

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