

# The Case of Female Orgasm: Bias in the Science of Evolution

Elisabeth A. Lloyd

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This study received a great deal of media attention after its release in April, 2005. *The New York Times*, *The Manchester Guardian*, *Lancet*, *Nature*, *New Scientist*, *Slate*, and all of Australia's leading newspapers covered it. The author, who is Tanis Professor of the History and Philosophy of Science and Professor of Biology at the University of Indiana, was interviewed by Barbara Walters and by the ABC's Natasha Mitchell on *All in the Mind*. Interest was provoked partly by Lloyd's emphasis on the well-known fact that about 25% of women seldom or never experience vaginal orgasm during intercourse, whereas the frequency of orgasm by clitoral stimulation is much higher — on the order of 90%. Why is this? One influential answer is that women who fail to achieve vaginal orgasm suffer some dysfunction. Not necessarily so according to Lloyd. That there are two orgasmic loci, and that the frequency of orgasm distributes in the ratios just stated, she argues is the consequence of the developmental basis of male and female genitalia. This complex story, briefly stated, is that the clitoris is a developmental 'byproduct' (or homology) of the penis. The homologous neural organization of the clitoris and penis explains why both are easily stimulated to orgasm. There's nothing new here: embryologists have known the facts since about 1820. Donald Symons introduced the idea, and the 'byproduct' designation, into the evolution literature in his *The Evolution of Human Sexuality* (1979). Lloyd's contribution begins here, with the question: Is the

female orgasm an adaptation, selected because of its contribution to reproductive advantage? Or is it, like the male nipple, fitness neutral, as Symons believes? That it is the latter would seem to be indicated by the fact that orgasm is not part of the insemination process. Yet this 'seeming' is challenged by empirically supported hypotheses purporting to establish connections between female orgasm and reproductive fitness. Lloyd examines each of these hypotheses (20 by her count) and concludes that none satisfies the evidential criteria of proof of adaptation. The negative result does not mean that the question is closed. On the contrary, Lloyd proposes new investigations to test the byproduct account against rival adaptive accounts.

To argue this position is to oppose a key element in the mainstream of evolutionary studies over the past several decades, especially the strong consensus supporting the sperm competition hypothesis. But there is further provocation, signalled by Lloyd's subtitle. Weak evidence, as she assesses it to be, would not have enjoyed such strong uptake were it not supported by bias. A major bias is that female orgasm is, or ought to be, like the male orgasm. When it is not, the absence is styled 'dysfunctional'. The bias figures prominently in male expectations of their marital partners, who conciliate their men by prosocial deceptions such as faked orgasm. But according to Lloyd, the bias is also incorporated by researchers of both genders. Ouch! This has provoked angry responses, notably a dismissive

review by David Barash in the online journal, *Human Nature Review*. Barash levelled a counter-charge: Lloyd embodies the antiadaptationist bias of her mentor, S. J. Gould (to whom this book is dedicated), which is in odium among Neo-Darwinians. To this she adds, Barash believes, a feminist bias so damaging to her scholarship that he derided her knowledge of the relevant biology and ostentatiously declared that the only aspect of the book that he could commend was the bibliography. Nonplussed, Lloyd composed a detailed, point-for-point response that is now part of the literature and requires consideration in assessing her case.

The book is organized into eight chapters: Introduction, The Basics of Female Orgasm, Pair-Bond Accounts of Female Orgasm, Further Evolutionary Accounts of Female Orgasm, The Byproduct Account, Warring Approaches to Adaptation, Sperm-Competition Accounts, and Bias. The concluding chapter is an exercise in Lloyd's forte, philosophy of science. Her own 'bias', if I may so express it, is a decided preference for objectivity, and in the Bias chapter she explains why the hypotheses she examines fall so short of accepted standards of scientific evidence; and why this faulty science is so widely received with little or no criticism.

This is an ambitious enterprise, in character with the philosopher's high aspirations but perhaps not so congenial to the more mundane habits of the bench scientist. We are asked to examine with equanimity criticisms of our extensively

researched views and to concur that the female orgasm is a not an adaptation. Perhaps a personal reference is in order. My commitment was to the ethological pair bond, which is unique among primates. I was aware that the interpretation is 'androcentric' (it presupposes male dominance) and that the diversity of marriage customs, human sexual adventures and preferences render the ethological interpretation at best an approximation. Lloyd's well marshalled evidence reinforced that conviction by displaying more loose ends than I had imagined. I am persuaded that the female orgasm is probably a developmental byproduct and that the pair bond interpretation should not be promoted as if it were established science. But I remain convinced that the pair bond interpretation is an important heuristic for understanding the gender compromises and tensions in contemporary society, and indispensable for conceptualizing the now threatening conflict between the decidedly androcentric Islamic law and our gender-neutral social values.

The central conflict featured is the clash between the byproduct and the sperm competition hypotheses. Lloyd pays close attention to the first high profile exposure of the conflict. It was instigated by S. J. Gould in a 1987 *Natural History* column that promoted the Lloyd-Symons theory. The 'ardent adaptationist' (self-designation) John Alcock wrote a sharp rebuttal, which stirred others to enter the fray. Lloyd devotes a chapter (Warring Approaches to Adaptation) to this suite of exchanges because it exemplifies current ambiguities and differences of opinion about what constitutes an adaptation, and the evidence needed to prove it. If a trait can be shown to promote differential reproductive success under current conditions, may it be inferred that it is currently selected? Should longitudinal studies be pursued to support a selection inference? Assuming evidence for current selection, can it be inferred that the

trait was historically selected by the same differential reproductive success as happens now? Might a trait that originated as a byproduct nevertheless become a selected trait in a changed environment? Is the byproduct account to be rejected in principle because it closes off the search for adaptive explanations; or can a byproduct account generate tests for that account? Lloyd's detailed exposition of these and other questions is a treat for those who wish to acquaint themselves with the variety and complexity of current adaptationist thinking. It also lays bare the evidential assumptions of proponents of the sperm competition hypothesis.

Lloyd devotes 11 pages to what is probably the most influential argument for sperm competition in the human species, the two article suite by Robin Baker and Mark Bellis in *Animal Behavior* (1993). By that time the sperm competition literature was extensive, even though the concept was only introduced in 1984. Baker and Bellis hoped to establish that 'nocturnal, masturbatory and copulatory orgasms are the primary mechanisms by which the female influences the ability of sperm in the next and/or current ejaculate to remain in, and travel through, her reproductive tract. We predict that by altering the occurrence, sequence and timing of the different types of orgasm, the female can influence both the probability of conceptions in monandrous situations and the outcomes of sperm competition in polyandrous situations' (p. 199). The resolution of the adaptive lens to this level of detail was what sperm competition research on lower taxa was about; but to achieve it for the diversity of human sexuality was very ambitious. While Lloyd commends the goal, her analysis of the execution of the project demolishes the purported findings in their entirety. The demolition begins with criticism of selection bias of the survey that Baker and Bellis assembled. She then scrutinizes the data and argument for the sperm upsuck hypothesis and concludes that they

have established no relation between upsuck and orgasm (p. 204). Finally, Lloyd identifies numerous flaws in the statistical analysis of data, and concludes that 'the problems with their skewed samples and sample size, their handpicking of subsamples, and their use of statistical tests requiring a normal distribution call every one of their conclusions into serious doubt' (p. 208). Thus, an exposition that enjoys consensus level acceptance is dismissed as bad science.

Let me turn now to her treatment of another model study, the 1995 Thornhill, Gangestad and Comer article meant to show that female orgasm is a response to male symmetry, which is in turn a marker of genetic quality. Since orgasm correlates with sperm retention and hence fertility (as per Baker and Bellis), orgasm is an adaptation that promotes female fitness in an environment of matings with multiple partners and hence sperm competition (p. 211). Thornhill and colleagues established a cohort of 86 heterosexual couples to whom they administered a questionnaire to ascertain rates of copulation, orgasm, the timing of orgasm and other information. Lloyd finds faults with their evaluation of the questionnaire that undermines their conclusions. Thus, while the study found a correlation between symmetry and orgasm rate, there was no correlation of sperm retention with symmetry, although that is the crucial connection. Furthermore, since the study did not include ex-pair copulations, there was no test of sperm competition. Lloyd also objects that the hypothesis, if proved, would apply to the 50% of women for whom the incidence of orgasm is variable. There is accordingly no proof of association of orgasm with fertility.

The final chapter sets out the biases that control the study of female orgasm. One is that orgasm is linked to female reproductive success. Another is that only orgasms occurring during coitus are relevant. Another is that sexual

response in nonhuman primates is determined by hormones and that nonhuman primates do not experience orgasm (Lloyd covers this subject in detail). Altogether there are eight assumptions that she believes to have been shown to be categorically false by evidence that researchers ignore or contradict. This is followed by a philosophical review of adaptationism, especially its strong version ('ardent adaptation'). In his critique mentioned

above, Barash lustily identified himself as an ardent adaptationist. Lloyd's no-compromise demolition is likely to stir even lacklustre adaptationists to the spirit of defence. Such readers may wish to consult the Barash-Lloyd exchange.

Most Neo-Darwinians consider the Gould-Lewontin criticism of adaptationism to be thoroughly rebutted and decidedly stale. I modestly suggest, however, that

Lloyd is an independent voice who warrants careful attention.

### Note

David Barash's review, 'Let a thousand orgasms bloom!' and Lloyd's response can be found at <http://mypage.iu.edu/%7Eealloyd/BarashReview.htm>

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## Multiple Pregnancy: Epidemiology, Gestation, and Perinatal Outcome

Editors: Isaac Blickstein and Louis G. Keith

(2005). Taylor and Francis (Abingdon), 2nd ed., 943 pp, US\$169.95, ISBN 1-84214-239-9.

This book aims to provide a comprehensive, international view of all aspects of multiple pregnancy and births in a way relevant to a wide range of readers. Such an ambitious challenge could easily fail to be met, but the text is imbued by the enthusiasm and knowledge of an international, multidisciplinary cast of authors under the editorship of two authors who have spent both a professional lifetime, and in the case of one of the authors who is a twin, a personal lifetime fascinated by all aspects of multiples. This is a truly international text, written by leading academics, practitioners and parents all driven by a love of the topic. It is a truly remarkable achievement. The book is almost unrelated to the first edition which was written before the explosion in the numbers of multiple births occasioned by artificial reproductive technology (ART) and the concurrent major improvements in fetal assessment and management.

Just who is the readership intended to be and are you one of them? The range of topics covered is very comprehensive. With 110 chapters written by over 80 authors one might expect some duplication and lack of clarity. The chapters are arranged in 11 sections which provide a clear and logical structure and each chapter is relatively short and self-contained and provides a clear view of any particular topic. The editorial process has been kept reasonably tight so that there is a uniform feel and style to the book. The index is well constructed and helps in this process of finding information, possibly in multiple sections. Where there are multiple chapters on the one topic, which particularly occurs in section 1 on epidemiology, it is clear that the topic is being approached from different parts of the world and the reader has a choice of selecting information most relevant to their interests. Geographic diversity has been celebrated by the

presentation of short chapters rather than attempting to synthesize an international view into a single long chapter. This is a strength of the book.

The book brings together disparate data sources. An international league table showing differences in the mean number of embryos transferred (range 1.9 to 3.46) emphasizes data presented in individual chapters. Superb text and photographs on placentation, examination and special cases strip away much of the mystery the novice clinician is faced with. These chapters are complemented with a later section on fetal management of clinical conditions related to placentation. Only occasionally are there annoyances which occur only when the book is read from cover to cover. Chapter 28 and 32 duplicated much of Chapter 15. Why is Chapter 34 'Trends in malformation' not with the basic epidemiology section earlier in the book? The text is up to date with references. Examples are the inclusion of the 2003 combined