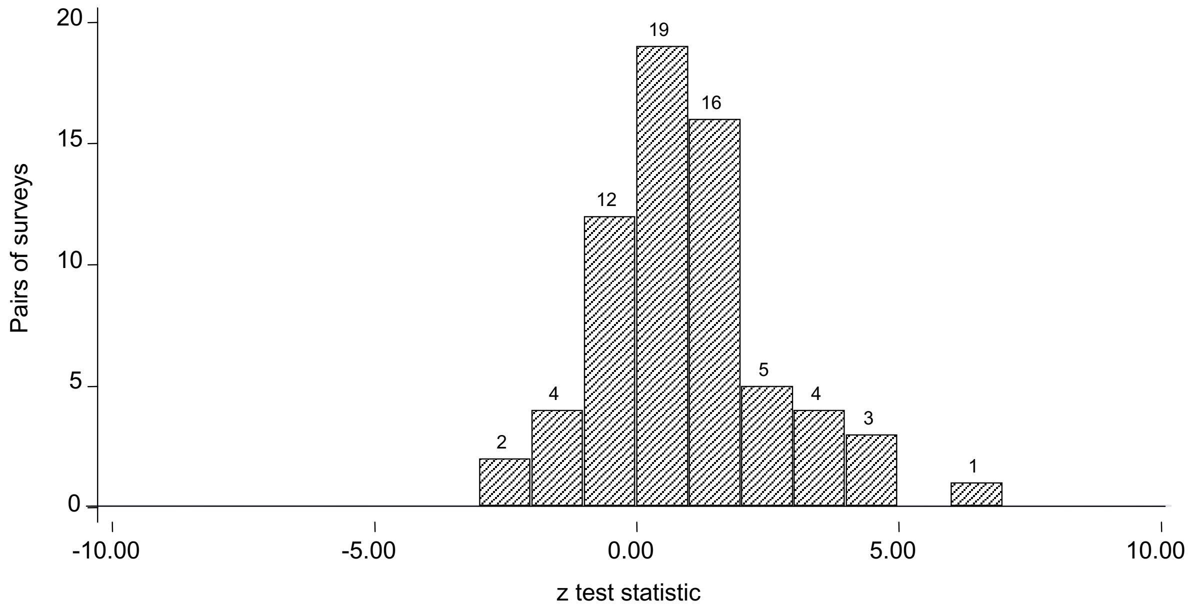


Table 4.8 Pairs of surveys with statistically significant (at the .05 level) discrepancies between estimates of infant mortality during a reference period preceding the first survey

Country	Median year of first survey	Century month code: Beginning month of 5-year window	Century month code: Final month of 5-year window	Median year of second survey	IMR estimate for reference period: First survey	IMR estimate for reference period: Second survey	Difference	Relative difference	z statistic
Bolivia	1989	1009	1068	1993	83	97	15	17.84	2.02
Brazil	1991	1033	1092	1996	77	57	-20	-26.03	-2.41
Cameroon	1991	1033	1092	1998	62	79	18	28.41	2.03
Colombia	1990	1021	1080	1995	17	34	17	102.77	4.31
Egypt	1992	1045	1104	1995	63	78	15	23.48	3.07
Haiti	1994	1069	1128	2000	74	109	35	46.96	4.37
Indonesia	1997	1105	1164	2002	44	53	9	20.32	2.10
Mali	1987	985	1044	1995	106	153	47	44.85	4.60
Namibia	1992	1045	1104	2000	58	42	-16	-27.81	-2.07
Nepal[1]	2052	1765	1824	2057	82	94	12	15.05	2.12
Niger	1992	1045	1104	1998	123	147	25	20.06	3.03
Nigeria	1999	1129	1188	2003	74	124	49	66.73	6.96
Tanzania	1996	1093	1152	1999	83	117	33	40.14	3.14

[1] The Nepalese surveys were conducted in 2052 and 2057 in the Nepalese calendar, 1996 and 2001 in the Western calendar.

5 Effect of Education on Misreporting

It is likely that many characteristics are associated with the probability that a response will be inaccurate, and would provide partial explanations, at least in the statistical sense, of the observed variations in data quality. Some of these characteristics are aggregate level and would account for variation between countries, and others are individual level and would account for variation within specific surveys. In this report, some interpretations have referred to the interviewers' natural desire to reduce their workload, but it could be argued that even this source of misreporting can be traced back to the social context and the respondents' backgrounds. The role of the interviewer in age transfers, for example, is probably contingent on the respondents being uncertain or vague about their own ages or the ages of children or other household members.

It would be possible to conduct a multivariate analysis of the association between several respondent characteristics and various indicators of misreporting, but this report focuses on the role of education, i.e., years of schooling, measured as an interval-level variable (hv108 in the household survey and v133 in the survey of women). We will examine how education is related to age heaping, incompleteness of reporting of ages and dates, heaping of age at death at 12 months, and age transfers. This is one of the few parts of this report in which test statistics will be reported because there is probably less risk here of confusing reporting errors with genuine irregularities in age distributions. We will employ a conservative .01 level of significance rather than the .05 level used earlier.

5.1 Aggregate-level Effects

In the household survey, it is reasonable to look for variation according to education of the household head and education of the household respondent—the person who provides the information on the household. The education of individual household members would be unlikely to matter, except to the extent that it is correlated with the education of the household head or household respondent.

The data file that consists of one record for each of the 102 household surveys used in Chapter 2 includes the mean education of the household head and usually includes the mean education of the household respondent. We would expect data quality to be more strongly associated with the education of the household respondent than the household head. Both measures will be used because they are highly correlated, and in some surveys it is not possible to link to the education of the respondent.

Myers' Index for the age heaping of household members is regressed on these two measures of education (separately) using ordinary least squares (OLS) regression, with results given in the first two lines of Table 5.1. The table shows that when Myers' Index of age heaping for a survey is regressed on the mean years of schooling for the household heads in that survey, the slope is -0.6208. That is, an increase of one year in that mean is associated with a decline of .6208 in Myers' index, and this variable accounts for 11.16 percent of the variation in Myers' Index. If the predictor variable comes from the household respondent rather than the household head, the effect increases in magnitude to -0.6680 and the percentage of variation explained increases to 18.15 percent. As expected, the effect of education on heaping is greater when it refers to the household respondent than when it refers to the household head.

As with a multilevel analysis, the appropriate way to obtain unbiased estimates and evaluate statistical significance is to inflate each household survey by the number of cases in the sample. When education of the household head is the predictor, the combined sample size is 5,408,173; when education of the household respondent is the predictor, the total number of cases is 4,719,118 (2 of the 102 surveys are missing the mean education of household head and 15 are missing the mean education of the household respondent). Because of the huge combined sample size, all coefficients given in Table 5.1 are highly significant. This analysis has not made a correction for within-survey clustering, which would not alter the estimates but would reduce the effective sample sizes somewhat.

Turning to the 128 surveys of woman age 15-49, Table 5.1 also gives the results of regressing the three measures of incompleteness for each survey on the mean years of schooling for the women in the survey. Overall, an increase of one year in the mean years of schooling tends to reduce the level of incompleteness by 0.0923 for age and birth date, 0.0666 for marriage, and 0.1145 for birth dates. Mean education accounts for a substantial amount of the variation between surveys, with R^2 values of .6196, .4952, and .2860, respectively. The association with the woman's own age and birth date, in particular, is very strong.

Finally, using the 128 files of birth histories as cases, we regress the measure of heaping of child deaths at 12 months (the ratio of the observed number to the expected number at 12 months) on the mean years of schooling of the mother. Again, there is a negative relationship, with a coefficient of -0.3756, but R^2 is small, only .0299.

Table 5.1 Between-survey effect of mean years of schooling on selected measures of misreporting (results of OLS regressions)

Measures of education and misreporting	Slope	R^2
Education of household head		
Myer's Index of age heaping	-0.6208	.1116
Education of household respondent		
Myers' Index of age heaping	-0.6680	.1815
Education of woman		
Incompleteness of age and birth date	-0.0923	.6196
Incompleteness of marriage age and date	-0.0666	.4952
Incompleteness of birth dates of children	-0.1145	.2860
Education of mother		
Heaping of child deaths at 12 months	-0.3756	.0299

To summarize, we find consistently negative and statistically significant effects of mean education on misreporting. The strongest effect, by far, is on the measures of incompleteness. For example, it is far more common to give an age but not a birth date, or to give ages and birth dates that are inconsistent in contexts where the mean level of education is low. The next strongest relationship is the negative effect of respondent's schooling on age heaping in the household surveys. This kind of heaping, like incompleteness, also indicates poor knowledge of ages and birth dates. Age heaping and heaping of age at death at 12 months are also negatively related to mean education but to a much lower degree.

5.2 Individual-level Effects

Another way to measure the effect of education on misreporting is with individuals as units of analysis. All the coefficients in this report have been calculated with individual-level statistical models, and covariates can be added to those models. Logit regression will now be used to estimate the within-survey effects of woman's education on the incompleteness of her age/birth date and marriage information, and the effects of the mother's education on incompleteness of birth dates and on the heaping of child deaths at 12 months. In this section, incompleteness of birth date information comes from the child file rather than from the woman file, because it is only in the child file that it is coded as a binary variable.

A code of 1 is assigned if a type of date or age information is missing, incomplete, or inconsistent, and must be given at least some degree of imputation, and a code of 0 otherwise. Within each survey, the code is regressed on years of schooling (v133) using logit regression. The results will be presented on a logit scale. As an aid to interpretation, a coefficient of -0.2 indicates that, on average, a one-year increase in education corresponds with an 18 percent reduction in the odds of giving complete information. A coefficient of -0.50 indicates a 39 percent reduction, and a coefficient of -1.00 indicates a 63 percent reduction. (If *b* is the logit regression coefficient, then the percentage reduction in the odds is $100[1-\exp(b)]$.) Figures 5.1, 5.2, and 5.3 show the distributions of these logit regression coefficients for the three kinds of incompleteness across all the surveys of women.

The effect of education on age incompleteness is always negative and almost always large and statistically significant. The chi-square statistics for the logit regressions described in Figure 5.1 are often very large (the one for India 1998-99 is 2525, with one degree of freedom, partly because of the size of that survey.) The largest (most negative) coefficient is -1.16, for Guatemala 1987, and it is below -0.50 for 27 other surveys. The distribution of coefficients for the effect of education on marriage incompleteness is more concentrated, and never below -0.50, although for two surveys, Kazakhstan 1995 and Uzbekistan 1996, it is actually in the opposite direction from what would be expected. The chi square test statistics are large, reaching a maximum of 1520. The effect of education on children's birth dates is always negative, and is below -0.50 for three countries. The chi-square values tend again to be very large, reaching a maximum of 1048.

Table 5.2 lists the 27 surveys, from 17 countries, for which at least one of the three coefficients in Figures 5.1-5.3 is below the arbitrary threshold of -0.50. There is a relative preponderance of Latin American countries in this category, with at least one survey each from Bolivia, Brazil, Colombia, Dominican Republic, Ecuador, Guatemala, Haiti, Mexico, Peru, Paraguay, and Trinidad and Tobago. The countries outside Latin America include Burkina Faso, Benin, Comoros, and Togo in sub-Saharan Africa, and Armenia and the Philippines. These countries differ greatly in their levels and internal variation of education, and many of them have low levels of incompleteness.

The three coefficients to describe the education effect in these logit regressions are strongly and positively correlated with one another. The correlation between the first and second coefficient is 0.35; between the first and third is 0.60; and between the second and third is 0.50. That is, education has a generalized effect on knowledge of ages and dates, which is expressed with all three indices but most clearly with the reporting of the woman's own date and age.

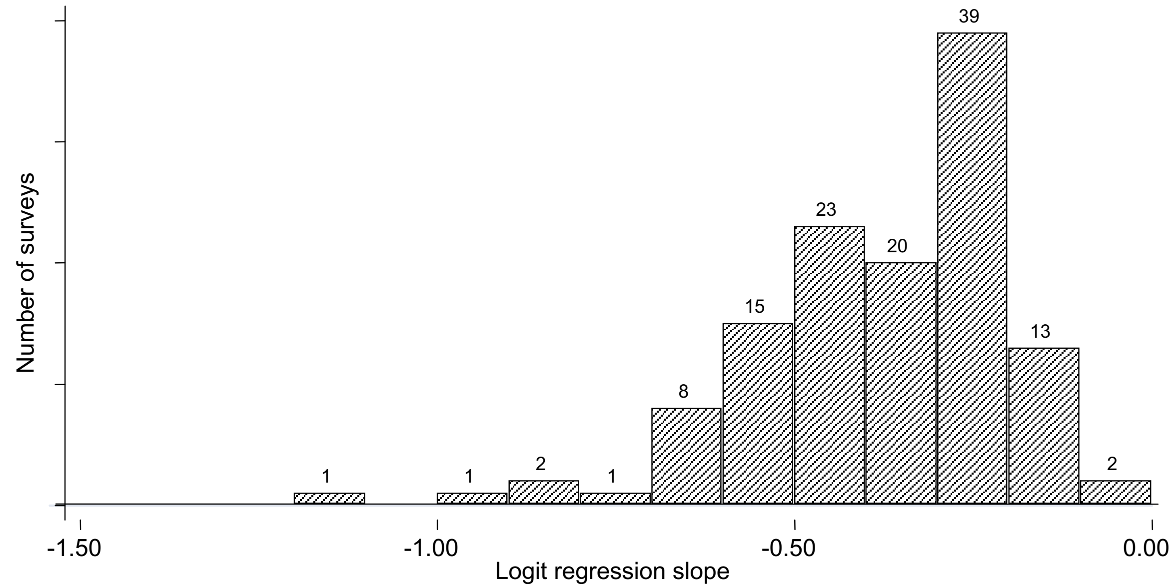


Figure 5.1 Effect of women's education on age incompleteness

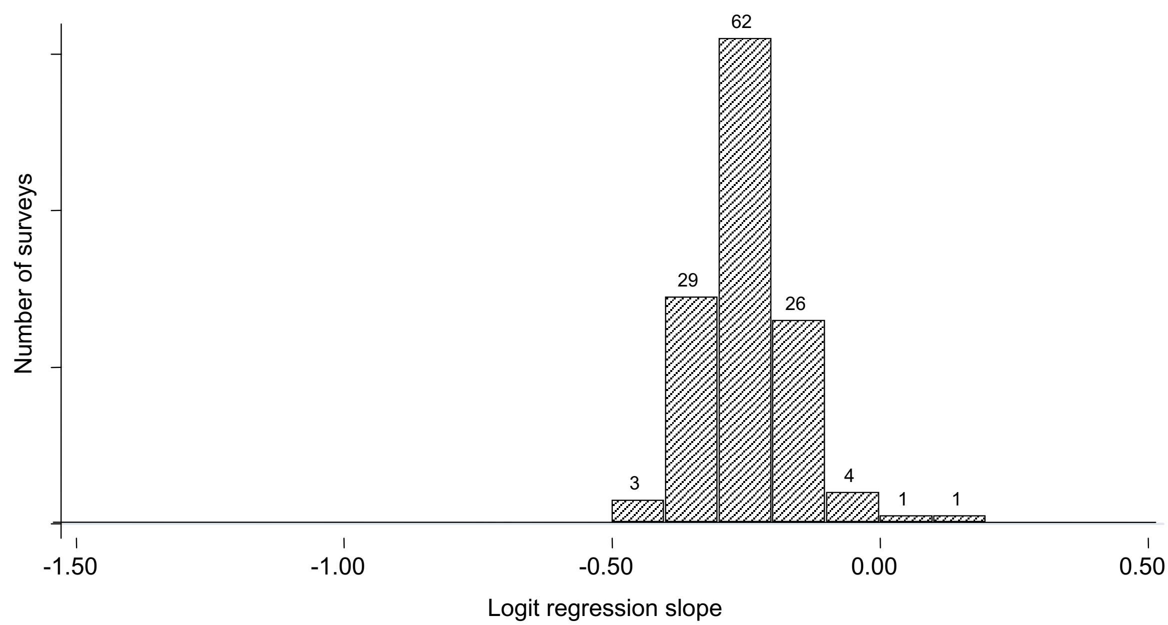


Figure 5.2 Effect of women's education on marriage incompleteness

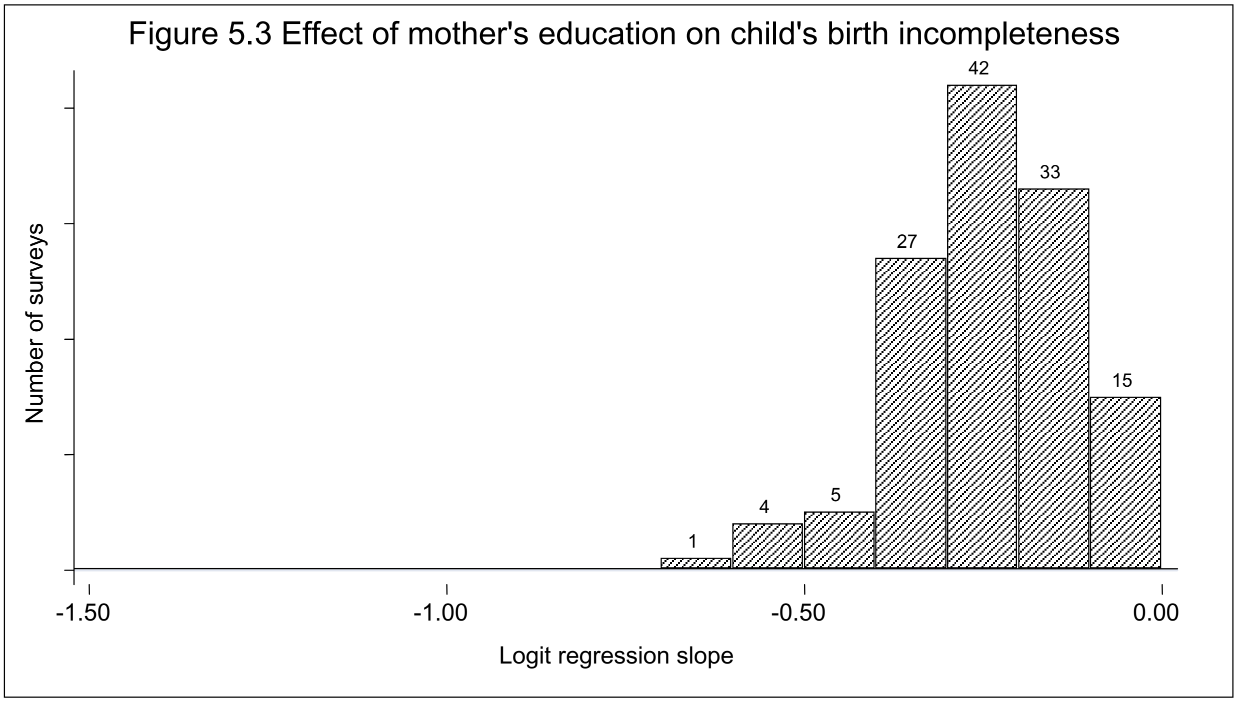


Table 5.2 Surveys with the strongest negative effect of women's education on incompleteness of dates

Country	Median year of survey	Logit regression coefficient for incompleteness: Age/birth	Marriage	Children's births
Armenia	2000	-0.52	-0.19	-0.19
Benin	1996	-0.62	-0.27	-0.35
Benin	2001	-0.57	-0.25	-0.30
Bolivia	1989	-0.51	-0.27	-0.33
Bolivia	1998	-0.51	-0.28	-0.35
Brazil	1991	-0.86	-0.24	-0.29
Burkina Faso	1999	-0.54	-0.33	-0.31
Burkina Faso	2003	-0.53	-0.35	-0.16
Cameroon	1991	-0.67	-0.38	-0.54
Cameroon	1998	-0.55	-0.31	-0.14
Colombia	1986	-0.66	-0.33	-0.37
Colombia	1995	-0.88	-0.28	-0.36
Dominican Republic	1999	-0.54	-0.24	-0.38
Ecuador	1987	-0.53	-0.31	-0.41
Guatemala	1987	-1.16	-0.39	-0.57
Haiti	1994	-0.53	-0.22	-0.40
Mexico	1987	-0.57	-0.41	-0.54
Paraguay	1990	-0.64	-0.27	-0.35
Peru	1986	-0.93	-0.36	-0.61
Peru	1991	-0.60	-0.33	-0.43
Peru	1996	-0.75	-0.26	-0.29
Peru	2000	-0.60	-0.26	-0.20
Philippines	1993	-0.51	-0.10	-0.12
Philippines	1998	-0.62	-0.34	-0.21
Togo	1988	-0.54	-0.31	-0.33
Togo	1998	-0.65	-0.29	-0.33
Trinidad and Tobago	1987	-0.57	-0.14	-0.31

Note: The table lists surveys in which any of the three coefficients are less than -0.50.

The effect of mother's education on heaping of child's age at death at 12 months can also be assessed by adding years of schooling to the logit regression that produced the ratio of observed to expected cases at month 12. Figure 5.4 shows the distribution of the logit regression coefficients while Table 5.3 lists the countries in which the coefficient is less than -0.50 or the coefficient is significant at the .01 level, for which the critical value of chi-square with one degree of freedom is 6.63. There are only a few countries for which more education tends to reduce this kind of heaping by a statistically significant amount, and the chi-square values are much smaller than for incompleteness, reaching a maximum of only 8.83. Some coefficients are statistically significant even though they are close to zero, simply because of the large sample sizes. For 32 surveys, the effect is actually positive, such that women with more education tend to have more heaping at 12 months, but those positive coefficients are never significant. Two of the most negative coefficients are for the two surveys of Vietnam.

Figure 5.4 Effect of mother's education on month 12 heaping

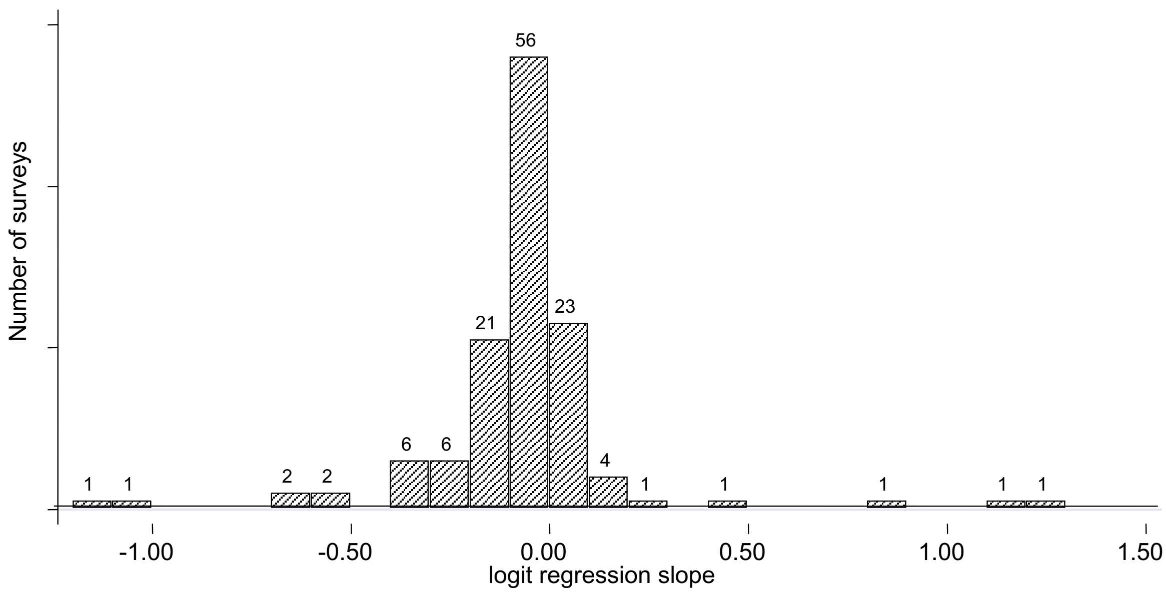


Table 5.3 Surveys with the strongest negative effect of women's education on heaping of child deaths at 12 months

Country	Median year of survey	Logit regression coefficient	Chi-square test statistic (has one degree of freedom)
Egypt	1992	-0.16	8.35
India	1999	-0.08	7.28
Peru	1996	-0.17	8.83
Rwanda	1992	-0.17	8.72
Trinidad & Tobago	1987	-1.02	2.51
Turkey	1998	-0.39	8.31
Uzbekistan	1996	-0.60	1.60
Vietnam	1997	-0.54	2.34
Vietnam	2002	-0.69	4.16
Zambia	1992	-0.13	7.59

Note: The table lists surveys in which the coefficient is less than -0.50 or chi-square is greater than 6.63.

We use a simpler strategy to analyze the individual-level effect of education on upward and downward age transfers of women in the household survey and the backward age transfers of children in the birth histories. Using the household surveys, the estimates of upward and downward transfers of women are recalculated separately for households in which the household head is below the median in years of schooling, or above the median; and separately for households in which the household respondent is below the median in years of schooling, or above the median. As mentioned above, we expect data quality to be more strongly associated with the characteristics of the household respondent than the household head.

In the birth histories, the measure of backward transfers will be recalculated separately for children whose mother is below the median in years of schooling, or above the median. (As before, the measure of schooling for the household survey is hv108 and for the mothers is v133. Note that hv108 is sometimes missing for an entire survey or cannot be linked to the household head or respondent, and both hv108 and v133 are missing for some individuals.)

The results of these comparisons between the lower and upper halves of the education distribution are shown in Table 5.4. The table lists only the surveys in which the difference between the two halves exceeds 10 percent in the expected direction and is statistically significant with a one-tailed .01 test. The expected direction of transfers is downward for younger women, upward for older women, and backward for children, and the hypothesized effect of more education is to reduce the level of such transfers. The z statistic given in the table is based on a test of the difference between the two "transfer" statistics, t, described in Appendix D.

There are a few surveys in which the displacement is reversed or the education effect is reversed, but rarely with statistical significance. A high standard for statistical significance is also required for inclusion in the table. For these reasons, the table includes only 16 surveys. One of them, Bolivia 1998, appears in two panels. Two of them, Peru 2000 and Zimbabwe 1999, appear in three panels each.

Only one of the surveys listed in Table 5.4 (Malawi 2000) was listed earlier as having high displacement. There is a tendency for surveys with a large differential by education to have low overall displacement levels. If education (or presumably another covariate) has a large effect on age transfers, then the serious transfers will be concentrated in a smaller subgroup. Hence there is actually a negative correlation between the overall net level of transfers and the inhibiting effect of education upon displacement.

Although there are good reasons to believe that the education of the household respondent is more relevant than that of the household head, there is very little difference in terms of whose education is more strongly associated with age displacement. In many countries, the household respondent tends to be female and to have very little education, and the household head tends to be male and to have greater variation in years of schooling as well as a higher mean. Greater dispersion in the head's years of schooling will in itself induce a stronger relationship.

This section serves mainly to suggest a strategy for identifying important covariates of misreporting. It could be applied to type of place of residence and other background variables, many of which are country-specific. As noted above, a covariate may actually be most important in a context in which there is a low level of misreporting, so a search for useful covariates should not be limited to countries in which the net level of misreporting is high.

Interviewers may affect the quality of the data, but we are not aware of earlier studies of DHS reporting that have examined this issue and we are unable to examine it here. There may be some settings in which either fixed or random effects for interviewers could be added to the various statistical models that produce our summary measures. In most contexts, however, interviewers are confounded with clustering and other characteristics of the survey design. This is because a specific interviewer will generally work in only a few clusters, nested within one region or part of a country, and not assigned at random. Some inferences might be possible if DHS could identify at least one survey with relatively poor quality data and a relatively small number of interviewers who tended to cover relatively large areas. We suspect that some kinds of misreporting, especially age displacement, are caused primarily by the interviewer.

Table 5.4 Differences between the upper and lower halves of the education distribution in the net level of displacement

Country	Median year of survey	Estimated percentage transfers: Up, down, backward	Estimated percentage transfers: For low education	Estimated percentage transfers: For high education	Difference (high education minus low education)	z test statistic (absolute value >1.96)
Surveys with a significant effect of education of household head on downward displacement						
Bolivia	1998	2.83	10.06	-3.08	-13.14	-3.80
Peru	2000	5.52	13.33	2.19	-11.14	-4.09
Zimbabwe	1999	9.88	14.89	3.11	-11.78	-2.50
Surveys with a significant effect of education of household head on upward displacement						
Brazil	1991	4.78	14.27	-8.82	-23.09	-2.96
Gabon	2000	17.86	30.31	3.03	-27.27	-3.23
Tanzania	1996	6.24	13.26	-2.90	-16.17	-2.35
Surveys with a significant effect of education of household respondent on downward displacement						
Dominican Republic	1999	4.58	20.43	-6.36	-26.79	-2.62
Guatemala	1995	1.05	7.74	-3.51	-11.25	-3.28
Peru	1991	1.10	9.84	-5.00	-14.84	-5.25
Peru	1996	4.73	14.90	0.32	-14.57	-5.31
Peru	2000	5.52	13.97	1.66	-12.31	-4.33
Philippines	1993	6.24	14.51	3.65	-10.87	-3.12
Philippines	1998	-1.40	8.36	-8.03	-16.39	-4.81
Philippines	2003	8.54	14.14	3.98	-10.16	-3.00
Zimbabwe	1994	9.53	17.91	2.13	-15.78	-3.41
Zimbabwe	1999	9.88	16.58	2.86	-13.71	-2.83
Surveys with a significant effect of education of household respondent on upward displacement						
NO SURVEYS						
Surveys with a significant effect of education of mother on backward displacement						
Bolivia	1998	6.68	12.83	1.61	-11.22	-2.61
Malawi	2000	22.97	29.31	16.30	-13.01	-3.38
Peru	2000	4.40	11.74	0.15	-11.59	-3.51
Turkey	1998	9.28	21.70	3.47	-18.23	-2.93
Zambia	1998	8.00	15.94	2.30	-13.64	-2.35
Zambia	2002	9.53	17.52	4.48	-13.04	-2.98

Note: Restricted to surveys in which the displacement is at least 10 percent less for the more educated than for the less educated and the difference is statistically significant at the .01 level.

6 CONCLUSIONS

In an attempt to summarize and integrate the findings in Chapters 2, 3, and 4, we now focus on nine specific symptoms of reporting errors. First we describe how these indicators have varied according to region and time period, or phase of DHS. Then we identify the specific surveys that have the highest overall levels on these indicators.

6.1 Variation by Region, Time Period, and Type of Problem

This analysis covers 128 surveys of women and 102 household surveys. Table 6.1 describes their distribution across four major geographic regions and time periods. The regions are sub-Saharan Africa, Latin America and the Caribbean, South and Southeast Asia, and "other." These are the same regions DHS uses in comparative analyses, except that "other" consists of a consolidation of regions that are sometimes given separately as North Africa, West Asia, and Central Asia. Even with consolidation, "other" includes fewer surveys than the other three regions.

Time period refers to the four phases of DHS data collection: DHS-I (1985-89), DHS-II (1990-92), DHS-III (1993-98), and MEASURE *DHS*+ (1999-2003). The omitted household surveys are the 26 conducted during DHS-I.

Table 6.1 Distribution of the surveys in this analysis according to major region and time period

	Time period				
Region	DHS-I (1985-89)	DHS-II (1990-92)	DHS-III (1993-98)	MEASURE DHS+ (1999-2003)	Total
Sub-Saharan Africa	11	11	22	19	63
Latin America/Caribbean	10	5	9	7	31
South and Southeast Asia	3	3	8	6	20
Other	2	3	5	4	14
Total	26	22	44	36	128

Of the nine selected indicators of misreporting, three refer to household data, three to the women's data, and three to the birth histories. The three indicators based on the household data are Myers' Index for age heaping, the measure of downward transfers of women across age 15, and the measure of upward transfers of women across age 50. The three based on the survey of women refer to incompleteness of reports of age, marriage, and births. The three based on the birth histories refer to upward transfers of children's ages, Myers' Index for heaping of the preceding birth interval, and heaping of age at death on 12 months.

Selected indicators of misreporting			
	Source of data		
Indicator	Household survey	Women's survey	Birth histories
1. Age heaping	X		
2. Downward age transfers of women across age 15	X		
3. Upward age transfers of women across age 50	X		
4. Age incompleteness		X	
5. Marriage incompleteness		X	
6. Birth history incompleteness		X	
7. Upward age transfers of children's age			X
8. Myers' Index for heaping of preceding birth interval			X
9. Heaping of age of death on 12 months			X

The nine indicators were selected because they refer to manifestations of age and date reporting that would generally be considered important, but are relatively independent of one another. For example, evidence of age heaping in the household survey is included, but age heaping in the survey of women is *not* included. Incompleteness of birth dates is included for the women files, but not for the birth files. No indicator is included for discrepancies between fertility rates or infant mortality rates in successive surveys. Such discrepancies are important, but it generally is not possible to say whether the problem is with the first survey or the second one, or both, and it would be misleading to assume that both are at fault.

Each indicator is coded "1" if the underlying continuous measure of misreporting was in its worst quartile; otherwise it is coded "0." For example, the first indicator refers to age heaping in the household survey, measured with Myers' Index. Twenty-five surveys (which rounds to 25 percent of the 102 household surveys) exceeded 7.660. A household survey is coded "1" for this indicator if Myers' Index exceeded that value; otherwise it is coded "0." As another example, the fourth indicator refers to age incompleteness. It is coded "1" if the incompleteness index exceeded .542, because 31 surveys (which rounds to 25 percent of the surveys of women) exceeded that value; otherwise it is coded "0."

For each indicator, and within each cell of Table 6.1, we then calculate the percentage of surveys that are coded "1." For example, of the 11 surveys in sub-Saharan Africa during DHS-II, 3 (or 27 percent) were in the overall worst quartile on the first indicator. Four (or 36 percent) were in the worst quartile on the fourth indicator. Table 6.2, with nine panels, gives the results of these calculations.

Some combinations of region and time period contain very few surveys. Indeed, half of the combinations contain five or fewer surveys. The interpretation of the panels of Table 6.2 will focus on the margins and on the trends for sub-Saharan Africa and Latin America, since these two regions account for the great majority of the surveys.

Table 6.2 Percentage of surveys in the worst quartile of nine measures of data quality, by region and time period

Region	DHS-I (1985-89)	DHS-II (1990-92)	DHS-III (1993-98)	MEASURE DHS+ (1999-2003)	Total
	Time period				
Indicator 1: Percentage of household surveys with age heaping in worst quartile					
Sub-Saharan Africa		27	27	37	31
Latin America/Caribbean		0	0	0	0
South and Southeast Asia		100	25	33	41
Other		33	20	25	25
Total		32	20	28	25
Indicator 2: Percentage of household surveys with downward age transfers of women in worst quartile					
Sub-Saharan Africa		36	50	47	46
Latin America/Caribbean		0	0	0	0
South and Southeast Asia		0	0	17	6
Other		0	20	0	8
Total		18	30	24	25
Indicator 3: Percentage of household surveys with upward age transfers of women in worst quartile					
Sub-Saharan Africa		73	41	37	46
Latin America/Caribbean		0	11	0	5
South and Southeast Asia		33	0	0	6
Other		0	0	0	0
Total		41	23	19	25
Indicator 4: Percentage of women surveys with age incompleteness in worst quartile					
Sub-Saharan Africa	55	36	32	26	35
Latin America/Caribbean	0	0	0	0	0
South and Southeast Asia	0	67	38	60	40
Other	50	33	0	0	14
Total	27	32	23	22	25
Indicator 5: Percentage of women surveys with marriage incompleteness in worst quartile					
Sub-Saharan Africa	55	36	41	35	41
Latin America/Caribbean	0	0	0	0	0
South and Southeast Asia	0	0	13	50	20
Other	50	33	0	0	14
Total	27	23	23	28	25
Indicator 6: Percentage of women surveys with birth history incompleteness in worst quartile					
Sub-Saharan Africa	55	64	41	11	38
Latin America/Caribbean	0	0	0	0	0
South and Southeast Asia	33	33	13	0	15
Other	50	67	20	25	36
Total	31	45	25	8	25

Continued...

Table 6.2—*Continued*					
	Time period				
Region	DHS-I (1985-89)	DHS-II (1990-92)	DHS-III (1993-98)	MEASURE DHS+ (1999-2003)	Total
Indicator 7: Percentage of surveys with upward age transfers of children in worst quartile					
Sub-Saharan Africa	45	27	45	32	38
Latin America/Caribbean	0	0	20	25	14
South and Southeast Asia	0	33	13	20	17
Other	11	0	11	0	7
Total	25	18	30	23	25
Indicator 8: Percentage of surveys with heaping of previous birth interval in worst quartile					
Sub-Saharan Africa	36	9	18	21	21
Latin America/Caribbean	0	40	0	0	6
South and Southeast Asia	0	100	25	33	35
Other	100	100	40	50	64
Total	23	45	18	22	25
Indicator 9: Percentage of surveys with heaping ratio of child deaths at 12 months in worst quartile					
Sub-Saharan Africa	82	0	9	26	25
Latin America/Caribbean	30	0	0	14	13
South and Southeast Asia	33	33	38	0	25
Other	100	67	20	50	50
Total	58	14	14	22	25

Five of the nine indices either had their lowest level (sometimes by only a small amount) in the most recent time period, or declined monotonically across the last three time periods. Perhaps the greatest improvement has been in upward age transfers of women, which was serious for 41 percent of DHS-II surveys but only 18 percent of MEASURE DHS+ surveys. No real improvement is observed for age heaping, heaping of previous birth interval, downward age transfers of women, and incompleteness of date of marriage.

As a region, South and Southeast Asia has the most serious incidence of age heaping and incompleteness of age. Sub-Saharan Africa is above average on most indicators, but especially age transfers—downward for women, upward for women, and upward for children—and incompleteness of all kinds of dates. Over time, countries in this region show progressively less of a problem with upward age transfers of women and incompleteness. Across the board, the Latin American surveys show the fewest problems, frequently having no surveys at all in the worst quartile of an indicator.

Table 6.3 summarizes the distribution of the nine types of problems listed in Table 6.2. An overall index is constructed which is the total number of indicators that are in the worst quartile. Thus a score of 0 means that a survey was not in the worst quartile for any of the nine indicators. Table 6.3 shows that 33 of the 128 surveys had a score of 0 on this overall index. A score of 9 (never observed) would mean that a survey was in the worst quartile on all nine indicators. Because the DHS-I surveys do not include the three household indicators, the maximum possible score for those surveys would be 6, and two surveys receive that maximum. Across DHS-II, DHS-III, and MEASURE DHS+ there is a steady increase in the proportion of surveys with favorable scores on all indicators.

Table 6.3 Frequency distribution and percent distribution of the number of indicators that are in the worst quartile, by time period										
Overall index	DHS-I		DHS-II		DHS-III		MEASURE DHS+		Total	
	n	%	n	%	n	%	n	%	n	%
0	7	27	3	14	11	25	12	36	33	26
1	9	35	6	27	10	23	5	14	30	23
2	2	8	2	9	8	18	5	14	17	13
3	1	4	5	23	4	9	6	17	16	13
4	3	12	0	0	6	14	5	14	14	11
5	3	12	2	9	3	7	1	3	9	7
6	1	4	4	18	2	5	0	0	7	5
7	0	0	0	0	0	0	2	6	2	2
Total	26	100	22	100	44	100	36	100	128	100

Note: Percentages may not add to 100 percent because of rounding.

Next we will identify the specific surveys that have had the highest incidence of reporting problems. These surveys are, of course, familiar from the various lists in Chapters 2, 3, and 4. Table 6.4 includes 8 DHS-I surveys with 3 or more indicators in the worst quartile, and 14 later surveys with 5 or more indicators in the worst quartile. Somewhat different lists would be obtained with different indices and different thresholds, but most of the surveys listed in table 6.1.3 would probably show up on most plausible alternatives.

The only surveys outside of Sub-Saharan Africa that appear in Table 6.4 are Morocco 1987, Egypt 2000, and Yemen 1991-92. Surveys from Burkina Faso, Ghana, Mali, and Togo appear twice on the list. There is some overlap, but not a great deal, with the lists of successive surveys that had discrepancies in estimates of fertility rates or infant mortality rates. For example, the two surveys from Burkina Faso that are listed in Table 6.4 are the same two surveys that had the greatest of all inconsistencies in successive TFR estimates. The two surveys from Mali that are listed in Table 6.4 were involved in inconsistencies of fertility estimates or infant mortality estimates. Surveys from Comoros, Egypt, Ghana, Morocco, Niger, Nigeria, and Senegal appear in Table 6.4 and also in the tables on fertility and mortality estimates, but are not always the same surveys.

Table 6.4 Surveys with the highest levels of age and date misreporting											
		Indicator[1]									
Country	Region	1	2	3	4	5	6	7	8	9	Total
SURVEYS CONDUCTED AS PART OF DHS-I											
Ghana 1987	Sub-Saharan Africa	.	.	.	0	1	1	0	0	1	3
Liberia 1986	Sub-Saharan Africa	.	.	.	1	1	0	1	0	1	4
Mali 1987	Sub-Saharan Africa	.	.	.	1	1	1	0	0	1	4
Togo 1988	Sub-Saharan Africa	.	.	.	1	1	1	0	0	1	4
Burundi 1987	Sub-Saharan Africa	.	.	.	1	0	1	1	1	1	5
Morocco 1987	Other	.	.	.	1	1	1	0	1	1	5
Senegal 1986	Sub-Saharan Africa	.	.	.	1	1	1	0	1	1	5
Sudan 1990	Sub-Saharan Africa	.	.	.	1	1	1	1	1	1	6
SURVEYS CONDUCTED AS PART OF DHS-II, DHS-III, and MEASURE DHS+											
Cameroon 1991	Sub-Saharan Africa	1	0	1	0	1	1	0	1	0	5
Comoros 1996	Sub-Saharan Africa	0	0	1	1	0	1	1	1	0	5
Egypt 2000	Other	1	0	0	0	0	1	1	1	1	5
Ghana 1993	Sub-Saharan Africa	0	1	1	0	1	1	1	0	0	5
Mozambique 1997	Sub-Saharan Africa	0	1	0	0	1	1	1	1	0	5
Nigeria 1990	Sub-Saharan Africa	1	1	1	0	0	1	1	0	0	5
Burkina Faso 1992-93	Sub-Saharan Africa	0	1	1	1	1	1	1	0	0	6
Burkina Faso 1998-99	Sub-Saharan Africa	0	1	1	1	1	1	1	0	0	6
Niger 1992	Sub-Saharan Africa	1	0	1	1	1	1	1	0	0	6
Pakistan 1990-91	South/Southeast Asia	1	0	1	1	0	0	1	1	1	6
Togo 1998	Sub-Saharan Africa	1	0	1	1	1	1	0	1	0	6
Yemen 1991-92	Other	1	0	0	1	1	1	0	1	1	6
Guinea 1999	Sub-Saharan Africa	1	1	0	1	1	1	0	1	1	7
Mali 2001	Sub-Saharan Africa	1	1	1	1	1	0	0	1	1	7

[1] Key to indicators:
1: Age heaping in worst quartile
2: Downward age transfers of women in worst quartile
3: Upward age transfers of women in worst quartile
4: Age incompleteness in worst quartile
5: Marriage incompleteness in worst quartile
6: Birth history incompleteness in worst quartile
7: Upward age transfers of children in worst quartile
8: Heaping of previous birth interval in worst quartile
9: Heaping ratio of child deaths at 12 months in worst quartile

This report has focused on omission and inconsistencies, using somewhat arbitrary thresholds for evidence of problems. It is important to recognize that most surveys had no apparent problems or only a few problems. Surveys done in Latin America or Southeast Asia, in particular, are relatively free of problems. The problems found in sub-Saharan Africa and South Asia can almost certainly be traced to the low salience of ages and dates and the relatively low levels of education, especially for women respondents. There are limits to what can be achieved in such contexts, even with careful construction of survey instruments, careful training of interviewers, and maintenance of the highest standards in all aspects of survey implementation.

Finally, the most important caveat in the first section of this report must be repeated: almost all of the measures confound misreporting with real, or genuine, variation. The appearance of age heaping and age transfers may be spurious and be the result of genuine irregularities in the history of fertility and mortality. On the other hand, some heaping and age transfers may go unrecognized because they are effectively cancelled out by the history of fertility and mortality. The only exceptions to this confounding of real variation with errors in reporting occur when there are two reports on the same objective reality, as with estimates of fertility rates or mortality rates in the same reference period using successive surveys.

6.2 Recommendations

This analysis leads to three sets of recommendations regarding fieldwork, imputation, and research into the sensitivity of important output measures to misreporting of ages and dates.

First, regarding the implementation of surveys, it is recommended that additional checks be developed and applied during fieldwork. DHS is well known for its high standards of training and fieldwork, but improvement is always possible, especially in countries that have previously had surveys with high levels of apparent misreporting.

Second, it is possible that the date imputation procedures can be extended to mitigate some of the negative consequences of misreporting. For example, the age of a child at death could perhaps be adjusted with a probabilistic model, even when a valid age is reported, in order to avoid heaping at 12 months. Again, DHS imputation procedures are already unusually sophisticated, especially as applied to dates in the birth histories, but some further adjustments may be feasible.

Third, and perhaps most important, is the issue of whether the level of problems observed in even the worst settings has much of an impact on the proportions, means, and rates that constitute the main output of these surveys. DHS could undertake a sensitivity analysis, for example, in which the output measures would be calculated for a survey with good data, and then recalculated after varying degrees of random displacement were artificially imposed on the data. It is quite possible that considerable shifting of eligible respondents outside of the boundaries of eligibility will have only a negligible effect on these measures. Age heaping, in itself, may not actually produce much distortion of rates unless it has a systematic component of bias. Age transfers of young children outside of the calendar may have little effect on estimates of the treatment of childhood illnesses, for example. There may also be analytic strategies to cope with such transfers, such as shifting the reference period or window for the calculation of rates, which would reduce their impact.

There has already been some research into the sensitivity of fertility and mortality rates to misreporting of ages and dates, but new strategies could relate more directly to the specific characteristics of DHS surveys and to reporting requirements. It would be cost effective to resolve these analytical issues before implementing significant changes in the fieldwork and imputation procedures.

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Appendix A Surveys Conducted by DHS 1985-2003 and Surveys Included in This Report

Table A.1 Surveys conducted by DHS 1985-2003. This report includes all surveys of women except for Senegal 1999, Peru 2003, and restricted surveys. It also includes all corresponding household surveys except those conducted as part of DHS-I. Age displacement in 55 of the surveys of men is examined in section 2.4.

Country/year	Code or status	Household sample	Female sample	Male respondents	Age of men	Male sample
DHS-I 1985-89						
Sub-Saharan Africa						
Botswana 1988	Restricted	4,473	4,368	na	na	na
Burundi 1987	BU01	3,868	3,970	Husbands	na	542
Ghana 1988	GH02	4,406	4,488	Husbands	na	943
Kenya 1989	KE03	8,173	7,150	Husbands	na	1,133
Liberia 1986	LB01	5,023	5,239	na	na	na
Mali 1987	ML01	3,048	3,200	All men	20-55	970
Ondo State, Nigeria 1986	OS01	3,437	4,213	na	na	na
Senegal 1986	SN02	3,736	4,415	na	na	na
Sudan 1990	SD02	6,891	5,860	na	na	na
Togo 1988	TG01	3,432	3,360	na	na	na
Uganda 1988	UG01	5,101	4,730	na	na	na
Zimbabwe 1988	ZW01	4,107	4,201	na	na	na
North Africa/West Asia/Europe						
Egypt 1988	Restricted	9,805	8,911	na	na	na
Morocco 1987	MA01	6,960	5,982	na	na	na
Tunisia 1988	TN02	5,645	4,184	na	na	na
South and Southeast Asia						
Indonesia 1987	ID01	14,142	11,884	na	na	na
Sri Lanka 1987	LK02	7,669	5,865	na	na	na
Thailand 1987	TH01	9,045	6,775	na	na	na
Latin America/Caribbean						
Bolivia 1989	BO01	8,439	7,923	na	na	na
Brazil 1986	BR01	13,283	5,892	na	na	na
Colombia 1986	CO01	4,273	5,329	na	na	na
Dominican Republic 1986	DR01	7,152	7,649	na	na	na
Ecuador 1987	EC01	4,578	4,713	na	na	na
El Salvador 1985	ES00	4,922	5,207	na	na	na
Guatemala 1987	GU01	5,459	5,160	na	na	na
Mexico 1987	MX00	7,786	9,310	na	na	na
Peru 1986	PE01	4,497	4,999	na	na	na
Trinidad & Tobago 1987	TT01	4,122	3,806	na	na	na
DHS-II 1990-92						
Sub-Saharan Africa						
Burkina Faso 1992-93	BF21	5,143	6,354	All men	18+	1,845
Cameroon 1991	CM22	3,538	3,871	Husbands	na	814
Madagascar 1992	MD21	5,944	6,260	na	na	na
Malawi 1992	MW22	5,323	4,850	All men	20-54	1,151
Namibia 1992	NM21	4,101	5,421	na	na	na
Niger 1992	NI22	5,242	6,503	Husbands	na	1,570
Nigeria 1990	NG21	8,999	8,781	na	na	na
Rwanda 1992	RW21	6,252	6,551	Husbands	na	598
Senegal 1992-93	SN21	3,528	6,310	All men	20+	1,436
Tanzania 1992	TZ21	8,327	9,238	All men	15-60	2,114
Zambia 1992	ZM21	6,209	7,060	na	na	na

Continued...

Table A.1—*Continued*

Country/year	Code or status	Household sample	Female sample	Male respondents	Age of men	Male sample
DHS-II 1990-92 (continued)						
North Africa/West Asia/Europe						
Egypt 1992	EG21	10,760	9,864	Husbands	na	2,466
Jordan 1990	Restricted	8,333	6,461	na	na	na
Morocco 1992	MA21	6,577	9,256	All men	20-70	1,336
Yemen 1991-92	YE21	12,836	5,687	na	na	na
South and Southeast Asia						
India 1992-93	IA22	88,562	89,777	na	na	na
Indonesia 1991	ID21	26,858	22,909	na	na	na
Pakistan 1990-91	PK21	7,193	6,611	Husbands	na	1,354
Latin America/Caribbean						
Brazil 1991 (Northeast)	BR21	6,064	6,222	Husbands	na	1,266
Colombia 1990	CO21	7,412	8,644	na	na	na
Dominican Republic 1991	DR21	7,144	7,320	na	na	na
Paraguay 1990	PY21	5,683	5,827	na	na	na
Peru 1992	PE21	13,479	15,882	na	na	na
DHS-III 1993-98						
Sub-Saharan Africa						
Benin 1996	BJ31	4,499	5,491	All men	20-64	1,535
Burkina Faso 1998-99	BF31	4,812	6,445	All men	15-59	2,641
Cameroon 1998	CM31	4,697	5,501	All men	15-59	2,562
CAR 1994-95	CF31	5,551	5,884	All men	15-59	1,729
Chad 1996-97	TD31	6,840	7,454	All men	15-59	2,320
Comoros 1996	KM32	2,252	3,050	All men	15-64	795
Côte d'Ivoire 1998-99	CI3A	2,122	3,040	All men	15-59	886
Côte d'Ivoire 1994	CI34	5,935	8,099	All men	15-59	2,552
Eritrea 1995	Restricted	5,469	5,054	All men	15-59	1,114
Ghana 1993	GH31	5,822	4,562	All men	15-59	1,302
Kenya 1998	KE3A	8,380	7,881	All men	15-54	3,407
Kenya 1993	KE33	7,950	7,540	All men	20-54	2,336
Madagascar 1997	MD31	7,171	7,060	na	na	na
Mali 1995-96	ML32	8,716	9,704	All men	15-59	2,474
Mozambique 1997	MZ31	9,282	8,779	All men	15-59	2,335
Niger 1998	NI31	5,928	7,577	All men	15-59	3,542
Senegal 1997	SN32	4,772	8,593	All men	20+	4,306
South Africa 1998	ZA31	12,247	11,735	na	na	na
Tanzania 1996	TZ3A	7,969	8,120	All men	15-59	2,256
Togo 1998	TG31	7,517	8,569	All men	Dec-59	3,819
Uganda 1995	UG33	7,550	7,070	All men	15-54	1,996
Zambia 1996	ZM31	7,286	8,021	All men	15-59	1,849
Zimbabwe 1994	ZW31	5,984	6,128	All men	15-54	2,141
North Africa/West Asia/Europe						
Egypt 1995	EG33	15,567	14,779	na	na	na
Jordan 1997	Restricted	7,335	5,548	na	na	na
Turkey 1993	TR41	8,619	6,519	na	na	na
Yemen 1997	Restricted	10,701	10,414	na	na	na
Central Asia						
Kazakhstan 1995	KK31	4,178	3,771	na	na	na
Kyrgyz Republic 1997	KY31	3,672	3,848	na	na	na
Uzbekistan 1996	UZ31	3,703	4,415	na	na	na

Continued...

Table A.1—*Continued*

Country/year	Code or status	Household sample	Female sample	Male respondents	Age of men	Male sample
DHS-II 1993-98 (continued)						
South and Southeast Asia						
Bangladesh 1996-97	BD3A	8,682	9,127	Currently married men	15-59	3,346
Bangladesh 1993-94	BD31	9,174	9,640	Husbands	na	3,284
Indonesia 1997	ID3A	34,255	28,810	na	na	na
Indonesia 1994	ID31	33,738	28,168	na	na	na
Nepal 1996	NP31	8,082	8,429	na	na	na
Philippines 1998	PH3A	12,407	13,983	na	na	na
Philippines 1993	PH31	12,995	15,029	na	na	na
Vietnam 1997	VN31	7,001	5,664	na	na	na
Latin America/Caribbean						
Bolivia 1998	BO3B	12,109	11,187	All men	15-64	3,780
Bolivia 1994	BO31	9,114	8,603	na	na	na
Brazil 1996	BR31	13,283	12,612	All men	15-59	2,949
Colombia 1995	CO31	10,112	11,140	na	na	na
Dominican Republic 1996	DR32	8,831	8,422	All men	15-64	2,279
Guatemala 1995	GU33	11,754	12,403	na	na	na
Haiti 1994-95	HT31	4,818	5,356	All men	15-59	1,610
Nicaragua 1997-98	NC31	11,528	13,634	All men	15-59	2,912
Peru 1996	PE31	28,122	28,951	All men	15-59	2,487
MEASURE *DHS*+ 1999-2003						
Sub-Saharan Africa						
Benin 2001	BJ41	5,796	6,219	All men	15-64	2,709
Burkina Faso 2003	BF41	10,000	12,000	All men	15-59	4,000
Eritrea 2002	Restricted	9,389	8,754	na	na	na
Ethiopia 2000	ET41	14,072	15,367	All men	15-59	2,607
Gabon 2000	GA41	6,203	6,183	All men	15-59	2,004
Ghana 2003	GH4Z	6,500	4,500	All men	15-59	4,500
Ghana 1998	GH41	6,003	4,843	All men	15-59	1,546
Guinea 1999	GN41	5,090	6,753	All men	15-59	1,980
Kenya 2003	KE40	8,561	8,195	All men	15-54	3,578
Malawi 2000	MW41	14,213	13,220	All men	15-54	3,092
Mali 2001	ML41	12,285	12,817	All men	15-59	3,390
Mozambique 2003	MZ41	12,087	12,193	All men	15-59	2,849
Namibia 2000	NM41	6,392	6,755	All men	15-59	2,954
Nigeria 2003	NG4A	7,225	7,620	All men	15-59	2,346
Nigeria 1999	NG41	7,647	9,810	All men	15-64	2,680
Rwanda 2000	RW41	9,696	10,421	All men	15-59	2,717
Senegal 1999	No SR	9,085	17,189	All men	15-59	7,850
Tanzania 1999	TZ41	3,615	4,029	All men	15-59	3,542
Uganda 2000-01	UG41	7,885	7,246	All men	15-54	1,962
Zambia 2001-02	ZM41	7,126	7,658	All men	15-59	2,145
Zimbabwe 1999	ZW41	6,369	5,907	All men	15-54	2,609
North Africa/West Asia/Europe						
Armenia 2000	AM41	5,980	6,430	All men	15-54	1,719
Egypt 2000	EG41	16,957	15,573	na	na	na
Egypt 1998	Restricted	6,759	6,406	na	na	na
Jordan 2002	Restricted	7,825	6,006	na	na	na
Turkey 1998	TR41	8,059	8,576	Husbands	na	1,971

Continued...

Table A.1—*Continued*

Country/year	Code or status	Household sample	Female sample	Male respondents	Age of men	Male sample
			MEASURE *DHS*+ 1999-2003 (continued)			
Central Asia						
Kazakhstan 1999	KK41	5,844	4,800	All men	15-59	1,440
Turkmenistan 2000	Restricted	6,303	7,919	na	na	na
South and Southeast Asia						
Bangladesh 1999-2000	BD41	9,854	10,544	Currently married men	15-59	2,556
Cambodia 2000	Restricted	12,236	15,351	na	na	na
India 1998-99	IA42	92,486	90,303	na	na	na
Indonesia 2002	ID41	33,088	29,483	Currently married men	15-54	8,310
Nepal 2001	NP41	8,602	8,726	Ever-married men	15-59	2,261
Philippines 2003	PH40	12,586	13,633	All men	15-54	4,766
Vietnam 2002	VN41	7,048	5,665	na	na	na
Latin America/Caribbean						
Colombia 2000	CO41	10,907	11,585	na	na	na
Dominican Republic 2002	DR4A	27,135	23,384	All men	15-59	2,833
Dominican Republic 1999	DR41	1,381	1,286	All men	15-64	1,112
Guatemala 1998-99	GU41	5,587	6,021	na	na	na
Haiti 2000	HT41	9,595	10,159	All men	15-59	3,171
Nicaragua 2001	NC41	11,328	13,060	na	na	na
Peru 2003	New design	6,000	6,000	na	na	na
Peru 2000	PE41	28,900	27,843	na	na	na

Note: All surveys of women include ages 15-49 except for India 1992-93 (ages 13-49).
na = Not applicable
Restricted = Country will not allow release of data without special permission

Appendix B Previous Assessments of Age and Date Reporting in DHS Surveys

As background to this report it is helpful to summarize the relevant methods of earlier assessments of DHS surveys. For the sake of completeness, the report's bibliography includes references to assessments of World Fertility Survey data and to some assessments of specific topics not included in this report, such as maternal mortality, but this appendix is confined to five DHS reports, listed below in chronological order and with an acronym for easy reference. The most recent assessment was published in 1996, and it appears that no surveys conducted after 1993 have been included in any general assessment.

Methodological Report No. 1, 1990 (MR1)

Institute for Resource Development. 1990. *An assessment of DHS-I data quality.* DHS Methodological Reports No. 1. Columbia, Maryland: Institute for Resource Development/Macro Systems Inc.

The authors of this report, in alphabetical order, are Fred Arnold, George Bicego, Ann Blanc, Naomi Rutenberg, Shea Rutstein, and Jeremiah Sullivan. A revision of the report by Fred Arnold was presented at the DHS World Conference in 1991 and appears in Volume II of the Conference Proceedings, pp. 785-806.

The report is a collection of four papers covering 22 DHS-I surveys between 1986 and 1989. The approach and measures for the present assessment of DHS data are fundamentally similar to those in MR1, which are outlined below.

Household survey
Completeness: Non-response rates
Misclassification: Household residency (misclassification of de jure and de facto residence)
Age heaping (using Myers' Index and a UN index of age-sex composition)
Age displacement across ages 15 and 49 (using age ratios and sex ratios)

The report includes some simulations of the effect of net age transfers outside the 15-49 age interval on the TFR, under-five mortality, and contraceptive use.

Individual survey, age and date of birth
Completeness
Age heaping (using Myers' Index)
Distortion of age distribution (simply identifies deviations from monotonicity)

Individual survey, age at first marriage
Completeness
Heaping of calendar year and years ago on 0 or 5
Consistency of trends in median age at first marriage between DHS and previous WFS surveys
Age bias due to marriage (trends in percentage of women ever-married by age 15-19 and 20-24 and comparison with previous survey or census)

Individual survey, age at first birth
Completeness
Heaping of calendar year and years ago on 0 or 5
Consistency of trends in median age at first birth between DHS and previous WFS survey
Age bias due to childbearing (trends in percentage of women with a first birth by age 15-19 and 20-24 and comparison with previous survey or census)

Individual survey, age at first sexual intercourse
Completeness, indicated by omission of age (date not asked)
Consistency of dates, indicated by age at first sex greater than age at first union or first birth

Birth histories
Completeness
Birth displacement (excess/deficit of births five or six years before survey, with three-year ratio, separately for dead children and living children)
Age heaping (using three-year ratio)
Miscalculation of year of birth (for children with age and birth year reported, month imputed)
Omission of births (using age at first birth by current age, and sex ratios at birth by years since birth)
Comparison of cumulative fertility rates for ages 15-34 for same reference period, measured by DHS and previous WFS survey

Mortality data in the birth histories
Completeness of date of birth, as above, separately for dead and living children
Birth displacement, as above, for dead and living children
Completeness of age at death
Heaping of age at death at 12 months for children who died at less than two years of age
Internal consistency among mortality rates
Comparison of under-five mortality rates for same reference period, measured by DHS and previous WFS survey

Occasional Paper No. 1, 1994 (OP1)

Curtis, Siân L. and Fred Arnold. 1994. *An evaluation of the Pakistan DHS Survey based on the reinterview survey.* DHS Occasional Papers No. 1. Calverton, Maryland: Macro International Inc.

This report is limited to a comparison between matched cases in the Pakistan DHS survey and a reinterview survey of 528 cases approximately six months later. Topics involving dates include agreement between year, month, and age of the woman's birth, first marriage, first live birth, and most recent live birth.

Occasional Paper No. 3, 1995 (OP3)

Curtis, Siân L. 1995. *Assessment of the quality of data used for direct estimation of infant and child mortality in DHS-II Surveys.* DHS Occasional Papers No. 3. Calverton, Maryland: Macro International Inc.

The relevant topics in this assessment are the dates of birth and dates of death in the birth histories that could affect the calculation of infant and child death rates. It included all 50 surveys conducted between 1986 and 1993 (28 DHS-I surveys and all 22 DHS-II surveys).

Birth histories
Completeness of reporting of births, by survival status
Displacement of births in the fifth calendar year before the start of the survey
Completeness of reporting of deaths at 0-24 months
Heaping on 12 months for deaths at 0-24 months (five point ratio)
Comparison between rates for ages 0 and 1-4, unadjusted and adjusted for heaping at 12 months
(adjustment is to shift 25 percent of excess at 12 months to the period 6-11 months)
Internal consistency among mortality rates
Comparison of under-five mortality rates for same reference period, measured by successive DHS surveys

Occasional Paper No. 4, 1995 (OP4)

Gage, Anastasia J. 1995. *An assessment of the quality of data on age at first union, first birth, and first sexual intercourse for Phase II of the Demographic and Health Surveys Program.* DHS Occasional Papers No. 4. Calverton, Maryland: Macro International Inc.

This occasional paper is largely a repetition of the chapter in MR1 assessing data on first union, first birth, and first sexual intercourse, but utilizing data primarily from DHS-II. It included a total of 26 surveys conducted between 1988 and 1993 (4 from DHS-I; 21 from DHS-II; and one, a repeat survey of Kenya, from DHS-III).

Individual survey, age at first union
Completeness
Heaping of calendar year and years ago on 0 or 5
Internal consistency of trends in median age at first marriage
Consistency between successive surveys in estimates of percent marrying by age 20
Age bias due to marriage (trends in percentage of women ever-married by age 15-19 and 20-24)

Individual survey, age at first birth
Completeness
Heaping of calendar year and years ago on 0 or 5
Internal consistency of trends in median age at first birth
Age bias due to childbearing (trends in percentage of women with a first birth by age 15-19 and 20-24)

Individual survey, age at first sexual intercourse
Response rate
Internal consistency of trends in median age at first sex
Consistency of dates, indicated by age at first sex greater than age at first union or first birth

Working Paper No. 19, 1996 (WP19)

Marckwardt, Albert M. and Shea Oscar Rutstein. 1996. *Accuracy of DHS-II demographic data: Gains and losses in comparison with earlier surveys.* DHS Working Papers No. 19. Calverton, Maryland: Macro International Inc.

This assessment included 25 DHS surveys done between 1989 and 1993. The main interest was in evidence of improvements in reporting when WFS, DHS-I, and DHS-II surveys are compared.

Household survey
Misclassification: Household residency (misclassification of de jure and de facto residence)
Age displacement across ages 15 and 50 (using age ratios and sex ratios)

Birth histories
Completeness of reporting of births, by survival status
Birth displacement (excess/deficit of births five or six years before survey, with three-year ratio, separately for dead children and living children)
Heaping on 12 months for deaths at 0-24 months (five point ratio)

Indexes of heaping in these earlier assessments

Earlier DHS assessments made considerable use of age ratios, sex ratios, and Myers' Index. Two other measures of heaping that have been used are based on a type of three-point ratio and five-point ratio. These are clarified below.

Three-point ratio: Let *a, b,* and *c* be the number of events in three consecutive intervals (e.g., ages 4, 5, 6). The "correct" value of b is estimated to be $(a+c)/2$; the ratio of observed to "correct" is

$$r = \frac{b}{(a+c)/2} = \frac{2b}{a+c}$$. (This may be multiplied by 100.)

The relative excess in *b* is described with $r-1$; if this is 0 then the progression across the three intervals is exactly linear.

Five-point ratio: This is the standard basis for identifying heaping at 12 months for age at death 0-24 months. Let *a, b, c, d, e* be the number of events in five consecutive intervals (e.g., months 10, 11, 12, 13, 14).

In MR1 and WP19, the "correct" value of *c* is estimated to be $(a+b+d+e)/4$; the ratio of observed to "correct" is

$$r = \frac{c}{(a+b+d+e)/4} = \frac{4c}{a+b+d+e}.$$

In OP3, the "correct" value of *c* is estimated to be $(a+b+c+d+e)/5$ and the ratio of observed to "correct" is

$$r = \frac{c}{(a+b+c+d+e)/5} = \frac{5c}{a+b+c+d+e}.$$

Singh Index of heaping: This measure first appeared in a chapter by Susheela Singh in a WFS publication (Goldman, Rutstein, and Singh, 1985). It is based on the second five-point ratio, calculated over a total time span of 25 years, and typically used to describe the "excess" reporting at final digits 0 and 5. For example, in its original use the time span was 1963-1987 and the interest was in possible heaping on 1965, 1970, 1975, 1980, and 1985. The underlying ratio of observed to "correct" was

$$r = \frac{c}{(a+b+c+d+e)/5} = \frac{5c}{a+b+c+d+e},$$

centered successively on 1965, 1970, 1975, 1980, and 1985.

The index is the average of these five ratios.

Appendix C Data Quality Tabulations Included in DHS Reports

The final report for each DHS survey includes an appendix with a standard set of data quality tables, usually C.1-C-6. If the survey includes men, Table C.2 has a panel for women and a panel for men. The formats and titles of these tables have been fixed since about 1990, when MR1 was published. Some reports have slightly different subtitles for the tables, but the content is always the same.

Usually, the only reference to data quality in the body of a DHS report is the discussion of response rates in the introductory chapter and a brief discussion in the chapter on infant and child mortality.

The tables in the appendix provide the most useful information on the quality of age and date data, but the appendix usually does not include any analysis or interpretation of the data. The following is a description of the DHS data quality tables and some comments on their use.

Table C.1. Household age distribution. Single-year age distribution of the de facto household population by sex (weighted)

This table can be used for calculation of Myers' Index, heaping at multiples of 0 and 5, and all measures of transfers across the age boundaries of eligibility. Measures of transfers based on age-specific sex ratios, not used in this report, are also possible with this table.

Table C.2. Age distribution of eligible and interviewed women. Five-year age distribution of the de facto household population of women age 10-54, and of interviewed women age 15-49, and percentage of eligible women who were interviewed (weighted)

The main value of this table is to identify whether the percentage of eligible women who were interviewed is constant across age, that is, whether the age distribution of respondents is consistent with the age distribution of eligible respondents. If there are substantial variations across age, it would be possible to revise the sample weights (v005) to improve some estimates, but it is not normal practice to do this.

Table C.3. Completeness of reporting. Percentage of observations missing information for selected demographic and health questions (weighted) [The selected items are birth date for births in the past 15 years, age at death for dead children in past 15 years, age/date at first union for ever-married women 15-49, and some additional selected variables.]

Completeness is a very important indicator of data quality, and it is essential to include this table.

Table C.4. Births by calendar years. Number of births, percentage with complete birth date, sex ratio at birth, and calendar year ratio by calendar year, according to living, dead, and total children (weighted) [The "calendar year ratio" is based on a three-year ratio. The calendar years are in single years for the most recent ten years, and then in five-year intervals going back to the earliest births in the birth histories.]

This table can be used to identify possible displacement or transfers of births. It can suggest possible omission of births, especially if the child died, but not reliably. Note that the sex ratio at birth, particularly for dead children, has a large standard error. A confidence interval can be obtained from a logit regression in which the dependent variable is sex of child, coded 1 for males and 0 for females, with no covariates (repeated within each combination of year of birth and survival status). The confidence interval for the intercept, after exponentiation and multiplication by 100, would be a confidence interval for the sex ratio.

Table C.5. Reporting of age at death in days. Distribution of reported deaths under one month of age by age at death in days and the percentage of neonatal deaths reported to occur at ages 0-6 days, for five-year periods of birth preceding the survey (weighted)

This table typically shows substantial heaping at 7, 14, and 21 days that could have some effect on estimates of very early mortality, but DHS reports do not generally give rates for shorter intervals than the first month (neonatal mortality). It is not clear that much can be done with this table with respect to the particular mortality rates currently included in DHS reports.

Table C.6. Reporting of age at death in months. Distribution of reported deaths under two years of age by age at death in months and the percentage of infant deaths reported to occur at age under one month, for five-year periods of birth preceding the survey (weighted)

In most surveys, this table shows a very high degree of heaping at 12 months, and substantial heaping at other months such as 6 and 18. DHS staff sometimes attempt to adjust the infant mortality rate (IMR) for heaping at 12 months by shifting one-fourth of the excess at 12 months down to 0-11. (The excess is the observed frequency at 12 months minus the expected frequency, which is calculated as the average at months 10, 11, 13, and 14.) Although this approach does give an impression of the sensitivity of ${}_1q_0$ and ${}_4q_1$ to the heaping at 12 months, it does not seem to have a good rationale. As noted, the ratio of observed to expected at 12 months is often 10 or more, and the heaping probably extends over a broader range than the two months below and above. Procedures for applying a model to Table C.6 to get a more robust adjustment are given in Pullum (2005).

Although we do not recommend changes to these tables, we suggest that the most important ones, namely Tables C.1 and C.4, be used routinely to calculate measures of heaping and transfers using the procedures in this report.

Appendix D Detailed Description of Method to Detect Age Displacement

Section 1.2 of this report described a procedure for detecting age displacement across a boundary such as age 15, or age 50, or the beginning month of the reproductive health calendar. This appendix provides more details on the procedure, for both aggregated and individual-level data, and will describe an empirical validation.

Application to aggregated data

As described in Section 1.3, the method uses four successive age intervals of equal width (single years or five-year age groups), two of which come before the boundary and two of which come afterwards. The intervals can be numbered 1, 2, 3, and 4, in increasing order of age. For example, for boundary 15, the intervals would be (1) age 5-9, (2) 10-14, (3) 15-19, (4) 20-24. The observed frequencies in the four intervals are a, b, c, d and the fitted frequencies are $\hat{a}, \hat{b}, \hat{c}, \hat{d}$, respectively.

There are two crucial assumptions. The first is that the only error of reporting is in the allocation across the second and third intervals. That is, the first and fourth frequencies are "correct," $\hat{a} = a$ and $\hat{d} = d$, and the sum of the middle two frequencies is "correct," $\hat{b} + \hat{c} = b + c$. Thus it is important that the intervals be wide enough for it to be plausible that displacement does not extend beyond the middle two intervals.

The second assumption is that the "correct" frequencies have a linear pattern of progression on a log scale (most models for frequencies or odds assume some form of linearity on a log scale). That is, it is possible to find constants α and β such that

$$\ln(\hat{b}/\hat{a}) = \alpha, \ \ln(\hat{c}/\hat{b}) = \alpha + \beta, \text{ and } \ln(\hat{d}/\hat{c}) = \alpha + 2\beta .$$

It is not actually necessary to solve for the two constants, because the sum of the three equations gives

$$\ln(\hat{d}/\hat{c}) + \ln(\hat{c}/\hat{b}) + \ln(\hat{b}/\hat{a}) = 3\alpha + 3\beta = 3(\alpha + \beta), \text{ or } \ln(\hat{d}/\hat{a}) = 3\ln(\hat{c}/\hat{b}). \text{ Therefore}$$

$$\hat{c}/\hat{b} = (\hat{d}/\hat{a})^{1/3} = (d/a)^{1/3}, \text{ which leads to}$$

$$\hat{c} = (\hat{b} + \hat{c})\frac{(d/a)^{1/3}}{1 + (d/a)^{1/3}} = (b + c)\frac{(d/a)^{1/3}}{1 + (d/a)^{1/3}} \text{ and } \hat{b} = b + c - \hat{c} .$$

A coefficient for the amount of transfer could be calculated as the log of the observed ratio in the middle two categories minus the log of the expected ratio: $t = \ln(c/b) - [\ln(d/a)]/3$, which (with the usual Poisson-based approximations) has an estimated variance of $s^2 = (1/9a) + (1/b) + (1/c) + 1/9d)$. A simple test of the null hypothesis that there are no net transfers between categories 1 and 2 would be given by $z = t/s$, which would have an approximately normal sampling distribution if the null hypothesis is true.

The proportion of cases in category 2 that are estimated to be shifted out of that category and into category 3 will be $(\hat{b} - b)/\hat{b} = 1 - (b/\hat{b})$. Similarly, the proportion of cases in category 3 that are estimated to be shifted out of that category and into category 2 will be $(\hat{c} - c)/\hat{c} = 1 - (c/\hat{c})$.

This aggregated-data form of the model can be used to conduct a simple check for early evidence of age displacement in contexts where it seems likely, using unweighted numbers of cases in four successive age intervals.

Application to individual-level data

We now describe how this method may be easily applied to individual-level data using logit regression. We construct two binary variables, *yad* and *ybc*. Both refer to females only; they are "missing" for males. Variable *yad* is defined to be 1 if the woman is 20-24 and 0 if she is 5-9; otherwise it is missing. Variable *ybc* is defined to be 1 if the woman is 15-19 and 0 if she is 10-14; otherwise it is missing.

In a logit regression of *yad*, with no covariates, the intercept is equal to ln(*d*/*a*). If this coefficient is divided by 3, we have $\ln(\hat{c}/\hat{b})$. In a logit regression of *ybc*, with no covariates, the intercept is equal to $\ln(c/b)$. Using the relationship between odds and proportions, we can then readily obtain the measure of upward shift, $1-(b/\hat{b})$, or the measure of downward shift, $1-(c/\hat{c})$. One of the advantages of the individual-level format with logit regression is that it allows us to incorporate sampling weights and clustering. It also can produce a test statistic of whether the measures of shift are significantly different from zero. That test statistic was not included in this report, but all evidence of shifting that was reported is highly significant.

The logit regression approach will be illustrated with Stata code to estimate net transfers from age 15-19 to age 10-14 in the household survey. Here, agevar is age coded in five year intervals, with ages 0-4 coded 0, ages 5-9 coded 1, etc. The weight variable is hv005 and the cluster variable is hv001.

```
gen yad=.
gen ybc=.
replace yad=0 if agevar==1
replace yad=1 if agevar==4
replace ybc=0 if agevar==2
replace ybc=1 if agevar==3
logit yad [pweight=hv005],cluster(hv001)
matrix b=e(b)
matrix V=e(V)
scalar slopead=b[1,1]
scalar varad=V[1,1]
scalar slopebchat=slopead/3
scalar varbchat=varad/9
logit ybc [pweight=hv005], cluster(hv001)
matrix b=e(b)
matrix V=e(V)
scalar slopebc=b[1,1]
scalar varbc=V[1,1]
* t is the measure of transfer, log of ratio of observed odds to expected odds
* where odds is category above boundary to category below boundary
* negative for downward shift, positive for upwards shift
* set is the estimated standard error of t
scalar t=slopebc-slopebchat
scalar set=sqrt(varbc+varbchat)
scalar z=t/set
```

```
scalar list t set z
scalar bobs=1/(1+exp(slopebc))
scalar cobs=1-bobs
scalar bhat=1/(1+exp(slopebchat))
scalar chat=1-bhat
scalar pshiftb=1-(bobs/bhat)
scalar pshiftc=1-(cobs/chat)
scalar list pshiftb pshiftc
```

Validation of the method

To assess the validity of this procedure, the version for aggregated data was applied to several five-year age distributions estimated for 1995 by the Population Division of the United Nations (United Nations, 2003). The year 1995 was selected partly because it was within the range of the DHS survey dates. It also preceded the 2000-2001 censuses that most countries conducted and was sufficiently in advance of the UN publication date (2003) that the tabulations are probably final estimates for 1995 and not contaminated with any projections. It is possible that in some cases the UN data were smoothed or interpolated with statistical and demographic methods, which would tend to make them more consistent with our model, but we have tried to avoid smoothed data.

Nine tabulated age distributions were selected, representing countries that showed the highest levels of transfers in the report. Six distributions refer to women in African countries. The other three refer to men in Armenia, Kazakhstan, and Kyrgyzstan. The model was applied to five-year age groups in the range from 0-4 to 55-59, leading to an estimate of outward shifts for ages 5-9 through 50-54. The results are given in Table D.1. The percentages in column 3 are estimates of the percentage of "correct" cases that were shifted upward to the next interval, assuming that the age interval occupied position 2 in the four-interval model. The percentages in column 4 are estimates of the percentage of "correct" cases that were shifted downward to the next interval, assuming that the age interval occupied position 3 in the model. If the UN tabulations were correct, and if the assumptions of the model were correct, all of the percentages in columns 3 and 4 would be zero. Our interest is mainly in whether the model gives spurious evidence of shifts in the vicinity of ages 15 and 50, specifically in the estimates for age 15-19 in column 4 and for age 45-49 in column 3.

The results given in Table D.1 indicate that the model worked very well in the African countries. The spurious estimates of downward transfers from age 15-19 into 10-14 range from -0.3 percent to 2.5 percent. These are well below the threshold of 10 percent used in Chapter 2. The spurious evidence of upward transfers from 45-49 into 50-54 range from -4.0 percent to 2.8 percent, consistently well below the thresholds of 20 percent used in Chapter 2.

Considering that these results come from the African countries with greatest evidence of age transfers in Chapter 2, we infer that the method was successful in that context. A somewhat different conclusion is reached for men in Armenia, Kazakhstan, and Kyrgyzstan, given in the last two panels of Table D.1. The percentages in columns 3 and 4 for these panels are generally larger in magnitude and more erratic than for the African women. These spurious estimates of transfers do not exceed the thresholds of 10 percent and 20 percent used in Chapter 2, but they would exceed thresholds of, say, 5 percent and 10 percent. There appear to be some genuine inconsistencies between the assumptions of our model and the actual age distributions for these countries, which may have caused an exaggerated appearance of transfers.

Table D.1 Estimated percentages shifted out (+) or in (-) when the four-category method is applied to UN age distributions (United Nations, 2003)

Beginning of age interval	Population (in thousands)	Estimate when the age interval is the second in the sequence of four	Estimate when the age interval is the third in the sequence of four
	Burkina Faso Females 1995		
0	1000	na	na
5	813	0.7	na
10	687	-0.3	-0.8
15	577	1.0	0.3
20	490	-0.1	-1.2
25	394	0.3	0.2
30	303	-2.2	-0.3
35	219	0.5	3.0
40	174	0.6	-0.6
45	147	0.0	-0.7
50	127	na	-0.0
55	112	na	na
	Ghana Females 1995		
0	1385	na	na
5	1261	2.1	na
10	1168	-2.1	-2.4
15	960	0.9	2.5
20	805	-1.4	-1.1
25	647	1.0	1.7
30	546	-0.1	-1.2
35	453	-0.4	0.1
40	371	0.4	0.0
45	307	-0.1	-0.5
50	250	na	0.2
55	202	na	na
	Kenya Females 1995		
0	2214	na	na
5	2200	-1.1	na
10	1931	-0.4	1.2
15	1605	-0.1	0.4
20	1295	-0.9	0.1
25	1019	0.6	1.1
30	830	0.1	-0.8
35	672	1.2	-0.1
40	538	-0.7	-1.6
45	392	-4.0	1.0
50	274	na	5.2
55	243	na	na

Continued...

Table D.1—*Continued*			
Beginning of age interval	Population (in thousands)	Estimate when the age interval is the second in the sequence of four	Estimate when the age interval is the third in the sequence of four
	Madagascar Females 1995		
0	1238	na	na
5	999	0.7	na
10	846	0.2	-0.8
15	719	-0.2	-0.3
20	604	-0.2	0.2
25	507	0.2	0.0
30	430	0.4	-0.2
35	364	1.1	-0.5
40	299	-3.8	-1.3
45	222	2.8	4.7
50	192	na	-3.5
55	160	na	na
	Nigeria Females 1995		
0	8843	na	na
5	7386	0.7	na
10	6288	-0.2	-0.8
15	5207	-0.4	0.3
20	4255	-0.0	0.4
25	3513	0.0	0.0
30	2938	0.2	-0.0
35	2487	1.5	-0.3
40	2099	-3.2	-1.8
45	1602	2.3	3.9
50	1371	na	-2.8
55	1128	na	na
	Uganda Females 1995		
0	2031	na	na
5	1610	0.6	na
10	1329	-0.1	-0.8
15	1095	0.8	0.1
20	904	-0.2	-1.0
25	708	-1.9	0.3
30	535	1.1	2.4
35	443	0.4	-1.3
40	374	0.4	-0.5
45	314	1.1	-0.4
50	256	na	-1.4
55	188	na	na

Continued...

Table D.1—*Continued*			
Beginning of age interval	Population (in thousands)	Estimate when the age interval is the second in the sequence of four	Estimate when the age interval is the third in the sequence of four
	Armenia Males 1995		
0	140	na	na
5	176	-3.0	na
10	167	-2.2	3.0
15	143	-0.9	2.4
20	127	-1.3	1.0
25	124	5.8	1.3
30	144	1.7	-5.6
35	141	-2.4	-1.8
40	105	-0.7	3.0
45	70	-17.3	1.0
50	44	na	19.0
55	79	na	na
	Kazakhstan Males 1995		
0	753	na	na
5	940	-7.4	na
10	810	1.1	7.4
15	750	1.2	-1.2
20	696	-6.5	-1.4
25	599	9.6	6.6
30	708	-3.3	-9.8
35	642	3.6	3.4
40	545	-7.8	-4.5
45	340	-5.5	10.4
50	271	na	6.2
55	393	na	na
	Kyrgyzstan Males 1995		
0	287	na	na
5	297	-2.5	na
10	259	-1.2	2.7
15	222	1.9	1.4
20	202	-3.3	-2.1
25	173	4.0	3.6
30	172	0.9	-4.4
35	154	-0.5	-1.1
40	117	-1.0	0.6
45	78	-14.1	1.4
50	49	na	16.5
55	74	na	na

na = Not applicable

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2. Macro International Inc. 1993. **An Assessment of the Quality of Health Data in DHS-I Surveys.**

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