Comment

Fat and Deception

Response to Caro and Sellen's (1990) Comments on Low, Alexander, and Noonan (1987)

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Caro and Sellen (1989) developed several arguments that they believed were contrary to the hypothesis of Low et al. (1987) that fat deposits on the breasts and hips of reproductive-aged human females are ambiguous sexual signals and are at least potentially deceptive. Some of these arguments had been presented previously by Caro (1987) and Anderson (1988). Caro and Sellen (1989, hereafter referred to as CS), also put forth a hypothesis which they argued is an alterantive to that of Low et al. (hereafter referred to as LAN).

There are several problems in the CS article. Those criticisms that are repetitions have been answered by Low et al. (1988); while CS cite Low et al. (1988), they did not address our points in their text. Some internal inconsistencies in CS result in 1) dubious interpretation of earlier papers, and 2) citation of data to refute LAN which actually support LAN. I will point out several of the structural and logical flaws in CS, then argue that their "alternative" hypothesis is not an alternative but actually a subset of the LAN hypothesis, clearly stated in the original paper.

In the section entitled "Theoretical Weaknesses of the Deception Hypothesis," CS stated that "morphological features that function as dishonest signals are not likely to be evolutionarily stable because selection will eventually favor receivers of the signal (males) who come to recognize that the signal conveys dishonest information." CS cite Trivers (1985) and Krebs and Davies (1987), both general texts, in support of their statement. However, if the cost to males of failing to recognize the deception is less than the benefit to females of being deceptive, then the deception would, in fact,

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be evolutionarily stable; this is the condition we expect if fat itself is valuable. This asymmetry in cost/benefit would be predicted in any polygynous organism. Males are simply the "less choosy" sex, although males are expected to engage in some degree of mate choice in cases where they invest parentally, a point made by Trivers (1972, 1985), Low (1979), and LAN.

In the section "Detecting Cheating Females," CS contend that since physicians can detect the degree to which hips are composed of fat, human males should evolve to do the same. We can also detect ovulation medically, and their logic would argue that we should have evolved to be aware of it also; arguments like CS's here are kin to wondering why owls don't have sonar, since it would obviously be useful (see Williams 1966, p. 30, for the fallacy in such an approach). Beyond this general problem, their argument is further flawed; CS cited Broude and Green (1976) as allegedly reporting on the great degree of touching during courtship and prior to copulation. Broude and Greene do not, in fact report on the degree of touching in courtship; they have a code on "male sexual aggressiveness," in which they report that in 41/65 societies, men are diffident, or make no advances, or make verbal but not physical advances. In only 7/65 societies are men physically aggressive in sexual overtures, and in 17 societies, men's advances are reported as hostile or involving rape. Broude and Greene (1976) also report for 39 societies on the degree of foreplay, but this is generally reported within marriage, and in the code, marital or courtship status is not specified. It is difficult to see how such information relates to the kind of physical touching in courtship that CS imply. In a majority of societies, then, there is little or no evidence of much touching prior to a marriage contract, and there is also no "courtship" as Western industrial societies are likely to understand it, prior to such a contract. Furthermore, such contracts are frequently made by coalitions of the bride's and groom's male relatives (cf. Flinn and Low 1986). Some Western societies may be exceptions to this rule, but only in modern times. Chaperoning was common, even given the comparatively "loose" social norms of the West, until very recently. Thus, it seems safe to conclude that men in many societies can see but cannot touch. CS's argument represents confusion of current condition with evolutionary pressures.

CS then stated that if there is a cost to females of having very large breasts, and males choose mates with the same criterion, females' breasts should converge on some optimum shape. We answered this point in our response to Anderson (Low et al. 1988). The reference CS cited to suggest that this is not the case is a study on obesity in modern humans (Garn et al. 1987). Again, such evidence is not relevant in any case, as obesity in modern humans is an evolutionarily novel event and does not reflect the selective forces operating on humans in predindustrial societies. In the next paragraph, CS stated, "We need to analyse anthropometric data to determine whether fat is preferentially deposited on breasts and hips as opposed to

other female-favored areas." These data do exist and were cited by LAN, a fact ignored by CS.

In the section entitled "Costs of Cheating," CS implied that extra fat deposits, if ambiguous or deceptive, must be costly to females. There is nothing in the LAN hypothesis that would require this being true. In fact, the converse is true, as we specified: The deception should evolve only if it benefits females, and it should remain only if it benefits females more than it costs males who do not recognize it as deceptive.

In a subsequent section, CS addressed some alleged assumptions of the LAN hypothesis. The first of these regards male investment in offspring. CS stated that there is no evidence that human males invested parentally during evolutionary time or among modern hunter-gatherer societies. This is nonsense, as human males are known to invest parentally in virtually all societies for which there are data (although in a few societies, males invest in their sister's offspring, apparently because of a low confidence of paternity [cf. Alexander 1979]). CS stated that "among extant hunter-gatherers, an arguable analog for these extinct societies, there is little property, either real or moveable, that can be transferred to offspring." The authors apparently believe that the only possible investment is that of property inheritance. Were this the case, *females* in virtually all societies must not invest in their offspring either, as they very rarely control property in any society (e.g. Low in press; Whyte 1978). Of course, this is not so, and, almost universally among humans, males can and do invest by protecting and provisioning their offspring. The widespread use of the term *bastard* as derogatory attests to this fact (cf. Laslett et al. 1980).

Several arguments should be critically reviewed here, as the CS assertions are serious, and their treatment of the literature and LAN is questionable. In the section in question ("Evidence that Males Universally Invest Heavily in Offspring"), CS cited Kaplan and Hill (1985) as their main example and stated, "Ache men, for example, do not preferentially feed their own wives or children in comparison to other members of the group." What Kaplan and Hill (1985) actually said was

> Within the band, close kin receive no more meat and honey from their acquirers than do more distantly related and unrelated individuals. *Spouses and children do, however, receive more fruits, larvae, palm products, and mission-bought foods* from their wives, husbands, and parents than do other band members. (p. 235, italics added)

Furthermore, Kaplan and Hill noted that this relatively equal food sharing is largely an artifact of the small, closely related band structure; that is, within the band (in which all members are closely related), the differences are small, as described. Across bands, it is clearly true that men share more of all kinds of food with close relatives.

Kaplan and Hill further stated, "High return hunters are reported to have *more extra-marital sexual relationships* than poor hunters, and *their children survive in significantly greater numbers*" (p. 237, italics added). In

another article, Hill and Kaplan (1988) noted that the increase in childhood survivorship is due to a father's involvement with his children—another form of paternal investment. Other studies (e.g., Flinn 1988; Hames 1988; Hewlett 1988); have found important impacts of paternal care (increased survivorship, increased ability to form suitable marriages, etc.), and they take up the more interesting questions of relative levels of maternal versus paternal direct care, the relative payoffs to males of parental versus mating effort (see also Low 1978; Trivers 1972), and the ecology of paternal care. CS, however, concluded that "we know of no studies where quantitative data unequivocally show that male investment in offspring increases their reproductive success." CS grossly misquoted and misrepresented the original text, and ignored other relevant literature. Kaplan and Hill's results are fully consistent with our argument and with other studies that show that men do invest parentally and profit reproductively from doing so.

In a second subsection treating "assumptions," CS cited several studies that showed that sex differences in pelvic structure do not produce differences in the mechanics of pelvic function. This is supportive of the LAN hypothesis and counter to the CS conclusion. LAN would directly predict that the (deceptively) wide hips (because of fat deposits) of human females would not interfere with locomotion. Locomotion would be hindered if hips were nondeceptively wide (i.e., pelvic structure so wide as to be costly for upright posture); this was discussed in LAN.

CS then questioned whether fat deposits are signals to males, stating that "there is evidence from several sources to show that some men in certain subsets of Western societies find breasts attractive" and citing an older work to support the statement (Wiggins et al. 1968), but then questioning whether men in other societies do likewise. Their statements are loaded and ignore the fact that LAN (and Low [1979]) argued that many cultural differences in what is considered attractive should, in fact, exist and that they should have ecological correlates, but that all should reflect high reproductive value.

The fact that pornography is a multimillion dollar yearly enterprise in the United States and Western Europe, and that much of it shows young women of high reproductive value with well-endowed breasts and hips, but who are otherwise thin (see Low 1979), and the fact that virtually all of it is aimed at a male audience (for good discussions of pornography, see Daly and Wilson 1983; Symons 1979), would argue that it is more than "some men in certain subsets of Western society" who are interested in signals of reproductive value. These observations support the LAN hypothesis. Furthermore, cross-cultural codes exist on pornography and sexual jokes in the Human Relations Area Files, and non-Western parts of the world (consider Asian decorative architecture and erotic art) have a long history of thriving sexually explicit markets; such phenomena suggest that these are not simply modern Western inventions; these observations also support the LAN hypothesis.

CS then went on to claim that the deception hypothesis ignores the

possibilities of choice based on other criteria. They gave examples of men choosing unambiguously fatter women, citing Daniella Seiff about the Kel Tamasheq, nobles of the Tuareg, who live in an arid and extreme environment in Mali. This, contrary to CS's discussion, does not contradict, but supports, the LAN hypothesis. In fact, LAN argued that in extreme environments, nondeceptive fat displays should be preferred; Seiff's example is similar to the example of the Nama, discussed in LAN as a predicted situation in which men should prefer unambiguous displays of fat, signalling that "even in this extreme and poor environment, I can acquire enough nutrition to store fat," a predictor of sufficient nutrition to lactate successfully. Interestingly, in both cases, there is evidence that the preference for unambiguous fat is due to sexual selection and actually imposes a cost of females (really desirable Nama women may have trouble rising from a sitting position on flat ground, and the most desirable Kel Tamasheq women may be so obese as to find any movement difficult). The LAN hypothesis does not preclude the other possibilities mentioned by CS, and LAN actually discussed several of them in their 1987 article and specified the predicted environmental correlates, a fact not acknowledged by CS.

CS then asserted that the LAN hypothesis rests on the unsupported assumption that "fat laid down in breasts or on hips is not advantageous to the bearer independent of its putative effects through mate choice." The LAN hypothesis, in fact, does not require this assumption, and Low et al. (1987) never made it. Such an assumption is unnecessary, as any trait could be beneficial in more ways than that for which it originally evolved. In fact, this is a good example of the consistent misinterpretation of LAN's argument. Our original paper 1) began with the observation that fat is more useful to reproductive-aged women than to men; 2) addressed alternate hypotheses about the unique placement of fat in reproductive-aged women; 3) specified the appropriate tests to distinguish among these hypotheses; and 4) reviewed existing data, commenting on their inadequacy for solving this problem. We considered this an interesting unsolved problem, which might profit from explicit presentation of alternative hypotheses. CS consistently assert that we made assumptions, in cases in which a reading of our article will show that we simply made an "if . . . then" logical construction—not an assertion of fact. Similarly, Caro (1987) earlier mis-quoted Low (1979), saying, "She regards large breasts as a means by which females deceive males of their lactational potential." In fact, what I said was "If amount of mammary tissue correlates with maternal fitness . . . females with large breasts and males who prefer them will be favored. . . . Thus one might predict that men would be especially attracted to large-breasted but otherwise slender women."

Again, Caro misread an if–then logical construction as an assertion. In this case, he failed to include a citation, meaning that interested readers could not easily find the original.

In a subsequent section on assumptions, CS cited numerous medical references that show that there is currently little evidence that breast volume

(they do not specify mammary tissue) predicts lactational output. They discussed these observations as contrary to the LAN hypothesis. The LAN hypothesis predicted that there should be no correlation between apparent breast size, including fat, and lactational output. Again, both LAN and Low et al.'s response to Anderson addressed this problem. If CS meant that mammary tissue volume does not correlate with lactational ability, they have again confused current abilities of well-nourished Western women with the selective forces during the evolution of humans. Nonetheless, even in modern societies, mammary tissue insufficiency, if severe, does result in lactational insufficiency (Niefort et al. 1985, cited in Low et al.).

Most of the references for the next section ("Data Show Pelvic and Iliac Canal Width Are Not Linked To Obstetric Complications") are from modern medical sources and, again, are largely irrelevant. Data do exist, (cited both in LAN and the response to Anderson) that in fact, while a number of factors are important for successful delivery, too-narrow pelvises can be problematic even today. More information on pelvic measurements and birthing complications in hunter-gatherer societies with little available modern medicine would be needed to present a convincing argument one way or the other here.

CS then presented their "alternative" hypothesis: that body fat has direct positive effects on reproduction in women. This is, in fact, not an alternative at all and was discussed by LAN, who began with this proposition. As we stated quite clearly, it was not the *amount*, but the *distribution*, of fat that concerned us. If all fat in reproductive-aged human females were an unambiguous display of fitness (as CS apparently believe), there would be no reason to expect the observed differential distribution of fat in reproductive-aged females. In fact, reproductive-aged females would be expected to put on fat exactly as do males of all ages, prepubescent females, and postmenopausal females, and males would be expected to be attracted to unambiguous displays of fat (such as the amount of fat on the upper arms and buttocks) if fat *per se* were the only criterion. Fat *per se* is one criterion for mate choice in some cases—extreme environments. LAN discussed this and the importance of unambiguous displays of fat (e.g., on the buttocks).

In summary, the arguments presented by LAN to explain the differential distribution of fat on the hips and breasts of reproductive-aged females are not refuted by any of the "weaknesses" or "assumptions" discussed by CS. Some of the data cited by CS directly support the LAN hypothesis, contrary to conclusions in the CS paper. CS rehash several arguments that LAN had discussed and refuted, without acknowledging the arguments given by LAN, and, in some cases, they misquoted other authors. The "alternative" hypothesis presented in the paper is not, in fact, alternative and was originally presented in the LAN paper, although with more specification.

From several standpoints, the LAN hypothesis remains the most convincing argument to explain the unique fat distribution in reproductive-aged women. The main insufficiency of LAN is difficulty of coming to firm con-

clusions because of the dearth of data, but although CS repeatedly paraded lack of evidence as a criticism (a strategy usually attributed to creationists [Nelkin 1982]), LAN themselves discussed the limitations of the data and then specified the kinds of observations that should be made and that could refute their hypothesis. The dearth of data from other societies is actually indirect support of the LAN hypothesis. If breasts were not sexual signals, if men weren't interested in them, if they really didn't matter at all, why are they so often covered, and why is it so difficult to get data on them? In Nepal, for example, the only reproductive-aged women who bathe topless at village taps are 1) married, 2) mothers several times over, and 3) approaching middle age (i.e., their reproductive value is low) (Heinen, personal communication). However, all boys, men, prepubescent girls, and old women bathe topless. Only unmarried reproductive-aged women remain covered. The same situation exists among the Dogon of Mali (B. Strassman, personal communication). Observations like this are curious if human female breasts do not function as sexual signals and if men are not interested in them. Consider this: If breasts and hip fat did function as deceptive sexual signals, then it could be riskier for anthropologists to take data on them compared with taking data on unambiguous fat displays, such as the upper-arm skin-fold test used by development agencies worldwide—which seems to be the case. If CS are convinced of their arguments, I would challenge them to go to the Subcontinent and ask permission to pinch the breasts of random sample of reproductive-aged women.

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