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**Sociobiology and Incest
Avoidance: A Critical Look at
a Critical Review**

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I sympathize with Gregory C. Leavitt's stated goal in critiquing the evidence upon which early sociobiological analyses of human incest avoidance are based (AA 92:971-993, 1990). In fact, Rauf Ali and I wrote an analogous critique of the data used to support the inbreeding-avoidance hypothesis for sex-biased dispersal among animals (Moore and Ali 1984), which he cites favorably. Unfortunately, Leavitt goes on to evaluate sociobiological theories themselves (e.g., p. 984). The distinction is important because most of the articles he criticizes were written between 1975 and 1983. Thus Leavitt's stated goal is not his actual destination, and his rejection of a sociobiological theory is accomplished without considering most of the relevant biological and ethological literature of the last decade.

A second problem is that there are a number of misleading statements in the presentation of material he *does* consider. If all anthropologists read widely in these fields, the paper would be read for its good points, these errors quietly forgotten. This is not the case: How many *American Anthropologist* readers are familiar with Baker's (1982) work on white-crowned sparrows, which Leavitt (mis-)cites? Since incest avoidance is one of anthropology's central problems, a comment on some of those errors seems worthwhile.

Cultural Universals

Leavitt states that sociobiologists have dealt with "noncompliance" with incest taboos by "redefining incest." Both social and biological sciences use the terms *inbreeding* and *incest*, often imprecisely, and this does lead to confusion, as is clearly illustrated throughout Leavitt's article by his failure to distinguish inbreeding from incest (e.g., his discussion [p. 975] of "outbreeding mechanism[s] posited by human sociobiology" [avoiding incest \neq outbreeding]). (See Shields 1987 and Thornhill 1991 for discussion of definitional issues.)

Earlier in his article, Leavitt states that:

The argument that environmental factors can produce exceptional cases . . . only further complicates the specification problems discussed above; for example, what is unusual in the Roman, Egyptian, Arapesh, or Mormon environments that *nullifies the genetic imperative* hypothesized? The hypothetical *interjection of environmental variables also produces a set of theoretical problems that natural selection theory seems as yet ill-equipped to handle.* [pp. 973-974, emphasis added]

By analogy he presumably would argue that the existence of a secular trend in adult height

demonstrates the absence of any genetic influence on stature. (For discussion of the sociobiological "problem" of environmentally correlated variation in behavior, see Wilson 1975, Krebs and Davies 1987, Hrdy 1990, or any issue of *Behavioral Ecology and Sociobiology*.) Space precludes full discussion of factors that can promote incestuous behavior. However, two are sexual asymmetry in parental investment, leading to the prediction that the "less-investing sex" (males in most mammals) should be relatively more tolerant of incest; and the distribution of resources important to members of a relatively K-selected species, such that inbreeding may result from territory retention or resource monopolization within a kin group (cf. "royal incest" in humans) (see, e.g., Smith 1979, Moore and Ali 1984, Thornhill 1991).

Inbreeding Effects

According to Leavitt, among Pleistocene hominids genetic loads would have been low but "any system of inbreeding that is reasonably possible would not greatly reduce the heterozygosity of the population" (p. 975). However, genetic load is reduced by the cumulative "weeding" effect of exposure of lethal recessives in *homozygotes*. Unreduced levels of heterozygosity and genetic loads approaching zero are simply incompatible. Even one or two transfers between social groups per generation would prevent the elimination of genetic load that he postulates; such groups are *socially* "isolated" and they would probably be somewhat inbred, but one cannot conclude from this that nuclear family incest would not result in inbreeding depression. (See Ralls, Harvey, and Lyles [1986:52-54] for a lucid discussion.)

Ethological Studies

The distinction between cited early evidence and current theory is vital here; claims of virtually complete endogamy among baboon and macaque groups reported during the 1960s have been rejected for more than a decade, a fact Leavitt fails to mention (for a review see Pusey and Packer 1987). There are also problems with the representation of articles he does discuss.

Leavitt implies that Erickson (1989) was misleading in his treatment of Itoigawa, Negayama, and Kondo (1981), and emphasizes that Itoigawa et al. reported mother-son copulation in Japanese macaques. However, the methods employed by Itoigawa et al. were explicitly designed to promote mother-son copulation; even so, incestuous copulations were rare and behaviorally atypical. Under severe

sociosexual deprivation, sexual disinterest (including incest avoidance) apparently can be overcome. Leavitt asked for examples of "the environmental factors that produce incestuous behavior" (p. 974); here is one.

Erickson's handling of Sugiyama and Koman's work on chimpanzees is also criticized: "What Erickson fails to report is that Sugiyama and Koman . . . also observed ten cases of mother-son copulation" (Leavitt, p. 979). Leavitt fails to report that all ten cases involved two infant males (Sugiyama and Koman 1979). This hardly sounds like sexual aversion, but mother-son nonreproductive sexual behavior is still consistent with a sociobiological framework. If nonhuman primates can be allowed psyches, the psychological aspects of such behavior should prove a fascinating area for study.

Most troubling in his review of primate literature is his failure to mention Pusey's work on chimpanzees (Pusey 1980), which was discussed by all of Leavitt's "target authors" writing post-1980. Furthermore, in our data review (cited by Leavitt), Ali and I explicitly state that "chimpanzees fit the inbreeding-avoidance model [for dispersal] precisely," in part because among chimpanzees "behavioral incest avoidance is well developed" (1984:104), as shown by Pusey's presentation of clear evidence for the behavioral avoidance of sibling, mother-son, and (potential) father-daughter incest. More recently, Goodall describes the few cases of mother-adult son copulation observed at Gombe National Park: in two of three such interactions in which intromission was observed, the "mother protested violently, screamed, [and] pulled away prior to ejaculation" (Goodall 1986:466-467).

Leavitt concludes his summary of the ethological data with a list of 16 studies "(largely ignored by human sociobiology) that report close inbreeding" (pp. 979-980, emphasis added). Here is what one of them, an analysis of the genetic consequences of song dialect recognition in sparrows, really says:

The relationship among the calculated F -statistics [measures of inbreeding] is that the total inbreeding coefficient of an average individual within a dialect (F_{IT}) is primarily a result of the population being subdivided into relatively isolated dialects (F_{ST}) and is not at all due to consanguineous mating patterns within the dialect (F_{IS}). *In fact, the average F_{IS} is negative which indicates that close inbreeding is actually avoided.* [Baker 1982:567, emphasis added]

In others of the 16, close inbreeding is merely inferred (e.g., R. Smith 1979) or

clearly results from lack of choice (wolves isolated on Isle Royale cited by Livingstone [1980]; captive rhesus macaques in a pen [D. Smith 1982]). Lack of options for an island-bound group is certainly an environmental factor, and behavioral mechanisms for incest avoidance are not expected to be strong in animals that (for demographic reasons) rarely encounter opportunities for incest (Moore and Ali 1984).

Many studies (some of which appear in Leavitt's list) have found evidence of inbreeding in animal populations, and these have led to the concept of optimal inbreeding (Shields 1982) or optimal outbreeding (Bateson 1983). Bateson's discussion should be of interest to anthropologists: he demonstrates a mating preference for unfamiliar first cousins among Japanese quail, and argues on biological grounds that cousins might be preferred mates in other taxa as well (see Hoogland 1992 and Moore 1993 for recent review and data).

The field of human sociobiology certainly has its problems (see commentaries following Thornhill 1991), but Leavitt's article does little to help solve them.

Notes

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Sociobiology and Incest Avoidance: A Critical Look at a Critical Review Critique

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Contrary to Moore's criticism, in my article I state (p. 971) that "to entirely separate theoretical arguments from the evidence cited to support them is not possible." I do review every article written by human sociobiologists on the subject of incest avoidance between 1975 and 1989. If there are articles I missed, I wish Moore would have cited them. (In Note 1 [p. 984] I do distinguish *inbreeding* from *incest*.)

The fact that human sociobiology is inferior to the sociobiology of nonhuman animals is widely recognized (Kitcher 1985:14-15, 156-157). If this is what Moore is saying, then I would essentially agree. Furthermore, I state (p. 976) that I am only scrutinizing the ethological evidence cited by the human sociobiologists in question, and note that the research reviewed may be dated and unrepresentative. I also include (p. 980) studies that *do* report avoidance behaviors.

If by "genetic influence" Moore is claiming that incest or inbreeding is avoided via the capacities of the human organism (i.e., there is a genetic inheritance allowing particular muscle movements), then I have no problem with his statement. But if Moore is saying that there is a specific (yet unidentified) biological mechanism, naturally selected to move the organism out of the way when inbreeding or incest is possible, then I do have a problem.

Human sociobiologists speculate considerably about biologically instructed behaviors,