didn't instead involve "time" as an equally compelling explanation.

Food production wasn't *invented* in most places. That it arose "at nearly the same time" refers to geological time (marked in this case by transition from Pleistocene to Holocene climates, and the culmination of two million years of cultural development). In fact, agriculture began at different times (over a span of some 5,000 years) and spread at greatly differing rates across the world. By Graber's reasoning, one might have expected that an ecological "shock wave," prompted by population growth, would have emerged (and agriculture with it) first in Africa, where hominid populations had experienced a much longer reproductive history than elsewhere.

We did not explain agricultural origins globally by invoking climate change, but we have little doubt that the latter altered vegetation, seasonality, and water resources. The effects of climate change in each region depend on latitude, topography, native vegetation, and the technology and organization of the local people; thus, we gain little understanding of a particular historical event by simply noting that climate was important. By identifying variables important in the Near East, we suggest that others may explore the importance of these variables in their regions, some of which still have only sketchy data. (The global climate models we used emerged within the past decade, as did many ecological studies in Mediterranean ecosystems.) Unlike Graber ("When *will* there be enough case studies?"), we prefer not to generalize from a sample of one. We will know when we have a few more with the detail we now enjoy for the Near East. Such case studies (with excellent data) are available from North America, where population stress has been ruled out and some archeologists even implicate social factors (see Watson 1991). Global theories, too, deserve testing, and parsimony is not the only test.

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## A Critique of Leavitt's Review of Sociobiological Explanations of Incest Avoidance

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In his article, "Sociobiological Explanations of Incest Avoidance: A Critical Review of Evidential Claims" (*AA* 92:971–993, 1990), Gregory C. Leavitt criticizes the position of "human sociobiology" on human incest avoidance. In what follows I will argue that Leavitt's criticisms are ineffective.

Sociobiological analyses of incest can be roughly divided into two areas of study. One is the micro-level, in which attempts are made to substantiate various aspects of the Westermarck hypothesis, namely that when two individuals are brought up together in an unrestricted physical proximity, the two will be sexually uninterested in one another at puberty. This is said to be the outcome of natural selection (Westermarck 1921:chs. 17–20; Shepher 1983; Van den Berghe 1983). The second is the macro-level, in which sociobiologists try to explain social practices and cultural prohibitions and taboos as an outcome of natural selection (e.g., Lumsden and Wilson 1980a, 1980b, 1981, 1982). Although related, the two are distinct issues.

Leavitt's analysis of the latter aspect (pp. 972–974) overlooks major sources (e.g., Lumsden and Wilson, mentioned above) and the issues they raise. He advances various examples to discredit the sociobiological assumption that incest avoidance is shaped by natural selection and is genetically predisposed. These examples, however, are of questionable utility. The example of the Arapesh does not involve close inbreeding (defined here as $r \geqslant .25$, $r$ being the proportion of genes shared between two individuals by common descent), which is a crucial aspect of the sociobiological understanding of incest. In the Iranian case, the question of how widespread the practice was is far from resolved, and it may well have been rare and confined to a very small group (Hopkins 1980:354, n. 89; Middleton 1962:609, n. 45). The Roman Egyptian case is indeed one with which sociobiology has difficulty since it involves a socially sanctioned, widely practiced, full sibling marriage, marriage that involved an erotic and romantic aspect and was more than just symbolic. However, the Iranian and Roman Egyptian cases may be viewed by sociobiologists as exceptions that do not invalidate the general ethnographic reality.

Leavitt disagrees with the claim that close inbreeding necessarily has negative consequences for individuals. He argues that recurrent inbreeding over generations would serve to reduce the genetic load through increased exposure of deleterious recessives, and that in cases of reduced genetic load (taken here to mean the average number of lethal and sublethal equivalents per individual in a popula-

tion) the selective pressure would be negligible. Yet, empirical studies have failed to confirm that recurrent inbreeding does lead to a reduced genetic load (Bittles 1980:775–776; Bittles et al. 1987). As for the second part of Leavitt's genetic argument—namely, that in cases of a low genetic load selective pressures would be negligible—as long as the genetic load is greater than zero (i.e., invariably) there will be a selective pressure toward outbreeding in the form of inbreeding depression.[1] Thus, even if cleansing of the gene pool did take place, selective pressure against close inbreeding would not cease to exist. (This pressure may, nonetheless, be contravened by other selective pressures.)

Further, Leavitt relies on Slater (1959) and takes up the hypothesis that demographic factors prevented incest among early hominids. However, Busch and Gundlach (1977) have shown this hypothesis to be based on faulty modeling.

Leavitt is right in claiming that the fact that other species also avoid close inbreeding does not in itself prove the existence of identical selective pressures in humans (p. 980). But considered with the phenomenon of inbreeding depression, such data can be said to lend strong support to a possible selective pressure for avoidance of close inbreeding.

Leavitt argues that other ultimate factors may result in organisms avoiding close inbreeding, and that in some species the proximate mechanisms behind the avoidance of close inbreeding are other than innate mechanisms dedicated specifically to that goal (pp. 976–977, and Arthur Wolf's suggested proximate causes, described on p. 982). The fact that other ultimate factors may be involved does not undermine the claim that such an avoidance is of great survival value. Nor does advancing possible proximate mechanisms invalidate the sociobiological claim. The only thing the sociobiological argument requires in this context is that whatever the proximate mechanism, it should be genetically biased, that is, the chances of its occurrence should be, however indirectly and however slightly, genetically affected. Otherwise it could not be selected for by natural selection. This requirement may prove to be the crucial test of the sociobiological view of incest avoidance. Leavitt, however, does not take the sociobiologists to task on this issue.

Shepher's study of peer groups in the kibbutzim in Israel (1971) is fundamental to the human sociobiological evidential claim. Leavitt seems to have misunderstood Shepher's description of the ontogenetic process that leads to incest avoidance. Shepher never claimed that the "genetic barrier to sexual activity among those raised together only manifests itself in the first six years of life" (pp. 980–981); quite the opposite. The claim that heterosexual activity is absent clearly relates to pubescent and adult peers who had been brought up together, not to prepubescent peers. The intimate heterosexual activity in the first six years is, according to Shepher, the precondition for the appearance of sex barriers about eight years later (1971:300). Other studies of the kibbutzim that Leavitt cites in order to discredit Shepher's observations fall short of shaking Shepher's grounds. Talmon's study (1964) was a functionalist interpretation of marital practices, not an observational study of sexual attitudes and practices. Kaffman does see the rarity of enduring romantic relationship among co-socialized kibbutzniks as remarkable (Kaffman 1977:216). He differs from Shepher in that he argued that in his experience such relationships are rare, though they do occur, whereas Shepher argued that in his kibbutz they did not occur at all.

As for the studies of minor marriages in Taiwan, Leavitt notes that Wolf mentions some "compelling sociocultural reasons for why minor marriages are less successful" (p. 982). This point is curious, as Wolf himself in his articles (1966, 1968, 1970) examines them and rules them out as alternative explanations to his own. Leavitt gives no reason why we should not accept Wolf's refutation.

Leavitt argues that the instance of the Arapesh contradicts Shepher's model, since the Arapesh "have a customary marriage practice where a young girl goes to live with her future husband and is raised as his sister, until the couple is old enough to marry" (p. 983). But as Robin Fox pointed out (1962:143–144; 1980:40–42), Mead describes "A marked muting of the sex relationship in marriage amounting to an unwillingness to copulate. The sex act itself has to be slow and unexcited; no climax is aimed at" (Fox 1980:42). If anything, this case may be added to minor marriages in Taiwan and peer-group avoidance in kibbutzim in support of the sociobiological position (cf. Fox 1980:46, 48–51).

To sum up, Leavitt's arguments do not adequately challenge sociobiological evidential claims or theoretical views of human incest avoidance.

## Notes

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[1]Inbreeding is defined as "the crossing of closely related plants or animals," and inbreeding depression as "decreased vigor in terms of growth, survival, or fertility following one or more generations of inbreeding" (King 1972:148).

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## Inbreeding Fitness: A Reply to Uhlmann

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In 1963, Edward O. Wilson published a paper on rare ant species with small effective population sizes. Because of "the danger of irreversible reduction of genetic variability due to inbreeding" (1963:250), Wilson expected that natural selection would provide mechanisms to encourage outbreeding. As Wilson (1975:80) was to later note, "inbreeding lowers individual fitness and imperils group survival by the depression of performance and the loss of genetic adaptability."

Instead of mechanisms that promoted outbreeding, Wilson found species characteristics that encouraged close inbreeding (e.g., wing-