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## Critical Notice

ELISABETH A. LLOYD, *The Case of the Female Orgasm: Bias in the Science of Evolution*. Cambridge, MA and London, UK: Harvard University Press 2005.

### I Introduction

In *The Case of the Female Orgasm: Bias in the Science of Evolution*, Elisabeth Lloyd carefully examines evolutionary accounts of female orgasm advanced over the past four decades — a total of 21. All save one of these treat female orgasm as an evolutionary adaptation that has arisen as a result of natural selection. The single nonadaptive account is the developmental explanation of female orgasm put forward by Donald Symons in his 1979 book *The Evolution of Human Sexuality*. Lloyd argues that the evidence is decisive in favour of Symons' explanation and that all 20 of the competing adaptive hypotheses should have been rejected by scientists, whether for faulty logic or lack of empirical support. She concludes that the case of the female orgasm is 'patently a story of scientific dysfunction' (2005a, 229), and that the failure to arrive at the rational outcome in support of Symons' account is due to the operation of 'pernicious' biases that have led researchers to overlook empirical data that fail to support, and even contradict, the adaptive hypotheses they advance. Lloyd describes her book as 'a case study on how biases and background assumptions can affect the practice of science' (18).

In this project, Lloyd joins with other philosophers and science studies scholars in paying closer attention to scientific practice and the intersections of science and society than has been traditional in the philosophy of science. Feminist theorists have been especially influential

in this regard. *The Case of the Female Orgasm* engages with feminism in several ways — with the wider social significance of scientific accounts of female orgasm for women, with feminist biologists who have been critical of Symons' account and/or defended adaptive explanations of female orgasm, and with feminist philosophers of science concerned with the epistemology of theory choice. Lloyd correctly notes that facts about the evolutionary origins of female orgasm come to figure in cultural understandings of sexuality, even if fallaciously, and that recognizing female orgasm as a nonadaptive rather than an adaptive trait in no way diminishes its importance, and is even potentially liberating. However, Lloyd wrongly surmises that feminist biologists critical of Symons have succumbed to the naturalistic fallacy (by assuming that only adaptive traits can be important) and allowed their politics to get the better of their science. Inasmuch as Lloyd's own theoretical presuppositions in deciding the case of the female orgasm in Symons' favour are open to reasonable challenge, feminist biologists need not be pulled just yet from the case. I argue that Lloyd's expressed allegiance to the contextual empiricism defended by feminist philosophers of science is undermined in two ways: more seriously, by retaining the traditional distinction between good and bad science based in separating what is empirical and logical from what is social and political; and less seriously, by appropriately telling this 'story of scientific dysfunction' as a failure of objectivity at the community rather than individual level, but without sufficient sociological analysis to do so convincingly.

## II Female Orgasm and Feminist Biology

An evolutionary adaptation is a trait that has arisen as a result of natural selection operating in ancestral environments — whether experienced by that particular species (e.g. *Homo sapiens*) or earlier members of the lineage (e.g. hominids, primates, etc.). Scientists have hypothesized a range of possible modes of selection that could be responsible for the origin of female orgasm, and Lloyd covers these in detail. Pair-bond accounts hold that orgasm has encouraged females to maintain pairing relationships, with the pair bond, in turn, adaptive for a variety of possible reasons: promoting male cooperation in hunting by sharing females as mates, satisfying increased female need for male protection and economic support in the move from forest to savannah, facilitating the rearing of offspring, increasing frequency of copulation once estrus was lost, or providing a means of facilitating female choice by assessing male contribution to the emotional quality of the relationship. Nonpair-bond accounts have also been proposed: orgasm could encourage females to remain horizontal post-coitus which would increase likelihood

of fertilization; orgasm could facilitate abortion in environments where this is adaptive; orgasm could contribute to an intermittent reward system for females that encourages sexual intercourse; or orgasm could provide nonpair-bond means of female choice through use of orgasmic contractions to facilitate male orgasm or to aid in the transportation of sperm through the cervical canal to the uterus. Lloyd reports that support by scientists has currently coalesced around this last mechanism of female choice, advanced by Robin Baker and Mark Bellis (1993), and known as the 'upsuck' hypothesis.

Symons denies that female orgasm has arisen as an adaptation in any of these ways. He offers a developmental explanation that treats female orgasm and male nipples as analogous. There are strong selection pressures for male orgasm and female nipples: we can easily see that males who have difficulty ejaculating will tend to be less successful at reproducing than those who do not, and females who have difficulty breastfeeding will have offspring less likely to survive than those who do not — at least in some environments. In contrast, Symons claims, female orgasm and male nipples convey no fitness benefits and did not arise due to natural selection. Instead, they are the byproduct of natural selection in the opposite sex, the happy accident of the developmental fact of their origin in bipotential structures common to both sexes. This is because sexual differentiation occurs in such a way that certain features start off as undifferentiated in genetic males and females until, as a result of hormonal influences that begin in the womb and continue through adolescence, they develop into those traits characteristic of either sex. For example, during fetal development, the genetic tubercle will develop into a clitoris or penis, and the urogenital swellings will develop into the vaginal labia or scrotum.

Lloyd pronounces Symons' nonadaptive account the clear winner over its adaptive rivals given the empirical data. '[A]ll the sexology literature,' she writes, says that, unlike men, women fail to experience orgasm with intercourse because of the physiological mechanics involved (225). Sex differences in ease and time to orgasm disappear with masturbation. Lloyd refers to this as the orgasm/intercourse discrepancy, and faults various adaptive explanations for ignoring it. The observed variability in orgasmic potential among women is also important. On Lloyd's analysis, this cuts both ways. Some adaptive explanations ignore this variability altogether. Other adaptive explanations, by accommodating the orgasm/intercourse discrepancy and variability in orgasmic potential, fail to account for those women for whom this experience is not variable, because they either never or always experience orgasm. But in either case, existing variability undercuts adaptive explanations and favours the developmental account: 'The high degree of variability within women of the ease and frequency with which they experience

orgasm under any circumstances is enough to make the point that female orgasm does not look like a typical adaptation' (146).<sup>1</sup>

Lloyd identifies the operation of four kinds of bias that have led scientists to ignore evidence lent by such facts and reject Symons' developmental account in favour of less well supported adaptive explanations of female orgasm: adaptationism, androcentrism, procreative focus, and human uniqueness. These various biases can be blamed for a number of unwarranted assumptions made by researchers: that orgasm occurs only with intercourse, that (think Bill Clinton) sex is intercourse, that sex is heterosexual, that only human females have sex outside of estrus, that female and male sexual responses are alike, etc. In *The Case of the Female Orgasm*, Lloyd is primarily concerned with adaptationist and androcentric biases. For example, the hypothesis that orgasm encourages females to remain horizontal post-coitus to increase the likelihood of fertilization receives support from the androcentric assumption that women like men experience post-orgasmic sleepiness, whereas there is evidence that women remain in an aroused and wakeful state after orgasm. Most recently, because of adaptationist commitments, scientists have wrongly embraced an evolutionary account of female orgasm based on statistically unacceptable studies (by Baker and Bellis) despite the availability of an empirically validated alternative (that of Symons).

Certainly, the major audience Lloyd targets is the scientists who are themselves pursuing evolutionary explanations of female orgasm. However, Lloyd insists that this research does not exist in a vacuum but carries social implications; consequently, this 'story of scientific dysfunction' matters more widely. Lloyd's approach to the social significance of the different evolutionary explanations of female orgasm seems correct to me. She reminds us that '*historical genesis does not dictate our cultural attitudes toward female orgasm*' (142), and I agree with her on this: the joys of sex can proceed quite unimpeded by whatever the evolutionary origins of orgasm and various erogenous tissues may be. But Lloyd also recognizes that while there is no logical basis (because of the naturalistic fallacy) that makes it necessarily of social significance

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1 I'm not sure what makes an adaptation 'typical,' though certainly the many ways in which women's orgasms differ from their opposable thumbs include variability across individuals and across time and place for a given individual. In comments made since the book was published, Lloyd emphasizes that variability also provides the key constraint on what possibilities remain for future adaptive hypotheses: the flat distribution curve associated with orgasmic variability rules out directional selection but leaves multi-strategy selection open as a possibility (Lloyd 2005b).

whether scientists conclude that female orgasm is an adaptation or a developmental byproduct, it inevitably will, because human sexuality is so bound up with cultural norms (18-19). So we might expect *The Case of the Female Orgasm* to matter to women, and in many ways what it brings are good vibrations. Given western culture's long history of founding social and behavioural norms — especially those concerning sexuality — in biological functions, the nonadaptive account Lloyd favours can be viewed as liberating, a counsel for women's enjoyment of the pleasures of the flesh without regard to any second-guessing as to what constitutes a normal response.

Interestingly, feminist scientists like Anne Fausto-Sterling and Sarah Blaffer Hrdy, both of whom have been trailblazers in challenging androcentric biases in biology, reject Symons' account and prefer adaptive explanations of female orgasm. Lloyd construes this as the product of poor reasoning, writing: 'I believe that much of the feminist reaction against the thesis that female orgasm is an embryological byproduct of selection on the male orgasm is based on a false equation of what is *important* with what is *naturally selected*' (142). This seems an accurate enough diagnosis of much of the feminist reaction to Lloyd's book in the often less-than-civil blogosphere. However, Fausto-Sterling's and Hrdy's opposition to Symons cannot be entirely accounted for by this 'false equation' of what is adaptive with what is important. Fausto-Sterling's and Hrdy's work, like Lloyd's, uncovers androcentric bias in mainstream biology. Lloyd locates androcentrism in the assumptions of some researchers that female sexuality is no different from male sexuality — for example, wrongly surmising that women like men feel exhausted after orgasm. She emphasizes the need to treat female sexuality as autonomous. The scepticism Fausto-Sterling and Hrdy share concerning the developmental account similarly reflects a desire to treat female sexuality as autonomous, in this case by seeking an evolutionary explanation of female orgasm that takes as its starting point female rather than male sexuality. For historical context, recall Darwin's account of 'the difference in the mental powers of the two sexes' in *Descent of Man*, especially these ringing words:

[P]artly through sexual selection, —that is, through the contest of rival males, and partly through natural selection, —that is, from success in the general struggle for life ... man has ultimately become superior [in energy, perseverance, courage, imagination, and reason] to woman. It is, indeed, fortunate that the law of the equal transmission of characters to both sexes has commonly prevailed throughout the whole class of mammals; otherwise it is probable that man would have become as superior in mental endowment to woman, as the peacock is in ornamental plumage to the peahen. (328-329)

Women, quite literally according to Darwin, have their forefathers to thank for any mental gifts they are lucky to have received. The structural similarity of Symons' account of orgasm with Darwin's account of mental powers is evident. For both, the selective pressures responsible for the trait's development in the species operate entirely on males, with females possessing the trait derivatively, due to constraints associated with mechanisms of embryological development for Symons and with mechanisms of inheritance for Darwin.<sup>2</sup>

Lloyd faults a 1997 essay review article by Fausto-Sterling and co-authors Patricia Adair Gowaty and Marlene Zuk for accepting the truth of the Baker and Bellis hypothesis of uterine 'upsuck' despite the poor statistical evidence provided. The article, 'Evolutionary Psychology and Darwinian Feminism,' has its own sexist Darwinian historical context, beginning as a reply to Robert Wright's article 'Feminists, Meet Mr. Darwin' which appeared in the *New Republic*.<sup>3</sup> After taking on Wright's claim that feminists refuse to acknowledge the truth about human nature (for example, that in cases of sexual harassment, because women evolved to be more sexually reserved than men, they become traumatized by what for men is normal behaviour), the authors criticize Symons' theory that 'women didn't even evolve their own orgasms' (404) and his associated contrast between the 'sexually predatory male' and the 'passive female' (405). In an accompanying footnote, noting that 'not all evolutionary psychologists think alike' (416), the authors compare David Buss favourably to Symons because his theory that orgasm increases sperm retention (based on Baker and Bellis) ascribes more agency to women. Lloyd contends that this wrongly 'equat[es] human agency with a trait's status as a contributor to current fitness' (142). This dismissal is too quick. The Fausto-Sterling et al. article explains how various 'Darwinian feminists' have challenged longstanding evolutionary dogma that frames sexual selection in terms of active, desiring males and passive, coy females. Lloyd recognizes that Symons has such views: she notes a chapter in the 1979 book titled 'Copulation as

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2 In a May 30, 2006 Canadian Philosophical Association/Canadian Society for Women in Philosophy (CPA/CSWIP) session on Lloyd's book, Kathleen Okruhlik drew attention to the different intuitions that arise when we consider the developmental account of female orgasm as analogous to female intelligence rather than, as most frequently portrayed, male nipples.

3 Wright is a science writer, a senior editor of the *New Republic*, and author of the 1994 book *The Moral Animal: Why We Are the Way We Are: The New Science of Evolutionary Psychology*. Fausto-Sterling et al. note that their reply was initially accepted at a lower editorial level and then rejected at the highest editorial level of the *New Republic*.

a Female Service' (141). Her response is to distinguish as 'logically independent' theories about 'what motivates the behavior of intercourse in females' and the 'evolutionary origins of female orgasm itself.' But are such theories really logically independent? They might be considered logically independent in the sense that the proposition 'Women are motivated to have intercourse with men because they want to provide a service' ('Fries with that? Have a nice day!') could be false while the proposition 'Female orgasm arose as the developmental outcome of selection pressure for male orgasm' could be true. But they are not logically independent insofar as they are mutually reinforcing propositions within that web of evolutionary beliefs Fausto-Sterling et al. are challenging. If female orgasm arose as a developmental byproduct of male orgasm, and male orgasm arose as an adaptation because the pleasure experienced encouraged the frequent pursuit of sex, then we are presented with an evolutionary account of why — in Fausto-Sterling et al.'s words — males might be expected to be 'sexually predatory' and females might be expected to be 'passive.'<sup>4</sup> On the causal account of biological functions accepted by Lloyd, what is normal and abnormal in a biological sense depends on whether or not a trait is an adaptation. This means only male sexual agency (taken here as actively seeking out orgasmic sex) can be represented as the biological norm for the species. Is it merely coincidental that Symons (1979) characterizes multiple orgasm in women as 'found primarily, if not exclusively, in the ideology of feminism, the hopes of boys, and the fears of men' (92)? Darwin is said to have reacted to John Stuart Mill's *On the Subjection of Women* by saying that Mill had something to learn from biology (Desmond and Moore, 572). Plus ça change...

However, wishful thinking doesn't make something true, and Lloyd's argument is that Fausto-Sterling et al. and many others should not be accepting Baker and Bellis' conclusions as fact given the poor statistical analysis (216). Although we need not accept with Lloyd that Fausto-Sterling, Gowaty, and Zuk's expressed preference (in a footnote) for a particular adaptive hypothesis necessarily implies that they believe it to be true, are they nevertheless being irrational in preferring a hypoth-

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4 Rebecca Kukla made the important point during the CPA/CSWIP session that those feminists who reject the active-passive essentialization of male and female should not uncritically embrace activity over passivity because activity can often be pernicious. However, in this context, Symon's developmental account decouples female orgasm from any evolutionary grounding in terms of functional norms by assuming that the species adaptive norm is for females to be less sexually assertive than males, rendering those females who actively seek out orgasmic pleasure abnormal.

esis that has less empirical support but coheres better with background beliefs and social values which they are reluctant to relinquish? Have they let not just adaptationist but feminist, even gynocentric, biases get the better of them? To assess this, we need to look more closely at the epistemology of theory choice.

### **III Epistemology of Female Orgasm: Not Social Enough**

Lloyd is not a naïve empiricist who believes that the data alone justify or falsify theories. She recognizes that, because of the underdetermination of theories by data, background assumptions always operate in science. Without background assumptions, data cannot become evidence for or against theories. The challenge is to identify when background assumptions become biases that result in 'bad science.' To this end, Lloyd's analysis treats androcentric and adaptationist biases/background assumptions differently. Because androcentrism is a social bias, she believes its influence in the science of the evolution of female orgasm can be adequately captured by traditional empiricist approaches to bias — for example, by logical positivists. Such approaches, she says, strive to eliminate the effects of social — nonepistemic — considerations that are injected as a result of the 'preconceptions and special interests' of individual scientists (239). This nevertheless leaves room to recognize that feminist biases will be beneficial for science inasmuch as they bring to the fore and thereby neutralize androcentric ones. But since adaptationism is a legitimate approach to research, a theoretical (and therefore properly scientific) not social bias, a different approach is required. Lloyd appeals to the contextual empiricism developed by feminist philosophers of science Elizabeth Anderson and Helen Longino.

Lloyd favours Anderson's characterization of harmful background assumptions as those that 'lead to partiality in the treatment of data' (256). Impartiality is conceived by Anderson as a methodological standard whereby all (or a representative subset of) truths relevant to the research question posed are taken into account. Thus far, we are not so removed from traditional empiricist approaches to bias. For example, Lloyd provides as an example of a beneficial background assumption the counsel that not every biological character is adaptive. This serves to neutralize adaptationist tendencies, just as feminist tendencies serve to neutralize androcentric ones. Lloyd thereby assumes that there is a neutral or impartial stance that is possible for researchers to take, or at least aspire to, before the facts. Whether social or theoretical, biases that compromise this neutrality or impartiality are bad for science. For her

part, Lloyd believes that the byproduct account is consistent with feminist values, but she emphasizes that this is entirely incidental to her support for it, and the support she recommends for it, which are based on empirical grounds. Lloyd drives this point home not just in the book but in her response to feminist-bashing reviewer David Barash, where she writes, for example: 'I am arguing against the many feminists who oppose the byproduct account because it makes female orgasm sound derivative or denigrated ... . I myself am arguing *against* anyone drawing conclusions on the basis of political inclinations ... . I support the byproduct theory on the basis of all the empirical evidence that I have amassed, discussed, and analyzed in my book' (Lloyd 2005b).

Accordingly, Lloyd's approach might better be captured by Sandra Harding's (1993) expression 'spontaneous feminist empiricism' than the contextual empiricism with which she herself identifies.<sup>5</sup> Harding introduced this expression to order to distinguish the position she previously labelled 'feminist empiricism' in *The Science Question in Feminism* from philosophically distinct feminist empiricist approaches developed subsequently — namely, Longino's contextual empiricism and the holism of Lynn Hankinson Nelson. According to Harding, spontaneous feminist empiricism emerged in the late-1970s and early-1980s as a product of the experiences of feminist scientists who were recognizing and confronting the role of sexist and androcentric biases in their fields of biology and the social sciences. Their envisaged solution was to rid science of such biases by promoting better adherence to existing methodological norms. This contrasts with Longino's argument that, because of underdetermination, there are no methodological rules that can provide an in-principle exclusion of either constitutive or contextual values from the context of justification. Longino (1983) defines constitutive values as norms conducive to cognitive or epistemic aims which are accepted to operate internal to science: accuracy, scope, simplicity, etc. Contextual values are those personal, social, or cultural

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5 Letitia Meynell characterized Lloyd's approach this way in the paper she presented at the CPA/CSWIP session. This proved to be contentious during the ensuing discussion. However, I agree with Meynell. Like Harding's 'spontaneous feminist empiricists,' Lloyd's critique arises within a science with which she is well familiar, recommends adherence to existing methodological norms, and retains the distinction between 'good' and 'bad' science. The point can certainly be made that this more conservative version of feminist empiricism will be most effective for engaging a target audience of scientists, and consequently, Harding unfairly paints the more recent philosophical versions of feminist empiricism as 'sophisticated and valuable' and 'important' (1993, 51-2). I discuss the possibility that Lloyd is merely being pragmatic in the approach she adopts in *The Case of the Female Orgasm* in what follows.

preferences which are traditionally considered to operate external to science: sexism, racism, profitability, etc. According to Longino, theory choice is often underdetermined by available empirical evidence, with the result that contextual as well as constitutive values come into play in the acceptance of a given theory over its rivals. Longino (1995) contends that constitutive values may themselves be contextually determined, placing into contention their privileged cognitive or epistemic status: for example, the preference for unidirectional causality (i.e. simplicity) replicates social relations of domination.

To the extent that Lloyd retains the distinction between 'good' and 'bad' science (shared by spontaneous feminist empiricism but rejected by Longino's contextual empiricism), this takes her some distance from the epistemological views she expresses in an earlier, 1993, treatment of the topic of female orgasm where she writes:

Current "purist" philosophy of science actually *contributes to* that political power [which attacks the women's movement by appealing to gender differences and women's biology] by reinforcing myths of the insulation of scientific endeavors from social influences. A more sophisticated understanding of the production and evaluation of scientific knowledge would mean seeing science as (partly) a continuation of politics. (Lloyd 1993, 151)

These claims that science is '(partly) a continuation of politics' and that the ideal of science as pure and insulated from social influences is a myth, indeed a harmful myth, become mitigated in *The Case of the Female Orgasm* by Lloyd's appeal to wholly empirical facts insulated from social and political aims to justify her conclusions and to distinguish her contribution from the work of feminist scientists like Hrdy and Fausto-Sterling and its reception by feminist nonscientists who are ostensibly motivated by such aims. How are we to understand this apparent shift in views? It may be pragmatic. Lloyd's primary audience in *The Case of the Female Orgasm* is scientists; her goal is to encourage and facilitate a reassessment of the science of the evolution of female orgasm, and there is no point in using a complex tool when a simple one will do the job and is likely to yield better results.<sup>6</sup> Appealing to widely shared methodological standards will be more effective than casting such standards into doubt. Insofar as *The Case of the Female Orgasm* is directed to a philosophical audience, it is to challenge those who defend the objectivity of science by denying that biases and background assumptions ever interfere with outcome. This seems to me a rare bird. Even

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<sup>6</sup> This point was made by Alison Wylie at the CPA/CSWIP session.

the 'purist' philosophers of science Lloyd criticizes in the 1993 article are prepared to draw a distinction between good and bad science, with Lloyd's case of female orgasm challenging their epistemological views only insofar as they believe that adequate time has elapsed for the self-correcting nature of science to be observed in this field. But this just further substantiates the point that philosophers are not Lloyd's target in the book, whereas they were for the article (which was published in *Philosophical Studies*).

Counting against the pragmatic interpretation is Lloyd's own mode of analysis in the book. She believes that the empirical support in favour of Symons' developmental explanation is completely unequivocal. Were pervasive and 'pernicious' biases not rendering this community of researchers 'dysfunctional,' they would have converged on the properly rational outcome. Feminists are no less guilty than androcentrists. This does not mean that Lloyd believes that the case of the female orgasm is (or should be) closed, and that with the evidence favouring Symons' account a definitive explanation has been reached. She identifies several directions for future research, and so, on the one hand, the case seems very much open.<sup>7</sup> On the other hand, given that Lloyd calls for a 'fresh start' (website FAQs), and considers none of the 20 adaptive hypotheses she canvasses to be worth pursuing further, the theoretical landscape has been narrowed considerably. This means that some degree of closure to the case has been reached, and brings to mind Peter Galison's 1987 book *How Experiments End*. Writes Galison:

[T]here are many theoretical and experimental presuppositions brought to the laboratory, some of them tacit. Many of these commitments are used in the establishment of scientific "facts." This prior knowledge is worked out in the measurement, interpretation, and construction stages of experimental work ... Moreover, these theoretical presuppositions that "subvert" the full autonomy of experimental procedure are more than mere "biases," interfering with an otherwise clear view of nature. Without presuppositions, experiments can neither start nor finish. (12-13)

Just as experiments cannot be brought to an end without theoretical presuppositions, theoretical presuppositions are implicated in closing the case of the female orgasm, at least to the extent this has occurred. As Galison's work suggests, we should expect to find presuppositions of Lloyd's that support her treatment of the sexology data — the orgasm/

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7 Lloyd recommends further studies into the possible relationship between oxytocin secretion and orgasm. She suggests that scientists not ignore the possibility that female orgasm is a secondary adaptation, meaning that its contribution to fitness explains its maintenance but not its origin in the population.

intercourse discrepancy and observed variability in orgasmic potential — as decisive evidence for Symons' developmental account and against the not just current but future viability of the 20 adaptive hypotheses.

Presuppositions are involved in how Lloyd delineates female orgasm as a trait. Women are classified according to whether they always, often, seldom, or never experience orgasm with intercourse (also whether intercourse is assisted or unassisted by manual stimulation of the clitoris), and this is compared to masturbation. This focus on the clitoris ignores the other tissues involved in orgasm (as sites both where stimulation occurs and pleasurable sensations are experienced), as does Lloyd's definition of orgasm as a basic spinal reflex. Lloyd provides a good reason for her physiological definition: she wishes to compare the trait across primate, hominid, and human lineages. But this leaves out the supraspinal physiological, neurological, and psychological aspects that, in contrast to the knee jerk response, make orgasm pleasurable and more and less easy to achieve depending on sleepiness, intoxication, medication, anxiety, fear of being overheard, etc. Nor does it include the supraindividual social and psychological dimensions of orgasm — that sex is complicated for people, as a function of past experiences, moral and religious beliefs, etc.; that partners are more and less experienced and knowledgeable, with varying degrees of interest in the other's pleasures; that people are often too embarrassed to discuss what's happening in bed, more and less comfortable at taking initiative, and sometimes uncomfortable with physical and emotional intimacy. These factors may contribute to the orgasm/intercourse discrepancy and explain at least some of the observed variability in orgasmic potential, complicating how evidentiary relations between sexology data and competing evolutionary hypotheses are to be construed. In addition, Lloyd's preference for a 'scientific' sexology which leads her to focus on quantitative studies rules out those insights that might come from qualitative research based on women's (or men's) own understandings of their bodies and could potentially lead to different, nonphysiological, interpretations of the observed variability.<sup>8</sup>

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8 The large surveys carried out by Kinsey in the 1940s-50s and Masters and Johnson in the 1960s were pivotal in bringing about what has come to be called 'the sexual revolution' and, due to the activism of second-wave feminists, contributed to a profound change in western thinking about women's sexuality in which the clitoris came to take its rightful place. But exactly because of the sexual revolution, these data seem inadequate today. Not in a quantitative sense: recent studies have confirmed Kinsey's and Masters and Johnson's results concerning what Lloyd refers to as the orgasm/intercourse discrepancy. However, in a qualitative sense, it is possible that women's understandings of their bodies have improved, given that from the 1960s until today, at least in North America and Europe, trends have been

Further presuppositions are involved when this variability in orgasmic potential conceived in wholly physiological terms as a basic spinal reflex is used to assess the validity of competing evolutionary hypotheses, whether adaptive or nonadaptive. Lloyd assumes that fixation of traits occurs even if selection pressure is low (145), or at least that balancing as well as directional selection yield peaks in the population distribution (website FAQs), and so that the observed variability with accompanying flat distribution curve falsifies all the adaptive hypotheses (website FAQs). This ignores that there might be countervailing selection pressures, fluctuating selection coefficients, or more than one evolutionary factor operating — for example, both selection and developmental constraints. If an adaptive hypothesis abandoned for lack of empirical support is getting part of the story right, an opportunity to obtain knowledge which could prove useful for constructing more complex models in the future is lost with it. Although Lloyd faults adaptationists for ignoring those orgasms experienced by females (human and otherwise) apart from heterosexual intercourse — for example, involving same-sex interactions or masturbation — she does something similar by ignoring inclusive fitness and thereby the possible adaptive value of orgasms reliably associated with the frequent same-sex interactions that occur among females of some nonhuman primate species like stump-tail macaques and bonobos. Lloyd faults adaptationists who wrongly interpret such observations as evidence that female orgasm in heterosexual population also reliably occurs in these species, or inexplicably continue to maintain that female orgasm evolved in hominid lines. Brief mention is made of the possibility that homosexual sex may promote reproductive fitness in bonobos by establishing female coalitions that influence male behaviour (234), but there is no further exploration of the possibility that orgasm as a basis for maintaining social relations among female kin could promote inclusive fitness. Instead, Lloyd concludes that these findings support Symons' account insofar as they indicate a developmental potential for orgasm in non-human as well as human females if the right sort of stimulation occurs. An additional presupposition involved in deciding the case of the female orgasm in Symons' favour is Lloyd's incorporation of what is the dominant account of sexual differentiation in human developmental biology: that the embryo masculinizes as the result of effects of the so-

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for women and men alike to have a greater number of sexual partners — at a given period in their lives as well as over the course of their lives, for sexual activity to begin at earlier ages, for increased bisexuality, for more open homosexuality, for greater encouragement of sexual experimentation, for heightened expectations of sexual reciprocity brought about by greater gender equality, etc.

called male hormones, and in the absence of these, the default female pathway will be taken. However, Fausto-Sterling (2000) and other feminist biologists argue that the focus on masculinization itself reflects androcentric bias by dichotomizing maleness as presence and femaleness as absence, and urge that hormonal influences in female embryos also be sought. This presupposition is significant because it helps to protect Symons' account by neglecting along with specific mechanisms of female embryological development the evolutionary history that might have shaped these.

My purpose in outlining some of the presuppositions which support Lloyd's treatment of data as decisive evidence is not to disagree that a pluralistic approach to evolutionary theorizing is preferable to a pan-adaptationist one. I agree entirely with Lloyd's argument that non-adaptive explanations — whether due to genetic drift, developmental constraints, etc. — should not be regarded by evolutionists as inferior, as purely negative findings that call for further research, equivalent to null hypotheses that are never accepted as true no matter how many times they are found to be untrue. Nor do I want to label these presuppositions biases, and nor am I suggesting that they are necessarily false. My contesting them merely shows that they are contestable. In many cases, this will affect how evidential relations are judged, and unless presuppositions which affect judgments about evidential support are shared, there is likely to be disagreement about whether some end to research has been reached. Hrdy's work is a case in point. Hrdy is dubious of the adaptive value of orgasm in human females — like Lloyd because of the orgasm/intercourse discrepancy, and also because of the mismatch in men's and women's capacities for multiple sequential orgasms. But she believes it may be adaptive in primates operating as a variable reward system that would encourage females to seek out one mate after another in a given bout of sex because confusing males about paternity would enhance fitness by increasing the likelihood of parental investment and decreasing the likelihood of infanticide. Lloyd makes several criticisms. One criticism appeals to a study that shows that successful behaviour modification requires rats to receive a reward each time they behave a certain way. Another criticism appeals to sexology reports that, beyond a certain prolongation of intercourse, women are unlikely to reach orgasm. How seriously you take Lloyd's objections will depend on whether or not you share her background assumptions about the validity of drawing inferences about chimpanzee, baboon, or macaque behaviour from rat or human behaviour. It seems to me that there are times when intermittent reinforcement seems to work in humans if not rats. A recent gambling addiction study (Turner et al. 2006) showed that early payout is a significant risk factor: people get hooked because they keep coming back hoping for a return of that magic mo-

ment. How similar is a VLT player at my neighbourhood bar to a chimp in the wild? I have no idea. Nor do I know how similar the prospects for orgasm between human and macaque worlds given a single partner on Viagra versus multiple mates varying in anatomy and technique. But that's the point. Background assumptions can vary among reasonable people.

*The Case of the Female Orgasm* reveals to the reader one contradiction and fallacy after another. Yet, such a litany of contradictions and fallacies somehow belies the holism of the web of beliefs within which we and the science we evaluate as philosophers are entangled. The Hrdy example leaves me sceptical that *all* 20 adaptive hypotheses are *equally* characterizable as 'completely unsupported by evidence' and 'hopelessly bad science,' as Lloyd says in an interview (Sutherland 2005), and 'that everyone involved in this case had fallen down on the job' (website FAQs). Think of this as a digital-analogue mismatch. As Pierre Duhem emphasized long ago, even so-called crucial experiments with their contradictory findings lack a rational decision procedure that is deductivist in structure. What remains is 'good sense.' And good sense comes in degrees. These degrees of good sense and the absence of an a priori basis for eliminating contextual as well as constitutive values from the justification of theories support Longino's claim that, where multiple theoretical models have empirical support, it is legitimate to choose one model over another for political reasons. For example, feminist scientists may well prefer interactionist over linear models in neurobiology because these allow for the intentionality and agency their emancipatory goals require (Longino 1987). Accordingly, insofar as the presuppositions which lead Lloyd to construe the sexology data as decisive evidence in Symons' favour can be contested, epistemic space remains for feminist biologists to continue to explore adaptive explanations that treat female sexuality as autonomous and permit female sexual agency, even if such preferences — and accompanying antipathy toward Symons' service-with-a-smile account of female orgasm — are inspired in part by feminist politics.

Lloyd departs more radically from traditional empiricist approaches to epistemology in her adoption of another aspect of Longino's approach which relocates objectivity from being a property of individuals to being a property of communities. Longino (1993) identifies four criteria important to objectivity: publicly recognized forums for criticism, demonstrated responsiveness to such criticism, shared methodological standards that make criticism possible, and equality of intellectual authority within the community. Objectivity comes in degrees, and a community of scientists is objective to the extent that these criteria are fulfilled. Lloyd finds the third criterion — that there be shared criteria for the evaluation of theories, hypotheses, modes of data collection, etc.

— most severely compromised in her case study. This is because those she identifies as ‘ardent’ adaptationists differ from ‘more mainstream’ adaptationists and developmentalists in their standards of evidence and explanation. Concerning standards of evidence, positive and negative evidence are treated differently. Developmentalists take the lack of evidence for a link between female orgasm and reproductive success to be telling, whereas ardent adaptationists insist there must be such a link. They also refuse to acknowledge positive evidence in support of the developmental account. Concerning standards of explanation, ardent adaptationists rule out nonadaptationist explanations of female orgasm a priori, treating the byproduct account as merely the null hypothesis that is to be disproved.

In an article in *The Globe and Mail*, science writer Alison Motluck offers this apt description of Lloyd: ‘She is the Sheila Fraser of science, and she’s holding a public inquiry.’<sup>9</sup> Indeed, Lloyd’s careful and thorough methodological criticisms make a contribution that is similar to that of a good survey article which we might expect to find in a scientific discipline that takes internal critique seriously. Reception from scientists is telling; some responses exhibit the androcentric (even sexist) and adaptationist biases she criticizes.<sup>10</sup> Lloyd believes that ‘this particular topic’ — female orgasm — ‘seems to attract deficient scientific explanations’ (18), and yet, adaptationism has contributed to many charged controversies in evolutionary biology: the drift-selection debate, the classical-balance debate, the neutralist-selectionist debate, the ‘Spandrels’ debate, etc. However, for Longino’s model to shed light on such controversies, a more robustly sociological approach than Lloyd uses in *The Case of the Female Orgasm* is needed. The extent to which standards and background assumptions are subjected to critical evaluation is a function of the social organization of scientific communities. Lloyd assumes that the community she finds dysfunctional is one organized around the problem of female orgasm or female sexuality more generally. But without appeal to social factors — to journals, conferences, correspondence, exchanges of personnel, etc. — we cannot be sure that this designates a genuine community. The complete evolutionary ex-

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9 Sheila Fraser is Auditor General of Canada, and generally characterized by the media as a pointed (even sometimes devastating) but always careful and fair-minded critic of the government’s management of its affairs.

10 Although others do not — for example, Marlene Zuk’s (2006) essay review is generous and fair, even though as a co-author of the Fausto-Sterling et al. article she was criticized in the book. Zuk’s article also makes a further contribution to critical discourse, arguing that androcentric and anthropocentric biases are implicated in considering female and not male orgasm a problem in the first place.

planation of female orgasm requires contributions from many fields: sexology (which includes physicians, psychologists, nurses, and social workers), physiology, gynecology, anthropology, evolutionary biology, primatology, animal behaviour, etc. Interest in female orgasm can be more and less tangential to these researchers' interest in other questions. And for a theory to become widely accepted, it must have effective and influential defenders, not just empirical support.

The move that relocates objectivity from individual scientists to scientific communities calls not only for the incorporation of sociological inquiry but the theorizing of relations between individuals and communities. Presumably, a community structured in a way that promotes critical discourse can tolerate what might be considered biased approaches by individual scientists or research teams, granting (though perhaps lauding less) Paul Feyerabend's celebration, in *Against Method*, of how 'prejudice, passion, conceit, errors, [and] sheer pigheadedness ... [have] opposed the dictates of reason' (1988, 121). Lloyd characterizes Longino's work as being about 'how scientific communities produce objective scientific results' (220), and in her post-publication responses refers to the 'objective empirical evidence' and 'objective data' which support the byproduct account (Lloyd 2005b; website FAQs ). But we might instead understand Longino to be emphasizing process over product, that is, to be interested in how scientific communities *objectively produce* scientific results. In her description of the case of the female orgasm as 'hopelessly bad science,' Lloyd draws a distinction between good and bad science that rests on evaluating the extent to which a rationally discernible outcome is achieved. There is a sense in which scientists collectively become that single, homogeneous mind capable of evaluating the empirical fit of hypotheses by logical means alone — in Longino's (1993) words, 'the original ideal of uncontaminated or unconditioned subjectivity' — which feminist epistemologists and philosophers of science (and others) have rejected.<sup>11</sup>

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11 Longino uses these words to describe western epistemology's paradigmatic knower — whether rationalist, empiricist, or Kantian — as a disembodied and detached individual whose subjectivity is 'uncontaminated' by emotions, desires, etc. and 'unconditioned' by social and historical location. Amongst philosophers, in addition to feminist epistemologists and philosophers of science, pragmatists and, more recently, social epistemologists and philosophers of science embracing a science studies perspective have rejected this paradigm.

#### IV Conclusion

I have argued that Lloyd's account of bias and objectivity in this case study of the evolution of the female orgasm doesn't go far enough in recognizing the social dimensions of scientific research — the role of social values in theory choice and the contribution of social organization to the objectivity of the community. We are left with the question whether the lack of emphasis on the social in *The Case of the Female Orgasm* was a strategic choice on Lloyd's part since scientists were the book's target audience or reflects her adherence to a fairly standard empiricist epistemology. An answer may be forthcoming in Lloyd's future writings, some of which I hope will be directed to a science studies audience and not just those rare philosophers who would deny any role exercised by biases and background assumptions in science. Given the social significance of the science of the evolution of female orgasm — the likelihood of scientific findings engendering cultural understandings of sexuality and gendered expectations about sexuality finding their way into scientific theorizing from the wider culture in which science is practised — such an undertaking will be enriched by Lloyd's engagement with the range of positions staked out by feminist biologists, epistemologists, and philosophers of science. She would do well to resist being forced into a corner by Barash-style feminist-bashing masquerading as scientific truth from which the only apparent escape becomes to distance herself from feminists like Fausto-Sterling and Hrdy by portraying them as motivated by political rather than scientific concerns. This would mean returning to, and taking seriously, her 1993 analysis that '[a] more sophisticated understanding of the production and evaluation of scientific knowledge would mean seeing science as (partly) a continuation of politics.' On such a view, 'biases' that compromise the neutrality and impartiality of science cannot be so easily disentangled from the 'background assumptions' necessary for empirical data to become evidence. Although feminists have contributed greatly to theorizing the social and political dimensions of scientific research, and it is some of these contributions on which I have focused here, the project of building a genuinely social epistemology of science is a big-tent affair. Lloyd's careful examination of evolutionary accounts of female orgasm advanced over the past four decades in *The Case of the Female Orgasm* reveals the importance of, and continuing need for, any adequate social epistemology of science to incorporate a normative stance as has been characteristic of the philosophical tradition. The shortcomings of Lloyd's case study in recognizing the social dimensions of scientific research reveal the need for philosophers to anchor this normative stance theoretically elsewhere than in the traditional dichotomies (rational — social, facts—values, etc.) and to foster interdisciplinary collabora-

tion with sociologists that would provide a more empirically grounded examination of social structure.

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