Monitoring Child Mortality Change Through Household Surveys

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Abstract

Under-5 mortality is a major component of the global development agenda, and there is increasing need for rapid mortality monitoring to evaluate program impact. Most low- and middle-income countries do not have fully functional civil registration systems, so monitoring has largely been through periodic household surveys using full birth histories. However, such surveys are impractical for rapid monitoring because of cost considerations. Surveys using summary birth histories (SBHs) are much cheaper, and can therefore in principle be carried out more frequently for larger samples, but existing methods to analyze such data do not provide estimates for short time periods. We have developed and tested in five countries two new methods of analysis that can provide estimates for single year periods, one using imputed birth histories and the other using cohort changes between surveys. Estimates from both methods are critically dependent on the quality of the underlying SBHs, giving good estimates from high quality surveys.

Introduction

Child mortality in low- and middle-income countries has declined dramatically over the last two decades; the Under-5 Mortality Rate (U5MR) for this group of countries is estimated to have declined by 50% from 1990 to 2013, an annual rate of reduction of 3.0% [1]. Most of these countries, however, lack fully functional civil registration systems, and estimates of U5MR and other measures of childhood mortality are not derived from continuous recording systems but rather from periodic household surveys collecting birth histories from surveyed women. Although such surveys generally provide an adequate basis for monitoring progress at the country level towards, for example, Millennium Development Goal 4 [2], they do not provide a basis for monitoring annual changes in U5MR, partly because of sample size limitations and partly because the surveys are rarely conducted more frequently than every five years. However, demand for annual monitoring has increased, particularly among development partners that wish to be able to demonstrate the effectiveness or otherwise of investments in child survival projects in "real time". Regular, rapid monitoring of child mortality also provides an alert system for emerging health threats, and a basis for prioritizing health interventions.

With funding from Canadian DFATD, the Johns Hopkins Institute of International Programs initiated a research project in 2008 to evaluate methods for rapid mortality monitoring (RMM) in five sub-Saharan African countries lacking functional civil registration systems. The project tested two broad approaches: one to provide a low-cost alternative to a civil registration system using community based health workers to record

births and child deaths in their catchment areas; and the other to explore innovative ways of using household surveys for RMM. In this paper we report on two such innovative survey-based approaches.

In principle, surveys with full birth histories (FBH), pioneered by the World Fertility Survey [3] in the 1970s and widely implemented since the late 1980s by, among others, the Demographic and Health Surveys program [4], offer the potential for RMM. The FBH asks a representative sample of women about the date of birth, survival status, and (if the child has died) age at death of every one of her live-born children. However, such questions are interview time- and interviewer training-intensive, and therefore expensive per interview. In settings of moderate to low child mortality, particularly where combined with low fertility, large samples of women are required to limit sampling errors. Thus the FBH methodology is expensive to implement, the periodicity of such surveys is typically five years or more, and adequately precise estimates of U5MR and other indicators cannot be calculated for short time periods [5]. As a result, the FBH is not in practice appropriate for RMM; large annual surveys would be prohibitively expensive.

Other ways of using birth histories to measure child mortality have been developed. The most widely-applied is the summary birth history (SBH), whereby women are asked about the number of children they have given birth to and the number of those children that have died. In combination with the age (or duration of marriage, or time since first birth) of the woman, probabilities of dying in childhood can be derived from proportions dead of children ever born through modeled relationships [6-9]. However, the number of children ever born to women of a given age, and the number of those children that have died, clearly reflect births over a number of years prior to a survey, and mortality rates of those children over the same number of years and over a range of ages. The mortality estimates derived from proportions dead of children ever born are thus weighted averages of mortality risks over both age and time period, and are neither time-period nor age-range specific, limiting their value for RMM purposes. On the other hand, the SBH questions are much less interview time- and interviewer training-intensive than FBH questions, and therefore much less expensive per interview (so much so that SBH questions are often included in population censuses in low- and middle-income countries).

In view of the cost advantages of the SBH, and therefore the ability to collect information frequently from much larger numbers of women than would be possible with an FBH, the RMM Project decided to test two new approaches based on SBHs that it was hoped would overcome the two key SBH problems of being neither age range- nor time period-specific. In practice, cost savings would be even larger, because SBHs would not need specially-conducted surveys but could be included opportunistically in other representative household surveys. We will refer to the two methods as the Birth History Imputation method (BHI) and the Cohort Change method (CC). The performance of both methods is critically dependent on the quality of the SBH; methods for evaluating the quality of such histories are fully described elsewhere [9].

Methods

The BHI Method

The BHI method, first explored by Montana [10] but closely related to the maternal age cohort method proposed by Rajaratnam et al. [11], is predicated on the observation that almost all low- and middle-income countries have carried out at least one FBH survey, and on the assumption that the internal dynamics (that is, birth intervals between children and ages of death of children that die) of individual birth histories change fairly slowly as fertility and child mortality change, whereas the distribution of birth histories by numbers of children ever borne (CEB) and died (CD) across women will change more rapidly. Given this assumption, it is possible to improve the specificity of estimates derived from an SBH by borrowing FBHs from an earlier FBH survey and imputing them onto SBH women, retaining the numbers of children born or dead from the SBH, but retaining the time distribution of events of a given type from the FBH. However, the BHI method can never provide true period-specific estimates; suppose for example that child mortality fell to zero in a year; the imputation, using FBHs from an earlier survey, would still impute both births and child deaths into the year, and therefore produce non-zero mortality estimates. The method will in theory work best in situations in which child mortality is changing fairly steadily over time, and mortality differentials by, for example, age of mother or preceding birth interval, are not changing.

The specifics of the method are as follows. Women in each survey are categorized by 5-year age (or duration of marriage or time since first birth) group, number of CEB and number of CD; a woman in a given SBH category (for example, age 25 to 29, 3 CEB, 1 CD) will be assigned at random an FBH from a woman in the same category in the FBH. To reduce random variation, the matching is repeated 10 times; the matching is carried out with replacement, so if there are fewer cases in the FBH dataset than in the SBH dataset, the FBH dataset is expanded by the required factor prior to matching. The SBH with the appended FBHs is then be analyzed by time period and age range as if it had been collected as an FBH. The explicit assumption made by the method is that, for women in a given category, the time distribution of births and ages and dates of child deaths do not change over time. In practice, the effect of this assumption is mitigated by geographic and temporal proximity; we use FBHs from the same country and for a time point as close before the date of the SBH as possible, though we also test the method using an FBH from a neighboring country and from an FBH survey held a decade before the SBH.

The BHI method is entirely straightforward to implement, except in one respect: how to treat non-matches, that is, cases in the SBH that do not have matches in the FBH (cases in the FBH that do not have matches in the SBH are irrelevant). We tested two approaches: first, to reduce the number of matching categories to CEB and CD only, dropping age of woman, and then drop any small number of cases that still remain unmatched; and second, to create from all available DHS FBH surveys a compendium of all recorded category combinations, each with up to 10 histories, and then borrow from this master set for all unmatched cases; the very small number of cases still unmatched are then dropped.

The compendium method performed much better in terms of proportions ultimately matched and in approximating the proportions dead of children ever born by age of mother (the key measure for standard indirect estimation) in the original data set, so we only present results based on the compendium method here.

The BHI method was validated in all five RMM Project countries (Ethiopia, Ghana, Malawi, Mali and Niger), in one case more than once. For the purposes of this paper, we have applied the method to nationally-representative surveys from three additional countries in sub-Saharan Africa (Cameroon, DRC and Kenya); these additional applications were selected because they had SBHs from Unicef's Multiple Indicator Cluster Survey (MICS) program that produced indirect estimates of U5MR good enough to be included in the U.N. IGME [1] child mortality estimation process and also had DHS FBHs from surveys both before (to provide histories) and after (for validation) of the MICS survey. In principle, the estimation and validation approach followed was to take a nationally-representative SBH, impute onto it FBHs from an earlier DHS FBH survey, compute child mortality indicators (neonatal, infant and under-5 mortality) from the imputed FBH for 10 calendar years before the (SBH) survey, and then compare these estimates with those computed for the same calendar years from a subsequent DHS or other FBH survey. Clearly, estimates from the subsequent survey will be affected by sampling errors (and possibly by systematic errors as well), but estimates from such surveys are regarded as current best practice in countries lacking full civil registration.

The Cohort Change Method

The CC method is an extension of a method proposed by Zlotnik and Hill [12], who show that cumulated cohort changes in average numbers of children ever born and children dead between surveys will be closely related to the levels of fertility and child mortality in the inter-survey period. The method primarily addresses the issue of lack of time specificity in SBH estimates, and does not produce estimates that are age group specific. The key assumption of the method is that repeated survey cross-sections can be treated as quasi-cohorts; for example, that women age 27 at representative survey 1 in 2010 and women age 28 at representative survey 2 in 2011 can be regarded as being a cohort of women born in 1983. Other things being equal, change in average number of children ever born for each cohort from one year to the next is driven by the fertility rate for the cohort during the year, and change in average number of children dead is driven by child deaths (at any age) during the year. The basic idea is illustrated in Figure 1. Changes in average CEB and average CD from one survey to the next for all quasi-cohorts of women of reproductive age can then be cumulated, and the sums will reflect fertility (the sum of the changes in CEB will be equal to the period total fertility rate) and child deaths during the year, but will be unaffected by births and deaths before the start of the year. The ratio of cumulated CD change (c-CD) to cumulated CEB change (c-CEB) will largely, though not exclusively, be determined by the risk of child mortality during the year.

To explore the relationship between the c-CD/c-CEB ratio and standard measures of child mortality, we extracted data from 154 DHS surveys covering 69 low- and middle-income countries on numbers of births and deaths (at any age) of women's children for each of

the 5 calendar years before each survey, and also calculated period measures of infant and child mortality using standard methods [9] for the same 5 years. Figure 2a shows the relationship between the natural log of the c-CD/c-CEB ratio and the natural log of the under-5 mortality rate (U5MR) for each of the 5 calendar years. The relationship is remarkably close; as a first-order approximation, the ratio for a particular year measures U5MR for that year. Figure 2b shows the relationship for two year as opposed to one year periods; again, correspondence is close.

However, the first-order approximation is not perfect, and since our objective is to estimate U5MR, we explore reasons for divergence. For true cohorts, cumulated changes in average numbers of children born between two surveys will perfectly reflect fertility between the two surveys; cumulated changes in average numbers of children dead will also reflect deaths of children between the surveys, but those deaths will occur at a wide range of ages, from zero to over 30 (for women in their late 40s). The relationship between the ratio of the two cumulated changes and the U5MR will therefore depend on, or be confounded by, the age distribution of the deaths of the women's children, which will be determined by the age distribution of children and the age pattern of mortality. The biggest factor in determining the age pattern of (surviving) children will be recent fertility. Consider for example a case in which fertility in a year falls to very low levels; the c-CEB value will be very small, but the c-CD value will still be affected by deaths of children born in previous years; the ratio will then over-estimate U5MR, partly because the age distribution of deaths will be shifted from below 5 to above 5. Similarly, the age distribution of women of reproductive age will also affect the age distribution of intersurvey deaths: a high proportion of women of older reproductive age (say 35 to 49) who have largely completed their childbearing will tend to increase c-CD relative to c-CEB as a result of deaths of children at ages over 5 relative to those under age 5. An age pattern of child mortality with high mortality above age 5 relative to that before age 5 will have the same effect; one factor that might contribute to such a mortality pattern is high HIV prevalence.

Indicators of some of these potential confounders can be found in the SBHs themselves, and can therefore be incorporated into the analysis. Fertility change will be reflected in the difference between period TFR (measured by c-CEB) and lifetime fertility, as measured by the average CEB of women over age 40. The age distribution of women can be incorporated as the ratio of the number of women aged 20-24 to the number of women aged 40-44. A rough indicator of the age pattern of child mortality is available as the ratio of the proportion dead of children ever born to women aged 25-29 to that of women aged 45-49; this indicator will also be affected by any trend in child mortality level, which in turn will affect the age distribution of surviving children. The age pattern of deaths of women may also be affected by HIV prevalence (as a result of deaths of young adult children), and national-level annual estimates of HIV prevalence are available from UNAIDS [14].

An additional complication arises from the common error in surveys of low- and middle-income countries of age heaping, that is, differential reporting of ages ending in digits 0 and 5. This error is a problem because it is also associated with reported values of CEB

and CD: there is often a sharp step up or down in average CEB and average CD between one single year age and the next, heaped year. Since the size of cohorts ending in 0 and 5 is larger than that of neighboring cohorts, the effects of cohort changes between one survey and the next will be exaggerated or diminished. To reduce the impact of this error, we use rolling five year cohorts incremented by single years of age, thus computing cohort change for 15-19 (16-20, 17-21, etc.) year olds at the first survey and 16-20 (17-21, 18-22, etc.) year olds at the second.

We use the same data set from DHS on which Figure 2 is based to develop a regression model of the relationship between U5MR and the c-CD/c-CEB ratio, controlling for confounders using only indicators collected in an SBH or available from external sources such as UNAIDS. For estimation purposes, we use the following model:

$$\ln\left(U5MR_t\right) = \beta_0 + \beta_1 \ln\left(\frac{c-CD_t}{c-CEB_t}\right) + \beta_i X_{it} \qquad (1)$$

where the X_{it} are variables controlling for factors likely to influence the age distribution of deaths of children in year *t*. As discussed above, we use the ratio of total fertility (c-CEB) in year *t* to cohort lifetime fertility (average parity of women aged 40 and over) to control for fertility change, the ratio of number of women aged 20-24 to the number aged 40-44 to control for age distribution of women of reproductive age, the ratio of the proportion dead of children ever borne by women aged 25-29 to that of women aged 45-49 as a rough measure of the age pattern of child mortality, and HIV prevalence 5 years before year t to control for any effect of HIV on the age distribution of deaths.

Table 1 shows results of regressions using the formulation in equation (1). Results are presented for survey intervals of both one year and two years. The first and third columns show the regressions without controls, the second and fourth with controls. As Figure 2 suggests, the simple relationship between ln(U5MR) and ln(c-CD/c-CEB) is close to one to one, with intercepts close to 0 and slopes only slightly greater than one; the simple relationship accounts for over 95% of the variance in ln(U5MR). As expected from the discussion above, all four of the variables intended to control for confounding are highly significant ($p < .001$ except for the HIV prevalence variable), with coefficients in the expected direction. Adding the four controls improves the fit somewhat (over 96% of variance); interestingly, the coefficient on the main independent variable becomes insignificantly different than 1.0.

Results

The methods presented above were developed for the RMM project, and were applied to data sets from all five RMM countries. However, for the purpose of this paper, we have added some applications to non-RMM countries, to test potential uses not covered by the RMM work, and dropped one application (BHI for Niger, for which in the RMM work we treated a 2010 FBH survey as if it were an SBH survey). In each case, we apply the method in as realistic a way as possible, principally by applying them to SBH surveys

and not treating FBH surveys as if they were SBH ones, and evaluate the estimates of U5MR obtained by comparison with independent estimates from a later data source or estimation exercise.

Birth History Imputation

For this method, validation consists of comparing calendar year estimates of mortality indicators derived from an imputed full birth history to estimates for the same calendar year drawn from a later full birth history. The values against which we validate are thus only as good as the surveys from which they are taken, and are potentially affected by both sampling and non-sampling errors; however, the full birth history is regarded as the current best practice for measuring child mortality in countries lacking accurate vital statistics.

We apply the BHI method to data from 12 surveys conducted in seven countries, four of which were part of the RMM project. Six of these applications are to data from nationally-representative surveys (with SBH data from population census samples (2) or MICS (4), and FBH data from DHSs or MICS). Three applications are to RMM project areas only, with SBH data either from population census samples or baseline RMM surveys, FBH surveys for imputation from DHSs and FBH data for evaluation from endline RMM surveys. The remaining 3 applications are to data from nationally-representative surveys (with SBH data from MICS, but testing the robustness or otherwise of the method to the contemporaneity of the FBH used for imputation, with time lapses of 11 or more years, or geographic proximity, using FBH data for imputation from a neighboring country). One RMM country (Niger) is not included because no large-scale SBH survey has been conducted.

Table 2 summarizes the results of the applications in terms of estimates of under-5 mortality rates. Results are very disappointing. Across the 12 applications, the average U5MR across 10 single year periods estimated by imputation is only within 10% of the average of the verification estimates in two cases, and differs by more than 20% in 3 cases. On the positive side, there doesn't seem to be any clear bias, such that of the 10 applications on average off by more than 10%, 5 over-estimate and 5 under-estimate. Applying an arbitrary standard of ± 20 % for an individual annual estimate relative to the corresponding verification estimate as being acceptable (this may seem like a very loose standard, but it is important to remember in evaluating these results that both the imputation and validation data sets have potential sampling errors), in only one application (Malawi) is every estimate "acceptable,", and in only 2 more are 8 or 9 out of 10 "acceptable." In 3 of the applications, half or more of the estimates differ from the verification estimate by over 20%. In the 3 applications that do not follow the normal method of using an FBH from the same country from shortly before the SBH survey, results using birth histories from a survey more than a decade before the SBH survey are both worse than those from using a more contemporaneous FBH, but the one test of using FBHs from a survey of a neighboring country gives results very similar to the normal application.

The question that naturally arises from these results is why they are so bad. There are a number of potential sources of error in the BHI method, but the most basic source of difference from a validation data set may have nothing to do with the method, but may arise from the quality of the summary birth history itself; if proportions of children dead are very different in the two data sets, the U5MR estimates will also be very different. There is some indication that this may be the case in the three applications to Kenya, where different FBHs are imputed onto the same SBH: results from the three applications are quite similar, but they all under-estimate, suggesting that the problem is not the imputation but the underlying SBH. This possibility is explored further in Table 3, which compares proportions dead of children ever born by age group of mother from the SBH survey and the validation survey for 3 applications, one a relatively successful one (Malawi), and two unsuccessful ones (Mali and Ghana). As can be seen, the SBH proportions dead are similar to those from the validation survey for Malawi, but substantially lower for the other two cases. It is also interesting to note that when we applied the method to a summarized FBH instead of an SBH in Niger, results were very good, with all 10 estimates 'acceptable' and a mean relative error of zero.

Another potential source of error in the BHI method is the treatment of first step non-matches. As discussed in the Methods section, we used two approaches to such non-matches: first, relaxing the matching criteria to agreement only on CEB and CD, but not age of mother; and second, matching the cases to a compendium of CEB/CD/Age of mother types drawn from all DHS surveys. The second approach was found to be superior on two key criteria: the proportion of cases ultimately matched; and the replication of proportions dead of children ever born by age of mother, the key metric for indirect estimation. We therefore report only on the second (compendium) method. Table 4 shows by application the proportions of all cases that matched on the first step, and the additional proportions that ultimately matched on the second step. In general, the proportion of first step matches is high, 93% to 99%, and the additional matches from the compendium range from 0.9% to 6.7%, and the final proportion of non-matches is extremely low in all cases except Mali, where 0.6% of cases never matched Further exploration of the somewhat lower match rate for Mali indicated that it was largely due to some really extreme CEB/CD types for women under 25 in the 2009 Census.

Cohort Change

The Cohort Change (CC) method was developed for application to SBH surveys separated by one or two years. Unfortunately, there were no suitably-spaced pairs of SBH surveys in the RMM countries, so we used combinations of SBH surveys and FBH surveys treated as if they were just SBH surveys. This is problematic as a way of testing the methodology, since consistency of data collection is crucial to ensuring that any errors in the surveys are broadly similar. We have therefore added exploratory applications to data from Brazil, which has conducted a large, standardized household survey (PNAD) that included an SBH annually on comparable sample frames since 2004, with the exception of 2010. These data allow the application of the CC method exactly as it was originally conceptualized (to broad, general household sample survey data) to 5 single-year and 5 two-year intervals. For the RMM countries, we applied the CC method

twice in three of the five RMM countries, for a total of six additional applications; in four cases, we applied the method to a pair of surveys, one of which was an SBH survey, and one an FBH survey, and in the other two we applied the method to a pair of FBH surveys. Table 5 shows the data sources used and summarizes the results.

The results are extremely disappointing. Of the RMM applications, only one of the six resulted in positive cumulated cohort change in average numbers of dead children (a negative number suggests under-reporting of dead children at the second survey relative to the first), although in that one application (Malawi 2008 Census to 2010 DHS), the estimates U5MR of 102 per 1,000 was within 10% of the RMM validation estimate, 109. Of the 10 applications to PNAD data, cumulated cohort change in average numbers of dead children was negative in two cases (2007-08 and 2008-09); in the remaining 8 cases the estimated U5MR varied from 0% (2007 to 2009) to 386% of the validation measure (U5MR as estimated by the United Nations Inter-Agency Group on Mortality Estimation [1]), though one estimate, 23.2 for 2004-06, was very close to the validation measure of 23.0. Despite the rather poor results, there is no evidence of systematic bias – 4 estimates are too high, 5 (including the negative cohort change case) are too low, and one as noted above is very close. Across all the applications, it seems that reports of children ever born and children dead are just not precise enough to survive the challenge of differencing, even when survey instruments and methods are highly standardized, as in the case of the Brazilian PNAD surveys. The main problem seems to be the consistency of reporting of dead children. The TFRs estimated from the cumulated cohort changes in CEB for the PNAD surveys range from 1.77 to 2.42, bracketing the UN population Division estimate of 1.90 for the period 2005-10 [14].

We use an example from Ghana to illustrate the problem of small differences between surveys being exacerbated by differencing. The initial SBH comes from the 2006 Multiple Indicator Cluster Survey, the second from FBHs from the 2007 Maternal Mortality Survey. Figure 3 shows the average number of children dead by rolling 5-year cohorts of women incrementing by one year of age from 15-19 to 40-44 from each source, and the increments in average number of children dead calculated from one survey to the next for each cohort. Although the patterns of average numbers of children dead by age are very similar, age by age the 2006 numbers are higher than the 2007 numbers (as could be the case as a result of declining child mortality); however, all the cohort increments after age 28 except two are negative, which cannot be explained by declining child mortality, and the sum of cohort changes is marginally negative at -0.0100 of a dead child (Table 5).

Discussion

We have developed and tested the validity of two methods for analyzing summary birth history data to obtain more timely or detailed estimates of under-5 mortality than standard indirect methods provide. More effective use of such data is potentially attractive because SBHs are relatively inexpensive to collect, thus allowing larger

samples and more frequent implementation. SBHs can also be included opportunistically in any representative household survey at modest marginal cost. It would thus be possible in principle to monitor and evaluate child survival programs over short periods of time, and also to identify underperforming areas for management purposes. The analysis contributes to the very small number of studies that attempt to validate the performance of measurement methods.

Unfortunately, our results have been disappointing. The birth history imputation method, which in its standard form borrows full birth histories from an earlier FBH survey and imputes them onto an SBH dataset, gave even moderately satisfactory results in only three out of 12 applications. The primary problem seems not to be with the method but with the quality of the SBHs themselves. Estimates can be no better than the quality of the underlying SBHs, and the quality of the histories, as indicated by proportions dead of children ever born, varies surprisingly widely from one survey to another. In the one application (Malawi) in which the proportions dead by age of mother in the SBH survey and the FBH survey used for validation were similar, the annual estimates obtained by BHI were also similar to those from the validation data set, so with high quality SBH data the method can work. Even with perfect data, however, the imputation method does not give true period-specific estimates of childhood mortality; the best it can do is control the resulting mortality estimates for observed changes in the numbers of children born and died to women classified by age (or time since first birth).

The cohort change method does in principle provide a true period-specific estimate of the U5MR, though it does not have the capacity to estimate mortality for different age ranges of childhood. However, our applications of the method did not produce satisfactory results, primarily because of the finding that SBHs collected by successive surveys do not produce mutually consistent results. A major factor contributing to the inconsistencies in the RMM applications is the difference in data collection methodologies between the first and the second surveys; however, in the Brazil setting where we were able to test the method with data from highly similar surveys, results were still poor.

We reluctantly conclude from these analyses that summary birth histories do not offer the potential for low-cost monitoring of child mortality. We have been surprised by how much variability there is in the quality of such histories, even when collected in standardized surveys. In at least one case (Malawi), the quality of SBHs collected in a national census appears to be high, and in at least one other case (Ghana), the quality of SBHs collected in a MICS survey appears to be higher than the quality of FBHs collected in a subsequent survey. In the multi-year series of SBHs collected in Brazil, the quality appears to be quite good, but not adequate to support the differencing from one survey to the next required by the cohort change method; it may be that SBHs are simply not an

appropriate base for measuring child mortality when that mortality reaches low levels such as those in Brazil, and proportions of children dead are around 2%. In other cases (e.g. the Mali 2009 census), the quality of the SBHs appears to be quite low. Without consistent high quality in SBHs, their potential to monitor changes in child mortality over time is very low.

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Figure 1: Schematic Showing the Principle of the Intersurvey Cohort Change Method

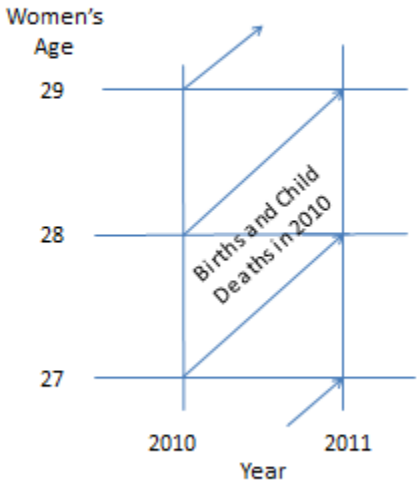
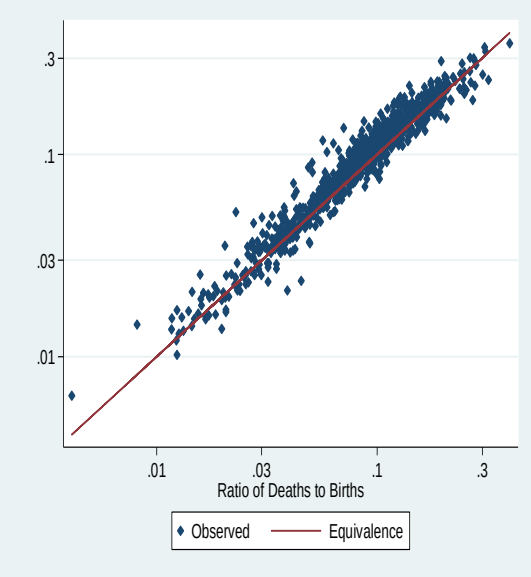


Figure 2: Relationship between Ratios of Cumulated Cohort Changes in CD to CEB and U5MR in DHS: One Year and Two Year Periods (log scales).

a) One Year Period



b) Two Year Period

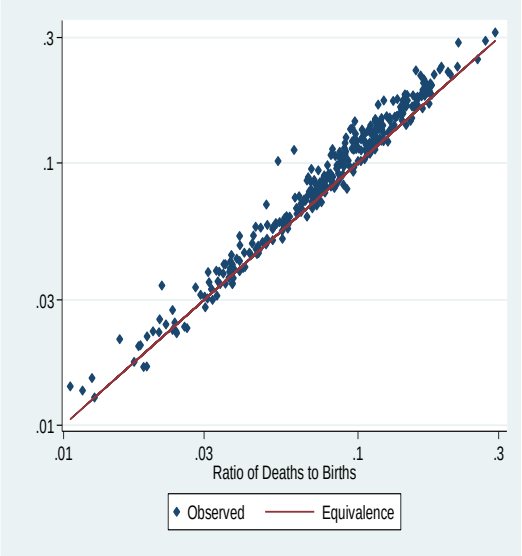


Figure 3: Average Numbers of Children Dead by Rolling 5-Year Cohorts: Ghana 2006 MICS and 2007 MMS

Table 1: Regression Coefficients for Relationship between Ratios of Cumulated Cohort Changes in CD to CEB and U5MR in DHS

Dependent Variable: ln($U5MR_t$)				
	One-Year Intersurvey Interval		Two-Year Intersurvey Interval	
$\ln(\text{c-CD/c-CEB})_t$	1.041	0.981	1.062	0.996
	[1.025,1.056]	[0.961,1.000]	[1.043,1.081]	[0.973,1.020]
Female Population 20-24/Female Population 40-44		0.066		0.068
		[0.041,0.090]		[0.040,0.097]
Female HIV Prevalence, 5 Years before survey		-0.0026		-0.0033
		[-0.0046, -0.0005]		[-0.0056,-0.0010]
TFR/Cohort Lifetime Fertility, Year *t*		0.364		0.313
		[0.276,0.452]		[0.199,0.426]
Prop. Dead (25-29)/Prop. Dead(45-49) (survey)		0.183		0.170
		[0.091,0.274]		[0.066,0.274]
Intercept	-0.050	-0.721	-0.034	-0.667
	[-0.088,-0.012]	[-0.840,-0.602]	[-0.080,0.012]	[-0.816,-0.518]
R^2	0.955	0.966	0.974	0.981
N of observations	793	708	316	280

95% CI in parentheses. Results are drawn from 154 DHSs covering 69 countries.

Table 2: Summary of Birth History Imputation Results

Country	Type of Analysis	Surveys Used			U5MR Estimates				
					10-Year Average		Mean Absolute Relative Error	Number of Imputed Estimates	
		SBH	FBH Imp.	FBH Ver.	Imp.	Ver.		10% or more below Ver.	10% or more above Ver.
a) RMM Applications									
Ethiopia	Normal	2007 Census	2005 DHS	2013 Endline	115	130	13.3%	4	0
	Normal	2011 Baseline	2011 DHS	2013 Endline	127	107	24.3%	0	4
Ghana	Normal	2010 Census	2008 DHS	2011 MICS	109	98	13.6%	0	2
Malawi	Normal	2008 Census	2004 DHS	2010 DHS	158	157	9.7%	0	0
Mali	Normal	2009 Census	2006 DHS	2013 Endline	172	217	21.3%	5	0
b) Additional Applications									
Cameroon	Normal	2000 MICS	1998 DHS	2004 DHS	0.151	0.142	15.3%	1	2
Dem. Rep. Congo	Normal	2010 MICS	2007 DHS	2013 DHS	0.161	0.127	27.6%	0	7
Ghana	Normal	2006 MICS	2003 DHS	2008 DHS	0.103	0.091	15.2%	0	3
	Contemp.	2006 MICS	1993 DHS	2008 DHS	0.113	0.091	23.7%	0	6
Kenya	Normal	2000 MICS	1998 DHS	2003 DHS	0.097	0.110	15.4%	3	0
	Contemp.	2000 MICS	1989 DHS	2003 DHS	0.092	0.110	14.7%	4	0
	Geograph.	2000 MICS	2001 Uganda DHS	2003 DHS	0.097	0.110	12.1%	1	0

Notes on Table 2:

Absolute mean relative error is computed as $0.1 * \sum_{i=1}^{10} \left| \left((Imputed_i - Verification_i) / Verifiction_i \right) \right|$

Ethiopia: Census, Baseline and Endline are from surveys of two districts, Jimma and West Hararghe; DHS data are for Oromia Province.

Ghana: Census, DHS and MICS data are for Northern Province.

Malawi: Data are nationally representative; 2008 Census are 1% sample.

Mali: Census and Endline Survey data are for two districts; DHS data are representative of rural Mali.

Additional Applications: All are nationally-representative MICS or DHS data as specified.

Table 3: Proportions dead of Children Born reported by all BHI and comparison surveys

Age Group of Mothers	Malawi		Ghana		Mali	
	2008 Census	2010 DHS	2010 Census	2011 MICS	2009 Census	2013 Endline Survey
15-19	0.100	0.091	0.121	0.075	0.165	0.173
20-24	0.107	0.106	0.102	0.095	0.155	0.195
2529	0.143	0.114	0.106	0.085	0.166	0.213
30-34	0.179	0.152	0.113	0.096	0.177	0.226
35-39	0.200	0.173	0.128	0.110	0.187	0.247
40-44	0.231	0.195	0.155	0.135	0.205	0.270
45-49	0.270	0.235	0.166	0.160	0.217	0.284

Table 4: Birth History Imputation Method: Proportions of cases matched

	First stage match	Second stage match: Compendium	Percentage unmatched
Ethiopia (Census 2007)	96.1%	3.9%	0.0%
Ethiopia (Baseline 2011)	94.2%	5.8%	0.1%
Ghana	95.3%	4.7%	0.0%
Malawi	98.0%	2.0%	0.0%
Mali	92.8%	6.7%	0.6%
Cameroon	97.2%	2.7%	0.1%
DRC	98.8%	1.1%	0.1%
Ghana (DHS 2003)	98.3%	1.6%	0.0%
Ghana (DHS 1993)	98.5%	1.4%	0.0%
Kenya (DHS 1998)	98.9%	1.0%	0.1%
Kenya (DHS 1989)	98.8%	1.1%	0.1%
Kenya (Uganda DHS 2001)	99.0%	0.9%	0.1%

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Kenya (Uganda DHS 2001)	99.0%	0.9%	0.1%

Table 5: Results of Cohort Change Method

Country	Data Sources			Cumulated Cohort Change		Estimated U5MR ('000)	
	First Survey	*Second Survey*	*Validation*	CEB	CD	Cohort Change	Validation Dataset
Ethiopia	DHS 2005	Census 2007	DHS 2011	-9.86	-2.96	a	75.1
Ethiopia	Baseline 2011	Endline 2013	Endline 2013	2.27	-3.67	b	68.9
Ghana	MICS 2006	MMS 2007	MICS 2011	5.089	-0.0100	b	79.6
Ghana	MMS 2007	DHS 2008	MICS 2011	0.15	-0.22	b	83.2
Malawi	Census 2008	DHS 2010	Midline 2011 ("current best practice")	13.580	1.502	101.7	108.7
Malawi	DHS 2010	Midline 2011 ("current best practice")	Midline 2011 ("current best practice")	3.78	-1.14	b	102.8
Brazil	PNAD 2004	PNAD 2005	IGME 2004-05	1.948	0.0329	13.3	23.8
Brazil	PNAD 2005	PNAD 2006	IGME 2005-06	2.166	0.0549	20.4	22.2
Brazil	PNAD 2006	PNAD 2007	IGME 2006-07	2.415	0.2368	80.0	20.7
Brazil	PNAD 2007	PNAD 2008	IGME 2007-08	1.765	-0.0298	b	19.3
Brazil	PNAD 2008	PNAD 2009	IGME 2008-09	2.056	-0.0170	b	18.0
Brazil	PNAD 2004	PNAD 2006	IGME 2004-06	4.302	0.1299	23.2	23.0
Brazil	PNAD 2005	PNAD 2007	IGME 2005-07	4.532	0.3294	56.8	21.4
Brazil	PNAD 2006	PNAD 2008	IGME 2006-08	4.080	0.2225	41.5	20.0
Brazil	PNAD 2007	PNAD 2009	IGME 2007-09	3.770	0.0000	0.0	18.7
Brazil	PNAD 2009	PNAD 2011	IGME 2009-11	3.724	0.2535	51.4	16.3

a: Cumulated increments of both CEB and CD negative, no estimate possible

b: Cumulated increments of CD negative, no estimate possible