

Book Review

A Biased, Incomplete Perspective on the Evolution of Human Mating Systems

A review of Alan F. Dixson, *Sexual Selection and the Origins of Human Mating Systems*. Oxford University Press: New York, 2009, 232 pp., US\$70.00, ISBN 978-0-19-955943-5 (paperback).

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Alan F. Dixson's *Sexual Selection and the Origins of Human Mating Systems* presents an incomplete and biased review of the evidence relevant to the evolution of human mating systems. Despite a pretense of drawing on information from multiple disciplines, Dixson largely ignores research conducted within evolutionary psychology as it pertains to human mating. Instead, he focuses almost exclusively on comparative physiology and anatomy in primates to build a case that human evolutionary history is characterized by monogamy and polygamy, rather than the multi-male/multi-female mating system—and associated intense sperm competition—of chimpanzees, for example. This is a well-established conclusion, however. Humans, as a species, tend towards monogamy and polygamy, rather than multi-male/multi-female mating systems, and Dixson builds a strong case for this long-known conclusion.

A key problem is that Dixson's conclusion does not allow for *degrees* of sperm competition. The frequent refrain throughout the book is that sperm competition played no selective role in the evolution of human mating systems. To use Dixson's own words (although he directed them at evolutionary psychologists), this conclusion regarding the role of sperm competition is "a case of wishful thinking." The conclusion that sperm competition played no role in the evolution of human mating strategies biases the way Dixson views research and presents data throughout the book. Here we address several of the most telling examples of bias: the construction of a false mating system dichotomy, the discussion of female orgasm, and the charges of academic xenophobia against those who disagree with Dixson's conclusion that sperm competition played no selective role in human evolutionary history.

Construction of a False Mating System Dichotomy

When presenting data, Dixson fashions dichotomous relationships between certain physiological traits and mating systems. The mating systems of the species are labeled either as

monogamous/polygamous or as multi-male/multi-female. Then Dixson shows how, in many but not in all cases, anatomical and physiological data collected from humans fits the monogamous/polygamous group better than the multi-male/multi-female group. This is a false dichotomy. Humans, chimps, and gorillas share a common ancestor and display different mating systems that include varying levels of sperm competition. These mating systems must have evolved from the ancestral mating system of that common ancestor. Unless mating system evolution was made in a single leap from no sperm competition to intense sperm competition, there must have been intermediate mating systems (e.g., social monogamy, with mild sperm competition) that ancestral humans might have practiced.

In his efforts to construct and justify this false mating system dichotomy, Dixson attempts to fit the data to his conclusion that sperm competition played no selective role in the evolution of human mating systems. Any evidence that does not directly support his conclusion is ignored or downplayed in importance. For example, Dixson states that “human testes sizes are unexceptional and consistent with an evolutionary history which involved pair formation or polygyny as the principal mating system” (p. 35). Although monogamy and polygyny are the principal human mating systems, that does not exclude the possibility of varying levels of sperm competition in human mating systems. As a proportion of body weight, human testes are more than twice as large as those of gorillas, suggesting some degree of sperm competition (in comparison, human testes are about one-third the size of chimpanzees; Smith, 1984). Dixson downplays this difference between humans and gorillas perhaps so that humans can be more plausibly lumped with gorillas into the monogamy/polygyny side of the mating system dichotomy. Dixson also notes that human penis length more closely approximates the penis length of males in multi-male/multi-female systems than in single-partner mating systems, but he does not grant this finding any significance, perhaps because it does not fit simply with his conclusion (p. 67).

Dixson also downplays the significance of human non-paternity data, perhaps because these data are not consistent with the mating system dichotomy he favors. He claims, “The data are *not sufficient* to justify generalizations about the occurrence, or importance, of sperm competition in human reproduction” (p. 37, italics added). Despite this description of the data, Dixson states in the sentence preceding the quoted sentence that non-paternity rates are *very low* (a subjective judgment and generalization). How high non-paternity rates must be for Dixson to consider them significant is not specified. The numerous and substantial reproductive and social costs of non-paternity for cuckolded men (see Shackelford, Pound, and Goetz, 2005) are likewise not mentioned. Without considering the associated costs, the rate of non-paternity is not a particularly meaningful metric for determining its significance. Even relatively infrequent events can act as a powerful selective pressure if they occur with some predictability and carry large fitness costs.

Of course, sperm competition need not always result in non-paternity, and neither is sperm competition required for non-paternity. As such, non-paternity is a crude estimate of the occurrence of sperm competition. The frequency of extra-pair copulations (EPCs) may provide a more accurate estimate of the intensity of sperm competition. Dixson reports that surveys of the United Kingdom, France, Australia, and the United States indicate that 5% to about 52% of women self-report EPCs. Due to the sensitive nature of the subject, these reports should be considered conservative estimates of the occurrence of EPCs. Nevertheless, these estimates indicate the potential for sperm competition to occur. Attempting to downplay or invalidate the data, Dixson claims without citing relevant evidence that, “Events in modern-day New York or

Paris probably bear little or no relation to patterns of extra pair copulations and paternity in remote ancestral populations” (p. 36). No reasoning is provided for this speculation.

Largely absent from Dixson’s account of the evolution of human mating systems is a consideration of empirical and theoretical work on human psychological adaptations that would make little sense without reference to an evolutionary history of sperm competition (see, e.g., Shackelford and Pound, 2006). This conspicuous omission from Dixson’s analyses may be attributable to his efforts to retain the false mating system dichotomy he favors, which does not allow for variable levels of sperm competition. Because sperm competition is not as intense a selective factor in human mating systems as it is in multi-male/multi-female mating systems, Dixson reaches the conclusion that it must not have played a role in human evolutionary history. He allows this conclusion to bias the way he examines and presents data.

Female Orgasm

Dixson’s discussion of female orgasm provides a fine example of the bias that distorts his assessment of the origins of human mating systems. He argues against the sperm retention hypothesis presented by Smith (1984) and elaborated by Baker and Bellis (1993b)—that female orgasm may facilitate retention of sperm from particular males. Because Dixson has already concluded that sperm competition has not played a role in human evolutionary history, he views the sperm retention hypothesis with blinding skepticism. Dixson instead favors the hypothesis that female orgasm is a non-functional byproduct of male orgasm. In the course of discussion, Dixson cites Wallen and Lloyd (2008) in support of this byproduct hypothesis. A closer investigation of Wallen and Lloyd is important, as it reveals the relaxed standards of evidence that Dixson extends to work that supports his conclusion.

Wallen and Lloyd’s (2008) arguments betray a misunderstanding of the sperm retention hypothesis. They claim the *primary* evidence consistent with the byproduct hypothesis is that female orgasm occurs less reliably than male orgasm. A key problem with this argument is that if female orgasm facilitates sperm retention, then female orgasms that reliably and consistently occurred during sexual intercourse would facilitate equally the retention of sperm from all partners. This would give no selective advantage to the sperm of any specific male. However, if female orgasm occurred more frequently with partners of high quality, for example, cryptic female choice could favor the gametes of higher-quality men. Selective orgasm would have been ancestrally adaptive in such situations, whereas consistent orgasm would be less effective in supporting cryptic female choice.

Dixson presents several examples of the selective occurrence of female orgasm throughout the book. Unfortunately, Dixson’s bias does not allow him to place these findings in their proper contexts. For example, Dixson notes that lower-ranking female Japanese macaques more frequently display orgasm when copulating with higher-status males than when copulating with lower-status males. Although this finding is consistent with the sperm retention hypothesis, allowing the female to favor sperm from a higher-quality male, Dixson fails to attribute to this finding any significance with regard to the sperm retention hypothesis. Another such finding is that human females report a higher proportion of copulatory orgasms with more symmetrical partners (Thornhill *et al.*, 1995). Such a finding makes good sense in light of the potential for female orgasm to favor the sperm of higher-quality men. Dixson further notes that “...the best documented examples of female orgasmic responses occur in species with multi-male/multi-female mating systems” (p. 78). If female orgasm affected the outcome of sperm competition by

facilitating retention of the sperm from a particular male, multi-male/multi-female mating systems would grant female orgasm the most power in cryptic female choice.

After presenting the initial misunderstanding regarding the evidence of selective orgasm, Wallen and Lloyd (2008) claim that the human clitoris displays substantial phenotypic variability in length. This variability suggests to Wallen and Lloyd that the clitoris was not under selective pressure, implying a lack of function for female orgasm. This argument was dismantled by Lynch (2008), who noted that clitoral length is unrelated to the ability of females to achieve orgasm. Further, Lynch questions the validity of using length as the sole measure of clitoral variability. When considering clitoral and penile volume, rather than just length, Lynch documents that standardized variation in clitoral and penile volume is not significantly different. Using Wallen and Lloyd's logic, this suggests that the human clitoris has been subjected to selective pressures similar to the selective pressures on the human penis.

Dixson's willingness to accept without question the results and arguments presented by Wallen and Lloyd (2008)—and without acknowledging the several problems with the Wallen and Lloyd work highlighted in subsequent publications (e.g., Lynch, 2008)—provides an example of the relaxed evidentiary standards applied to research that favors his conclusions. Dixson does not similarly relax his skepticism when considering the work of Baker and Bellis (1993a, 1993b, 1995)—which does not support his conclusions—quoting Lloyd (2005), who claims that Baker and Bellis's test of the sperm retention hypothesis for female orgasm “fails to meet basic scientific standards of evidence” (p.76). Dixson's conclusion regarding the occurrence of sperm competition biases what he is willing to consider and as accept as evidence.

Charges of Academic Xenophobia

Dixson repeatedly chastises evolutionary psychologists (typically without citing particular studies) for speculation. His charges are reminiscent of the logically unsound charges of “just-so stories” leveled against evolutionary psychology in the past, which claimed that evolutionary psychology was mere speculation that did not generate testable hypotheses. Dixson's charge of speculation is odd, however, given that intense speculation is replete throughout his book. Dixson speculates on many topics, including: the evolution of the white sclera of the human eye (p. 13), the costs outweighing the benefits of female extra-pair copulation (pp. 122, 184), the causes of testes size differences across human populations (pp. 37-38), and the sufficiency of the evidence in favor of dual-mating strategies in females (p. 184). The charge of speculation is disingenuous given the frequency with which Dixson engages in speculation.

The final chapter of the book presents several of the most telling examples of Dixson's bias. Dixson repeats most of the prior claims made throughout the book, but here many new claims are suddenly introduced and not given proper attention. One case is the hypothesis of prudent sperm allocation in humans. Dixson notes that sperm count in many species, humans included, varies as a function of the sociosexual context. Dixson notes that “it is interesting that sperm numbers and sperm motility are significantly greater in ejaculates collected by condoms as a result of copulation [in humans, as contrasted with ejaculates obtained through masturbation]” (p. 53). As an alternative explanation to prudent sperm allocation that does not invoke sperm competition, Dixson proposes that sperm count in ejaculates varies as a function of the degree of sexual arousal. What Dixson does not address is *why* certain contexts might heighten arousal relative to other contexts and *why* heightened arousal should produce a greater sperm count. He mentions that specific contexts that may increase sexual arousal could include “the presence of a

highly attractive female, or the sight of other conspecifics mating” (p. 175). Claiming that the sight of conspecifics mating could increase male sexual arousal without an appeal to the potential for sperm competition is a strange claim indeed (for research on the effect of viewing episodes of sperm competition, see Kilgallon and Simmons, 2005; Pound, 2002).

The last section of the final chapter includes charges against evolutionary psychologists of scientific fraud and disciplinary xenophobia. Dixson accuses evolutionary psychologists of excluding outside sources of information to protect their collective academic interests. What the collective interests of evolutionary psychologists are, how they are protected by disregarding data, and why they are protected are not addressed. The charge of xenophobic bias—like Dixson’s charge of speculation—rings hollow, given Dixson’s demonstrated bias in examining and presenting data.

Dixson accuses evolutionary psychologists of presenting “misinterpretations of false and limited evidence” and “highly questionable findings” that are “deeply troubling in the scientific sense” (p. 185). Dixson does not explain how or why these findings (or even which specific findings) are troubling, questionable, or, more importantly, false. He simply lists 10 hypotheses and associated empirical findings without addressing most of them *at any point* in the book. What is deeply troubling in the scientific sense is the bias Dixson demonstrates when examining and presenting data. What is also troubling is the dismissive attitude Dixson displays towards evidence that does not support his conclusions, while he simultaneously credulously considers evidence that does support his conclusions without subjecting that work to the same critical analysis.

Although Dixson’s book does offer a reasonable overview of several primate mating systems and of comparative anatomy and physiology, it leaves much to be desired. Its tone is unnecessarily and unjustifiably hostile toward evolutionary psychologists and the research they have conducted. The presentation and examination of data suffers from deep bias. As a result, many interesting findings are downplayed in importance or ignored altogether, whereas the significance of methodologically questionable work that presents results consistent with his conclusions is showcased. A misleading and false mating system dichotomy is constructed that includes no effect of sperm competition and a massive effect of sperm competition. Dixson’s book offers an incomplete and biased picture of the evolution of human mating systems. Readers interested specifically in the evolution of human mating will find more complete and less biased treatments in David Buss’s (2003) *The Evolution of Desire* and Randy Thornhill and Steven Gangestad’s (2009) *The Evolutionary Biology of Human Female Sexuality*.

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