Adult mortality from sibling survival data : does the corrected method perform better?

Masquelier, Bruno
Office of Population Research
Wallace Hall, 261 - Princeton University
bmasquel@princeton.edu

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Abstract

Due to the lack of complete registration of deaths in most countries of sub-Saharan Africa, adult mortality is still measured through "unconventional" techniques. Among other estimates, those based on sibling survival have long been deemed implausibly low, but they have received increasing acceptance in recent years. According to some authors, they can provide valuable counterpoints to model-based estimates, which are typically derived from childhood mortality and standard age patterns of mortality. This more optimistic view in the literature is partly due to the work of Gakidou and King (2006). The weighting scheme they suggest to correct for selection biases in sibling histories has been applied to DHS surveys, and it yields much higher estimates than previous calculations based on the same data. After reviewing the main features of this procedure, this paper offers a methodological critique of its application to DHS data. Microsimulations are used to demonstrate that the "Corrected Sibling Survival" method may substantially overestimate mortality rates, especially among males.

1 Introduction

Vital registration systems still cover a small fraction of deaths in most parts of sub-Saharan Africa (often less than 25%), with the exception of South Africa and Zimbabwe. Owing to the lack of registration-type data, "unconventionnal" approaches play an important role in estimating adult mortality (Hill et al. 2005). Apart from intercensal survival techniques (Preston and Bennett 1983), such approachs are mostly based on data on survival of close relatives. But while mothers interviewed in fertility surveys provide first-hand information on the survival of their children, no equivalent source of data has proven to be fully satisfactory for adult mortality. Three types of data are currently used to elicit information on adult deaths : (1) orphanhood status, which is collected in both censuses and surveys, (2) recent household deaths reported by census respondents, and (3) survival of maternal siblings as collected in large-scale surveys. All such retrospective reports yield levels of mortality deemed to be underestimates (Gakidou et al. 2004, Timaeus and Jasseh 2004, Hill et al. 2005).

As a result, one often falls back on estimating adult deaths rates by extrapolating from child mortality. For instance, for approximately 4 African countries out of 5, the UN Population Division (UNPD) uses a two-step procedure; the background mortality is first inferred from a combination of child survival and model life tables (mostly from the Coale-Demeny system), and the demographic impact of AIDS is factored into the estimates by rescaling the mortality rates upward (United Nations 2009). This procedure yields model-based estimates of all-causes mortality, which are then compared with other available estimates derived from censuses or surveys. Because of the frequent underreporting of deaths, the empirical estimates typically serve as lower bounds of risks of dying in adulthood. There is a general consensus that model-based estimates should be *a little higher* than those obtained from survival of close relatives. If not, or when important discrepancies are apparent between the estimates, the background mortality underlying the model outputs is revised and the procedure is repeated until a "reasonable agreement" is achieved (United Nations 2005). The World Health Organization uses a similar approach to produce its own life tables, but with a different age pattern of mortality (a modified version of the two-parameter Brass logit model) (Murray et al. 2003).

Even though both organizations presumably use the same levels of childhood mortality as starting points, considerable discrepancies remain between their final estimates of adult mortality. For instance, in Niger, the female probability of death between ages 15 and 60 ($_{45}q_{15}$) is estimated at 0.31 by the UNPD for the year 2006, whereas the corresponding probability is 30% higher according to the WHO (0.44). In a dozen countries, the two sets of estimates for this female probability $_{45}q_{15}$ differ for 2006 by more than 20%. Such discrepancies arise because both UN and WHO procedures share similar and well-known limitations. Firstly, the estimates are overly sensitive to the choice of the model life tables, let alone the fact that these models predominantly reflect the historical experience of non-African countries. The development of standard age patterns of mortality from African data is only in its early stages (Sankoh et al. 2006). Secondly, it is well established that age

patterns of mortality vary markedly across countries. Childhood and adult deaths risks are thus less correlated in practice than one assumes when combining summary indices of child mortality to a few model life tables (Bradshaw and Timaeus 2006). Thirdly, in countries severely affected by HIV-aids, the cornerstone of the estimation of adult mortality is the calculation of AIDS deaths. This calculation is of considerable complexity and involves numerous assumptions about past trends of HIV prevalence, trends in the distribution of infections by age and sex, length of survival time after infection, or access to antiretroviral therapy (ART). Finally, in the most affected countries, population dynamics are so intertwined with the course of the HIV-epidemic that it has become increasingly problematic to make no-aids counterfactual scenarios (Heuveline 2003). For instance, the construction of a life tables net of AIDS entails the estimation of reasonably precise risks of dying in childhood, which are mainly obtained from fertility histories in DHS data. But even those estimates are prone to bias because of the correlation between deaths of mothers and young children (Hallett et al. 2010).

In this context, it is crucial to evaluate the reliability of empirical estimates used to check the plausibility of model results. In recent years, data on sibling survival, which are collected as part of a *maternal mortality module* in DHS surveys, have provided a counterpoint to model-based estimates. Starting with the Sudanese DHS conducted in 1990, sibling histories have been included in over 60 African DHS, covering more than 30 countries. Questions about sibling survival are also incorporated in the men's questionnaire in about one-fifth of these surveys. Along with Demographic and Health Surveys, sibling histories are collected in World Health Surveys and in some MICS surveys, even though the data of these two programs have hardly been analyzed (Obermeyer et al. 2008).

In general, a standardized set of questions is used to list all siblings born to the same mother by birth order, and then to elicit information about their gender, survival status, and current age, or age at death and years since death for the deceased. Some additional questions are aimed at identifying pregnancy-related deaths. A major advantage of sibling histories is that they provide "direct" estimates, since the observed number of deaths can be divided by the corresponding person-years of exposure. When civil registration-type data are deficient, this the only way occurrence/exposure deaths rates can be computed for the adult ages at the national level. Indirect techniques are also available, whereby proportions of surviving siblings are converted into survival probabilities (Timaeus et al. 2001). But the resulting estimates need to be time-located, and current dating procedures cannot cope with mortality reversals. The direct calculation should therefore be preferred whenever possible. The main limitation of this direct approach is that sample sizes are too small to allow for the calculation of age-specific rates by country without introducing some smoothing. But this can be overcome by merging different surveys in a regression model to borrow strength from neighboring countries.

Although they are widely collected, readily available, and similar in nature to birth histories, sibling histories have not been as extensively used. In Sub-Saharan Africa, we can point to only three papers in which a regression framework was used to produce es-

timates of overall mortality for different countries (Timaeus and Jasseh 2004, Obermeyer et al. 2010, Reniers et al. 2011). This contrasts with the profusion of papers exploiting DHS birth histories. Perhaps one reason for this relatively scant use of sibling histories is the suspicion that they are plagued with larger recall biases than data on child survival, because the respondent is more loosely related to the deceased. Even Hill and Trussell (1977), who first suggested collecting such data, expressed some reservations about their reliability. One of the problems foreseen was the fact that respondents may not know about siblings who died before they were born or while they were young. Respondents are also likely to under-report the deaths of siblings whose whereabouts are unknown. But such omissions will not affect the direct estimates if the omitted deaths occurred in childhood or many years prior to the survey. Besides, the extent of under-reporting of adult deaths can be assessed and partly corrected for. One way to do so is to compare mortality rates from successive surveys where there is an overlap in reference periods (Timaeus and Jasseh 2004, Obermeyer et al. 2010). Assuming that the extent of recall biases is related to the time elapsed since the occurrence of deaths, it possible to assess how the completeness of death reporting changes as the reference period extends farther back in time. In DHS surveys conducted in sub-Saharan Africa, the completeness of death reporting appears to decline rapidly as the interval between their occurrence and the survey increases, especially for brothers. Compared to the 3 years immediately preceding the survey, male deaths are significantly underreported 3 to 6 years prior to the survey (91%), and past that point, completeness of death reporting drops to less than 75% (Reniers et al. 2011). Estimates should therefore be adjusted and deaths rates in the distant past should be treated with caution. Unfortunately, even in the few years prior to the survey, simple internal data checks are suggestive of serious recall biases. For instance, reports on the number of years elapsed since the deaths are affected by heaping at round digits (5 and 10 years prior to the survey) (Stanton et al. 1997). But a systematic assessment of the quality of sibling histories has yet to be conducted, because previous work on this issue remains limited, and relies mainly on aggregate comparisons with UNPD estimates (Gakidou et al. 2004, Stanton et al. 1997).

An issue that has recently attracted more attention is the problem of selection biases. The work of Gakidou and King (2006) served as a stepping stone to developing a new method, called "Corrected Sibling Survival Method" (CSS), applied to DHS data by Obermeyer et al. (2010). According to these authors, this method yields levels of mortality that are around 20 to 30% higher than the estimates obtained without adjustment for structural biases. Other variants of procedure suggested by Gakidou and King (2006) have been applied to World Health Surveys and to the Iraq Family Health Survey (IFHS). In this paper, I offer a methodological critique of the "Corrected Sibling Survival Method". Sections 2 and 3 present the selection biases affecting sibling histories, and section 4 summarizes the main features of this CSS method. The adoption of the method is motivated by the presumption that mortality is associated with sibship size, which I demonstrate is not the case in adulthood (section 5). Moreover, I argue that failure to fine-tune the weighting scheme to survey data can translate into a substantial over-estimation of adult mortality (section 6).

2 The mathematical demonstration of Trussell and Rodriguez (1990)

At the outset, it must be noted that sibling histories suffer from three structural limitations:

1. First, groups of siblings (also referred to as sibships) with high mortality are under-represented because no information is available for sibships without a surviving member.

2. Second, low mortality sibships are over-represented because the experience of the respondent's siblings is counted multiple times when more than one sibling is interviewed - as is the case in DHS surveys. Obviously, since every eligible women report about her sibship, some sibships are duplicated in the data.

3. Third, in most of the researches based on sibling histories, the respondents themselves are not counted in the denominator, which produces upward bias in the mortality estimates.

Trussell and Rodriguez (1990) have shown mathematically that these three structural limitations neutralize each other, provided that (1) all siblings in the sampling frame are interviewed, (2) the experience of the respondents themselves is excluded from the calculation, and (3) there is no association between mortality and sibship size. In order to make connections with recent developments, this demonstration is summarized below.

Consider a probability of death p, which is assumed to be independent of n, the sibship size. The observed number of deaths in each sibship follows a binomial distribution (with parameter $\mu = np$). If every surviving member is interviewed and if own-reports are not counted (i.e. the respondents merely report on their siblings), the number of respondents in each sibships equals $(n - x)$, the number of deceased siblings equals x, and the size of the sibships considered for the exposure is $(n - 1)$. The proportion of deaths observed in each sibship (PD) can thus be expressed as below, where $f(x)$ stands for the probability mass function of the binomial distribution[1]:

$$PD = \frac{\sum_{x=0}^{n-1} x(n-x)f(x)}{\sum_{x=0}^{n-1}(n-1)(n-x)f(x)} \tag{1}$$

Though not elaborated here, Trussell and Rodriguez (1990) show that this can be simplified into p :

$$PD = \frac{npq(n-1)}{nq(n-1)} = p \tag{2}$$

Thus far, equations have been presented for sibships of size n. To complete the demonstration, the distribution of sibship sizes ($g(n)$) must be taken into account. If $N(n)$ stands

[1]The probability of getting exactly x successes in n trials is given by :

$$f(x) = Pr(X = n|n) = \frac{n!}{x!(n-x)!}p^x(1-p)^{n-x}$$

for the numerator of equation 1 and $D(n)$ for its denominator, the proportion of observed deaths amongst all sibships can then be expressed as :

$$PD = \frac{\sum g(n) \sum_{x=0}^{n-1} x(n-x)f(x)}{\sum g(n) \sum_{x=0}^{n-1} (n-1)(n-x)f(x)} = \frac{\sum g(n)N(n)}{\sum g(n)D(n)} \tag{3}$$

From equation 2, one sees that $N(n) = pD(n)$. Since p is identical in every sibship, it can be factorized. Equation 3 thus simplifies into $PD = p$, which demonstrates mathematically the cancelling out of the three selection biases mentioned above.

This demonstration can be illustrated with a simple numerical example comparable to the one presented in Trussell and Rodriguez (1990). In Table 1, 100 sibships are randomly drawn from a Poisson distribution (with a mean sibship size of 5). If the probability of death is 0.25 regardless of the sibship size, interviewing every survivor will provide a unbiased estimate.

	1	2	3	4	5	6	7	8	9	10
p	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25
N(n)	0.000	0.375	1.125	2.250	3.750	5.625	7.875	10.500	13.500	16.875
D(n)	0.0	1.5	4.5	9.0	15.0	22.5	31.5	42.0	54.0	67.5
nb	3	8	16	25	13	13	12	4	3	3
PD(real)							120.75/483		=	0.25
PD(observed)							426.75/1707		=	0.25

Table 1 – *Example with no association between mortality and sibship size, every survivor is interviewed, respondents merely report on their siblings.*

By contrast, Trussell and Rodriguez (1990) show that restricting the calculation to only one sister per sibship will translate into an overestimation of mortality, as can be seen in Table 2 (p. 7). This bias will be directly proportional to the size of the sibships and to the level of mortality. On that account, the DHS reports, as well as several papers based on DHS sibling histories, have not used any kind of weights to correct for selection biases (Bicego 1997, Gakidou et al. 2004, Timaeus and Jasseh 2004, Reniers et al. 2011). The common practice has been to derive estimates from all the respondents, regardless of the fact that some sibships were duplicated in the data. I can point to only one paper in which one respondent was randomly retained by sibship (de Walque and Verwimp 2010).

More recently, on the presumption that mortality was indeed related to sibship sizes, another range of papers have adopted a different approach, whereby the experience of the respondent is included in the calculation, but weights are used to correct for selection biases. This approach draws on the work of Gakidou and King (2006). We will return to this new approach in section 4. Before doing so, we extend the demonstration of Trussell and Rodriguez (1990) to conditions that are more illustrative of DHS survey data.

	1	2	3	4	5	6	7	8	9	10
p	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25
N(n)	0.000	0.375	0.703	0.984	1.245	1.499	1.750	2.000	2.250	2.500
D(n)	0.000	0.938	1.969	2.988	3.996	4.999	6.000	7.000	8.000	9.000
nb	3	8	16	25	13	13	12	4	3	3
PD(real)							120.75/483		=	0.25
PD(observed)							117.771/381.635		=	0.309

Table 2 – *Example with no association between mortality and sibship size, only one survivor is interviewed, respondent merely report on their siblings.*

3 Insights from microsimulations

The cancelling out shown by Trussell and Rodriguez (1990) is mathematically correct, but one might wonder whether it would be true in DHS data. In these surveys, only some siblings are eligible to respond to the sibling module (typically females aged 15 to 49) and respondents also report on the survival status of siblings of the opposite sex. Moreover, death risks vary over time, as well as by age and sex. Hence, even if it were correct to assume that mortality rates are unrelated to sibship sizes, the fact that deaths risks are drawn from different distributions adds some complexity. To evaluate if the cancelling out still holds true, a micro-simulation framework is used here.

Micro-simulations are stochastic models in which the unit of analysis are individuals. In each time step of the simulations, pre-defined vital rates are converted into waiting times before various events (e.g. deaths, births, marriages and transitions to various groups). These events are assigned to fictitious individuals. Some models are closed, in a way that marriage partners are found in the simulated population, as compared to created on an ad hoc basis for each individual in search of a spouse. This allows keeping track of the kinship links as they are generated during the simulations. This is the case of SOCSIM, a model developed in the 1970s at the University of California (Wachter et al. 1997, Murphy 2004). For our purpose, the model outputs from SOCSIM can be reshaped into sibling histories. The underlying mortality rates can serve as a "gold standard" against which to evaluate mortality rates obtained from sibling survival. The linchpin is the absence of association between mortality and sibship size.

The distribution of sibship sizes observed in DHS surveys, as well as the age and sex composition of siblings and respondents are shaped by past trends of mortality and fertility. To be as close as possible as DHS sibling histories, simulated populations are generated to mimic the demographic trajectories of 41 countries of Sub-Saharan Africa. The main features of this set of micro-simulations are discussed below, and full details of the model are given in Masquelier (2010).

Simulations are calibrated with estimates of the 2008 Revision of the World Popula-

tion Prospects (United Nations 2009). Age-specific fertility rates and non-AIDS life tables are derived from UNPD estimates, and HIV infection rates are computed from UNAIDS incidences. In its core version, SOCSIM makes no allowance for HIV-aids mortality, but different transitions rates can be set up from group to group, and each group can face its own demographic rates. Thus, we model the HIV disease progression as a staged process from HIV infection to full blown AIDS, allowing for reduced fertility of HIV-positive mothers and vertical transmission. For each country, simulations start in 1900 and they run under conditions of stability until 1951, at which stage deaths and birth rates start varying yearly until 2010. The size of the starting population (in 1900) is calculated to yield a final population of 30 000 surviving individuals in 2010. In order to reduce the random variability, each run is repeated 10 times, and the final populations are merged by country. For each run, SOCSIM creates a file with one record per individual who lived in the population. This file contains identifiers to parents, which permits to reconstruct sibships born from the same mother. Mortality rates analogue to those estimated from DHS data can be computed. The underlying mortality rates, as well as other aggregate indicators, are extracted with event history analysis. For example, Figure 1 compares the relative age composition of person-years in the simulated populations with the age structure estimated by the UNPD for Zambia and Botswana, in 1950, 1975 and 2009. It illustrates the good agreement between simulation outputs and UNPD estimates.

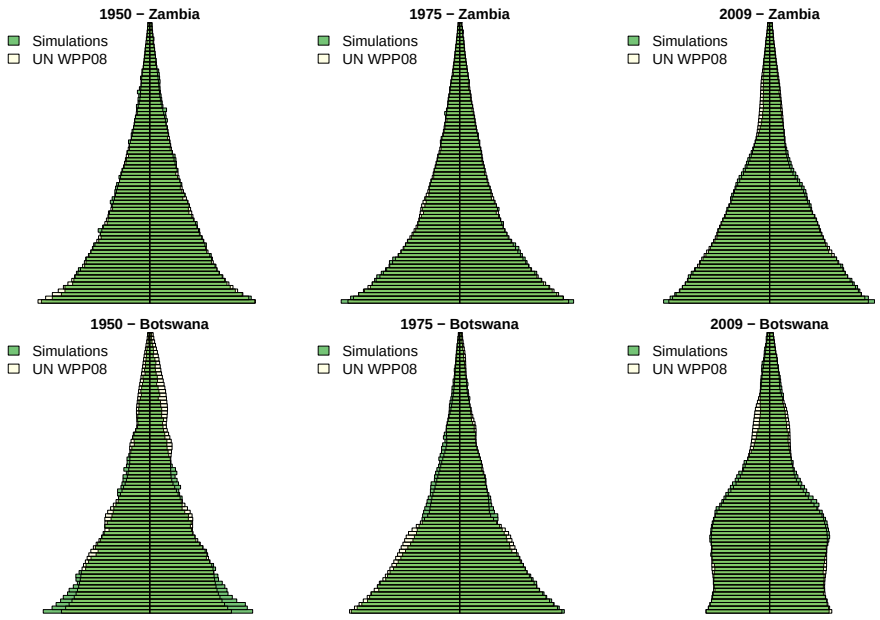

Figure 1 – *Relative age composition of the simulated populations and age structures estimated by the UNPD - Zambia and Zimbabwe - 1950, 1975, 2009*

The quantity of interest is the probability of death between ages 15 and 60 ($_{45}q_{15}$). Neither the age pattern nor the time trend of mortality are modeled here. Adult deaths are simply divided by the corresponding person-years of exposure, and the age-specific deaths

rates are then converted into ${}_{45}q_{15}$. To obtain sibling histories, each sibship is repeated in the file once for each member who survives and is eligible to the maternal mortality module. As in DHS reports, the respondents are not counted for computing the exposure. But unlike DHS data, every eligible individual is interviewed, as if a census was conducted. This is because the siblings are not organized in households, and it would require considerable ingenuity to replicate a DHS sampling procedure from the simulation outputs.

Taking as an example the case of a simulated population resembling Mozambique, and a census conducted in 2010, four different scenarios are presented in Figure 2, depending on the age range considered for eligibility (all ages versus 15-49 only) and the sex of respondents (females only versus both sexes):

- Scenario A : all individuals surviving at the end of the simulation provide information about their maternal siblings (thus totaling 300 000 respondents),
- Scenario B : individuals aged between 15 and 49 are eligible (both sexes),
- Scenario C : only females provide information on their sibships (regardless of their age),
- Scenario D : women of reproductive age are the only eligible respondents, as in most DHS surveys.

For the decade preceding the fictitious census, the sibling estimates are close to the underlying mortality rates, in each of the four scenarios (Fig.2, p. 10). Note that when the sibling histories are gathered from adults only (scenarios B and D), the scatter around the estimates is higher in the more distant past. This is because the number of siblings aged 15 to 59 of respondents aged 15 to 49 diminishes rapidly as the reference-period extends further back in time. The accuracy of the sibling estimates can be measured by drawing samples of respondents among the population of eligible survivors. With approximately 8000 adult female respondents, as in DHS surveys, the random variation would be increased in such a way that the percent root mean square error (RMSE/mean) would exceed 25% ten years prior to the survey (results not shown here). Along with the important under-reporting of deaths mentioned above, this confirms it would be unwise to use DHS sibling estimates to investigate mortality trends more than 10 years prior to the survey.

Coming back to the more recent periods, Fig.2 illustrates that the cancelling effect of selection biases remains true even though mortality varies overtime and differs by sex, and when females aged 15 to 49 are the only ones to be interviewed. As expected, the male mortality rates can be properly estimated from females, because :

1. counting or not counting the respondent in the exposure has no effect, because the calculations refers to individuals of the opposite sex.
2. sibships with low female mortality are underrepresented in the data relative to brothers (and *vice versa*), but this does not introduce a bias, since we assumed that deaths risks are not associated with sibship sizes.
3. for the same reason, the fact that some sibships have no surviving sister to report on them does not distort the estimation of male mortality rates (and *vice versa*).

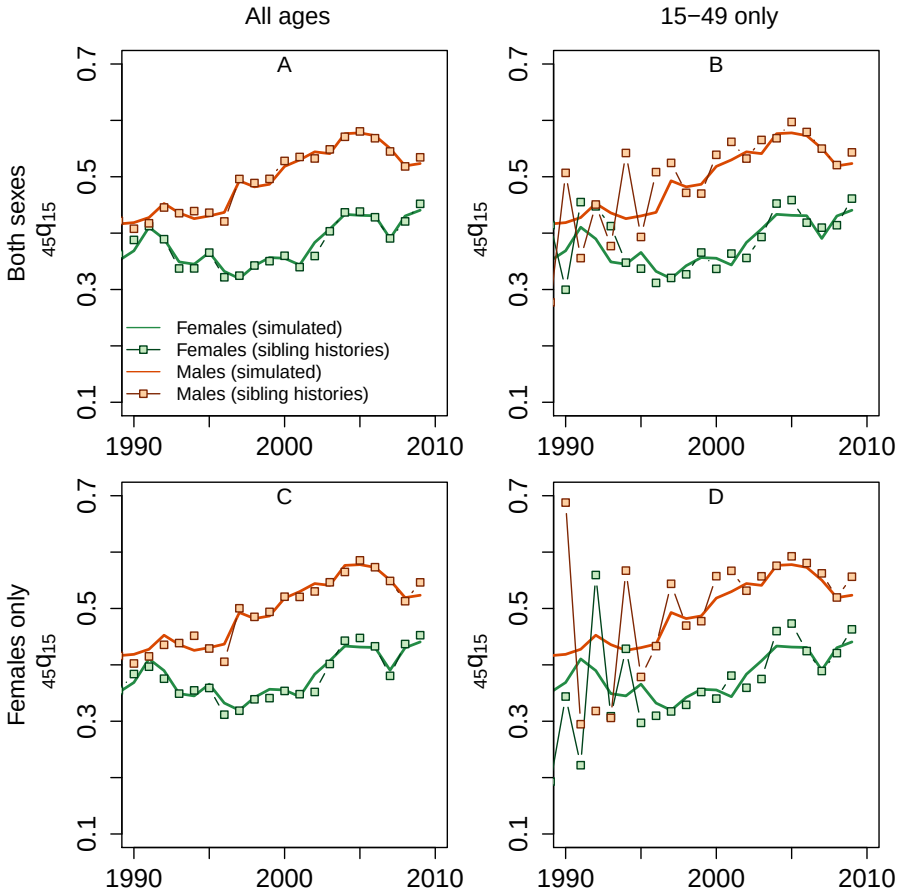

Figure 2 – *Probabilities of death between age 15 and 60 estimated from simulated populations (lines) and re-estimated from sibling histories (points) - microsimulations - Mozambique*

The next section introduces the alternative calculation, called the "Corrected Sibling Survival Method", which originates from the work of Gakidou and King (2006). We will return later to microsimulations when this alternative method will be applied to simulated sibling histories.

4 Association between sibship size and mortality

In the event that death risks *do vary with sibship size*, the standard calculation will produce either an underestimate or an overestimate[2]. A positive correlation between sibship sizes and mortality will translate into an overestimate, because larger sibships facing less

[2]Going back to equation 3, we see that the probabilities $p(n)$ cannot be factorized when they differ by sibship size. Since $N(n) = D(n)p(n)$, the standard calculation will give a weighted mean of the probabilities $p(n)$, with weights being the product of the distribution of sibships sizes by the number of siblings observed $(g(n)D(n))$. The correct calculation would be to take the weighted mean of the probabilities $p(n)$, with weights being the product of the distribution of sibship sizes by the sibship sizes $(g(n)\ n)$:

favorable mortality rates will be oversampled. Again, this can be illustrated with a simple numerical example. In Table 3, probabilities of deaths range from 0.03 to 0.075 according to the sibship size, and the mortality is overestimated by more than 7%.

n	1	2	3	4	5	6	7	8	9	10
p	0.030	0.035	0.040	0.045	0.050	0.055	0.060	0.065	0.070	0.075
N(n)	0.000	0.068	0.230	0.516	0.950	1.559	2.369	3.403	4.687	6.244
D(n)	0.00	1.93	5.76	11.46	19.00	28.35	39.48	52.36	66.96	83.25
nb sib.	3	8	16	25	13	13	12	4	3	3
PD(real)							25.87/483		=	**0.054**
PD(observed)							124.574/2143.48		=	**0.058**

Table 3 – *Example with a positive association between sibship size and mortality, every survivor is interviewed, respondent merely report on their siblings.*

To correct for this, Gakidou and King (2006) (henceforth GK) propose a correction procedure which is twofold. The first step is to weight the data, in order to recover deaths rates for sibships where there remains at least one surviving respondent. The second step relies on extrapolation and aims at correcting for the "zero-survivor bias", namely the fact that some sibships are not observed at all because no one survived.

4.1 Weighting scheme of Gakidou and King (2006)

The key idea of the GK procedure is to give less weight to sibships where many siblings survived, by computing family-level weights of the form B_i/S_i, where B_i is the number of siblings of individual i at the start of the observation period, and S_i is the number of surviving siblings at the time of the survey. A crucial point is that respondents are included in the calculation, both to compute B_i and S_i, as well as to compute the exposure. Another important point, but hitherto mostly overlooked, is that these B_i/S_i weights are designed to be applied to proportions of dead siblings reported by each survivor i.

To link this to previous equations, the *average proportion of dead siblings reported by each survivor* can be reformulated in the notation used by Trussell and Rodriguez (1990). If m individuals survived to the survey ($i = 1,\ldots, m$), this average can be expressed in two equivalent ways :

$$PD = \frac{\sum g(n)N(n)}{\sum g(n)D(n)} = \frac{\sum g(n)\left[D(n)p(n)\right]}{\sum g(n)D(n)} = \frac{\sum g(n)\left[n(1-p(n))(n-1)\right]p(n)}{\sum g(n)n(1-p(n))(n-1)} \neq \frac{\sum g(n)\left[p(n)n\right]}{\sum g(n)n} \tag{4}$$

From Equation 4, we see that the standard calculation will give more weights to larger sibships (through $n-1$) and to sibships with lower mortality (through $1-p$).

$$\frac{\sum_{i=1}^{m}(B_i - S_i)/B_i}{m} = \frac{\sum g(n) \sum_{x=0}^{n-1} \frac{x}{n}(n-x)f(x)}{\sum g(n) \sum_{x=0}^{n-1}(n-x)f(x)} \tag{5}$$

Without weights, this is clearly a biased estimate, as can be seen in Table 4. Note that in this case, the positive association between mortality and sibship size does no longer translate into an overestimate (as in Table 3), because the computation is different. The death rates obtained here are all underestimates, and the underestimation is larger in smaller sibships, because the chances that no one survives are higher in these sibships.

n	1	2	3	4	5	6	7	8	9	10
p	0.030	0.035	0.040	0.045	0.050	0.055	0.060	0.065	0.070	0.075
N(n)	0.000	0.034	0.077	0.129	0.190	0.260	0.338	0.425	0.521	0.624
D(n)	0.97	1.93	2.88	3.82	4.75	5.67	6.58	7.48	8.37	9.25
nb	3	8	16	25	13	13	12	4	3	3
PD(real)							25.87/483		=	**0.054**
PD(observed)							19.77/457.13		=	**0.043**

Table 4 – *Average of the proportion of dead siblings reported by each survivor, respondents are included in the calculation.*

The deaths rates in sibships with a least one survivor can be recovered by weighting the average proportion of dead siblings by B_i/S_i (or $n/(n-x)$ in the previous notation). If D_i stands for the number of dead siblings, we obtain :

$$\frac{\sum_{i=1}^{m} D_i/B_i \times B_i/S_i}{\sum_{i=1}^{m} B_i/S_i} = \frac{\sum g(n) \sum_{x=0}^{n-1} x\, f(x)}{\sum g(n) \sum_{x=0}^{n-1} n\, f(x)} \tag{6}$$

n	1	2	3	4	5	6	7	8	9	10
p	0.030	0.035	0.040	0.045	0.050	0.055	0.060	0.065	0.070	0.075
N(n)	0.000	0.068	0.120	0.180	0.250	0.330	0.420	0.520	0.630	0.750
D(n)	0.970	1.998	3.000	4.000	5.000	6.000	7.000	8.000	9.000	10.000
nb	3	8	16	25	13	13	12	4	3	3
PD(real)							25.87/483		=	**0.054**
PD(observed)							25.764/482.894		=	**0.053**

Table 5 – *Weighted average of the proportion of dead siblings reported by each survivor, respondents are included in the calculation.*

Table 5 shows that the observed proportion is now very close to the real probability of death, the difference being entirely attributable to the omission of sibships without survivors. This constitutes the first part of the GK correction procedure. The remaining

"zero-survivor bias" will be discussed in section 6.

The work of GK is an important step toward a better understanding of selection biases that could potentially plague sibling histories. But problems arise when it is not fine-tuned to survey data. To date, this weighting mechanism has been applied to a large range of surveys, including Demographic and Health Surveys, World Health Surveys, the CDC's Reproductive Health Surveys, and the Pan Arab Project for Family Health surveys (Obermeyer et al. 2008; 2010, de Walque and Verwimp 2010, Rajaratnam et al. 2010, Iraq Family Health Survey Study Group 2008). The higher mortality rates obtained have been endorsed in a paper recently published in the *Lancet*, in which new estimates of worldwide adult mortality are presented (Rajaratnam et al. 2010). A review of levels and trends of maternal mortality also makes indirect use of this weighting, by computing proportions of deaths attributable to maternal causes from DHS and applying these proportions to sibling estimates of all-causes mortality (Hogan et al. 2010).

Interestingly, the weights used in these different applications are only partly standardized, although this lack of consistency has not been noted in the literature. One aspect of the GK paper that seems to cause confusion is the computation of *weighted averages of proportions of dead siblings per survivor*. This is what DK call "family mortality rates at the individual level for all individuals in the population". In that case, the weights should indeed take the form B_i/S_i, as shown above. But the standard practice with sibling histories is to reshape the original files containing one observation per respondent into files containing one observation per sibling. The death rates are then obtained by dividing the observed deaths by the corresponding person-years of exposure, regardless of the sibships they come from. But if the data are to be analyzed at the sibling level, as compared to the sibship level, the weights should take the form $1/S_i$, and not B_i/S_i. This can be seen from equation 7, which reduces to equation 6 :

$$\frac{\sum g(n) \sum_{x=0}^{n-1} x \left[(n-x)/(n-x)\right] f(x)}{\sum g(n) \sum_{x=0}^{n-1} n \left[(n-x)/(n-x)\right] f(x)} = \frac{\sum_{i=1}^{m} D_i/S_i}{\sum_{i=1}^{m} B_i/S_i} \tag{7}$$

Apart from the GK paper introducing the weighting scheme, no applications of this weighting has computed "family mortality rates". But several papers have used B_i/S_i weights in person-period files, which seems to be an incorrect application of the GK weights (Obermeyer et al. 2008; 2010, Rajaratnam et al. 2010).

Perhaps more problematic is the fact that most papers applying this weighting scheme to survey data do not discriminate between adult siblings and siblings who died in childhood, and do not compute sex-specific weights. B_i is computed as the "original sibship size", that is, all children born from the same mother, and S_i is computed as the total number of surviving siblings at the time of the survey. This is clearly inappropriate since the sibling histories are collected from adults only, typically from women of reproductive age. GK warned that their procedure requires asking respondents about relatives of the same group (i.e. males aged 40-44 about males siblings age 40-44), unless considerable adaptation

is made to the weighting mechanism. But this has been overlooked in most practical applications, including by GK themselves when they use DHS data. We will show in the next section that these two errors (the use of B_i/S_i weights and the inclusion of all siblings) translate into larger biases than those arising from selection.

To our knowledge, the only appropriate use of the GK weights has been made by the Iraq Family Health Survey Study Group (2008). These authors weighted both sibling deaths and person-years lived by respondents' siblings by *the inverse of the number of female siblings aged 15-49 and alive at the time of the survey*. But as will be shown later, when applied to DHS data, an adjustment of this kind yields estimates that are slightly under the unadjusted estimates. This is because adult mortality rates are not correlated with adult sibship sizes, contrary to assertions in the literature.

5 Is the standard calculation biased ?

GK contend that sibship sizes are strongly and positively correlated with mortality in DHS data. They estimate a weighted proportion of dead siblings by sibship size and compute correlation coefficients between sibship sizes and this index of mortality. With the same sample of DHS surveys, we replicate their calculations and present the correlations in the third column of Table 6 (ρ_1)[3]. According to GK, "[These results] demonstrate unambiguously that mortality is not empirically independent of sibship size, as the standard estimator assumes. (p. 579)"

When considering the sign of the correlations, we should conclude that the standard calculation will undoubtedly *overestimate* mortality : sibships facing higher mortality risks will be *oversampled* since the deaths risks apparently rise with sibship sizes. Yet, according to Obermeyer et al. (2010), the reverse holds true[4]. Indeed, they argue that their adjusted estimates of ${}_{45}q_{15}$ are on average 27% higher than standard estimates.

How could this apparent contradiction be explained ? The explanation we suggest is twofold. First, the positive correlation between proportions of dead siblings and sibship sizes stems from structural effects such as fertility declines, and this correlation vanishes when restricting the sample to adult siblings and adult deaths. Secondly, because it is not tailored to characteristics of DHS data, the Corrected Sibling Survival Method will lead to overestimates.

Consider first the GK correlations. If the fertility declined over the last decades, the size of the sibships will be positively related to the age of the respondent, and thus to the probability of death of her siblings. Hence, a positive correlation between sibship size and

[3]Levels of significance : . p $\leq$ .10; *p $\leq$ .05; **p $\leq$ .01; ***p $\leq$ .001. Respondents under 25 are excluded because their mother have not necessarily terminated their reproduction period. Sibships of more than 10 members are excluded from the calculation. For the sake of precision, it is worth noting that these correlations are obtained from deaths rates corrected for the zero-survivor bias. The correlation without this correction are presented in the fourth column (ρ_2) of Table 6. These unadjusted correlations are logically higher, since the adjustment is more important in smaller sibships.

[4]"Underrepresentation of high mortality sibships is an important consideration when analyzing sibling survival data" (p.6)

	Country	Year	ρ_1	ρ_2	p-values (ρ_2)
1	Benin	1996	0.95	0.97	***
2	Burkina Faso	1999	0.95	0.97	***
3	Cameroon	1998	0.75	0.81	**
4	Chad	1997	0.93	0.96	***
5	Côte d'Ivoire	1994	0.75	0.89	**
6	Ethiopia	2000	0.71	0.90	***
7	Guinea	1999	0.80	0.97	***
8	Madagascar	1997	-0.19	-0.16	
9	Mali	1996	0.86	0.97	***
10	Nigeria	1999	0.93	0.96	***
11	Togo	1998	0.74	0.92	***
12	Uganda	1995	-0.06	0.33	
13	Tanzania	1996	0.82	0.93	***
14	Zambia	1996	0.47	0.73	*
15	Zimbabwe	1994	0.76	0.85	**
16	Zimbabwe	1999	0.69	0.67	*

Table 6 – *Correlations between sibship sizes and mortality DHS surveys (calculations replicated from GK) [. $p \leq .10$; *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$.]*

mortality will be observed. The same will be true if mortality has declined overtime. But, from this, it does not follow that the standard calculation is biased, as we have seen in section 3.

To show that the strong correlations identified by GK are spurious, we apply the calculations used to produce Table 6 to micro-simulated populations. As mentioned above, in these simulations, there is no association whatsoever between sibship size and mortality, and the standard calculation works fine. And yet, we observe very high correlations as in DHS data (Table 7).

It is important to realize that the only association which could potentially affect the estimates is an association between *adult* mortality and the number of potential *adult* respondents (typically the total of surviving sisters aged 15 to 49). Up to the present, there is no evidence of such association in DHS data. We can, however, refine the calculations made by GK to restrict them to adults. In order to do so, we fit a Poisson regression model to the observed deaths, including exposure time as an offset parameter, and the number of adult surviving sisters *at the start of the observation period* (B_i) as a covariate. To create a homogeneous cohort, we retain only women who were still alive 10 years prior to the survey and were aged 20 to 39 years at that time. Brothers are dropped from the calculations, as well as sisters of respondents under 30. The period under observation and the corresponding ages are illustrated in the Fig.3 via a Lexis diagram. In this example, four sisters survived to 1990, and two of them died during the reference period. The sister who died at age 24 shortly before 1990 is not counted. The "adult sibship size", which is

	Country	ρ_1	ρ_2	p-values (ρ_2)
1	Benin	0.29	0.56	
2	Burkina Faso	0.77	0.86	**
3	Cameroon	0.75	0.85	**
4	Côte d'Ivoire	0.66	0.91	***
5	Ethiopia	0.24	0.71	*
6	Guinea	0.69	0.84	**
7	Madagascar	0.58	0.70	*
8	Mali	0.15	0.82	**
9	Nigeria	0.33	0.75	*
10	Uganda	0.27	0.67	*
11	Tanzania	0.76	0.80	**
12	Chad	0.17	0.44	
13	Togo	0.28	0.52	
14	Zambia	0.56	0.84	**
15	Zimbabwe	0.91	0.95	***

Table 7 – *Correlations between sibship sizes and mortality - microsimulations [. $p \leq .10$; *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$.]*

B_i, will be 4. The older sister (who is the respondent) is not included in the person-years, and the only weights used are the sample weights.

To control for variations of mortality overtime, we use a simplified version of the Timaeus and Jasseh (2004) model, whereby the overall mortality level follows a log-linear trend and a standard mortality pattern is introduced to smooth the non-AIDS component of the mortality[5]. The age pattern of mortality is assumed to be fixed until 4 years after HIV prevalence reaches 1%. At that time, this age pattern is allowed to change along with the duration of the epidemic. However, unlike the model of Timaeus and Jasseh (2004), this model is fitted for each DHS survey separately. The age of respondents is included as well, because we suspect that older respondents (whose sibship sizes should be higher) tend to underreport deaths more frequently than younger respondents (Stanton et al. 2000). Moreover, we use a quasi-poisson model to account for overdispersion.

A likelihood ratio test is performed for the available African DHS with sibling histories[6], comparing the model including the sibship size B_i and the model without. The p-values of these tests are presented in Table 8 (p. 18). In 50 DHS surveys out of 57, female mortality between ages 20 and 49 does not appear to be significantly associated with the number of surviving sisters at the start of the observation period. Still, this is the case in 7 surveys, and the puzzling fact that Kenya and Mozambique account for 5 of these cases deserves more attention. But in any case, it seems premature to assert the standard calculation

[5]We use the General model of the United Nations Life Tables for Developing Countries (1982). See also note 7, p. 19 for a condensed description of the model.

[6]We use the `anova` command in `R`, after fitting the model with `glm`.

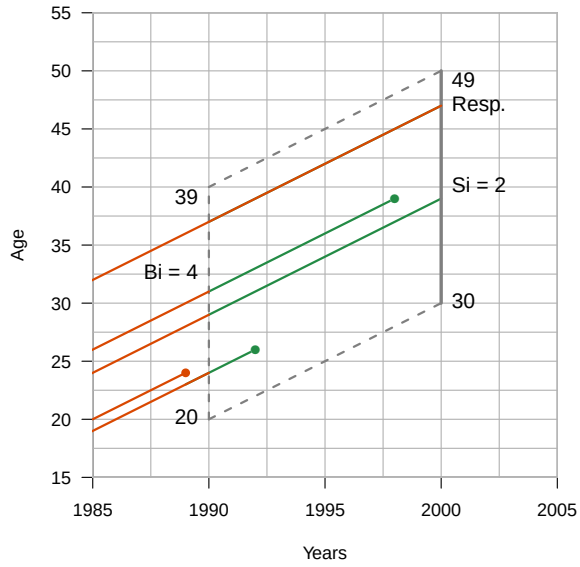

Figure 3 – *Selection of sisters included in the regression model : Green periods are included in computation of deaths and person-years, whereas orange periods are excluded.*

systematically provides biased estimates. Under most circumstances, this is unlikely not be the case.

Country	Year	p-values		Country	Year	p-values		Country	Year	p-values	
South Africa	1998	0.503		Lesotho	2004	0.751		Uganda	2001	0.221	
Benin	1996	0.857		Madagascar	1992	0.619		Uganda	2006	0.970	
Benin	2006	0.958		Madagascar	1997	0.059	.	Rwanda	2000	0.370	
Burkina Faso	1999	0.832		Madagascar	2004	0.791		Rwanda	2005	0.111	
Burkina Faso	2003	0.120		Madagascar	2009	0.202		Senegal	1993	0.704	
Cameroon	1998	0.369		Malawi	1992	0.499		Senegal	2005	0.541	
Cameroon	2004	0.147		Malawi	2000	0.003	**	Sierra Leone	2008	0.128	
CAR	1995	0.358		Malawi	2004	0.695		Swaziland	2006	0.077	.
Congo	2005	0.642		Mali	1996	0.415		Tanzania	1996	0.186	
Congo DRC	2007	0.419		Mali	2001	0.768		Tanzania	2004	0.084	.
Cote d'Ivoire	1994	0.683		Mali	2006	0.177		Chad	1997	0.000	***
Ethiopia	2000	0.068	.	Mozambique	1997	0.011	*	Chad	2004	0.580	
Ethiopia	2005	0.898		Mozambique	2003	0.002	**	Togo	1998	0.447	
Gabon	2000	0.741		Namibie	1992	0.326		Zambia	1996	0.062	.
Guinea	1999	0.748		Namibie	2000	0.099		Zambia	2002	0.764	
Guinea	2005	0.946		Niger	1992	0.602		Zambia	2007	0.118	
Kenya	1998	0.007	**	Niger	2006	0.282		Zimbabwe	1994	0.545	
Kenya	2003	0.013	*	Nigeria	2008	0.706		Zimbabwe	1999	0.412	
Kenya	2009	0.026	*	Uganda	1995	0.541		Zimbabwe	2006	0.463	

Table 8 – *P-values of likelihood ratios tests - comparison between model with 'adult sibshsip size' and model without - DHS data (n=57) [. $p \leq .10$; *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$.]*

6 Does the corrected method perform better ?

As we mentioned above, a prudent approach would probably be to derive estimates in two different ways : with the "standard approach", and with weights similar to the one used by GK, but restricted to adults. We will call the former "standard estimates", the latter "adjusted estimates". At this stage, we retain only the first part of the GK correction, which is by far the most important. Thus we make no attempt to estimate the number of deaths in sibships without survivor, an awkward task that is discussed in the next section. Consequently, if the standard calculation was unbiased, it should yield death rates that are *a little higher* than the adjusted rates, because the latter does not encompass deaths in sibships where everyone died.

In order to be able to isolate the effect of the weighting, we fit two Poisson regression models that differ only by the weights used (the respondents are excluded in the case of the standard calculation). Once again, we borrow the model developed by Timaeus and Jasseh (2004) and apply it to African DHS histories, this time pooling all the available datasets together[7]. Estimates of the probability ${}_{45}q_{15}$ obtained with the standard calculation are presented by country on pages 28-29[8].

In the left graph of Fig.4, the standard estimates are compared with the adjusted estimates, obtained by weighting both deaths and person-years of exposure *by the inverse number of potential surviving respondents*. That is, the weights take the form $1/S_i$, where S_i stands for the number of surviving sisters aged 15 to 49 at the time of the survey[9]. This is the weighting used by the Iraq Family Health Survey Study Group (2008), and it seems to be the best way to translate the GK weights to DHS data at the individual level. As we anticipated, adjusted estimates are lower than standard estimates. The median of the ratio of adjusted to standard values of ${}_{45}q_{15}$ is 0.92 for males (with an interquartile range of 0.10) and 0.85 for females (with 0.13 as interquartile range). As mentioned above, this can

[7]The model can be expressed as :

$$\begin{aligned} ln(\mu(x,g,i,t)) = \beta_0 + \beta_1(g,i) + \beta_2(g,i)t + \beta_3(i)ln(\mu_s(\bar{x},g)) \\ + \left\{(\beta_4(i) + \beta_5(\bar{x},g))\,(t-T_i) + \beta_6(i)(t-T_i)^2 I(i \in S)\right\} \\ I(t > T_i)I(x \geq 20) \end{aligned} \tag{8}$$

The overall mortality level and sex differences are allowed to vary by country (β_1), and background mortality follows a log-linear trend (β_2). A model age pattern is introduced to smooth the non-aids component of mortality (β_3). The corresponding coefficient is specific to each country, but is not assumed to be time-dependent. The level of mortality is allowed to rise 4 years (T_i) after the HIV prevalence reaches 1% (β_4). A change in the age pattern of mortality due to HIV is also introduced, and we assume it varies from region to region (β_5). A quadratic term is added for a subset of countries (S) with a stalling or decreasing HIV prevalence (β_6). Note that data for periods prior to nine completed years before the survey are discarded. A more elaborated discussion of this model is provided in Reniers et al. (2011) and Timaeus and Jasseh (2004).

[8]It is worth noting that estimates used in Fig.4 are not adjusted for underreporting of deaths, whereas values of ${}_{45}q_{15}$ in the Appendix are adjusted.

[9]When the data come from men's reports, the weights are adjusted consequently.

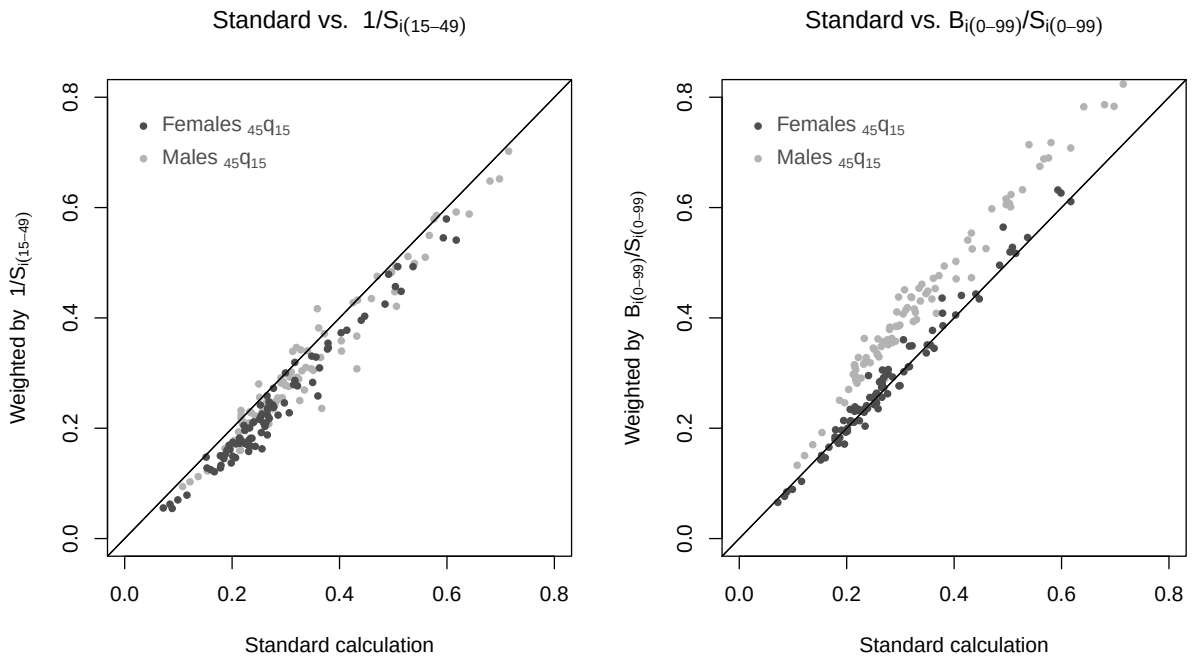

Figure 4 – *Comparison of* ${}_{45}q_{15}$ *for 1990, 1995, 2000 and 2005, obtained from sibling histories collected in Sub-Saharan Africa, with the standard calculation (x-axis) and with two kinds of applications of the GK weighting scheme (y-axis).*

be partly explained by the lack of information on sibships with no surviving member aged 15 to 49 at the time of the survey. To say the least, a revised version of the GK procedure does not yield higher estimates than the standard approach.

From this, we draw the inference that the high levels of mortality obtained in recent applications of the GK mechanism most likely stem from failure to adapt the weights to survey data (Obermeyer et al. 2010, Rajaratnam et al. 2010). As a matter of fact, the right graph of Fig.4 displays the same comparison, when applying weights of the form B_i/S_i and including all siblings in the computation of these weights, irrespective of their sex and age. Adjusted values of the female probability ${}_{45}q_{15}$ are in line with standard values, but it does not mean the two calculations are equivalent, because the former would then be adjusted upward to account for the "zero survivor bias". Since Obermeyer et al. (2010) evaluate this adjustment at approximately 0.2% to 4.0% on the ${}_{45}q_{15}$ probabilities, the overestimation of female mortality is apparently modest. However the adjusted values of the male ${}_{45}q_{15}$ probabilities are on average 28% higher than the standard estimates (with an interquartile range of 12%). This is a substantial overestimate.

This point is buttressed by microsimulations outputs. Figure 5 illustrates the impact of applying the same B_i/S_i weights to the population resembling Mozambique that we used previously (p.10). As expected, the weights work well in the first scenario, in which all

survivors are interviewed[10]. But when women of reproductive age are the only ones to be eligible (scenario D), adjusted sibling estimates follow the pattern observed with DHS surveys. Female mortality is slightly over-estimated (because further adjustment would be made for the zero-survivor bias), whereas male mortality is substantially over-estimated. In this case, the relative error made since 2000 averages 18%.

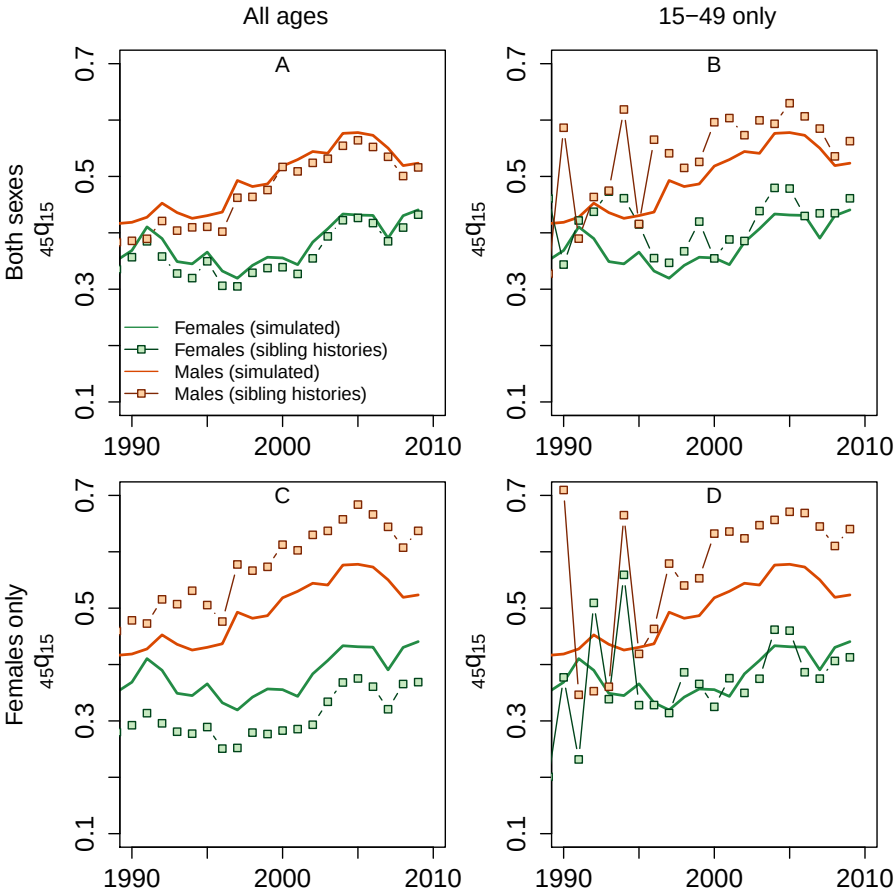

Figure 5 – *Probabilities of death between age 15 and 60 estimated from simulated populations (lines) and re-estimated from sibling histories with the CSS method (points) - microsimulations - Mozambique*

It is noteworthy that Obermeyer et al. (2010) do not mention a gender-differentiated effect of the weighting they use. Indeed, when comparing our estimates with those distributed by the authors, we find they obtain higher estimates for both sexes. This is presented in Fig.6. Both series are final estimates that incorporate some adjustment for underreporting of deaths. On average, their estimates of the female ${}_{45}q_{15}$ are 13% higher than ours, whereas the relative difference for males is as high as 20%. The CSS method

[10]Once again, the small difference between sibling estimates and underlying deaths risks is due to the zero-survivor bias.

seems to increase sex mortality ratios, although not as much as Figures 4 and 5 might suggest.

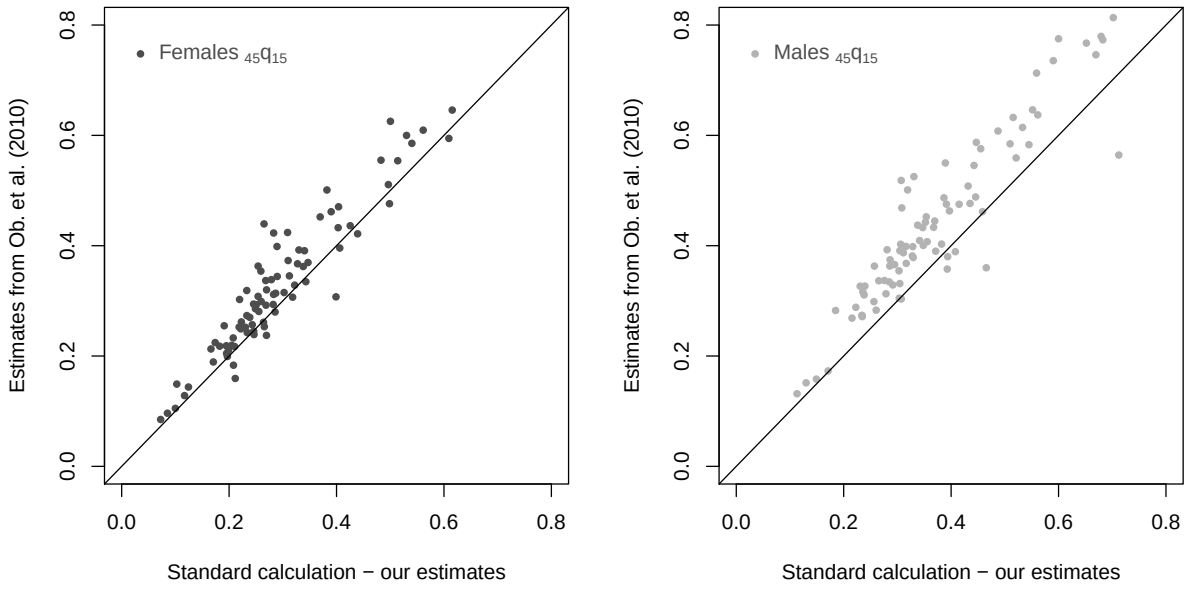

Figure 6 – *Comparison of* $_{45}q_{15}$ *for years 1985:2004, obtained from sibling histories collected in Sub-Saharan Africa: our estimates (x-axis) and estimates obtained by Obermeyer et al. (2010) (y-axis).*

Unfortunately, the computation of adult deaths rates from sibling histories entails considerable data processing and we were not able to replicate their estimates. In this last comparison, it is thus difficult to disentangle the relative contributions of different corrections made to the raw rates. For instance, their female estimates include an adjustment factor for the zero-survivor bias surrounded by uncertainties, even if it remains modest. We are also comparing results from two different models, since they use a logistic regression framework, capturing the trends in 5-years blocks. The model age patterns used to smooth the data are also different. More anecdotally, in 6 DHS out of 10 in Sub-Saharan Africa, the sibship sizes of respondents who are the only child are coded as missing, and not as zero. In these cases, Obermeyer et al. (2010) discard the observation. This is likely to translate into a slight overestimation of female mortality[11].

[11]For instance, the proportion of respondent who are the only child is highest in Congo-RDC, where it reaches 5%.

7 The challenge of estimating deaths occurring in "zero-survivor" sibships

When one wishes to refine the GK mechanism to be applied to survey data, then arises the problem of estimating the number of deaths in sibships without survivors. There are currently two approaches to do so. The first one relies on extrapolation from a regression model while the second one is based on distributional assumptions.

GK offer an elaborated discussion of the first approach. To sum it up, the key idea is to extrapolate the number of deaths in sibships without survivor from the number of deaths in sibships with at least one survivor. This number, noted ζ, is added to both the numerator and denominator of Equation 6, so that it is now expressed as :

$$\frac{\sum_{i=1}^{m} D_i/S_i + \hat{\zeta}}{\sum_{i=1}^{m} B_i/S_i + \hat{\zeta}} = \frac{\sum g(n) \left[\sum_{x=0}^{n-1} x\, f(x)\right] + nf(n)}{\sum g(n) \left[\sum_{x=0}^{n-1} n\, f(x)\right] + nf(n)} \tag{9}$$

In order to estimate ζ, they propose a regression model whereby the number of observed deaths is regressed on the number of surviving siblings ($s = 1, 2, 3\ldots$). This model is then used to extrapolate back to the case where $s = 0$. GK discuss various approaches and eventually retain a quadratic fit of the log of the absolute number of observed deaths versus the number of survivors, as shown in Equation 10. A transformation[12] of β_0 gives $\hat{\zeta}$.

$$ln(\sum_{i:S=s} D_i) = \beta_0 + \beta_1 s + \beta_2 s^2 \ (s = 1, \ldots, 7) \tag{10}$$

GK apply this method to DHS data and present model fits to sibship sizes by log proportion of deaths that look startlingly good. An illustration of such fit is provided in Fig.7a, based on the Ethiopian 2000 DHS. GK conclude that they found "a persistent, stable pattern that may be useful in extrapolating to deaths in families with zero survivors"(p.580). A similar regression model is used by the Iraq Family Health Survey Study Group (2008).

Unfortunately, as noted by GK, nothing can guarantee that the extrapolation to the point where $s = 0$ will be accurate. And it turns out the method performs poorly in the binomial case, despite the fact that the regression adjusts the data very well. To keep it simple, we retain our first example, where deaths risks were all drawn from the same distribution (with parameter $p = 0.25$) and sibship sizes followed a Poisson distribution (with parameter $\lambda = 5$). If we keep the distribution of sibship sizes presented in Table 1 (p. 6), we know that 121 deaths will occur, out of which 3 will remain unobserved. This can be seen in the last column of Table 9, where the number of deaths is distributed according to the number of survivors. Note that the unobserved deaths come predominantly from small sibships (n $\leq$ 4), whereas the distribution of deaths in sibships with more than one survivor is to a large extent determined by sibships whose original size exceeds 4.

The log numbers of deaths of this example are plotted in Figure 7.b. The curve resembles the one observed in Ethiopia, and here too, the quadratic model fits the data very well

[12] They suggest to compute $exp(\beta_0 + 0.5 \times \sigma^2)$, where σ is the standard error of the regression.

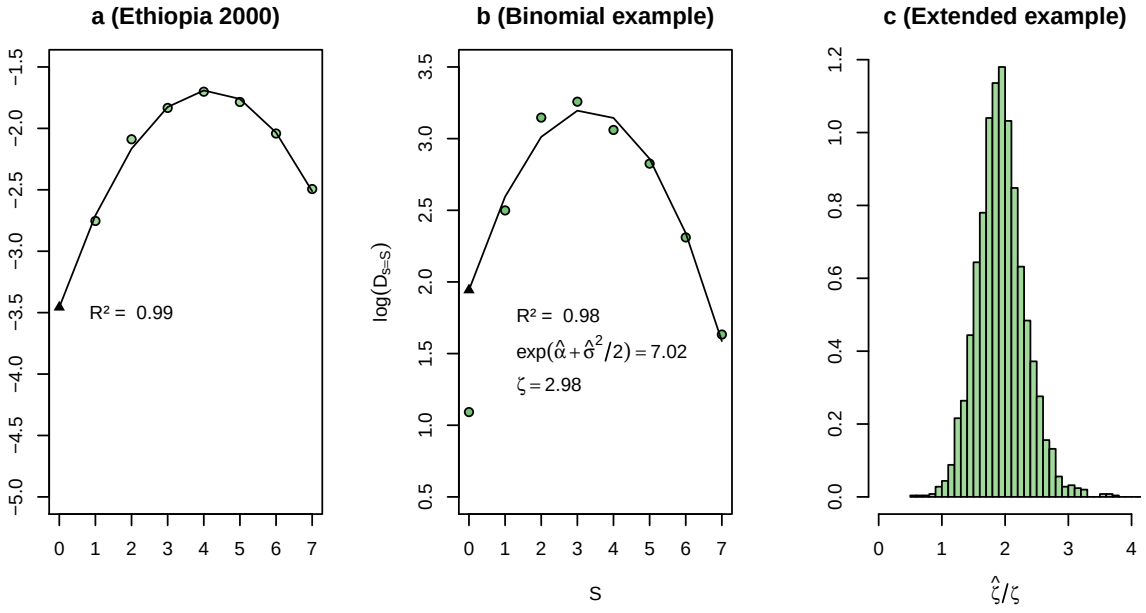

Figure 7 – *Quadratic fits to number of survivors, by log number of deaths (a & b) and density function of the ratio of predicted values over real numbers of unobserved deaths (c).*

for sibships with 1 to 7 survivors ($R^2 = 0.98$). And yet, reliance on goodness of fit is insufficient. This model cannot predict the number of deaths in zero-survivors families. In this case, the predicted number of unobserved deaths would be a substantial overestimate (7), more than twice the real value (3).

To assess the sensitivity of this finding to variations in levels of mortality or sibship sizes, this binomial example is replicated 2500 times, with probabilities of death ranging from 0.2 to 0.7 and mean sibship sizes ranging from 3 to 10, allowing the deaths risks to differ between sibship sizes. The extent of the overestimation does not appear to vary with either mortality levels or sibship sizes. Fig.7c presents the density of the ratio of predicted values to the actual numbers of unobserved deaths. The median of these ratios is 1.92, and 90% of them lie between 1.3 and 2.6.

Instead of using a regression-based extrapolation, it seems more appropriate to use distributional assumptions. Obermeyer et al. (2010) suggest a clever way to do so. They draw on the fact that for a given sibship size (n), the observed probability of death (p^{obs}) can be expressed as a function of the true probability of death (p) if both come from a binomial distribution. Their demonstration can be summarized as follows :

$$p^{obs} = \frac{\sum_{x=0}^{n-1} x\, f(x)}{n - n\, f(n)} = \frac{np - np^n}{n - np^n} = \frac{p - p^n}{1 - p^n} \tag{11}$$

	n=1	2	3	4	5	6	7	8	9	10	Pop.	Deaths
Si=0	0.25	0.12	0.05	0.02	0.00	0.00	0.00	0.00	0.00	0.00	2.98	2.98
1	0.75	0.75	0.42	0.19	0.07	0.03	0.01	0.00	0.00	0.00	21.11	12.17
2	0.00	1.12	1.27	0.84	0.44	0.20	0.08	0.03	0.01	0.00	59.76	23.26
3	0.00	0.00	1.27	1.69	1.32	0.79	0.40	0.18	0.08	0.03	95.77	25.99
4	0.00	0.00	0.00	1.27	1.98	1.78	1.21	0.69	0.35	0.16	99.33	21.34
5	0.00	0.00	0.00	0.00	1.19	2.14	2.18	1.66	1.05	0.58	80.90	16.87
6	0.00	0.00	0.00	0.00	0.00	1.07	2.18	2.49	2.10	1.46	60.70	10.08
7	0.00	0.00	0.00	0.00	0.00	0.00	0.93	2.14	2.70	2.50	35.37	5.12
8	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.80	2.03	2.82	17.73	2.37
9	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.68	1.88	7.66	0.56
10	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.56	1.69	0.00
g(n)	3.00	8.00	16.00	25.00	13.00	13.00	12.00	4.00	3.00	3.00		

Table 9 – *Distribution of sibships according to the number of survivors, number of siblings and number of deaths*

The value of p can be algebraically[13] computed from p^{obs} when the size of sibships equals 2 or 3. In larger sibships, the observed probability is virtually the same as the true value, because the chance that no one survived is close to zero. The logic is thus to correct the deaths rates observed in the sibships of size 2 and 3, and then to regress the corrected probabilities on the sibship sizes at the start of the observation period. This procedure makes it possible to compute the total number of deaths and to derive a correction factor.

But this procedure too has its drawbacks, of which the major one is the uncertainty around the correction factors. Once again, Obermeyer et al. (2010) apparently use correction factors that are sex-specific but not age-specific, and are based on all sibling deaths, even those occurring out of the observation period and in childhood. If the computation was made only from adult deaths observed in the last 10 years, the number of deaths would be much smaller and hence the uncertainty around the estimates would be much higher.

Fortunately, the standard calculation allows us to sidestep this difficult estimation, because the three structural biases cancel each other.

[13]Obermeyer et al. (2010) show that :

$$n = 2 \Rightarrow p = p_{obs}/(1 - p_{obs})$$
$$n = 3 \Rightarrow p = \frac{(1 - p_{obs}) - \sqrt{-3p_{obs}^2 + 2p_{obs} + 1}}{2p_{obs} - 2}$$

8 Conclusion

In their pioneering work making extensive use of empirical estimates of adult survival, Rajaratnam et al. (2010) show that recent trends of adult mortality have been substantially more diverse than what could be expected from child mortality. This stresses the importance of a direct estimation of adult deaths risks, instead of inferring them from child survival. In Sub-Saharan Africa, where demographic data remain scant and defective, sibling histories have emerged as a major source of estimates. They circumvent several limitations of orphanhood techniques, such as the issues revolving around the time-location of estimates when mortality has not evolved linearly, or the correlation of deaths risks between parents and young children. Compared to deaths distribution methods, they are less demanding in terms of data collection and they allow to reconstruct past trends of mortality from a single inquiry. Siblings estimates have thus found increased acceptance in the literature, even tough considerable uncertainties remain about their strengths and weaknesses.

In this paper, we showed that selection biases are not the predominant problem plaguing sibling histories. This is because the presumption that mortality is associated with sibship size is contradicted by survey data. Under most circumstances, the standard calculation should therefore be preferred. Of course, there are ways to improve correction procedures suggested by Gakidou and King (2006) and Obermeyer et al. (2010), especially as regards the zero-survivor bias when applied to adult siblings. Eventually, one might wish to carry out the calculation in several ways and compare the estimates. But at this stage, potential users should be aware that corrections for selection biases can artificially inflate mortality rates when they fail to take into account the age and sex of respondents and siblings. Such corrections will also distort the mortality sex ratios, and exaggerate the rates of mortality increases when combined with other types of estimates for the more distant past.

In sum, we call for a more cautious approach when dealing with sibling estimates. For instance, there is a need to conduct a thorough examination of the quality of the data. A number of unanswered questions also remain as regards the duplication of some sibships in the data files. Duplicated sibships are likely to be younger on average, since the probability that a sibship is observed multiple times diminishes with the age of the respondents (unmarried women aged 15-24 are more likely to be clustered in the same sampled households than women aged 40-49). Since the age at first marriage is higher in Eastern and Southern Africa, sibships are also more likely to be duplicated in these regions. This could affect some of the estimators routinely used by demographers, such as the proportion of deaths due to maternal causes. Besides, when regression models are used, the repetition of sibships in the files introduces some unobserved heterogeneity that will biases the standard errors downward. More attention should be paid to these issues.

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	1990	1995	2000	2005
Benin	0.208	0.213	0.222	0.237
Burkina Faso		0.283	0.281	
Cameroon	0.222	0.275	0.321	
CAR	0.364			
Chad	0.248	0.253	0.258	
Congo			0.404	0.249
Congo DRC			0.29	0.274
Cote d' Ivoire	0.278			
Ethiopia		0.391	0.322	0.221
Gabon		0.279	0.293	
Guinea		0.249	0.295	
Kenya	0.192	0.279	0.326	0.311
Lesotho			0.454	
Madagascar	0.294	0.269	0.24	0.211
Malawi	0.28	0.468	0.545	
Mali	0.24	0.25	0.26	0.272
Mozambique	0.173	0.21	0.254	
Namibia	0.16	0.244	0.368	
Niger	0.25	0.238	0.227	0.217
Nigeria			0.303	0.272
Rwanda		0.537	0.39	
Senegal	0.198	0.199	0.2	
Sierra Leone			0.234	0.267
South Africa	0.102	0.171		
Swaziland			0.505	0.625
Tanzania	0.223	0.278	0.328	
Togo	0.203	0.246		
Uganda	0.374	0.418	0.421	0.382
Zambia	0.315	0.537	0.609	0.495
Zimbabwe	0.206	0.374	0.523	0.596

Table 10 – *Estimates of female ${}_{45}q_{15}$ obtained from sibling data with the Poisson regression model of Timaeus and Jasseh (2004) (adjusted values for under-reporting of deaths in the more distant past)*

	1990	1995	2000	2005
Benin	0.302	0.3	0.304	0.314
Burkina Faso		0.348	0.335	
Cameroon	0.3	0.364	0.419	
CAR	0.464			
Chad	0.274	0.286	0.299	
Congo			0.558	0.308
Congo DRC			0.362	0.336
Cote d' Ivoire	0.371			
Ethiopia		0.477	0.376	0.248
Gabon		0.401	0.418	
Guinea		0.273	0.328	
Kenya	0.212	0.321	0.391	0.392
Lesotho			0.606	
Madagascar	0.361	0.327	0.296	0.27
Malawi	0.308	0.51	0.596	
Mali	0.247	0.275	0.308	0.344
Mozambique	0.254	0.282	0.315	
Namibia	0.278	0.382	0.518	
Niger	0.256	0.238	0.222	0.208
Nigeria			0.34	0.296
Rwanda		0.768	0.517	
Senegal	0.242	0.239	0.237	
Sierra Leone			0.256	0.309
South Africa	0.285	0.367		
Swaziland			0.625	0.684
Tanzania	0.317	0.373	0.418	
Togo	0.259	0.303		
Uganda	0.479	0.534	0.544	0.505
Zambia	0.359	0.593	0.666	0.549
Zimbabwe	0.291	0.488	0.639	0.7

Table 11 – *Estimates of male ${}_{45}q_{15}$ obtained from sibling data with the Poisson regression model of Timaeus and Jasseh (2004) (adjusted values for under-reporting of deaths in the more distant past)*

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