Courant Research Centre
'Poverty, Equity and Growth in Developing and Transition Countries: Statistical Methods and Empirical Analysis'

Georg-August-Universität Göttingen
(founded in 1737)

Discussion Papers

No. 133

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February 2013

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Missing Women: Age and Disease: A Correction

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Abstract

In a recent paper in the *Review of Economic Studies*, Siwan Anderson and Debraj Ray (Anderson and Ray, 2010) develop and apply a new 'flow' measure of 'missing women' to estimate the extent of gender bias in mortality in developing countries. Contrary to the existing literature, they find that the problem of gender bias in mortality is as severe among adults as it is among children in India, that gender bias in mortality is larger in Sub-Saharan Africa than in China and India, and that there was substantial evidence of gender bias in mortality in the US around 1900. These latter results are driven largely by the finding of substantial gender bias among adults. We show first that the data for Sub-Saharan Africa used in the paper are generated by simulations in ways that deliver their findings on Africa (and the US in 1900) by construction. Second, we show that the analysis is entirely dependent on a highly implausible reference standard that is inappropriately applied to settings where the overall disease and mortality environment differ greatly; the attempt to control for the disease environment by the authors is not able to address these issues. When a more appropriate reference standard is used, most of the new findings of Anderson and Ray disappear. Instead, the findings from the existing literature relying on stock measures of missing women are confirmed. The one finding that remains and deserves further attention is some evidence of gender bias in mortality among young adults in Africa (though of much lower magnitude than suggested by Anderson and Ray).

JEL Codes: J16, D63, I10
Keywords: Missing women, gender bias, mortality, disease, age, Sub-Saharan Africa, China, India

Acknowledgements: We would like to thank Sonia Bhalotra, Monica Dasgupta, Angus Deaton, Alan Lopez, Ashok Rai, and Debraj Ray for helpful comments and discussion. We would also like to thank Ken Harttgen for calculating the predicted adult mortality rates using the logit life tables.

1. Introduction

In a recent paper in the *Review of Economic Studies*, Siwan Anderson and Debraj Ray (AR, 2010) develop and apply a method to create a 'flow' measure of so-called 'missing women', i.e. women that appear to have died as a result of 'excess' female mortality. It is a flow measure in the sense that it compares the differential in female and male mortality rates for the year 2000 with a differential from a reference population to determine how many women have died due to 'excess' female mortality, i.e. they estimate an annual flow of 'missing women' for the year 2000. This is in contrast to most of the existing literature on 'missing women' which has tended to generate 'stock' estimates by comparing population sex ratios (males/females) in the affected countries with population sex ratios of some reference standard (e.g. Sen, 1989; Coale, 1991, Klasen and Wink, 2002, 2003). In the absence of reliable vital registration data (see below), such stock estimates using census data are generally held to be the only reliable information source to estimate gender bias in mortality. These studies, and many more using micro data, have generally found that the problem of missing women is much more prevalent in China and India than in Sub-Saharan Africa, and that in the two Asian countries, it is largely a problem of excess female mortality before birth or the first few years of life (e.g. Sen, 1989, Coale, 1991; Bannister and Coale, 1994; Das Gupta, 1987, Murthi, Guio and Dreze, 1995; Das Gupta, 2005; Klasen and Wink, 2002, 2003; Das Gupta, Chung, and Shuzhou, 2009).

To generate their flow measure of missing women, AR use the sex ratios (male-over-female) of age-specific mortality rate ratios in today's rich world as a reference standard. These ratios are above one for all age groups, but particularly high (1.8-3.0) from ages 15 to 75. Comparing actual sex ratios of age-specific mortality rate ratios in Sub-Saharan Africa, China and India in 2000, as well as the United States in 1900 with this reference standard, the rather startling conclusions from the 'flow' analysis of AR (2010) is that, contrary to existing research, the problem of gender bias in mortality is as severe among adults as it is among children in India; more surprisingly, the problem of gender bias in mortality is larger in Sub-Saharan Africa than in China and India and also here, the main contributor is gender bias in *adult* mortality rates. The paper also considers the role of the disease environment and finds that their new findings on the regional distribution of 'missing women' are not primarily driven by differences in the disease environment in these regions (compared to the disease environment in the reference standard, see below). Finally, they show that the United States in 1900 may have had a similar problem of missing women, driven by adult mortality, as Sub-Saharan Africa has today; again this is a new finding in the sense that previous literature had not found such substantial gender bias in mortality there (e.g. Johanssson, 1984, 1991; Klasen, 2003).

The essence of their key new findings on adult mortality in Sub Saharan Africa and India can be gleaned from Tables 1 and 2 below. As one can see in Table 1, the sex ratio of mortality rates for adults aged 15-60 reported in AR is 1.13 (row 1) and 1.37 (row 8) for Africa and India, respectively. Males thus die at 13-37% higher rates in these age groups. As the ratio in today's rich world, the preferred reference standard used by AR (see row 5) is about 1.90 (i.e. males die at 90% higher rates than females), adult females in Africa and India seem to suffer from a massive relative survival disadvantage (compared to today's rich world), leading to the finding of the large number of missing women among adults in these two regions. Based on this comparison, the first column of Table 2 presents the number of missing women by age group in Sub-Saharan Africa, India, and China (which is arrived at by multiplying the difference of the actual female death rate and the expected female death rate from the reference population with the number of women in that age group)[1]; it confirms that the problem of missing women (as a share of the female population) is worst in Sub-Saharan Africa (0.47% of females 'disappear' each year, compared to 'only' 0.35% in India and 0.28% in China), driven largely by adult mortality there; it also shows that, in India, there is also a massive problem of missing women by adults.

The results have received considerable attention, not only among researchers, but also among the policy community. In fact, based on this reference standard and using the same data, the World Bank produced and analyzed flow measures of missing women for 1990, 2000, and 2008 for all developing countries in its recent World Development Report (World Bank 2011). It finds that nearly 4 million women below age 60 die in excess every year, with about 40% accounted for by adult mortality, and Sub-Saharan Africa accounting for 1.2 million excess deaths, the vast majority among adults. While the stylized facts all mirror those of AR[2], the analysis by the World Bank adds that the problem of 'excess female mortality' has worsened between 1990 and 2008 pre-birth in China and India, and it also worsened considerably among adults in Sub-Saharan Africa.

In this note, we will primarily show three things. First, the flow analysis for Sub-Saharan Africa is highly unreliable as only 4 (of 46!) countries have *any* information on adult mortality (and most of that is outdated). As a result, the data used are nearly entirely based on simulations and imputations that are unlikely to be particularly accurate as far as age- and disease-specific relative mortality rates by sex are concerned. More seriously, we will show that these generate the findings of AR of the large excess female mortality among adults in Sub-Saharan Africa (and the US in 1900) by construction. Second, we will show that the analysis is entirely dependent on a highly implausible reference standard that systematically biases the results towards finding excess female mortality among adults. In particular, the use of the sex ratio of age-specific mortality rates from an environment with extremely low overall adult mortality rates make this reference standard highly sensitive to peculiarities of the mortally experience in rich countries (such as the role of non-natural causes of death or the peculiarities of the sex ratio of AIDS mortality) and therefore unsuitable for a reference standard for the sex ratio of age-specific mortality rates in high-mortality environments such as those prevailing in Africa and, to a lesser extent, India and China. Third, we will also show that the controls for differences in the disease environment used by AR are not able to address this problem due to differences *within* disease groups across regions. When using a wide variety of more reasonable reference standard that are more similar in overall

[1] The formula is actual minus 'expected' female death rate multiplied by population. Formally: Missing = $(DR_{fa} - (DR_{ma}/(DR_{mr}/DR_{fr}))*Pop_f$, where DR is the death rate, f, m are males and females and a refers to the actual death rate in an age group and r the death rate in the reference population, i.e. rich countries, while Pop is the population in that age bracket. For example, in Africa, the female death rate 1-5 is 1.39%, while the male one is 1.27%. Since in rich countries the male-female sex ratio is 1.25 in that age group, the number of 'missing females' in Africa using this procedure is (0.0139- (0.0127/1.25))*42 m. = 0.16 million missing females in that age group as shown in Table 2. For further details of the calculations and detailed tables on mortality rates used, see AR (2010).

[2] Note that AR find a total flow of 5 million missing females in the three areas alone, while the World Bank 'only' finds a flow of about 3.2 million in those three regions. The difference is mostly due to the fact the World Bank only considers excel female deaths up to age 60; as can be seen in Table 2 below, excess deaths above 60 indeed account for about 1.7 million missing women.

disease and mortality conditions to the countries studied, most of the new findings of AR disappear. We therefore find that, also in a 'flow' sense, gender bias in mortality is much more serious in India and China than in Sub-Saharan Africa. In China and India gender bias in mortality is largely driven by gender bias pre-birth and during the first few years of life. These 'corrected' findings are also in accordance with a wealth of socioeconomic and anthropological as well as anthropometric evidence. The only new finding from AR that remains in a qualitative sense is some evidence of gender bias among adults in Sub-Saharan Africa, although it is of much smaller magnitude than reported by AR.

2. Data Issues

An important reason why flow estimates of missing women have not been produced before is the lack of trustworthy data on age-specific mortality rates by sex in many developing countries. Disease-specific data are even harder to come by as they require a complete vital registration system (and competent medical staff entering the correct cause of death on death certificates). While acknowledging data gaps and problems in Sub Saharan Africa, AR claim that "recent vital registration data are only available for 20% of the countries in Sub-Saharan Africa. Otherwise, the main data sources include the Demographic and Health Surveys (which cover 80% of countries in Sub-Saharan Africa) as well as census data (available for 73% of countries). Other sources include the World Fertility Surveys, the Multiple Indicator Cluster Surveys and National Integrated Household Surveys. Using all the data at hand, together with regression techniques and a set of roughly 2000 life tables judged to be of good quality, the WHO computed estimates for mortality rates (excluding HIV/AIDS and war deaths) by age and gender for all Sub-Saharan African countries. HIV/AIDS deaths and war deaths were then added to the total mortality rates where necessary (AR 2010: 1295)."

The reality of data availability for Sub-Saharan Africa is unfortunately much more sobering. According to the document explaining the data sources (Mathers et al, 2004, see text and annex 6 and 7), vital registration data that is actually used in the analysis is available for only *one* country in Sub-Saharan Africa, the tiny island of Mauritius. In addition, there are some academic studies on deaths by age and cause based on sample disease surveillance systems and some national data for South Africa in 1996 (although complete vital registration is still lacking there). As reported in the underlying source for the WHO Life Tables (Lopez et al. 2003), there is actually *no* country in Sub Saharan Africa (except Mauritius) with complete vital registration system covering more than 95% of deaths. In the absence of vital registration data, mortality rates have to be estimated and Mathers et al. (2004) indeed provide such estimates that are then used by AR.

As explained in detail in Mathers et al. (2004), estimation of age-and-disease specific mortality rates first requires accurate calculations of overall age-specific mortality rates to provide the overall 'envelope' for total deaths that can then be attributed to different disease categories. But producing such overall age-specific mortality rates is extremely difficult in Sub-Saharan Africa. While indeed the majority of Sub Saharan African countries have fairly recent data on infant and child mortality from the DHS and related surveys (the modal time period being 1995-99, see Mathers et al. Table 3), data to estimate adult mortality are based on observations from only 4 out of 46 countries, two each for the early and the late 1990s (see Mathers et al. 2004: Table 4). Basically, we are back to the data from South Africa and

Mauritius and some older census information from very few countries.[3] Thus one of the main findings of the paper, on the importance of gender bias in adult mortality in Africa, is based on this largely non-existent data base.

If reliable data on adult mortality are actually not available, how does the WHO then estimate age-specific adult mortality rates by sex in Africa? This is explained in Mathers at al. (2004:11): "Based on the predicted level of child mortality in 2002, the most likely corresponding level of adult mortality (excluding HIV/AIDS deaths where necessary) was selected, along with uncertainty ranges, based on regression models of child versus adult mortality as observed in a set of almost 2000[4] life tables judged to be of good quality. These estimated levels of child and adult mortality were then applied to a modified logit life table model, using a global standard, to estimate the full life table in 2002, and HIV/AIDS deaths and war deaths added to total mortality rates where necessary."

These modified logit life tables go back to work by Murray et al. 2003 (a, b) who developed a method where one can assign 'plausible' age-specific mortality rates for all age groups associated with a particular level of child mortality. These logit life tables are themselves estimated econometrically from nearly 2000 existing historical life tables from across the world that are deemed reliable; they show that the patterns of mortality by age across high and low-mortality environments can be rather reliably estimated by a two-parameter logit function of survivorship probabilities. As shown by Murray et al. (2003a, b), this method performs well in simulations if one has reliable data on child mortality (the likelihood or reaching age five) and adult mortality (the likelihood of reaching age 60 given that one has reached age 15) for each sex; as the relationship between child and adult mortality differs between countries and across regions (see Coale, Demeny and Vaughan, 1983), these two pieces of information are crucial to generate the full set of age-sex specific mortality rates. If only child mortality is available, one basically has to guess the corresponding level of adult mortality. That is where the uncertainty ranges (not reported or even mentioned by AR) come in. There is no independent way of confirming these adult mortality rates generated by the logit life tables, let alone the relation between male and female adult mortality rates that are also generated in this way.

What is considerably more problematic in this approach and its application to sex-specific mortality rates is the database of actual life tables used to generate these logit life tables. As discussed and shown in Murray et al (2003a), the vast majority of these life tables come from North America and Europe between about 1900 and 1990. There are some observations from Latin America and Africa where we are back to the data from Mauritius and South Africa, the latter covering whites and coloureds only. It is critical to understand that the estimated system of logit life tables for high mortality environments (such as those prevailing in Sub Saharan Africa) will therefore heavily depend on the actual mortality

[3] To be sure, AR are correct in pointing out that census information (mostly from the 1970s and 1980s) is reported to be available for many more countries, but those data are actually not used for the calculation of adult mortality, presumably because they are deemed unreliable.

[4] Note that the 2000 life tables referred to here and lower down in the paper by Murray et al. (2003 a, b) are from all over the world, not, as implied in the quote by AR, from Africa alone.

experience of high-mortality situations in Europe and North America, which are contained in the early historical life tables from the early parts of the 20th century. During those times, the mortality conditions in Europe and North America were similar as presumed to be in Africa today. Thus when these logit life tables are used to simulate adult mortality rates for Sub Saharan Africa, the results will be heavily driven by levels and sex differentials in mortality prevailing in historical Europe and North America.

To see this, the following information on adult mortality is shown in Table 1. First, we show the data on mortality rates by sex for the ages 15-60 for Sub Saharan Africa as reported by Anderson and Ray, i.e. based on the WHO Life Tables.[5] Row 2 shows the predicted male and female mortality using AR's data on child mortality and predicting adult mortality using the logit life table approach. The third row uses AR's data on child mortality and uses the Model Life Tables West, based on the historical experiences of Western Europe and North America and Australia between 1870-1960 to generate adult mortality rates by sex (Coale, Demeny and Vaughan, 1983), a source routinely used in the past to assess gender bias in mortality (e.g. Coale, 1991; Klasen and Wink, 2002, 2003). The fourth row shows the data for the USA in 1900 (taken from AR), the fifth row for established market economies from AR. The last two rows we will discuss below. [6]

The key finding is that the *sex ratios* of adult mortality are remarkably similar across all the first four lines, around 1.1. So it seems that the logit life tables that are used to predict mortality rates for Africa and then used by AR generate a sex ratio of adult mortality rates that are remarkably similar to the historical ratios in the Princeton Model Life Tables, or in the United States in 1900 where AR also 'found' substantial numbers of missing women. This is, of course, as expected, as these historical data were used to predict the mortality rates in Africa in the first place. For the analysis in AR, this implies two things. First, the findings of 'missing women' among adults in Africa and in the United States in 1900 are not two separate findings. Since data from the United States in 1900 (and other now industrialized countries around that time) were primarily used to impute adult mortality rates for Africa using the logit life tables, the two findings are related by construction. Secondly, this implies that the 'finding' for Sub Saharan Africa on a comparatively low sex ratio of adult mortality rates (compared to rich countries today), upon which the claim of missing women is based, is basically derived from the US and European experience at similar levels of mortality to those prevailing in Africa today, where the male-female mortality ratios were also around 1.1 and much lower than today (compare the first four lines in Table 1 with the 5th where the sex ratio is 1.9 in established market economies today). In fact, given that the adult mortality rates in Africa are nearly entirely simulated, in AR there really is no finding on Africa; instead, African data are simulated based on European and US *historical* experience, and then compared to *present-day* experience in Europe and the US. We learn nothing about Africa, but only something about the time path of the sex ratio of adult mortality in Europe and the US as these countries moved from high to low-mortality populations.

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[5] From the information provided, we are generating the probability that a person does not reach age 60 given that she reached 15 (among demographers known as 45q15).

[6] Note first that the *level* of adult mortality for both males and females reported in AR is substantially higher than in the other estimates; this is due to the fact that AIDS mortality and war deaths, which together account for some 20-50% of deaths in the adult age groups in Africa around 2000, are added to mortality after the logit life tables have been used.

If the WHO Life Tables used by AR do not really provide reliable information on adult mortality rates by sex in Africa, is there any better information one could draw on? It remains the case that the complete absence of vital registration systems makes it impossible to come up with reliable age-specific mortality rates for specific age brackets; as such the stock estimates of missing women using the much more widely available (and reliable) census information continues to be the most reliable way to study the issue of missing women. As shown in previous work (e.g. Klasen and Wink, 2003), these stock estimates suggest that there is evidence for a small amount of gender bias in mortality in Sub-Saharan Africa, with around 1-2% of the female population missing, compared to 7-8% in India and 6-7% in China.

But there has recently been some progress in developing better estimates for adult mortality in countries without vital registration systems. Using the Demographic Health Surveys and related instruments, Rajaratnam et al. (2010) use information on the mortality of siblings of the (female) respondent in those surveys to generate a crude estimate for adult mortality for nearly all countries of the world. Since the precise age at death cannot be estimated using this procedure, they can only provide one mortality rate for the 15-59 age group (or more precisely, the likelihood of dying by 59 given that one has reached age 15). Given the inherent uncertainties in their estimates, the results come with very large standard errors which are, however, also reported. As these estimates are based on actual data from Africa, and not simulations based on historical Europe and the US, these estimates are likely to provide much greater insights into the true state of adult mortality in Africa. They will also obviate the need to add in AIDS and war mortality as these deaths would also be reported by the siblings.[7] In the last two lines we present their estimates of the (weighted average of) adult mortality in Sub Saharan Africa for 2000 (to compare to AR) and 2010. In the latter year, confidence intervals are also provided.

Two findings stand out. First, the male-female mortality ratio is substantially higher than found in AR or in historical Europe or the US. The point estimate is around 1.3 and has remained at that level in 2000 as well as 2010. The second point of note is the massive confidence intervals attached to these estimates. The sex ratio could be between 1.02 and thus slightly worse than in historical Europe or as high as 1.66 where the ratio is only somewhat lower than the ratio prevailing in today's rich world.

We also report in Table 1 the male-female mortality rate ratios for India in 2000 from the WHO Life Tables used by AR as well as the new estimates by Rajaratnam et al. (2010). The Indian data used by AR are based on a much better database than the African one as India actually has a sample vital registration system from which to generate these rates. Nevertheless, the sample registration system only covers a small part of the population (and, as reported by Mathers et al. 2004, fewer than 85% of deaths in those sample districts) resulting in questions of reliability. But in contrast to Africa, no historical European data are used to simulate those rates. Rajaratnam et al. (2010) also use these same data, correct them for under-reporting, and then provide confidence intervals. As shown in Table 1, the male-female ratio reported for 2000 by Rajaratnam et al. (2010) is slightly higher than reported by AR

[7] There are a number of estimation issues that need to be tackled to avoid biased estimates, including sample selection issues where entire families were wiped out and thus no one can report on a sibling death. These issues are all dealt with carefully using validation exercises in Rajaratnam et al. (2010).

(1.37 instead of 1.31). By 2010, the ratio is estimated to have risen to 1.58. The confidence intervals provided for 2010 are also huge, ranging from a ratio of 1.27 to 1.96. Thus they include actually the current value prevailing in rich countries (1.90). This suggests that also in India the finding of this massive female disadvantage among adults is estimated to be smaller in 2000, much smaller in 2010, and with huge confidence intervals attached to these point estimates. Thus the claim of massive numbers of missing women among adults (even when using today's rich countries as a standard) has to be treated with great caution also in India.

To summarize the data issues, it is clear that the finding of missing women in Africa in AR is based on simulated data using historical Europe and the US for the simulations. The best data available to date (which is still rather poor) suggest the problem to be substantially smaller and much more uncertain. The same applies, to a somewhat smaller extent, to the finding for India where the confidence intervals are similarly massive.

To be sure, if we ignored the confidence intervals and believed the point estimates of the male-female mortality ratios generated by Rajaratnam et al. (2010), the key findings from AR would not entirely disappear as these point estimates for male-female mortality ratios among adults are still much below those in today's rich world. The problem of missing women among adults has just become smaller, particularly in Africa, and much more uncertain in both regions. This leads to the second issue with AR: Is the use of age-specific female-male mortality rate ratios from today's rich world as a reference standard the best approach to identify 'missing women'?

3. Specifying an Appropriate Reference Standard

As discussed in detail in Coale (1991), Klasen and Wink (2002, 2003) and AR, the ideal counterfactual for assessing the amount of gender bias in mortality would be a society where males and females are treated equally and where there are no sex-based behavioral patterns with significant mortality implications. There is no such society past or present that exhibits this feature so that it is inherently difficult to generate a reference standard that it totally beyond reproach. But there are clearly more and less plausible reference standards that can be used as a counterfactual.

As already discussed above, AR (and World Bank 2011) use as the counterfactual the male-female age-specific mortality rate ratios that prevail in rich countries in 2000. As shown in Table 1 (row 5), they suggest that the probability of men not reaching age 60 (given that they reached 15) is about 90 percent higher than the corresponding likelihood for women. Disaggregating into smaller age groups (not shown here), one notices that the ratios are particularly high in the age groups between 15 and 35 when they are above 2. In fact, these high ratios are largely driven by the role of injuries which particularly drive up male death rates; in the demographic literature this has sometimes been referred to as the 'testosterone spike' linked to injuries related to the combination of alcohol abuse, dangerous traffic behavior, and violence of young men (Kalben, 2000). Due to overall low mortality and a relatively high fatality rate from injuries, the data provided by AR show that injuries make up 70% of all deaths of males (and 50% of females) in the age group of 15-29 in rich countries. The male-female ratio of death rate from injuries is

3.7; for all other deaths it is 1.6. Thus the counterfactual is heavily affected by this great relative importance of injuries in adult mortality in rich countries, where males are disproportionately affected.

Following from this, there are essentially three problems that make the use of the sex ratio of age-specific mortality rates in rich countries extremely problematic:

1) Using the sex ratio of age-specific death rates in a situation of very low mortality has dramatic implications for the 'expected' mortality in Africa and Asia. In rich countries, the death rates for females between the of ages 1 and 40 are below 1 per thousand; in the adult age groups were most of the alleged missing women are found (15-29), they are below 0.5 per thousand. Just slightly higher male rates immediately generate the large ratios observed.[8] By using them as a standard, these dramatic ratios that are based on tiny overall mortality rates are then held to be the 'expected' ratio in regions such as Sub Saharan Africa where overall mortality rates in those age groups are 10-20 times higher. So we are being told that just because males aged 25-29 have a mortality rate of 1.1/1000 while women have one of 0.4 per 1000 in rich countries, the expected female mortality rate in Sub Saharan Africa for that age group would barely be a third of that of men, i.e. 3.7/1000 for a given male rate of 10.8/1000. Assuming such massive relative disadvantages in situations of high overall mortality is, however, inconsistent with the entire literature about the biological and behavioral drivers of sex differential in mortality as will be discussed below. Use of the ratio as the particular functional form, in combination with the substantial relative mortality advantage in the low mortality environments of rich countries, leads to these massive numbers of missing women, particularly among adults in Africa. While AR briefly discuss functional forms, they merely state that they have a preference for the 'scale neutrality embodied in the use of relative death rates' (AR, 2010: 1290). To show the massive impact of functional forms, we will examine below if one used the absolute *difference* (rather than the *ratio*) of male and female mortality rates from rich countries.[9]
2) While it is true that there is no bias free-society that one could use as a reference standard, there is a huge literature that has investigated the role of biological versus behavioral factors that influence sex differential in mortality (e.g. Case and Paxon, 2005; Cawley and Ruhm, 2012; Rogers et al., 2010; Waldron, 1976, 1982, 1993, 1998; among many others). While estimates differ slightly, there is a large consensus in the literature that most of the survival advantage of females in industrialized countries is actually due to sex-specific behavioral patterns. Most relevant for the adult age group is the role of risky behaviors, strongly associated with alcohol and drug abuse and behavior in traffic, while in the older adult age groups the role of cigarette smoking, worse eating habits and poorer health seeking behavior among males weigh more heavily (Case and Paxson, 2005). As a result, the consensus in the literature is that women's biological advantage only amounts to 1-2 years of life expectancy. This is mostly based on

[8] In fact, it turns out that there is a significant inverse relationship between overall mortality and the sex ratio of mortality across age groups in rich countries; the higher overall mortality, the lower the sex ratio of mortality rates.
[9] To be sure we are not recommending the difference as a standard. The absolute difference would underestimate biological differences, particularly in infancy, and might overestimate these differences in older age groups where mortality levels are high. Also, the problem of male-biased injuries dominating the results would still be there (but muted through the functional form).

biological mortality advantages, particularly in infancy and beyond age 50. The most common estimates of this survival disadvantage are 10-15 % in infancy, below 10% between age 1 and about 45, and rising to above 20% in old age (e.g. Waldron, 1976, 1982, 1983; 1993; 1998). This is dramatically different from the ratios closer to 2 and beyond used by AR as the reference standard for the adult age groups.

3) The reference standard of today's industrialized world is, of course, based on the disease environment prevalent here. Clearly, the disease environment in high-mortality regions such as Sub-Saharan Africa and, to a lesser extent India, is dramatically different than in today's rich countries. In particular, in today's industrialized world, communicable diseases account for only about 5% of total deaths, while non-communicable diseases drive mortality in older ages, and injuries in younger ages. The disease data in Africa and, to a lesser extent, India, are also subject to serious questions. As discussed above, the African data are entirely based on incomplete vital registration data from South Africa, complemented by incomplete patchy survey information and information on AIDS and war mortality (Mathers et al. 2004). But assuming that these data roughly reflect the true distribution of mortality by cause, they suggest that, in Africa, communicable diseases account for over 50% of all deaths, with injuries playing a significant role in the younger age groups, and non-communicable in the older ones. The male-female mortality rate for injuries in rich countries is very high, driven by male risky behaviors and greater violence (including suicide). Take the two most important causes of death due to injuries, the male-female mortality rate for suicide is around three in all adult age groups in rich countries, and that for road traffic accidents is around 3 in the younger adult age groups. In contrast, for communicable diseases, the ratio is around 2, and for non-communicable diseases, it is around 1.5. So it is critical to control for the disease environment and the numbers for missing women by age cannot be trusted due to these massive differences in the disease environment. AR try to control for the disease environment but, as we show below, do not capture the essence of the differences in the disease environment which is due to within-disease rather than between-disease categories.

Are there better alternatives than using the sex ratio of mortality in rich countries as the reference standard? One could use the difference, as we do below, as a robustness check, or one could use the data from the consensus in the literature, such as it is, on the presumed biological mortality advantage of women. Or one could refer back to what the demographic literature prior to AR has commonly done when examining the question (e.g. Coale, 1991; Klasen, 1994, Klasen, 1998, Klasen and Wink, 2002, 2003): using so-called Model Life Tables as a reference standard. There are various versions of such Model Life Tables. The previous literature relied mostly on the Princeton Model Life Tables (Coale, Demeny, and Vaughan, 1983) which are based on comprehensive and high-quality life tables from Europe, North America, and some non-European countries from about 1870 to 1960. These life tables were then combined to generate Model Life Tables for different overall mortality conditions. As the pattern of mortality (esp. the relation between child and adult mortality) differed slightly among countries, four families of life tables were generated. The advantages of using those life tables as a reference standard for generating estimates for missing women are the following (see Coale, Demeny, and Vaughan, 1983; Coale, 1991; Klasen, 1994; Klasen and Wink, 2002, 2003): First, they span the entire spectrum of mortality conditions that include high-mortality environments such as Africa's but also medium mortality environments such as India's or China's.[10] Second, and closely related, they reflect the entire spectrum of the epidemiological transition from a preponderance of communicable diseases to a disease spectrum that is prevalent in places like China today. Third, they come from a time period where the main drivers of high male mortality today, especially related to nicotine abuse and traffic accidents, were much less serious and thus less likely to bias results. Lastly, they come from time periods where the overwhelming historical evidence suggests that gender discrimination in access to resources was comparatively low, certainly when compared to places such as India or China today (e.g. UN, 1998; Klasen, 1998; 2003; Johannsson, 1984, 1991).[11]

4. Using different reference standards

Table 2 summarizes how the results on missing women by age would change if one used either the difference in mortality rates in rich countries (rather than the ratio, column 4), relied on the consensus in the literature on the relative biological disadvantage of males (20% under age 5, 5 % until age 50, 30% until age 80 and 20% beyond see column 3 called 'expert'), or relied on the West (column 2) Model Life Tables (which are most commonly used for such assessments, Coale, 1991; Klasen 1994).[12] We also reproduce the findings from AR using the sex ratios of mortality in rich countries (column 1) and adopt their approach for identifying the number of missing women at birth.[13]

Regardless of which alternative reference standard we use, the key findings of AR, that the missing women problem is more severe in Sub Saharan Africa than in India or China, and that in Sub-Saharan Africa and India it is driven largely by gender bias in *adult* mortality, largely disappear.[14] The flows of missing women are now much larger in India and China, compared to Sub Saharan Africa. While AR found that nearly 0.5% of the female went missing every year, compared to 'only' around 0.3% in India

[10] They are less suitable for assessing very low mortality environments such as those prevailing in some industrialized countries today.

[11] To be sure there is a literature that has examined gender bias in mortality in historical Europe and that literature has indeed identified small episodes of such gender bias; but most episodes refer to the early 19th century, before the time period covered in the Model Life Tables; also the one episode in the late 19th century was focused on girls (aged 1-14) and has been linked to a particularly serious episode of tuberculosis that particularly affected them. But the presumed excess amounted to never more than 5-10% above expected mortality and was very small in Scandinavia and the UK (and larger in Ireland). For a survey, see for example, Klasen (1998, 2003), Johansson (1984), Johansson and Nygren (1991), among others.

[12] In a robustness check, we also use the Model Life Tables 'North' with virtually identical results. They are available on request. Note that AR also report on results using model life tables in appendix B of their paper and also find that most 'missing women' disappear in Africa but do not place any emphasis on these results in the text.

[13] As they also used the sex ratio of mortality prevailing in Latin America as a robustness check, we used this standard as well in results available on request. Note that using Latin America does little to solve the problem as mortality rates in Latin America today are much lower than in Africa or India and in fact relatively close to rich countries; the sex ratio of mortality in Latin America is even higher in some young adult age groups, related to particularly high male death rates due to injuries. So the same concerns apply to this standard.

[14] Note that using the difference of mortality rates as the reference standard leads to negative numbers of missing females in Africa. But this is mostly driven by the 0-1 age group. But here the epidemiological literature (e.g. Waldron 1983, 1998) is quite clear that the disadvantage in infancy appears to be a relative one (not an absolute one) so that one should treat the results with caution.

and China, now the numbers are drastically lower, but much more so for Sub Saharan Africa.[15] For example, using the Model Life Tables West (column 3), 0.07% of females die in excess every year in Sub Saharan Africa, compared to 0.14% in both India and China. Using the 'expert' reference standard leads to nearly identical findings as far as size and distribution of gender bias is concerned.[16] The second key finding, that the problem of missing females is as much a problem among adults in India has also almost entirely vanished. The vast share of missing females emerges pre-birth and in the first few years of life. Again using the Model Life Tables West as the example, the share of missing women accounted for by sex-selective abortions and elevated mortality rates in infancy and early childhood (< age 5) is over 70% (compared to less than 30% in AR, compare column 4 to column 1). Depending on the reference standard used, there might be some problems in selected adult age groups, but one should be cautious in interpreting these findings in light of the data issues discussed above. The third key finding, i.e. that the massive problem of gender bias in Sub-Saharan Africa is driven by excess mortality among adults, is also much reduced. But it remains the case that the largest *share* of missing women appears to go missing in the 15-29 age group. This is an issue we return to below.

Note also that our results change the least as compared to AR for China. This is not surprising as China already has a rather low mortality environment and thus the biases generated by using the ratios of mortality from rich countries are less serious. Of course, this standard would also longer find many missing females in the US in 1900. The US in 1900 was a high mortality environment, where the use of today's ratios is as deeply problematic as it is for Sub Saharan Africa; thus this finding of the surprisingly massive gender bias in mortality in the US also vanishes.[17]

To summarize, using today's male-female mortality rate ratios in rich countries vastly overstates the flow of missing women in Africa and Asia; this is due predominantly to much higher numbers of missing women among adults where the skewed sex ratios of mortality rates in rich countries have a huge impact. Using more reasonable reference standards, the total 'flow' of missing women, found by AR to be 5 million (and the World Bank to be 3 million per year up to age 60), is now 'only' around 1.2-1.5 million per year. Using these more reasonable reference standards basically confirms that the problem is more severe in India and China than in Africa, and is largely driven by gender bias pre-birth and in young ages in India and China, while in Africa there remains some evidence that there might also be a substantial problem of missing females (subject to the data problems described above) among young adults.

5. Controlling for the Disease Environment

AR are aware of some of the criticisms leveled above (and cite one of us as a source of these debates) but are confident that their way of controlling for the disease environment basically addresses most of the concerns raised above. Briefly, AR control for the disease environment by building up the number of

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[15] The numbers are therefore, of course, also much lower than the claims by World Bank (2011) of 4 million women going missing each year.

[16] Using the difference also shows qualitatively the same result; in fact, there are no more missing females in Africa. For reasons stated above, we consider the Model Life Tables and the expert standard to be the most reliable reference standard.

[17] Also, note that the US experience is part of the Model Life Tables West.

missing women, disease group by disease group, using the sex ratio of age-specific mortality rates for each disease group in rich countries as the standard, and calculate the number of missing women in that disease group (by taking the male fatality rate as a proxy for the prevalence of diseases overall). The key findings in AR are that the building up of the flow of missing women disease-by-disease hardly changes the numbers of missing women in the three regions. Moreover, the problem of missing women among adults in Africa is largely a result of high female deaths due to AIDS and maternal conditions; while in South Asia maternal conditions, injuries and cardiovascular diseases account for most of the missing women among adults. While the procedure to build up the number of missing women in this way seems reasonable, it is not able to capture the differences in the disease environment between rich countries on the one hand, and India and Africa on the other. This is due to the problem that *within* disease groups the conditions between rich and poor countries are drastically different.

The examples of AIDS, maternal mortality, injuries, and cardiovascular diseases, the key drivers of missing women in AR, illustrate the point. In rich countries, the sex ratio of mortality rates due to AIDS was an average 4 across the adult age groups, i.e. men died at four times the rate of women from AIDS. This is of course related to the fact that AIDS incidence and fatalities in the rich world was much higher among the gay community and IV drug users, while heterosexual transmission never played a significant role (World Bank 1997). In Africa, the ratio is around 1.5 in most adult age groups, but only about 0.6 in the 15-29 age group, i.e. women have higher mortality than men from AIDS in this age group. Given this vast difference in ratios, it is no wonder that female AIDS mortality accounts for the bulk of missing women among young adults in Africa. But this counterfactual presumes that without gender bias, AIDS in Africa would have also been primarily a disease affecting the gay community and IV drug users. But this presumption is clearly false. The fact that AIDS in Africa has been a disease that has spread mostly through heterosexual intercourse (and mother to child transmission) is due to the early emergence of the disease in Africa and its spread among heterosexual couples long before it was identified as a disease, combined with very high incidence of STDs, an important role of prostitution (which can indeed be interpreted as gender discrimination), and higher risk behavior of heterosexual people (Iliffe, 2006; World Bank 1997; Oster, 2005). It is likely to be true that the particularly high incidence and fatality of AIDS among young women is due to gender discrimination (particularly if related to unwanted sexual intercourse with older men), but this is not the *main* reason why AIDS is a disease affecting mostly heterosexual people. Thus assuming the rich world ratio as reference standard vastly overstates the relevance of AIDS in causing missing women. A much more reasonable assumption in line with what is known about AIDS, its transmission and fatality, would be a sex ratio of mortality 1-1.2 as a reference standard for AIDS in environments with a predominantly heterosexual transmission path; that would reduce the number of excess female deaths by about two thirds, but still leaving the higher female mortality from AIDS in the 15-29 age group as an issue worth investigating further, as indeed gender discrimination could contribute to the high female mortality in that age group. If one used an expected sex ratio of mortality due to AIDS of 1.2, the number of excess female deaths would fall from some 600,000 per year to about 200,000, all concentrated in the 15-29 age group.[18] As shown above, in the

[18] While partly this high mortality could indeed be due to discrimination linked to unwanted sexual intercourse of young women with older men, part of the high female mortality could also be due to earlier voluntary sexual

revised reference standards used above, high female mortality in the 15-29 age group continues to be a serious problem in Africa and AIDS is likely to be the reason. Thus discriminatory practices that push up female mortality from AIDS in this age group may play a role in accounting for excess female mortality by some 200,000 excess deaths in the 15-29 age group and thus most excess deaths in that group using the more reasonable reference standards in Table 2. But presuming AIDS had been a disease primarily affecting the gay community in Africa without gender discrimination, as implicitly done by AR, is certainly incorrect and vastly overstates the issue.

The calculation of excess female deaths from maternal mortality, another important cause of missing women in Africa and India, is also deeply problematic. Since there is no *male* mortality available to calculate a ratio, AR simply assume that *any* maternal death above the tiny rate prevailing in rich countries is an excess death, 'so that this procedure will treat practically all maternal deaths as excess female deaths, which is as it should be' (AR 2010:1279). Should it really be that way? This presumes that the high number of maternal deaths in Africa and India are entirely due to gender inequality rather than simply poor overall health conditions and health services. This is again vastly overstating the case. One way to see this, is to examine the relationship between maternal mortality rates and child mortality rates of both sexes, the latter being an overall indicator of health conditions in a country. This is done in Figure 1 using the best available data on maternal and child mortality. The Figure shows an extremely close fit between the two, suggesting that maternal mortality goes hand-in-hand with poor overall health. If African countries were outliers in this relationship, then this might be due to gender inequality in health access that goes beyond poor overall health conditions. We investigate this using the relationship in Figure 1 (using a simple linear regression and a dummy variable for SSA) and find that about 10-15% of the high maternal mortality in Africa (averaging 720/100000 live births there) is not explained by poor health conditions (proxied for by child mortality rates) and might be due to gender bias. So as with AIDS, a small share of deaths due to maternal mortality (around 20-30,000 rather than 200,000 annually as found by AR) might indeed be due to gender inequality, a subject worth investigating in much greater detail.

Third, there might be related issues in the category of injuries where within-disease issues might explain the substantial number of missing women in India. Deaths from fires, which is heavily male-biased in rich countries, but afflicts women more in India is likely to be largely based on the lack of access to safe energy for cooking and lighting, forcing households in India to use open fires, candles, kerosene, etc. which are the most likely cause of many domestic accidents; as women are much more likely to be at home doing these tasks, they are more prone to suffer from these accidents. Similarly, the more balanced male-female ratio of suicides in India (compared to rich countries where it is much more male-biased) might be related to a totally different origin of suicides where economic hardship apparently is the main driver of suicides of families (especially in rural areas), while in rich countries psychiatric conditions are the main causal factor for suicides (c.f. Aditjanyee, 1986; Hiroeh et al., 2001; Mortensen et al., 2000; Nordentoft et al., 1993; Qin et al., 2000).

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activities of females (and thus earlier AIDS risks) which is observable in most countries of the world, including rich countries; see World Bank (2011) for a discussion.

Lastly, AR note that in India women die relatively more than in the West from cardiovascular diseases. Again there is the question whether differences in the causes of these deaths between India and the West play a role. As shown by Case and Paxson (2005), the higher mortality of men in rich countries is heavily driven by higher rates of smoking and greater effects of smoking on male than female mortality, with cardiovascular diseases playing an important role. In India, smoking is much less prevalent, thus the smoking related death rates are therefore more gender-balanced than in rich countries.

Please note also that the results in Table 2 suggest that there is not much excess female mortality in the adult age groups in India. This is to be expected as the Model Life Tables are likely to capture the influence of overall health conditions and its influence on maternal mortality, the nature of injuries in high-mortality environments, and other relevant mortality conditions quite well. Thus these issues identified by AR to account for excess female mortality in India appear to be much less relevant when using the proper counterfactual.

This discussion shows that the disease correction by AR does not address the problems of different disease environments between rich countries and China, India, and Sub Saharan Africa. Either the reference standard is based on a different structure within a disease group (as in AIDS, injuries, or cardiovascular diseases), or the alleged excess mortality is largely a result of the overall worse disease environment (as in the case with maternal mortality). As a result, our results in Table 2 using more reasonable reference standards remain superior to the choice of the problematic standard of mortality ratios in rich countries.

6. Conclusions

Given the problems with the data, the analysis, and the control for the disease environment, what have we learned from the analysis in AR on the age and disease composition of the flow of missing women? At one level, one might say that the data issues alone which partly generated the results by construction and which are based on highly uncertain information on overall mortality and mortality by disease leads one to conclude that it is simply not possible to do what AR set out to do, i.e. to calculate a *flow* measure of missing women. As shown above, some of the data issues can, however, be partly addressed and the data problems for India are much less severe than for Africa. So another way to look at the results is to see what happens when a proper counterfactual is used for the analysis. The main conclusion from such an analysis is that the stylized facts from the previous literature on missing women have not been challenged but actually confirmed. In particular, the problem of flows of missing women is more serious in India and China than in Africa. Moreover, the problem of missing women in India is overwhelmingly a problem of pre-birth and post-birth discrimination among children, and not among adults. To be sure, confirming the findings of the previous literature on gender bias in mortality using a flow measure also confirms that gender bias in mortality is a serious issue that deserves urgent attention by policy-makers. According to our calculations, some 1.8 million females perish every year this way. But our results suggest that the focus of the literature and associated policy proposals on South Asia and China, and on pre-birth and young children, is largely correct as this is the places and age groups where this problem is most severe.

The one new finding that remains from AR is that there is some evidence of excess female mortality in Africa, mainly among young adults driven by excess mortality from AIDS and maternal mortality. In this age group, female mortality is higher than suggested by the overall disease environment and the expected mortality rate in a mainly heterosexual AIDS transmission channel. But this problem is much smaller than suggested by AR and provides some intriguing further detail to the issue of a gender bias in mortality in Africa that already existed in the literature (e.g. Klasen, 1996a, b; Svedberg, 1996; Klasen and Wink, 2002; 2003; World Bank, 1997). Further investigating this issue would surely be worthwhile, as would be further efforts to improve vital data from Sub Saharan Africa and India to be able to assess flow measures of gender bias in mortality more accurately in the future.

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Table 1: Adult Mortality Rates by Sex (15-59) in Populations and Model Life Tables

	(1) Male	(2) Female	(3) Sex Ratio	(4) Sex Ratio Lower Bound	(5) Sex Ratio Upper Bound	(6) Absolute M-F Difference
(1) AR-Sub Saharan Africa	0.542	0.478	1.133			0.063
(2) Logit Life Tables	0.377	0.355	1.062			0.022
(3) Model Life Tables 'West'	0.392	0.335	1.171			0.057
(4) USA 1900	0.405	0.380	1.066			0.025
(5) Est. Market. Econ. 2000	0.127	0.067	1.896			0.060
(6) Rajaratnam 2000-SSA	0.451	0.350	1.287			0.101
(7) Rajaratnam 2010-SSA	0.432	0.330	1.307	1.021	1.656	0.102
(8) AR-India	0.300	0.228	1.314			0.072
(9) Rajarantam 2000 India	0.258	0.1886	1.366			0.069
(10) Rajaratnam 2010 India	0.228	0.1446	1.580	1.271	1.963	0.084

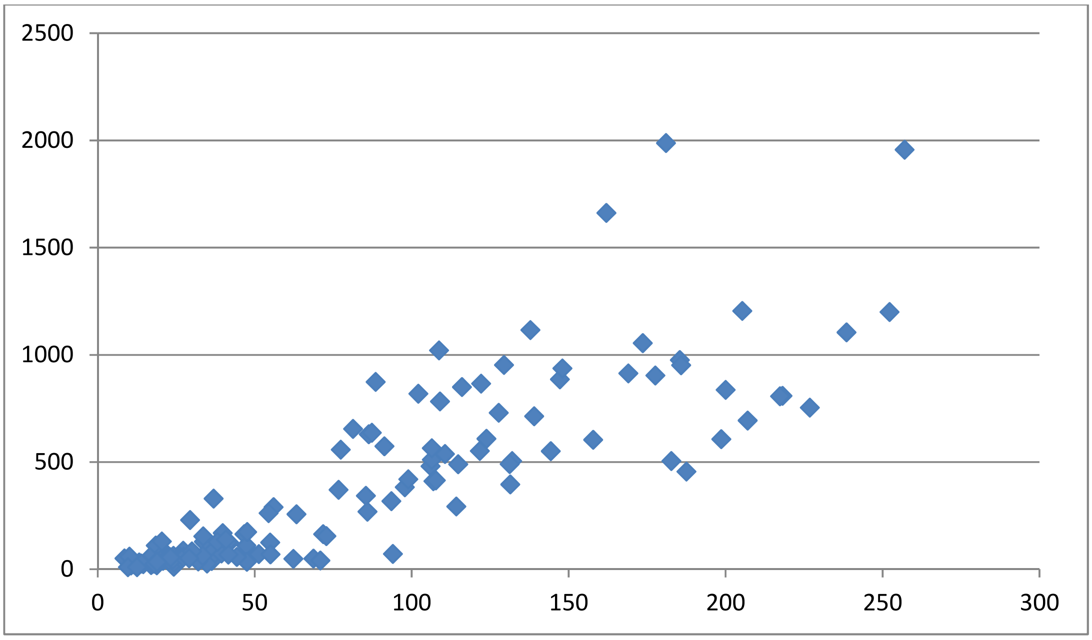
Note: Rates refer to the likelihood of having died by age 60 if one was alive at age 15.
Sources: Murray et al. (2003a, b), Coale, Demeny, and Vaughan (1983); Anderson and Ray (2010), Rajaratnam et al. (2010).

Table 2: Missing Women ('000) by Age Groups using various Reference Standards

	Ratios			Difference
	(1) EME (AR)	(2) MLT „West“	(3) „Expert“	(4) EME
Sub Saharan Africa				
At birth	0	0	0	0
0-1	32	22	23	-235
1-5	160	55	78	55
5-14	70	5	23	18
15-29	578	241	253	279
30-44	345	-3	-55	-24
45-59	84	-55	-151	-144
60-79	213	-40	6	-18
80+	44	-10	22	10
Total	1526	213	200	-59
Share Females ‚Missing‘	0.47%	0.07%	0.06%	-0.02%
India				
At birth	184	184	184	0
0-1	146	181	141	8
1-5	164	140	129	121
5-14	93	71	56	53
15-29	258	111	65	127
30-44	93	-70	-121	3
45-59	120	-39	-167	-114
60-79	541	63	100	103
80+	113	21	73	84
Total	1712	661	459	384
Share Females ‚Missing‘	0.35%	0.14%	0.09%	0.08%
China				
At birth	644	644	644	644
0-1	109	148	107	65
1-5	23	29	13	13
5-14	2	12	-12	-9
15-29	24	-9	-93	-9
30-44	73	-2	-61	86
45-59	89	-9	-150	15
60-79	490	27	-44	170
80+	272	64	175	165
Total	1727	903	579	1140
Share Females ‚Missing‘	0.28%	0.14%	0.09%	0.18%

Note: Ratios refer to reference standards that are based on sex ratios of age-specific mortality rates, difference to absolute differences. EME refers to established market economies, MLT are Model Life Tables, Expert is based on the 'expert' assumptions stated in the text.

Figure 1: Cross-country relationship between child mortality (x-axis) and maternal mortality (y-axis)



Source: World Development Indicators (child mortality) and Hogan et al. (2010). Child mortality is measured in deaths of children below 5 per 1000 children in the same age group while the maternal mortality rate is the number of maternal deaths per 100,000 live births.