Letters to the Editor

The ovopathy concept for explaining the secondary sex ratio

Dear Sir,

We thank James for his remarks on the statement of our hypothesis (James, 2004). According to him, our line of reasoning would not imply decisively that the ovopathy concept for explaining the secondary sex ratio (SSR) variation would be false, but, I summarize: (i) he disagrees with our statement that some adverse circumstances associated with female offspring have led to involvement of the notions of SSR ‘reversal’ and ‘inverted dose–response gradient’, which would suggest ‘a circular argument, i.e. one that assumes what is purportedly being demonstrated’; (ii) I would be unable to predict the SSR changes predicted by him, and particularly not in which direction; (iii) the data on the sexes of associated abortions would be needed in order to learn whether ‘sex ratio reversal’ occurs in accordance with a ‘dose–response fallacy’; (iv) the data on SSR variation with maternal age, social class, placental dysfunction, twinning and season have been misinterpreted; and, finally (v) he asks which findings would be required to falsify my hypothesis.

In response: (i) the notions of SSR ‘reversal’ and ‘inverted dose–response gradient’ are incorporated principles in the epidemiological literature, as referred to in my original debate paper (Jongbloet, 2004). Given that delayed ovulation and delayed fertilization in animal experiments result in gradually increasing attrition of the conceptuses, bad implantation and inherent loss of embryos and fetuses are the logic itself. If we are right that Y-bearing sperm are advantaged in the case of non-optimal liquefaction of the cervical mucus at the beginning and the end of the fertile window, a shift in the male direction and inherent loss of affected conceptuses should be the result. The sex ratios in pathological pregnancies appear to be higher, as illustrated in Downs’ syndrome: 115:100 when prenatally diagnosed and 128:100 at birth (Bishop et al., 1997). That means excessive loss of XY embryos and fetuses, i.e. more female surviving neonates or SSR reversal as shown in naturally breeding cattle (see Figure 1; Wolda, 1935). This association between sex ratio distortion with pathology of the conceptus is the central issue of the ovopathy concept and explains, in contrast to other hypotheses, a lot of the SSR variations in animals and humans.

(ii) Confirmation of James’ prediction does not prove a causal relationship as his premises are weak and not established, i.e. there is no biological basis for why parental hormone levels would control the probability of fertilization either by X- or by Y-bearing sperm, in other words, why low testosterone concentrations in fathers would bias their sperm or favour fertilization in one direction or the other. The ovopathy concept, in contrast, is founded on animal experiments and human observations. Low testosterone levels and inherent decreased libido imply decreased coital frequency and, thus, higher rates of ‘waiting’ oocytes in the oviduct, i.e. postovulatory over-ripeness ovopathy and probability of fertilization by Y-bearing sperm.

(iii) More data on the sexes of associated abortions are, of course, needed in order to learn whether ‘SSR reversal’ occurs in accordance with a ‘dose–response fallacy’. James allegedly presumes a skew in the female direction in spontaneous abortions. The ovopathy concept, however, does not predict selective female pregnancy loss, but disproportionally more male conceptions and loss of male pathological conceptuses. Disproportional loss of male-biased fetuses and fetal deaths is in fact general experience: 151 male to 100 female fetuses were assessed at the end of the second month; and 132 to 100 at the end of the third month (Kukharenko, 1970; Mizuno, 2000).

(iv) The scientific community will judge whether the data on SSR variation with maternal age, social class, placental
dysfunction, twinning and season have been misinterpreted. The SSR in mammals appears to approach equity at prime reproductive age, at the peak of their ovulatory seasons and in the case of affluent nutrition; the male proportion increases in less optimal conditions; but, in the utmost adverse conditions, a shift in the female direction is often revealed. Specific behavioural factors at very young and advanced age or lower socio-economic status, e.g. irregular coital frequency, unsafe contraceptive practices and deviating lifestyle (drugs and smoking), also endanger optimal maturation and fertilization. This accumulation of conceptopathology, of course, causes difficulties in interpretation, but does not render data ‘valueless in suggesting (or confirming) causes’, as contended by James, who adds ‘to know of no large sample which would support our interpretation of the variation with social class’. In fact, in a large study population controlled for ethnicity and birth order, Teitelbaum and Mantel (1971) concluded that the SSR increased significantly and positively from low to moderate socio-economic levels, but not further, and instead decreased again—a limit beyond which ‘improvements’ would not be biologically meaningful.

The few exceptions on the recent secular decline of the SSR in contemporary industrialized countries mentioned by James do not invalidate our concept. The overall decline in male births after the First and Second World Wars in both Europe and North America has been established to be highly significant (Grech et al., 2003), and has been attributed to gradual increases in socio-economic conditions (Jongbloet et al., 2001). The continuous rise of the male proportion before the First and Second World Wars was also due to the same diminishing conceptopathology as a consequence of less undernutrition and better health facilities, and, thus, to diminishing loss of male-biased pathological conceputures. The same line of thought may account for other phenomena, such as the SSR skew around the turning point in the early 1980s in south western Siberia (Jongbloet, 2003), an increasing trend in the black populations in the USA (Marcus et al., 1998) and less affluent (Southern) non-metropolitan areas in Italy (Astoni and Zonta, 1999).

In connection with James’ claims on SSR variation and placental dysfunction, I refer to earlier work of Iffy (1972) who pinpointed delayed ovulation as a causal relationship. James also contests our references related to male-biased perinatal mortality in monozygotic (MZ) twins by referring to Hall (2003) and suggests that this topic is ‘outside the scope of general theorizing about sex ratios’. Under close scrutiny, Hall’s review does not reveal any denial of our claim, but instead indicates a multitude of congenital anomalies and vanishing twins associated with MZ twinning. I disagree, and it should be remembered that one-egg twins were the paramount phenomenon after delayed fertilization in animal experiments and human observations. This was the reason to put forward MZ twinning and anomalous X-chromosome inactivation as consequences of one phenomenon, i.e. over-ripeness ovopathy (Jongbloet, 1971, 1988). The sex proportions in the survivors are not in contradiction to the determined timing of the various types of MZ twins (Chitnis et al., 1999).

In connection with the required falsification of our ovopathy concept as an explanation for the sex ratio variation, I will restrict myself to the mentioned seasonality of births, a phenomenon recognized by James, but disliked by him as ‘not related to sex ratio’ and ‘little to do with season’. If he were able to show—in contrast to what is known in naturally breeding animals (see Figure 1; Wolda, 1935)—in a stable population any higher male proportion at the seasonally bound birth peak(s) (instead of a decrease at the optimum ovulation rate), a lower one at the slopes (instead of amplification during the transitional stages) and again a higher one at the birth troughs (instead of ‘SSR reversal’ during the ‘anovulatory seasons’), I will concede.

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References


