

natural environments. His method involves a very brief systematic observation of a behavior (questionnaire responses) unique to our species, making species comparison difficult. Nevertheless, cross-species analysis of a sort could be achieved by comparing, wherever possible, humans' *statements* regarding their behavior with the actual behavior of other species. No systematic effort was made to do this. No attempt to relate the questionnaire responses to reproductive success was made.

Buss cites several of these limitations and is most cautious in his conclusions. It seems to me that he has generated several meaningful hypotheses about the role of natural selection in human mating strategies which could be tested using the historical/comparative method. Comparison requires finding in-frahuman equivalents of such concepts as "good financial prospect."

There are a number of other points worth mentioning:

1. The American results reported by Buss and Barnes (1986) demonstrate that both sexes find other characteristics more important than the sex differences Buss cites in his study. "Kind and understanding" was a clear preference for *both* sexes. (Who benefits from such a partner?) Women preferred eight characteristics, including "physically attractive" and "healthy," over "good earning capacity," and males preferred three traits over "physically attractive." One can only speculate how these various characteristics might interact in real life to produce mate preference and how various constellations are related to reproductive success.

2. Comparative analyses clearly support the male's preference for mature (proven) female mates, and the female's preference for supportive (kind and understanding?) and/or strong and dominant males (Anderson 1986; Berenstain & Wade 1983; Goodall 1986; Hrdy 1981; de Waal 1982).

3. Perhaps establishing the statistical significance of sex differences in the professed preference for each characteristic in a list is an improper use of an excellent tool. Buss points out what many seem to forget: There is a wide range of individual differences and marked overlap between the sexes. If we add the observations made in the first point above and the probability that we respond to *available* potential mates as intact phenotypes rather than as a composite of a list of characteristics, the statistical significance of sex differences in preferences for "good looks" and "ambition," for example, may be largely irrelevant if not misleading predictors of reproductive success. Mayr's remarks about mathematics and "intentions" (1982, pp. 39-42, 51) and about traits (1983, p. 327) may increase our ability to cope with these issues.

4. In our eagerness to find differences in behavior that we could correlate with reproductive success, we have greatly oversimplified the magnitude of sex differences. Hrdy (1981) has demonstrated that females compete; she concluded that it was quite naive to think that natural selection would not apply to them as well as to males. Buss now points to evidence that selective pressures are not the sole province of females. Unfortunately, there will be those who fail to recognize that "good looks" in a mate are very important to many women (see also Cunningham 1988; Freedman 1979) and that "industriousness" in a potential wife is crucial to many males.

• Typology and human mating preferences

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Buss is interested in testing hypotheses about the evolutionary basis for mate choice in humans. Most recent comparative animal and human studies in evolutionary biology have focused on between-group differences and tests of predicted relationships among variable traits. Buss has focused on similarity between groups, testing the hypothesis that there are species-

typical patterns of mate preference. He suggests that the evolutionary process has been sufficiently consistent over time to produce a pattern of choice that is, at least partly, culturally universal. Traits such as male preference for beautiful mates are predicted to be consistent across cultures, because historically these traits have been consistently associated with reproductive success. Buss then attempts to test this hypothesis with the prediction that he will find constant patterns of sex-related differences between mating preferences in different cultures.

Buss's analysis has several important difficulties. First, he implies a typology for "evolutionary" hypotheses about social behavior that does not exist. This results because his predictions assume certain facts about human societies that may not always be true. For example, the prediction of an asymmetry in the importance of preferences based on earning potential and ambition-industriousness assumes greater male access to resources. Although male domination of resources may occur in many societies, it is not always present, and the "evolutionary" prediction should not apply to these exceptions. Thus, predictions for particular behavioral traits, such as mating preference, are often complex and depend on the particular pattern of social arrangements existing in a society. Buss's typological analysis does not allow for these exceptions within the framework of an evolutionary hypothesis.

A common but incorrect criticism of evolutionary studies of human behavior is that they are typological in exactly this sense. Exceptions to general patterns are cited as refutations of the entire approach. The correct answer is that this is not true, because evolutionary biologists recognize that expected patterns often depend on special social contexts. Buss fails to give this important point its due.

Second, the analysis as presented is flawed because the author doesn't use a valid sample. Of the 32 societies in his comparisons, 27 are European or have had a predominantly European influence. Aboriginal cultures are so scarce in the sample that the geographical category "Oceania" is represented only by the transplanted British cultures in Australia and New Zealand. Long ago, anthropologists recognized the role of cultural transfer in creating between-group similarity in social traits. Murdock and many more recent workers have gone to great lengths to exclude the effects of cultural transfer as a cause of similarity. Buss's use of a sample with an overwhelmingly European bias and no significant variation in cultural traits that are likely to be associated with different patterns of mate preference (e.g., with variation in mating system, residence, or inheritance patterns) does not present a reasonable challenge to the hypotheses he claims to test. Buss claims as evidence for the independence of his sample the fact that in one variable, the preference for female chastity, there was some variation in response among the groups he sampled and that this differs from the patterns seen for other variables. This is not a compelling argument, however. There are many potential causes for variation in a sample apart from statistical independence. For example, the variance in the sampling practices he used could certainly account for the variation in preference for chastity. Moreover, even for this variable, most males in the social groups he sampled showed the same preference. Thus, Buss has failed to do what is necessary in this type of comparison: offer convincing evidence that the observed similarity in cross-cultural patterns of mate preference is due to convergent evolution and not to cultural transfer.

Third, there is an important inconsistency between the conclusions of this study and the history of the societies used in these tests. The majority of societies in Buss's sample are drawn from industrialized countries in which there has been a dramatic change in key reproductive characteristics (commonly referred to as the demographic transition), including changes in fecundity and age of first reproduction. The occurrence of the demographic transition contradicts Buss's suggestion that basic human reproductive patterns are invariant across cultures. It suggests that the consistency in patterns of preference found by

Buss is due to the homogeneity of his sample, rather than to the constancy of female reproductive patterns.

Buss claims that "this is the first study to examine human mate preferences across cultures on a broad scale." This statement ignores a large body of cross-cultural comparisons on mating preferences collected by anthropologists and evolutionary biologists (e.g. Flinn 1981). Perhaps if Buss were more familiar with that work, he could have avoided the obvious errors in his own analysis.

Mechanisms matter: The difference between sociobiology and evolutionary psychology

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I share Buss's objective of an integration between psychology¹ and evolution, but the synthesis cannot be – nor need it be – grounded in sociobiology. Any proposed solution to the long-standing impasse between advocates of "mostly cultural" versus "mostly biological" explanations for behavior will inevitably be evaluated by the traditional standards of the social and behavioral sciences for methodological rigor and explanatory validity.

Sociobiologists are likely to judge Buss's research to be a strong demonstration of sex differences. From a fairly simple model founded on genetic self-interest as the ultimate causal factor in behavior, he has derived a set of hypotheses about human preferences. The predictions are supported across 37 samples. The proximate mechanisms are unknown, but his findings of cross-cultural universality argue compellingly for biological factors and provide the grounds for inferring the differences in constraints faced by males and females during their evolutionary history.

Psychologists will find the target article problematic, largely because the traditional "rules of the game," from hypotheses to conclusions, are contravened to no apparent purpose. The evolutionary hypotheses do not describe some heretofore unnoticed but significant behavior, nor are they contrasted with nonevolutionary hypotheses. The same predictions could have been made from a random sample of newspaper advertisements, magazines, or soap operas. At the very least, the evolutionary framework could have been used heuristically to suggest novel comparisons. For example, is there a relationship between women's preferences for men who are good financial prospects and the level of state-supported maternity and child-care benefits? Nevertheless, this heuristic use would still leave open serious doubts as to the ultimate disconfirmability of the sociobiological framework (Kitcher 1985).

The data analysis also deviates from generally accepted research practice in psychology. It exploits the well-known fact that very large sample sizes produce statistically significant results even when the differences between groups are very small. Without an analysis of the magnitude or power of the effects, we have no way of distinguishing an important result from a trivial, albeit statistically significant, one. Furthermore, most psychologists will have serious reservations about the appropriateness of using a large number of *t*-tests.

What Buss is saying has evolved is also a problem. In most of his target article, he suggests he is studying preferences, which are psychological mechanisms. But toward the end, he says his work yields no information about proximate mechanisms – which may be preferences, genetic differences, socialization effects, or even "structural effects at a societal level such as those that limit female access to economic resources." But how can structural effects at a societal level be accommodated in a framework based on biological selection for heritable traits? Without a clue to the mechanism, there is no way to distinguish

evolved sex differences from the multitude of gender differences that have no basis in biological evolutionary processes.

For example, consider an alternative account of the relative preferences males and females show for financial prospects in a mate. Both sexes may want the same financial resources, but because women are systematically denied independent access to them, we may conclude that (a) women select the most practical remaining option – marriage to men who have the resources, and (b) men do not use irrelevant criteria for their mate preferences. This explanation suggests that there are no evolved sex differences: Males and females have identical preferences, but social structural arrangements produce gender differences. By itself, evolutionary theory cannot explain why the social structural arrangements exist, nor do the social structural arrangements reveal anything about evolutionary history (cf. Buss & Barnes 1986). An understanding of proximate mechanisms cannot be postponed, as Buss suggests, to a later date; we must understand the mechanism *before* we can determine whether it has an ultimate cause at all.

The emphasis on mechanism is a key distinction between sociobiology and what could be dubbed *evolutionary psychology*.² At least in the human case, there are two requirements for an evolutionary argument. The first is an account of the origins of the hypothesized mechanism given the species morphology and ecology in which the mechanism is presumed to evolve; the second is an account of the systematic engagement of the mechanism under a variety of social conditions. An evolutionary psychology would draw more from a functionalist paleoanthropology (Foley 1987) than from evolutionary theory *per se*. In contrast to the sociobiological strategy, which tends to provide evolutionary accounts of contemporary phenomena, research questions in evolutionary psychology would be developed from engineering design considerations. Given the specifications of morphology and ecology, what are the minimal functional requirements for reproduction and development to reproductive age? What possible mechanisms might we expect to evolve under those conditions? How would these mechanisms behave under changing historical, structural, and cultural conditions, such as during the shift from living in small groups to high residential density, from kinship loyalty to state loyalty, and from rare interactions with strangers to daily interactions in large numbers? What research designs allow us to choose among a variety of possible evolved mechanisms (which would allow us to elaborate the evolutionary scenario)? What research designs allow us to eliminate alternative explanations or show how the purportedly evolved mechanism interacts with historical and contemporary social structures? I suspect that the mechanisms that we conclude have evolved will be relatively "low-level" – that is, they will have to do with such things as attentional biases, maturational sequences, or rules for evaluating and integrating different categories of information. An evolutionary psychology will be most successful when it makes novel predictions, organizes previously inexplicable research results, and suggests novel comparisons.

In the final analysis, the results reported by Buss are too compelling. Consider a thought experiment. Suppose that the pattern of results were reversed; for example, that men were found to prefer women who were good financial prospects and that women were found to prefer men who were young and attractive. Most observers would probably conclude that the measuring instrument was seriously flawed. What then is the point of scientific investigation if only one pattern of results can be accepted as valid? Science has helped demythologize some purported biological sex differences (Maccoby & Jacklin 1974), but it has also facilitated the social construction of other differences supposedly based on biological imperatives (Harding & O'Barr 1987; Pfafflin 1984. [See also McGlone: "Sex Differences in Human Brain Asymmetry's *BBS* 3(2) 1980.] In a society that feels a stronger obligation to correct inequities arising from imperfections in the social system than to correct those resulting from differences in natural endowments (Lambert 1987), the