On Explaining Asia's "Missing Women": Comment on Das Gupta

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IN AN EARLIER issue of this journal, Monica Das Gupta (2005) comments on a recent paper of mine. Her comment was based on Oster (2005a), a working paper version of Oster (2005b); in general, the two versions do not differ, but I will distinguish between them when they do. In that paper, I attempt to connect the issue of the gender imbalance in Asia—the "missing women"—with the prevalence of the hepatitis B virus. I present a wide variety of evidence suggesting that women who are carriers of the hepatitis B virus give birth to more male children than do noncarriers. I argue that perhaps as much as 45 percent of the gender imbalance observed in the Sen (1992) missing women populations in the period 1980–90 can be accounted for by hepatitis B. Further, I argue that the explanatory power varies significantly across space: 75 percent of the missing women in China are accounted for, versus around 20 percent in India.

The connection between hepatitis B and sex ratios at birth relies on existing individual-level studies as well as on new analyses: a natural experiment based on recent vaccination campaigns and cross-country evidence (Oster 2005a, b). Hepatitis B (a viral disease of the liver) is common in China, with 10–15 percent of the population infected before vaccination. I argue that there is evidence that the sex ratio imbalance in China arises at birth, and, in addition to presenting evidence on the sex ratio–hepatitis connection, I conclude that some of the sex ratio imbalance is naturally occurring. The analysis in Oster (2005a, b) is much more extensive than the short summary I present here, and it includes important caveats, robustness checks, and alternative counterfactual analyses.

In her note, Das Gupta argues that the hepatitis B explanation is unlikely to be important, since sex ratios in China differ over time and among

families in ways that seem consistent with couples' sex preferences. For example, in times of resource constraints (when, presumably, families must choose between children) girls suffer more than boys, so the sex ratio moves to favor boys. Similarly, in families whose first children are girls, later births are more likely to be boys. Because this evidence indicates that cultural explanations have explanatory power, Das Gupta states that "governments of these countries have been correct to focus their policies on changing the cultural roots of son preference" (p. 533).

I do not disagree with her fundamental conclusion about government policy. In particular, given the increasing availability of sex-selection technologies, sex ratios have risen substantially in these countries, which may have widespread (possibly negative) consequences. (As detailed in Oster (2005a, b), hepatitis B does not explain these recent increases in sex ratios.) More generally, there is evidence that women in these regions have less access to education than men, have low levels of bargaining power in the household, and are generally limited in their choices. To the extent that these facts could be altered by changes in government policy, those might be very valuable. Having said this, I would argue that Das Gupta's other conclusion—in particular, that the support for cultural explanations allows us to conclude that the biological explanation is not particularly salient—is flawed. This note discusses the issues intuitively and provides a simple model to illustrate the fact that evidence in support of cultural explanations by no means rules out biological ones.

Before tackling this central issue, it is worth mentioning the data on sex ratios presented by Das Gupta. In brief, she argues that sex ratios in China are "normal" in situations where there is unlikely to be parental intervention. As I discuss in more detail in Oster (2005a, b), there is, in fact, significant evidence that sex ratios in China and among Chinese women outside of China are higher than "normal" even in situations where intervention is unlikely. This includes evidence from Coale and Banister (1994), older literature on sex ratios in China in the early part of the twentieth century, and, perhaps most importantly, new data on the sex ratio of births in the United States to women who were born in China (and are therefore likely to have an average hepatitis B prevalence similar to Chinese women in China but unlikely to undertake infanticide, underreporting, etc). This evidence does not, of course, say that sex ratios do not increase with birth order in China; it is intended only to note that there is reason to think sex ratios among Chinese women are higher than "normal" even without parental intervention.

Let us turn now to Das Gupta's primary argument, that support for cultural explanations largely rules out the possibility of biological ones. As she says in her conclusion: "It is hard to see how biological factors could play a significant role in determining the sex ratio at birth when that ratio

is so closely related to the sex composition of the children already born in the family" (p. 533). Put simply, this confuses marginals and averages. In the theoretical appendix to this note, I outline a model that illustrates this point. I analyze the choices of utility-maximizing (but not gender-neutral) parents and show that resource constraints and existing children may change the sex ratio, regardless of what the naturally occurring average is. The fact that we see support for cultural explanations does not give us ammunition to dismiss the biological explanation.

The intuition for the latter explanation is relatively simple and is perhaps best seen by stepping away from the issue of sex ratios. Consider two countries, one of which on average is hotter than the other (for example, one is in the desert, the other in the Arctic). Imagine also that we observe that the desert country is cooler when the weather is cloudy and the Arctic is warmer when the weather is sunny. Presumably, we would *not* therefore conclude that the entire difference between the desert and the Arctic was cloud cover; rather, we correctly perceive that there is a naturally occurring difference in the average temperature but on the margin the temperature in both places can move.

The same logic holds with sex ratios. Two countries may have different levels in the sex ratio at birth, but income constraints or parental preferences could still alter them within a country. Because Oster (2005a, b) did not claim that hepatitis B explained *all* of the sex ratio imbalance, there is no reason that both factors could not be acting simultaneously.

Das Gupta also makes a number of smaller points about my thesis. She argues that Africa is a counter-example to the hepatitis B–sex ratio connection. As I discuss in Oster (2005a) and analyze in more detail in Oster (2005b), there is evidence showing the connection holds within Africa and also holds when Africa is included in the overall regression, and the relationship is of a similar size. Further, she notes that I do not provide evidence of a cross-regional connection between hepatitis B and the sex ratio within China today. However, in Oster (2005a) I do present evidence for this relationship in data from the 1980s, and I expand on the evidence in Oster (2005b). In both papers I show that the cross-regional relationship is strongly statistically significant and of appropriate size; in Oster (2005b) I also demonstrate it is robust to controls for some simple regional income measures. Finally, Das Gupta suggests that Chahnazarian, Blumberg, and London (1988) argue against the hepatitis B–sex ratio connection, a contention that I believe misunderstands the conclusions of their paper.

Ultimately, of course, these last points are minor issues. The key to thinking about the relative potential of culture and biology to explain the over-representation of men in a population is understanding that marginal effects may be seen to operate and still tell us relatively little about the average. In the end, it seems better to think of these two explanations as

complementary. The issue of gender imbalance in Asia—the causes and consequences—is an important one; we should endeavor to have a complete understanding, not just a partial one.

Theoretical appendix

Consider the following simple (but not necessarily completely unrealistic) model. A measure 1 of girls are born, and a measure n of boys, so the sex ratio at birth (generally, the ratio of boys born to girls born) is equal to n. Parents can invest in their children to increase the probability of survival. If parents invest c in a boy, he will die with probability $p(c)$; if they invest c in a girl, she will die with probability $q(c)$ where $p'(c), q'(c)<0$ and $p''(c), q''(c)>0$. Parental utility is simply equal to the sum of utility for each of their children: b for each boy and g for each girl, with $b \geq g$, so there is at least weak gender preference overall. Assume parents have income y, which is spent solely on investment in child survival. For simplicity, I assume spending does not differ within gender.

Parents then have a simple maximization problem: choose spending on boys c_b and spending on girls c_g to maximize their utility, subject to the constraint that the total resources are y. The maximization problem and budget constraint are given by the following:

$$\max_{c_b, c_g} (bn(1 - p(c_b)) + g(1 - q(c_g)))$$

such that

$$nc_b + c_g = y$$

Solving this yields the first-order condition:

$$-bp'(c_b) + gq'(y - nc_b) = 0$$

This defines the optimal level of c_b, and the optimal c_g is immediate based on the budget constraint. Das Gupta presents two types of evidence that she argues favors a cultural explanation over a biological one: sex ratios differ over time as resources change, and sex ratios among later births differ based on the gender of early births. I consider each of these in turn in the context of this model.

Consider first the issue of resource constraints: as y decreases, what happens to c_b? In particular, the question is whether $\frac{dc_b}{dy}$ is smaller when $b>g$ than when the two are equal: does favoritism of boys mean that boys lose less when resources are constrained? Through total differentiation, we find that:

$$\frac{dc_b}{dy} = \frac{gq''(y - nc_b)}{bp''(c_b) + ngq''(y - nc_b)}.$$

This is clearly positive. As resources decrease, c_b decreases also. However, it is also easy to see from this expression that as b goes up, leaving g fixed, the expression decreases in magnitude. When boys are heavily favored, resource constraints affect them less and, conversely, affect girls more.

Now turn to the issue of sex ratio by birth order. At its core, the idea is that for some families, in some fertility situations, the difference between b and g is larger than for other families. If parents already have three girls, then the value of a boy relative to a girl in the next birth is larger than if parents start with three boys. In the context of this model, we can ask whether spending on boys is increased when b is increased—is $\frac{dc_b}{db} > 0$? Total differentiation implies that:

$$\frac{dc_b}{db} = \frac{-p'(c_b)}{bp''(c_b) + ngq''(y - nc_b)}$$

This expression is clearly positive, so, as the value of boys relative to girls in the family increases, spending on boys rises.

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