
Response to Judith Anderson's Comments on Low, Alexander, and Noonan (1987)

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Anderson develops six arguments that she believes counter those of Low et al (1987) in regard to sexual selection and the possibility that fat deposits on the hips, breasts, and buttocks of human females are deceptive. We think her arguments cannot be sustained.

1 She argues that sexual selection has not been an important influence on either the behavior or the physiology and morphology of human females, citing Gee (1982) that in nontechnological cultures most marriages are arranged, and in most cultures 95% of the women marry. Items of importance in sexual selection, however, are not evolved simply to attract mates, but also to attract the *best* mates. Wherever males vary significantly in quality (including especially, their promise of parental care—of all forms), females potentially can gain immensely by attracting the interest of superior males (see, e.g., Low 1979). Even in societies with arranged marriages, variations occur in the desirability of females, as is evidenced by differences in ages of marriage and bride price (e.g., Borgerhoff Mulder, 1988). Our hypothesis implies that across history, females have invested a great deal in mate attraction, and the principal reason is that human males do indeed vary dramatically in quality. There is evidence that women or their families in nontechnological societies exert considerable effort into the attraction of powerful or resource-rich males (e.g., Flinn and Low 1986).

2 She believes that evidence of variations in what human males apparently find attractive denies that some attributes in women are viewed as desirable by men generally, and that the fact that some of human males'

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interests in female attributes are learned denies that evolution and sexual selection have been involved. We are aware of cultural differences in male preference and argue that these variations in preference and the observable responses to such preferences constitute powerful evidence that sexual selection does act on human females as well as males. We do not think that particular cultural traditions (e.g., Chinese bound feet) or temporary fads (e.g., flappers) deny the existence of traits of general interest, and we doubt that anyone has eliminated the possibility that there are some attributes of the human female that virtually all men find attractive (e.g., Buss 1987). The traits likely to be broadly appreciated are those reflecting health, high reproductive value, and receptiveness (Low 1978).

On the second point Anderson seems to be confusing proximate and ultimate causes—surely it is no longer necessary to emphasize that the capacity to learn is evolved and that what is learned is not random (e.g., Alexander 1979; Cosmides and Tooby 1987).

3. She argues that there is no correlation between breast size outside lactation and success in lactation. The book she cites, however, is designed to promote breastfeeding in a modern technological society, and presents no data but only an assertion that size does not matter. In fact, the hypothetical illustration given in Minchin (1985, p. 112) supports our hypothesis that fat on the breasts may be deceptive, showing a small nonfatty breast compared to a large breast with little glandular tissue and much fat. As we noted, that breast size is currently unrelated to lactation success in technological societies (with supplemental feeding) is irrelevant to the hypotheses that breast size was important in evolutionary history and that patterns of breast size and lactation frequency might co-vary cross-culturally. Anderson is inconsistent in using a modern society to make her point while previously arguing (in connection with male preferences) that modern society cannot be used as a criterion.

Breastfeeding failure, while commonly due to insufficient information, lack of confidence, etc., may also occur because of insufficient glandular development of the breast, and such insufficiency may be heritable (Niefert et al. 1985). This again argues that breast development and size due to mammary tissue have not been irrelevant to success—that large breasts due to fat are deceptive and that sexual selection is likely to have operated.

She argues that breast size cannot indicate storage capacity because milk production and storage are antagonistic functions in humans, citing evidence that storage of milk for more than a few hours leads to a reduction in milk production. These two functions cannot, however, be adversarial at base unless nursing is continuous. We would rather describe this relationship as indicating that failure to use milk eventually results in lowered production of it. The data in the papers cited by Anderson do not suggest that larger breasts cannot both produce and store more milk—as we would suggest, except when the breast is large by virtue of fat deposits rather than glandular and storage tissues. As we noted, the point at which storage of

milk without use inhibits further production varies among species, depending on the pattern of lactation frequency. We suggested that it may vary among societies, depending on the history of lactational frequency patterns.

4 She believes that evidence that the birthing-functional aspects of the female pelvis cannot accurately be determined externally, and that the iliac crests yield a "false pelvis" effect, deny our hypothesis that the human female may have evolved to give the appearance of a wider pelvis than is actually the case. Both points, however, may support the deception argument. The more difficult is accurate assessment, the more difficult it is to detect deception. She also believes that difficulty in birth owing to large cephalic dimensions is largely owing to unfavorable birth postures required in modern hospitals. Needed here is a comparison of cephalic dimensions relative to maternal size and a compilation of information (currently lacking) about head size and birthing difficulties in nontechnological societies.

5 She argues that females could not gain reproductively by giving false impressions about pelvic width. We have already countered this argument by noting that what males favor and what is best for females may be different. Her argument, however, requires that females deceive males while keeping their pelvises narrower than would be to their own advantage, our argument involves males favoring wider pelvises than would be advantageous to females. Further, wide hips, whether or not they ever made birth easier, could increase in frequency if favored by males. For any trait to be favored through sexual selection, it need not render any advantage to the possessors of the trait, other than being favored by the choosing sex (Fisher 1958).

6 She presents four hypotheses as alternative to ours: that wide hips evolved to assist females in carrying babies, that fat evolved to insulate women's breasts and buttocks, that breast fat is "an adaptation for making the breast large and soft enough so as to be convenient for an infant to reach," and that the function of breast fat is contribution to a particular hormonal environment. She indicates that "most" of these hypotheses are "easily falsifiable" but makes no effort to falsify them. First, none of these hypotheses excludes ours and sexual selection does not seem to be excluded in any case. For example, Hottentot women carry their babies on their prominent buttocks, and men evidently have used buttock size as a criterion in sexual selection (e.g. Darwin 1871, II 345). Similarly, wide hips could easily be selected on several bases, including sexual selection.

Second, the hypothesis that breast fat evolved to insulate the mammary tissue, and that buttock fat functions to insulate the posteriors of women sitting on the ground is testable. Breast size due to fat and fat deposits on the buttocks should increase as cold stress increases. We specifically argued, however, that fat on the buttocks is not deceptive. If sexual selection were not involved, men living in cold climates should also have extra fat on the buttocks. In fact, steatopygia is most pronounced in women in a subtropical area, and there is evidence that sexual selection has been powerful even in opposition to selection on ability to move.

Third, she suggests that breast fat functions to make the breast available to infants who need a breast that will hang conveniently—as it rides on its mother's hips. If fat were advantageous in producing elongate, pendulous breasts, then such breasts should be unusually fatty; we know of no evidence suggesting this.

Finally, fat may contribute to a hormonal environment, but it is a confusion to suggest that it evolved in breasts because some particular hormonal environment was important. Not only are proximate and ultimate mechanisms being mixed (again) as in Mastia-Lees et al. (1986) criticised in our original paper, but if this is its function, one has to wonder why extensive elaboration of breast fat seems to have evolved only in humans.

Some of Anderson's arguments are also critically reviewed by Caro (1987) who cites additional references. Caro misstates the original deception hypothesis of Low (1979, cited but not referenced) but nevertheless fails to dismiss the hypothesis that fatty breasts may be deceptive.

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