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Comments on Paper by Emily Oster: Hepatitis B and the Case of the Missing Women

Main Message of the Paper
In her paper, Emily Oster suggests that a considerable share of the 'missing women', particularly in China, are not due to gender bias in mortality, but due to the prevalence of hepatitis B carriers among the adult population which has been found to increase the sex ratio at birth. Thus they did not die but were never born and thus are due to 'biology' rather than 'discrimination'. About half of the 60 million missing women that Coale had calculated in 1991 can be explained this way. The impact on the estimates of missing women is largest in China (75%), followed by Egypt and West Asia (50%), and lowest in South Asia (17% in India, 19% in Bangladesh, 28% in Nepal, 0% in Pakistan).

General Comments:

1) This paper approaches the question empirically from a number of vantage points, including cross-country evidence, time series evidence, and micro data evidence. These three pieces of evidence appear to tell a similar story although, as indicated below, they are all open to serious questions.
2) The paper's claims are grander than the evidence presented. Even if the evidence were credible, the interpretation should be much more careful. First, the range of estimates of the impact of HBV on the sex ratio at birth does vary considerably and she takes an estimate from the micro studies that are higher than the results from her various other analyses. Second, the claim that some 75% of Sen's 100 million missing women have been 'found' is totally erroneous, as Sen's 'estimate' was, unlike Coale's, not based on an assumption about the sex ratio at birth. Third, what the evidence suggests is that historical estimates of 'missing women' in China have been overestimated. The corollary to the claim (not discussed in the paper) about historical estimates from China is, that, consequently, the *increase* in the number of missing women in China in the last 10 years (estimated to be about 6 mission in Klasen and Wink 2002) has been much larger as the likely effect of the vaccination campaigns in China (begun in the 1980s and carried out with increasing intensity since) should have lowered the sex ratio at birth so that the actual increase of that ratio suggests that sex-selective abortions has been even more prevalent (and thus the effect of discrimination has been growing). Thus the central message I take from the paper is that the levels of discrimination against girls has possibly been lower in China than previously thought, but that the worsening of the situation for girls has been more dramatic in China.
3) Similarly, the paper takes as its baseline for the sex ratio at birth the assumption used by Ansley Coale in his 1991 paper (1.059), which is based on the average sex ratio at birth in rich countries. As pointed out by Klasen (1994), as well as Klasen and Wink (2002, 2003) and as shown by Oster herself in her cross-country regressions, there is a sizable and statistically significant positive link between life expectancy and the sex ratio at birth. The 'missing women' countries all have life expectancies lower than those in rich countries and thus their sex ratio at birth should consequently be lower as well. If one used this as a baseline and then adjusted it for the presumed link between HBV, the number of missing women would end up close to the total calculated by Coale, i.e. some 60 million. So the combined effect of the adjustments by Klasen et al. and by Oster would do little to change the grand total of 'missing women' but

would only change the geographical distribution, with South Asia being much worse affected, and Chine and Middle East being less affected.

4) The evidence presented is, however, much less convincing that one thinks at first blush. This will be dealt with below.
5) The empirical link between HIB and the sex ratio, is a black box relationship. As we know little about the causation in the areas where it occurs, we know little about its relevance in different contexts, particularly given that there are eight HIB genotypes with apparently different transmission rates.
6) Some of the arguments in the paper are a bit strange, including the claim that parents have been ‘helped’ by biology to fulfill their son preference. While this might appear to be the case in aggregate, at the household level many parents ended up with girls despite wanting a boy and being an HIB carrier; also non-carriers were not ‘helped’ at all by this. So the idea that parents could rely on this presumed relationship to fulfill their ‘son preference’, cannot be the case.
7) A lot of evidence that suggests that gender bias in mortality (post-birth) is a serious issue, including in China, is not discussed at all Thus a rather one sided picture is presented. This is largely driven by the fact that this is a purely demographic exercise focused entirely on one possible determinant of the sex ratio at birth. Among the evidence not considered are:

a) The large evidence on mortality rates by sex in the missing women countries which is consistent with both son preference and can explain the biased sex ratios in these countries. For China, there are a number of papers by Banister on this issue and historical papers Campbell and others.
b) A conclusive treatment of the issue of HIB and the sex ratio at birth in Africa (see below).
c) Cross-regional evidence on sex-specific mortality rates and sex ratios in India and China which also appear to be rather consistent with each other.

Detailed Comments on the Evidence

The author presents four pieces of evidence: about the sex ratio at birth in ‘missing women’ countries, about the sex ratio at birth of HBV carriers, about the effect of HBV eradication on sex ratios at birth, and about country and cross-country evidence on the link between hepatitis prevalence and the sex ratio at birth. Let me present and critique each piece of evidence:

1. Evidence of Sex Ratios at birth from Missing Women Countries and immigrants to the US from these countries

a) In Figure 1 she shows that in some missing women countries (particularly in China, but also in India, West Asia, and Egypt) the sex ratio at birth seems to be very high and that this might be an important factor driving the high sex ratio in these countries.

Critique: Only the China sex ratio at birth is really unusually high (given underreporting and measurement error) and here we know from a large literature (e.g. Johannsson and Nygren, 1991) that, as a result of the one-child policy, female children were hidden, adopted illegally away and parents tried to keep them out of sight at all cost to ensure that they could have a second child. The decline in the sex ratio after birth then happens because these girls ‘appear’ later in the statistics. There is strong evidence that this is driving the abnormally high sex ratio in China.

b) In Figure 2 she shows for specific cohorts in China and India that the sex ratio in China declines with age for particular cohorts, suggesting that the problem in China is the high sex ratio at birth and not discrimination in mortality. In India, the story looks very different where the sex ratio increases with age suggesting discrimination against girls in mortality.

Critique: The same argument applies for China as for Figure 1. In fact, one can show using the same data that there are more 5 year old Chinese girls in 1987 than there were girls born in 1982, suggesting that those girls were hidden in 1982 and 'surfaced' in 1987 (probably at their adoptive parents). Moreover, it is unclear why she uses the 1% sample of 1989, rather than the Census of 1990 to do the analysis.

c) Table 1 shows figures from Banister and Coale that suggests that the sex ratio at birth in China was already quite high (between 1.06-1.14) prior to the time where sex-selective abortions might have emerged as a common phenomenon.

Critique: The data are based on a retrospective fertility survey. They show that the sex ratio at birth rises the further back parents are asked to recall (they are highest for the 1936-40 cohort) typical for recall bias in a country with son preference where the births of daughters often go unreported. Close to the time of the survey (1982), the sex ratio is only slightly higher than usual (around 1.07-1.08 instead of 1.06) and this small difference could also easily be due to recall bias (which is smaller when the recall period is shorter). In fact, this Table is quite powerful evidence for the counterclaim that the sex ratio at birth in China has not been noticeably higher in the past, prior to the advent of sex-selective abortions !

d) She reports the sex ratio of mothers in the US whose race is coded "Chinese" as 1.082.

Critique: It is not clear that China-born mothers in the US have weaker son preference and thus it might well be that the (rather small) difference is due to underreporting of girls (esp. if one considers that girls were not given names but numbers in China up until very recently, see e.g. Elizabeth Croll's book).

e) Table 2 reports the sex ratio of young children of Chinese immigrants to the US and finds it to be higher although the numbers fluctuate wildly and the sample sizes are quite small. For non-Chinese immigrants, the sex ratios are noticeably lower (1.04)

Critique: For the 1940-70 period, these sex ratios are exactly as predicted (around 1.05). Since 1980 they increase and this could be the effect of the one-child policy (i.e. leading to sex-selective abortions as well as adoptions, gender bias in mortality etc) as quite a few of the births reported in the sample might have already taken place in China. To the extent the births took place in the US, the parents also had the ability to perform sex-selective abortions so that the increase in the sex ratio could also be linked to that. In any case, the data do not show a consistently high sex ratio at birth over time as they would have to if HVB is the main issue. Regarding the lower sex ratios of non-Chinese immigrants, this is presumably due to the impact of populations of African descent among the immigrant population (probably most are from the Caribbean) where there is overwhelming evidence of a slightly lower sex ratio at birth. The low sex ratio of the natives is also driven by the lower sex ratio of African-Americans. Among whites it is between 1.05 and 1.06.

2) Individual-Level Evidence on HVB Status and the Sex Ratio at Birth

In Table 3 she reproduces the results from six micro-level studies (from France, Greenland, Papua New Guinea, Greece, and Philippines) about the effect of HVB carrier status (not all infected children become carriers as adults) and the sex ratio at birth. These studies suggest that the sex ratio to carriers is noticeably higher than to non-carriers.

Critique: The evidence presented here is quite misleading for several reasons. First, the sample sizes are very small and the results thus quite unreliable. Second, it is not distinguished whether both parents or just one is a carrier. Third, the interaction with birth order is ignored. Anouch Chahnazarian did his Ph.D. in 1986 in Princeton on the determinants of the sex ratio at birth (using two of micro studies produced in Table 3) and found that the carrier status appears to increase the sex ratio at birth in low birth orders but not in higher parities. He concludes by saying that "the negative relationship observed here between birth order and the sex ratio at birth in children of carrier parents fails to provide an explanation of the unusually high sex ratios at birth observed at higher parities in China, a country of high hepatitis B prevalence." Fourth, there is at least one study (Hesser et al. 1976) that found a significantly lower sex ratio at birth in a four Melanesian populations. Fifth, none of the studies come from the 'missing women' countries. There are 9 types of the hepatitis B virus with different transmission rates and effects, which could easily include the sex ratio at birth. The types of virus differ by region. As a result, it is totally unclear what the evidence from these micro studies suggests about the impact of HVB status on the sex ratio at birth in the 'missing women' countries.

3) Time Series Evidence

Here she investigates the impact of HVB immunization programs that have been pursued in the US and in Taiwan on the observed sex ratio at birth. In the US, she focuses on native Alaskans who had a high known HVB status.

a) Figure 3 shows the sex ratio at birth in regions with high native populations and high HVB prevalence before and after vaccination, compared to other groups. It shows that the sex ratio at birth declined noticeably in the regions with high native and high prevalence populations.
   Critique: The sample sizes are very small, confounding effects are not investigated, and it is unclear whether the effects shown here are relevant for other regions. Also, is there evidence of gender bias among native populations?
b) Table 4 runs a regression on the likelihood of a child being male using data from Alaskan natives and non-natives and finds that, even if one controls for other factors, the chance of being male among natives is between 0.6 and 3% higher in the period prior to the vaccination than afterwards.
   Critique: First, these effects are quite small. In the second regression which should generate more precise estimates as it focuses on Eskimos (who were the ones with high HIB prevalence) being an Eskimo in 1990 leads to an 0.6% increase in the chance of being male (regression 2) which is rather small indeed and would affect the sex ratio at birth by about 1 percentage point. Second, there is a much larger effect of being an Eskimo in whatever time period. This goes against the claim that the unusually high sex ratio at birth among Eskimo's was mostly due to their high HIB rates. Third, one should try interactions between natives and birth order to see whether the effects are dependent on birth order as well. As before, it is unclear what this tells us about China and India, given the different types of the virus.
c) In Table 5, she analyzes panel data on the sex ratio at birth in Taiwan by cohort and year to see whether the effects of aggressive vaccination show up in affecting the sex ratio at birth. It shows that the HIB prevalence calculated for a cohort has a significant impact on the sex ratio at birth.
   Critique: The size of the coefficient is, again, very small and, more seriously, not significant and even smaller in the regression with year effects. The claim about countervailing behavior (i.e. sex-selective abortions) which led to the small effects is, as argued above, subject to serious questions. The proportion of women who, as a result of vaccination, now had a girl rather than a boy, is very small and only those women would be potential candidates for engaging in 'countervailing' behavior. It is likely that only a very small share of all women resort to sex-selective abortions and those are not likely to be concentrated among those few who 'additionally' had a girl, but among all women who happened to conceive a girl. Lastly, this countervailing behavior should be captured in the year effects.

4. Cross-Country Evidence

Lastly, the paper presents some cross-country evidence on the linkage between the sex ratio at birth and HVB prevalence.

a) First she presents in Figure 4 evidence based on WHO classification and shows that the sex ratio at birth is higher in high prevalence countries, even if one excludes China. The effect is also true within Europe.
   Critique: First, the effects are very small. If China is excluded the effect is a little more than one percentage point and would not change the missing women calculation by much. Second, there are no controls for other factors that affect the sex ratio at birth (including overall fertility and mortality conditions). Third, the data on the sex ratio at birth come from the Demographic Yearbook and it is not clear whether she uses all data, or only those based on 100% registration of births. There are serious problems of underreporting, including sex-selective under-reporting and one would have to be quite careful here.
b) Then she collects many studies about prevalence from the medical literature and generates prevalence rates by the weighted average of these studies by country. It yields a sample of 38 countries (where at least 1500 have been tested). In Figure 5 a scatterplot is presented (in Figure 6 for OECD countries only) and in Table 6 a regression of the sex ratio at birth on the left-hand side and HVB prevalence and GDP/capita and other controls on the right-hand side. She finds a significant positive influence on HVB prevalence on the sex ratio at birth (income has no effect, life expectancy a positive effect) in all countries and a sub-sample of OECD countries (which includes 16 countries) and in a sample where there is at least 90% complete birth registration.
   Critique: The scatterplotts and the regression have a number of very serious problems and the results cannot be seen as reliable for several reasons. First, the sample is very small and one does not know how representative it is. (In my analysis of the influence of life expectancy on the sex ratio at birth, we used more than 200 observations, see Klasen and Wink, 2002, 2003). It would be critical to report the HIB rates for all 63 countries (along with sample sizes) to reassure the reader that the sample is not a selected one. Second, the sample is entirely cross-section which raises all sorts of questions about unobserved heterogeneity. The Demographic Yearbook does have time series information on sex ratios at birth and one should try to investigate this issue in a panel framework. Third, the reliability of reported sex ratios at birth in the Demographic Yearbook is open to question. We know about sex-selective under-reporting of birth in China and India (e.g. Dyson, 2001; and Johannsson and Nygren, 1993) and it is likely to be an issue in other countries as well. Fourth, Figure 6 appears heavily influenced by China and South Korea, two countries with significant son preference, sex-selective abortions, underreporting of female births, etc. Both should be excluded (as should Mexico and Iran which suggest unusually low sex ratios at birth which can hardly be true). If excluded, the effect would be considerably smaller. Regarding Figure 7, South Korean ought to be excluded and then the effect would likely be much smaller again. Regarding the regressions, one should omit the countries I have just mentioned. Second, the reliability of the sex ratio at birth data needs to be checked. Third, other factors influencing the sex ratio at birth (e.g. fertility and mortality conditions) should be included in the regressions.
c) Third, she presents evidence on the sex ratio of various immigrant groups in the US and links them to HVB rates in their home countries (Figure 8 and Table 7) and finds again a positive relationship.
   Critique: First, South Korea, India, and China should be thrown out of Figure 8 and Table 7 for the reasons stated above. Once this is done, I think the effect would largely disappear. Second, it includes children, not births, with all the problems stated

above with such data. Third, it is bizarre to see that quite a few immigrant groups have sex ratios at birth below 1. Surely here are sample size or measurement problems. The regressions should control for birth order and mortality conditions as I mentioned above.

d) Lastly, she discusses a major empirical problem for the link between HIB and the sex ratio at birth which is evidence from Africa which points to a lower (rather than a higher) sex ratio at birth despite high HIB carrier rates. The author claims that there is a positive relationship (results are not shown) and that the low sex ratios at birth in Africa are due to other reasons.
Critique: It is the case that Africans have a slightly lower sex ratio at birth. This is well-documented in the US, where the difference is about 2-3 percentage points (i.e. 1.03 instead of 1.05-1.06); in the Caribbean, the effect is similar (in Africa the data is very sparse but points to similarly lower sex ratios at birth). Thus the race effect is small and, given the high prevalence of HIB in Africa (the rates are among the highest in the world), the presumed HIB effect should easily more than outweigh this race effect. Thus we are left to wonder how the high HIB in Africa does not lead to a higher sex ratio. This is further support for the view that the linkage is not the same everywhere and thus that one cannot generalize from individual studies.

5. New Missing Women Calculation

a) Table 8 summarizes the evidence from the three types of analyses.
Critique: For reasons stated above, none of the three are particularly convincing or relevant to the missing women calculation. Also, the table is incomplete. Using the second regression from the Alaska evidence would generate a much lower effect (although that estimate is likely to be more precise), as would the use of the Taiwan evidence. So this is a selective summary rather than a comprehensive one and biases the following assessments.

b) Table 9 then predicts sex ratios at birth based HVB prevalence rates and on the assumption that HVB carriers have a sex ratio at birth of 1.53
Critique: This hinges on the size of the linkage which, based on the criticisms above, is vastly overstated (and ignores some of the own evidence generated which show a much smaller effect). Also, this omits the influence of fertility and mortality on the sex ratio at birth which would generate effects in the opposite direction and thus reduce or possibly totally eliminate the effect of HVB prevalence on the sex ratio at birth.

c) Table 10 then calculates a new number of missing women based on the census information of the early 90s (using the data in Coale, 1991). All it does is to adjust the sex ratio at birth and then calculates a new number and share of missing women. The number of missing women drops by 75% in China, about 50% in West Asia and Egypt, and much less in the other countries. All told, about 46% of missing women are thus ‘found’. In the bottom panel, it then compares the number of missing women to the calculation by Sen (1989) and finds that about 70% of missing women are ‘found’ compared to Sen’ estimate.
Critique: The sex ratio assumptions rest on the previous analysis which has already been criticized. If, after correcting for all the problems above, there still remains an HVB effect, it is likely to be much smaller and will only affect the historical estimates of missing women in China in any significant amount. The comparison with the Sen’s estimates is misleading and confusing. Sen never has a sex ratio at birth assumption underlying his estimate. Thus adjusting his figures by something that never formed the basis of his calculation is adjusting apples by presumed biases in oranges.

d) Lastly, she presents some very limited evidence on HVB prevalence and the sex ratio at birth in regions of India and China and finds a positive correlation in Table 10. Critique: The data are, as the author admits, extremely weak, based on tiny samples, and open to measurement problems. No other controls for the sex ratio at birth are included. It is unclear why the data have not been pooled for the two countries. Little can be made of these observations. Particularly here, the question of the impact of vaccination on the estimate of 'missing women' would be useful to discuss.

Other comments:

1) p.5: the population sex ratio in rich countries is between 0.95-0.97, not 1 (largely due to age structure issues).
2) P.5: Missing women are those who have died of were never born due to discriminatory treatment (i.e. it includes the effect of sex-selective abortions)
3) P.6: equation 2 must refer to the cumulative mortality rate up to a particular point in time.
4) P.8: what is the source of the household surveys for the sex ratio at birth? Why not use the sample registration system for India?
5) P.8: Here would be a useful place to discuss the mortality rate evidence.
6) P.14: Here it would be useful to discuss the impact of changes in HIB prevalence (due to vaccination) on measured gender bias in mortality, in particular that it appears to have increased in some places (including in China and Taiwan and by more than was estimated by Klasen and Wink, 2002 and 2003).
7) P.21: There are serious issues about sex-selective underregistration. Thus the completeness issue is serious.
8) P.28: The interesting discussion about sex-selective stopping rules is illuminating, but is powerful evidence against the claims made in the paper. We know that the sex ratio of later-born children has been higher rather than lower as suggested here. If HIB was so important, it is clear that it should have been lower.
9) The conclusion really should discuss the impact these estimates have not only on historical estimates of missing women, but trends over time. As suggested above, the evidence implies that the rather optimistic assessment of changes over time presented in Klasen and Wink (2002, 2003) should be revised as the effect of spreading vaccination should have led to lower sex ratios at birth (and thus overall lower sex ratios) so that the actually observed lower sex ratios in a number of countries might be due to success in vaccination not reductions in gender bias. And in China, the observed increase would be even more worrying indeed.