

some signs that the authors might be aware of the deficiencies, but they do not suggest how to overcome them.

Fluctuating asymmetry (FA) is a marker of genetic fitness, not only for the eyes of the researcher but also for the eyes of women looking for a mate. If so, FA is also, as the target article demonstrates, an important, signal of genetic quality; hence males have a strong incentive to fake this signal. It is difficult to see why accordingly, G&S do not apply the first twelve sentences in section 3.2. which are on honest signaling, to the body of observations regarding FA.

One of the strongest evidence that Fa is more a signal than a marker, is its exclusive relevance for males. FA may be a weaker marker of genetic fitness in females, because the variance in male's reproductive success (RS) is greater, but it cannot be no marker at all. Should pathogen resistance and developmental precision not be a good predictor of female RS as well, likewise because FA is heritable? Do all the genes responsible for FA sit on the Y chromosome? Or, if we hypothesize genomic imprinting, why should evolution select the inactivation of genes favoring pathogen resistance and developmental precision?

It is also difficult to see how the factors leading to low FA should be direct causal factors for a body odor which pre-ovulatory women like (sect. 4.5.2). Moreover, of the three sexually dimorphic traits – body mass, physicality, and social dominance – which together account for 70% of the total FA effect of lifetime number of partners, neither body mass nor physicality can be direct effects of FA (sect. 4.4).

Still, given all the observations collected in G&S's target article, FA might prove one of the most important signals of a male's genetic quality for females making their choice. But then it has to be shown that it is an honest signal, and, if so, its precision as the signal has to be determined. For that, some internal male quality Q has to be identified which is (1) not directly (or only after long acquaintance) observable to a mate; (2) costly to fake; (3) heritable; (4) causal for FA. Then, it has to be established that Q is a determinant of RS. Next, it has to be demonstrated that a low degree of FA is more costly to produce the lower a male's Q. Since women do not need low FA for a high RS, evolution may also concede males some freedom about which level of FA to choose. These are the conditions for a signal's honesty to be evolutionarily stable, as Zahavi (1977) and Grafen (1990) have shown theoretically, and as Moeller (1987) has empirically demonstrated for the size of the chest spot being an honest signal of male sparrow's dominance, or as Mueller and Mazur (1997) have demonstrated for facial dominance being an honest signal of a man's dominance – the first such demonstration in humans.

I am sure that G&S know best what the good candidates would be for Q. They consequently owe us a rigorous demonstration that FA is an honest signal Q. That would greatly increase the value of all the valuable material on FA presented in their target article.

## The limits imposed by culture: Are symmetry preferences evidence of a recent reproductive strategy or a common primate inheritance?

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**Abstract:** Women's preference for symmetrical men need not have evolved as part of a good gene sexual selection (GGSS) reproductive strategy employed during recent human evolutionary history. It may be a remnant of the reproductive strategy of a perhaps promiscuous species which existed prior to the divergence of the human line from that of the bonobo and chimp.

When do Gangestad & Simpson (G&S) believe that the evolution of good genes versus good provider reproductive strategies oc-

curred? Is it likely that a good gene sexual selection (GGSS) reproductive strategy was important in our recent evolutionary past or should we be looking elsewhere? Not every evolved human characteristic is the result of selective pressures operating during the environment of evolutionary adaptedness (EEA).

For modern humans, the ability to acquire culture, the desire to enculturate children, the compulsion to behave according to cultural norms, and the tendency to reject individuals who behave "abnormally" are among the strongest and most important structural elements in what G&S refer to as "the psychological architecture that guides social interactions." Human psychological architecture undoubtedly has other design features but they must operate in conjunction with the powerful instinct to be cultured. Culture has a particularly profound influence on reproductive behaviour. The reproductive strategies used by its members are of great interest to everyone who belongs to a culture and the only strategies available to members are those which the culture recognises. This means that any hypothesis about the selective advantage of a particular strategy has to be plausible within the context of the particular culture likely to have been in force when selection was taking place.

The influence of culture does not mean that human behaviour remains the same when environments suddenly change. Humans do not slavishly continue to follow cultural norms when it is unnecessary or disadvantageous to do so. But neither do individuals respond to environmental change by consulting their evolved psychological mechanisms, inventing novel behaviours, and pursuing them on their own. Humans change their behavioural options at the population level by making adjustments to the cultural norms which already exist. This places limits on the kinds of new behaviours that become available when environments change. For example, men in the modern western "global" culture have the option of behaving polygamously by having a series of sex partners or having several sex partners at once, but their potential for polygamous behaviour is held within strict limits by cultural norms. They cannot, for example, purchase a harem of young teenage girls from the Philippines, even though the market price might well be within the reach of many of them.

Human behaviour strategies can be partly explained by looking at how they allow individuals to respond to variations in the physical and biological environment. To gain a fuller understanding, it is necessary to look at the cultural environment. The observations of female reproductive behaviour cited by G&S were mostly of women living in the United States in the 1980s and 1990s. What if ancestors of G&S had measured the Sociosexual Orientation Inventory (SOI) scores for the grandmothers of these women, living in the United States of the 1930s and 1940s? If they had managed to avoid being arrested for indecency, they would undoubtedly have found these women far less willing than their granddaughters to engage in sex without closeness or commitment. These women may well have preferred the look and smell of symmetrical men but their culture did not allow them to act on these preferences.

The changes that have taken place in women's behaviour over the last two generations have not been a response to changes in the physical or biological environment. They are owing to changes in the cultural environment. Modern women are surrounded by an essentially infinite number of available men. Having short-term sexual partners is condoned, even encouraged. We possess the technology for avoiding pregnancy and sexually transmitted diseases. In America today, a single woman can thrive and bring up her children alone. In all of human history, when have women had such an ideal environment for exercising a preference for symmetrical men? And only if a preference is exercised can it confer an advantage and be selected.

But this conclusion poses the question when in human prehistory could a preference for symmetrical men have conferred a selective advantage? Humans are social animals and, like other patrilocal social primates, they tend to associate in male kin bonded groups, though with the important difference that each man's clos-

est companion is a woman (Bailey & Aunger 1990). The sparseness of food resources kept population densities quite low prior to the development of agriculture. A group of about 500 might be recognised by its members as sharing a language or culture, but individuals would have spent most of their time in groups of between 10 and 30 or even smaller single family groups (Foley 1996). Such a small group would provide women with few opportunities for the kind of sexual shopping-around necessary to take advantage of GGSS and, since the men available are likely to be brothers, the genetic choice would be even more limited.

The existence of a weak preference for symmetrical men in modern women does not necessarily indicate that the use of short-term and extra-pair mating tactics has been a routine occurrence in recent evolutionary history. This preference could be a very old adaptation, the remnant of the GGSS reproductive strategy of a perhaps promiscuous species which existed prior to the divergence of the human line from that of the bonobo and chimp. As the hominid line evolved, infants became harder to raise and offspring were more likely to survive if their father made a contribution to their care. The more reproductively successful females would have been those who could ignore their preference for symmetry and mate enthusiastically with men willing and able to provide them and their offspring with resources. In this way, natural selection would have weakened the preference for symmetrical men but, as long as it was not strong enough to inhibit females from mating with slightly asymmetric good providers, there would be no selective pressure to eliminate it completely.

## Idealized human mating strategies versus social complexity

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**Abstract:** Gangestad & Simpson present an idealized model of human mate strategies based on rational economics and genetics that elides most social constraints on human sexuality. They do not deal with observable complexities of courtship nor with ambiguities in short- and long-term mating. The model successfully explicates a narrow set of premises, but cannot yet explain complex sexual behavior.

The greatest strength of Gangestad & Simpson's (G&S's) target article is its analysis of connections between long- and short-term mating strategies and sexual selection for good genes and/or for resources. Furthermore, they make the excellent point that choosing between the two strategies depends on complex contingencies of genetics and economics. However, on closer study, their argument becomes less persuasive.

G&S present a highly abstract, strongly idealized model of mate choice strategies that strips away a great many details. Indeed, the organisms that make mate choices in the model are so abstract and so idealized that they barely seem human. G&S discuss mating strategies as if enacted by completely rational economic and genetic monads devoid of all society and history, and who lack all religion, institutions, traditions, and laws concerning mate choice and its social consequences. No kinship systems, symbols, or customs influence or control mating. Instead, mate choices are analyzed as if isolated individuals made purely rational individual decisions. We doubt if such monadic purity of purpose has ever existed during human history or evolution.

This lack of social context for mate choice creates a serious definitional problem concerning the widely-used notions of short- and long-term mating. In actuality, the two concepts depend on a complex and modern moral and symbolic framework for assigning social value and meaning to sexual encounters after the fact. For example, Joe and Sally are each drawn to the other on a first date for reasons they themselves may label as "short-term" sexual in-

terest: he is symmetric, sexually available, and willing to wine and dine her; she is young, clear-skinned, lustrous-haired, and pretty. They have great sex – and decide to do it again. A year later, they are still together. Were they then enacting short- or long-term strategies? Had they stopped seeing each other after one date, they themselves would post facto speak of a "short-term" relationship, but that label becomes inappropriate if they remain together.

If one studies human courtship using ethnographic and ethological field observations (see Givens 1978; 1983; Moore 1985; 1995; 1998; Moore & Butler 1989; Perper 1985; 1989; 1994; Perper & Weis 1987), one sees why the couple cannot themselves determine if a relationship will prove to be short- or long-term. Initially, neither person fully knows what they themselves want or what the other person might offer – such knowledge emerges only as their interaction itself creates their feelings for each other. Such floating emotionality is characteristic of courtship, so that uncertainties are intrinsic, and one cannot predict the future of a love affair (Perper 1999). Accordingly, most relationships begin with "strategic ambiguity," where it is not clear what either person wants or expects. It is therefore not obvious what strategy each is actually enacting except in 20-20 hindsight.

The expressions "short-term" and "long-term" are also deeply problematic as evolutionary concepts. Consider a band of some 100 proto-humans 300,000 ybp, where we use an imaginary time machine to observe one female for a month or so. She mates nine times with one male, five times with another, and twice with yet another. Which strategy is she using for which male? We might say that the two matings with the last male represent the short-term strategy, but a year later, we still find her mating twice a month or so with that male. Infrequent, yes; short-term, no. The labels "short-term" and "long-term" do not unequivocally characterize her sexual behavior.

Even if, in modern times, we symbolically identify two polar types of sexual encounter – conveniently, if opaquely, labeled short- and long-term – their evolutionary origin is not elucidated solely by good genes and resources. For example, a married woman might obtain resources from her husband and yet maintain a lengthy secondary sexual relationship with a man who does not give her money but provides much appreciated love and emotional support. Are Gangestad & Simpson arguing that the "strategies" underlying her life-choices are only matters of resources versus good genes? The actual complexity of mate choices and sexual behavior does not easily fit into G&S's idealized analysis of dichotomized mating strategies. Nor do the complex spiritual/sexual lifestyles recently compiled by Francoeur et al. (1999). Probably few social scientists would accept the view that women choose men solely as providers of resources or solely for their genes.

The natural and social sciences have produced two general answers for how to study complex phenomena. One, illustrated by this paper, is to build theory from a few – and only a few – explicit principles that justify narrow but extensively analyzed conclusions. This approach is validly reductionist, first in seeing strength in theory developed from a few premises, and second in depending on biological and genetic principles deemed logically and causally prior to the complex phenomena themselves. The other approach can be called "grounded," because it starts with the phenomena and tries to identify generalities and principles whether or not these match the premises of reductionist theory. One of us (Perper) has used this approach for studying courtship. Grounded theory necessarily makes simplifications as it develops, whereas reductionist theory tends towards more complexity. Ideally, they will meet in the middle, but in the meantime it is worth pondering what is lost through the reductionist hope of identifying a few sovereign principles that regulate human sexual behavior.

Yet we cannot really fault Gangestad & Simpson for their narrow focus. Their paper is a serious effort to explore the implications of a small set of ideas, rather than to prepare a synoptic view of the evolution of human mating. If G&S's presentation seems abstract and idealized, or lacks the rich substance of human real-