

In any event, much of the research and speculation in evolutionary psychology revolves around sexual and reproductive strategies, in large part because much of human evolutionary psychology does in fact revolve around sexual and reproductive strategies (Gandolfi et al. 2002). On occasion, however, I have wondered whether the thrust of such efforts reflect the genuine, evolved predispositions of *Homo sapiens*, as opposed to the living conditions currently experienced by the great majority of practicing, publishing scientists. Granted that the projection of genes into the future is what natural selection is all about, and that reproduction (defined more inclusively to embrace assistance toward kin) is the means of achieving this end, it is also true that survival is typically a prerequisite for sexual selection, parenting, and so forth. Given the strong likelihood that during most of our evolutionary prehistory mortality factors were omnipresent, it seems equally likely that human nature has long been concerned with basic survival (resource accrual, predator avoidance, temperature regulation, suitable response to and avoidance of pathogens, etc.), at least as much as with reproduction *per se*. Although there may well be room for sex differences in survival selection, these promise to be less dramatic than sex differences in sexual selection, but no less important.

Because evolutionary psychologists and sociobiologists lead privileged lives (for the most part in affluent Western societies, in which food, shelter, and adequate medical are available, as well as a reasonable probability that researchers will not themselves be seriously menaced by predators), they are able to take survival pretty much for granted and focus their research energy on “sexier” topics, notably sex and reproduction. This in turn has led me to question whether evolutionary psychologists should focus more on those presumed mental modules – possibly including sex differences – that contribute to survival and perhaps less on sex and reproduction itself.

In this regard, once more Schmitt’s research is, if not conclusive, at least reassuring. His massive cross-cultural sample, which includes data from many developing countries, suggests that – as most of us have long intuited – sex is important, and so are sex differences, and not only for those in the affluent West. An important extension of the present study would therefore involve surveys of less privileged people in developing countries, among whom sheer survival cannot be taken for granted.

As evolutionary thinking matures, analysis of human mating patterns has been making headway in numerous disciplines within which it had previously been lacking. Thanks to the work of Schmitt and others, it seems likely that we are on the brink of a true multidisciplinary understanding of human sexuality, and not a moment too soon.

## Sociosexual strategies in tribes and nations

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**Abstract:** Extending the findings of this work: Tribal peoples need study. Monogamy as marital institution and monogamy as sociosexual orientation must be separated. Sociosexuality must be considered as an aspect of somatic as well as reproductive effort; third-party interventions in sociosexuality need attention; and multiple sociosexual orientations, with frequency-dependent fitness payoffs equal at equilibrium, need to be modeled.

The interesting and important work reported in the target article is a necessary step toward an evolutionary understanding of human mating. That my comments are directed largely at its limitations and their implications for further research should not be taken as deprecation of this essential research but as an attempt to locate its results in their wider anthropological context.

**Limitations of the sample.** As Schmitt is clearly aware, a sample of people from modern states (the great majority of which enforce monogamy as their only legal form of marriage) does not represent the full range of human mating systems. As Schmitt notes, it is an anthropological commonplace that in most tribal societies polygyny is considered the preferred form of marriage, even if it is usually achieved only by a favored minority of successful men. As he further notes, a sample of college-age people, who are mainly at the beginning of their sexual careers and largely unmarried, does not necessarily represent the opinions and behaviors of even the same people a few years down the road. Less obvious is the problem that any survey of this sort is necessarily restricted to people who are literate and comfortable with the idea of grading behaviors and opinions on a numerical scale: The instrument inherently eliminates the tribal people, who represent 95% of human history, the time in which in which our modern sociosexuality evolved. The need for an alternate instrument that can be administered to nonliterate peoples who may not be able to count beyond 2 or 3 is clear. The problem of developing such an instrument, and calibrating it to surveys such as the current one, is immense.

**Limitations of the descriptive apparatus.** Schmitt appears generally to use the word *monogamy* to mean a sexually exclusive arrangement between a single man and a single woman. Sometimes, he uses monogamy to mean a marriage between a single man and a single woman. It is important to note that the two uses are distinct, and one cannot take the presence of the latter as evidence of the former. It is fairly common in tribal societies (no one knows how common, because the subject is underinvestigated and underreported, for obvious reasons of ethnographer discretion) that a married man’s brothers have legitimate sexual access to his wife. In many societies, men classified as brothers include parallel cousins (e.g., father’s brothers’ sons and mother’s sisters’ sons), as well as children of the same mother and father. It also happens that a man may have legitimate sexual access to his wife’s sisters, whether or not they are married to other men. The lending of a wife to a visitor, even one who is not close kin to the husband, is also common in some tribal societies. In a number of tribes, married women accept socially sanctioned, long-term lovers. Finally, there are a number of societies with ceremonies or other regular occasions for sexual license. All of these practices are compatible with monogamy as a system of marriage. The distribution of monogamy as a marital institution tells us little about whether sexual attentions are restricted or unrestricted. The terminological confusion of mating system with marital institution is a recurring problem in discussions of the evolution of human mating. Eventually, there may have to be some sort of nomenclatural convention.

**Limitations of the theories.** The theories evaluated here are significant attempts to deal with the evolution and current manifestations of human mating strategies. However, they simplify the natural history of these strategies in at least three important ways. First, all of them except that of Eagly and Wood (1999) see human sexual behavior simply as reproductive effort, the imperative of finding mates and producing offspring who will themselves reach reproductive status. However, since the advent, very early in human history, of the sexual division of labor and food sharing, sexual behavior has also been, particularly for females, an aspect of somatic effort, of the basic need to get enough food and other resources to stay alive. Put simply, in virtually all tribal societies, making a sexual connection (usually marriage) with a man or men is an indispensable part of the way a woman makes a living, irrespective of her reproductive interests. In the substantial number of societies in which a man cannot survive without the foods or services a woman supplies, the same is true for males. Although a mate is not a fundamental survival necessity for any of the college students surveyed by Schmitt’s collaborators, one cannot ignore the occupational and other economic advantages that can be obtained by a successful mating strategy in the modern nation state.

Second, in focusing on the individual’s own sexual attitudes and

behaviors, the theories give short shrift to a peculiar human trait – third-party policing of other people's sexual behavior. From incest taboos to prescribed and arranged marriages to the rape or exile or execution of people who violate sexual rules, human beings have a uniquely complicated social environment in which to behave sexually. A complete theory of human sexual behavior needs to explore and account for this extraordinary species-typical elaboration of the social context. What, for example, is the role of parental pressure in sociosexuality, as parental interests respond to such externals as sex ratio, resource levels, and infant mortality? Even if parents attend to exactly the same cues as their offspring, *their* reproductive interests (as manifested largely in the number and survival of the grandchildren produced by all their children) will rarely correspond exactly to those of an individual child. There are major parent–offspring conflicts to be explored here, not only by administering the same instruments to both parents and children but also by asking parents to answer on behalf of their children.

Finally, the possibility of strategic pluralism in sociosexuality, as suggested by Gangestad and Simpson (2000), needs to be addressed in the context of plural alternatives within a single society. There is no a priori reason that one sociosexual orientation should be the single best adapted strategy for a given sociocultural context. On the contrary, particularly in large, complex societies, one might expect several successful alternative sociosexual strategies, probably with frequency dependent fitness payoffs.

## Who's zooming who?

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**Abstract:** Men and women report having significantly different numbers of sexual partners, which is impossible in a large sample. Schmitt's target article is no exception. This focuses discussion on the nature of the samples, their heterogeneity, and the locale they are drawn from. Further, we query how humans determine, for example, sex ratio, in the context of large numbers.

Schmitt and his many colleagues have provided us with an article that is rich both in terms of data and in the application of those data to test a number of theories. This is a monumental endeavour that will provide a source of debate for years to come. However, as with all monumental studies, there are weaknesses that need examination. I focus on the sampling and how it links into the claims made with respect to responses on the Sociosexual Orientation Inventory (SOI).

A number of authors, most notably Dorothy Eimon, have pointed out that there are often major discrepancies between the number of sexual partners claimed by men and women (Eimon 1994; Walsh 1993). The problem is, given the nature of sexual activity, these claims, although they may not be identical, should be relatively close. Despite this obvious fact, almost every study reports that men claim to have had more sexual partners than women. The present study is no exception. Men in every country claim that they have had or will have more sexual partners than do women. Of course, one would not expect these small samples to match up perfectly, but given that the sum must approach equality as the sample size increases, one would expect women in some countries to report that they have had or will have more partners than men.

Eimon makes the point that this difference might be the result of the relative difference in prostitution. There are more female prostitutes serving males than vice versa. However, her studies show quite clearly that this is not the case, and that the most likely explanation is that men are exaggerating and women are being coy. The truth lies somewhere in the middle.

This is important because it suggests that we need to look care-

fully at the samples that were employed to generate the data in the Schmitt article. To be fair, Schmitt notes some of these weaknesses. However, these weaknesses could have a profound effect on the outcomes that he observed and the conclusions he drew.

If Eimon is correct, then clearly men and women will not differ dramatically in terms of their mean number of sexual partners. There will be some variation, given the differences in sex ratio, as illustrated in Figure 1 of the target article, but these are small in comparison with the claims made. Unfortunately, the samples employed are unlikely to pick up outliers such as women who are working as prostitutes. Clearly, if women who are working as prostitutes make up the differences that are reported here and in other studies, and if such women are included in such studies, then we would expect to see considerable differences in the variability of reported sexual activity. Men are likely to be much more homogenous and women more heterogeneous in terms of number of sexual partners. What would be of interest is how these differences in variability are expressed as preferences. Do women who work as prostitutes have similar preferences to women who do not work as prostitutes, thereby preserving the differences in the SOI reported here?

We can take the issue of sampling one step further. The above focuses on differences between men and women. However, we should not assume that samples taken from different countries are necessarily homogenous, as is implied in the Schmitt article. Australia is a multicultural society that contains numerous religious and ethnic groupings, all of whom are likely to differ on the SOI. Therefore, it is important to know exactly where the sample was taken to determine the extent to which it is likely to be representative of the nation as a whole. Even large cities such as Sydney and Adelaide differ dramatically in their religious and ethnic makeup. What is true of Sydney would not necessarily be true of Adelaide and vice versa.

The locale of the sample raises the question of how people are able to gauge some of the posited causal factors that influence the SOI. For example, Schmitt notes that certain areas of the United States are likely to have significant imbalances in the number of men versus women because of likelihood that the former are incarcerated. It is easy to understand how such a local imbalance could affect behaviour. However, it is difficult to see how the marginal differences in sex ratio reflected in Figure 1 could affect behaviour. Schmitt and others assume that all men and all women will form a long-lasting partnership. Thus, like musical chairs, the absence of a partner will become obvious. This has never been the case, and it is certainly not the case at present, which leaves open the questions of how people know that there are differences in the number of men and women available as partners, and whether they alter their behaviour accordingly.

In summary, Schmitt has provided us with much food for thought. He provides us with answers to some questions and poses many more. Nevertheless, in examining the data produced, we must be mindful of the weaknesses inherent in the sampling. The jury must remain out until more evidence is provided.

## Sex differences in the design features of socially contingent mating adaptations

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**Abstract:** Schmitt's study provides strong support for sexual strategies theory (Buss & Schmitt 1993) – that men and women *both* have evolved a complex menu of mating strategies, selectively deployed depending on personal, social, and ecological contexts. It also simultaneously refutes social structural theories founded on the core premise that women and men are sexually monomorphic in their psychology of human mating. Further progress depends on identifying evolved psychological design features