

Book Review

Let a Thousand Orgasms Bloom! 

A review of *The Case of the Female Orgasm*, by Elisabeth A. Lloyd. 2005. Harvard University Press: Cambridge, Mass. 311 pp.

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In *The Case of the Female Orgasm*, Elisabeth A. Lloyd has taken a really terrific topic and written a really terrible book. I say this with sincere and manifold regret, first because female orgasms pose fascinating and important questions, that warrant serious attention from evolutionary biologists. Second, I particularly would have loved to find a worthwhile book on this matter, not least because I am working on my own book-length treatment of it and other “womanly mysteries.” Thus, for all the interest (scientific no less than personal as well as prurient) attaching to female orgasm, there are many other dimensions of womanhood that also remain to be explained in convincing evolutionary terms: Why is ovulation concealed? Why is it so often synchronized? Why does menopause occur? Ditto for menstruation. Why do women have prominent breasts even when not lactating? (Barash, in preparation).

And finally, I would like to be kinder and gentler with Lloyd’s book because – given the realities of reciprocal altruism - the more books I write, the more am I inclined to say good things about the efforts of others, hoping perhaps that they will return the favor ... or at least, not be terribly nasty about my next offering! But I need to be honest, too.

The topic is straight-forward enough: why do women experience orgasm? The male counterpart is easy enough for evolutionists to explain, but – despite an abundance of theorizing – no one has yet demonstrated a clear-cut fitness benefit that accrues to orgasm in women. Indeed, for many years, biologists including yours truly believed that female orgasm was unique to human beings, albeit no less a mystery. Now, we know that females of many nonhuman primates and maybe even some nonprimates experience orgasm. But we still don’t know why.

One possibility, championed most prominently by Donald Symons (1979), is that the female orgasm is an evolutionary by-product of male orgasm, a neutral tag-along trait that persists because it is adaptive in one sex, and, because of its developmental underpinnings, is difficult to lose in the other. The preferred metaphor is/are male nipples: clearly adaptive in women, nipples are nonfunctional and evidently nonadaptive in men, yet they presumably persist among the latter because

of their benefit to the former. This is the view that Dr. Lloyd favors.

So far, so good. The problem – among many – is that she doesn't simply favor it; rather, she is so ardent in her commitment and so dismissive of other, adaptive possibilities that her book is more a rant than an even-handed effort to expound upon and demystify a genuine, unsolved scientific controversy. Moreover, Lloyd's zealotry is compounded by a regrettable lack of biological sophistication plus plain old-fashioned lousy writing. Some examples of the latter: "The crucial point is that, whatever people's intuitions are regarding the obviousness of what should be the case in evolution, I have already argued, in relation to the adaptive stories current at the time, that there is no credible evidence that orgasm correlates with reproductive success (pg. 166)." "I should note that even though evidence can be used to challenge background assumptions, there are yet further background assumptions required to link the challenged background assumption to this evidence. In general background assumptions are challenged with relation to a particular set of other background assumptions and the public standards of a community (pg. 249)." How about this background assumption: decent writing? Perhaps we should take up a collection to help Harvard University Press hire an editor who edits.

Okay, so Dr. Lloyd is not a stylist. Sadly, she is not much of a biologist, either. Much of her argument turns on the question of whether a given phenotype is an adaptation, and in order to fit her Procrustean bed (pre-constructed to accord with her commitment that lots of traits aren't adaptations, she defines adaptations so narrowly as to exclude most of them! Thus, we are offered the extraordinary claim that sickle-cell disease is "the best, most elaborated case of a human adaptation." (pg. 12) What about binocular vision, or the human hand with its opposable thumb? Or bipedal locomotion, or the kidney's prowess at filtration, the heart as pump? Or our large, multi-faceted brains, for Darwin's sake? (Hint: since no iron-clad historical sequence for such phenotypes have yet been adduced, and, moreover, no clear Mendelian basis for such traits have been identified, they don't qualify in Lloyd's opinion as adaptations. With so limited a perspective, no wonder Dr. Lloyd has such a hard time perceiving female orgasm as a possible – indeed, a likely – adaptation.)

Ironically for a book with the subtitle "bias in the science of evolution," *The Case of the Female Orgasm* is marinated in bias: against "adaptationism," "androcentric thinking," and heterosexuality, and in favor postmodernist "social constructivism" as well as anything ever written by Stephen Jay Gould. Not surprisingly, Gould godfathered Lloyd's argument, basing one of his *Natural History* columns upon it, and – word has it – lobbying hard and successfully at Harvard University Press on behalf of her manuscript.

Moreover, this isn't really a book about the female orgasm; rather, it is about the debate over the female orgasm, consisting mostly of criticisms of other people's data and interpretations. There is much to be said for an occasional Hercules, cleaning out the Augean Stables of ill-founded scientific dogmatism. But Lloyd labors are less Herculean than they are self-defeating, as her single-minded determination to argue against any adaptive function for the female orgasm not only

diminishes her credibility but highlights her own biological naivety.

Thus, Lloyd makes much of the fact that orgasm varies “widely across women,” arguing that this is “exactly what would be expected if it were not under direct selection pressure” (pg. 133). She is correct that traits under very strong selection pressure generally show little variation (the variance in number of heads per person is indeed low). But a range of variability is definitely not, in itself, evidence that a trait is *not* adaptive: what about human height, weight, intelligence? Did they not almost certainly evolve under selection pressure? Admittedly, maybe the answer is no, but common sense plus basic biology suggests otherwise; the fact that such traits vary may be attributable to several factors, suggesting the maintenance of underlying genetic variation via a diversity of mechanisms.

Lloyd claims further that orgasm’s phenotypic plasticity “is evidence that selection has not acted on the trait at all” (pg. 135). Nonsense. Selection can *favor* phenotypic plasticity. Indeed, it often does! The fact that human beings speak thousands of different languages is testimony to vast plasticity, but is NOT evidence that selection hasn’t acted on the ability to speak languages.

Lloyd also makes much of the fact that most women do not experience orgasm every time they engage in sexual intercourse, claiming that this somehow casts doubt on the possibility that orgasm is an adaptation. This is like saying that because lions don’t always hunt, or don’t always succeed when they do hunt, hunting isn’t adaptive. The reality, of course, is that lions have been selected for phenotypic plasticity with regard to their hunting: being more likely to do so in circumstances than others. And indeed, one of the more plausible hypotheses for female orgasm relies on the observation that it occurs at some times and not at others ... wherein may lie its adaptive value:

Several decades ago, while a graduate student, I noted that when a subordinate male grizzly bear mounts a female, his head swivels constantly over his shoulder as he anticipates the arrival of any dominant boar, who would rapidly supplant him. Not surprisingly, subordinate grizzlies ejaculate quickly whereas dominants are substantially less rushed. I have no idea whether grizzly sows experience orgasm, but it seems likely that if they do, it would be more likely when copulating with the latter than with the former. And this, in turn, led me to wonder whether female orgasm is a signal whereby a female’s body tells her brain that she is sexually engaged with a dominant individual. If this is the adaptive significance of female orgasm, then we certainly would not expect orgasm to occur with regularity, and would be altogether misled if we took its seemingly erratic occurrence as evidence that it does not serve an evolutionary function.

Lloyd claims to marshal evidence to the contrary, which includes the claim that females cannot be assumed capable of discriminating “genetically-based differences among potential mates” and deciding, as a result, which phenotypes would constitute better mates (pg. 196). Perhaps she has not yet heard of sexual selection and the role of female choice. If so, I suspect most readers of this review could suggest some references, probably off the top of their heads.

Elsewhere, Lloyd points to the “puzzling data on the relative infrequency with which women experience orgasm with intercourse,” arguing that “under the common assumption that the capacity for orgasm is designed as an adaptation to encourage and reward intercourse, this infrequency must be seen as a design flaw” (pg. 112). What, pray tell, is wrong with design flaws? Evolutionists know that the biological world abounds in them: our narrow birth canal, the location of the prostate and of the exit site for the retinal nerve. It is the advocates of “intelligent design” and of similar drivel who have difficulty contending with design flaws; biologists understand that since there is no designer, flaws are to be expected, and their existence by no means comprises evidence against evolution by natural selection of the traits at issue. (At the same time, of course, flaws provide impressive evidence that a purported divine designer is either incompetent, indifferent, lazy, or, on occasion, downright malign.)

In her extended polemic, Lloyd makes much of the fact that female orgasms are typically clitoral and rarely vaginal, which once again is supposed to militate against the adaptationist contention that orgasm is related to fitness maximization, via heterosexual intercourse. There is little doubt that the clitoris is homologous to the penis (a connection that is fundamental to Symons’ by-product theory, so strongly favored by Lloyd), and that the two organs derive from the same embryologic substrate. Accordingly, it is unsurprising that clitoral stimulation is intimately connected to female orgasm, just as penile stimulation is to its male counterpart. That said, it is altogether consistent with a range of adaptive interpretations that female orgasm should be more readily evoked clitorally than vaginally; as all but the most blinkered nonevolutionists realize, evolution is an inefficient process, in which phenotypes are cobbled together from embryologic and historical antecedents. Under various scenarios, it might be more adaptive yet – and certainly, more desirable for those involved – if women could achieve orgasm by eating chocolate, painting their toenails, thinking pure thoughts, and so forth, but insofar as orgasms are keyed to penises and clitorises, that’s what natural selection has had to work with.

This, in turn, leads to another possibility – indeed, perhaps likelihood: What if female orgasm isn’t an evolutionary by-product, à la Symons and Lloyd, but rather, orgasm via masturbation is? Moreover, what if the biological significance of female orgasm derives from clitoral stimulation achieved – albeit inefficiently – via (gulp!) old-fashioned heterosexual intercourse, because of some yet-to-be clarified adaptive value? The horror, the horror!

Lloyd seems downright incensed by the fact that most analyses of female orgasm emphasize its occurrence during heterosexual intercourse and give short shrift to orgasms induced during female-female encounters or via masturbation. The reason for this emphasis, I submit, is simple, and not because of a male chauvinistic bias on the part of researchers (many of whom are women). Rather, evolutionary science suggests that insofar as there is likely to be an adaptive value to female orgasm, it is almost certainly manifest during heterosexual intercourse. I doubt that women have been endowed with the capacity for orgasm simply as a beneficent gift from a generous god, even though neither I nor my fellow ardent adaptationists (John

Alcock's phrase) are unaware of the inelegantly labelled "orgasm/intercourse discrepancy," supposed by Lloyd to weigh heavily against the adaptive significance of female orgasm itself. But by the same token, one might point out that male orgasms are more reliably achieved if a man stays home and masturbates than if he goes out on the town, seeking a sex partner; no one would seriously claim, however, that this is evidence suggesting that male orgasm during intercourse is nonadaptive! Changing the metaphor, I might be able to achieve a kind of satisfaction (albeit temporary) by filling my stomach with sand, or chewing coca leaves, or I might even force myself not to eat altogether, but this doesn't mean that consuming food is nonadaptive.

Don't misunderstand: I have nothing against masturbation or same-sex relationships, or carnal satisfaction achieved via poetry, sunsets, cooking utensils, antique harpsichords or even consenting animals. Quite the contrary. Let a thousand orgasms bloom! Indeed, in a world both overcrowded and increasingly infected with dangerous sexually transmitted diseases, I'd heartily recommend masturbation in particular as the epitome of safe sex. My point is that just because something (e.g., female orgasm) can be achieved in diverse ways (e.g., masturbation) does not argue against it having evolved because it is particularly adaptive in a specific, different context (e.g., heterosexual intercourse).

Is it androcentric, phallogentric, or some other kind of "centric" to suggest this? Maybe so, but the fact that a hypothesis might be "male centered" does not necessarily make it wrong, any more than the fact that population biology is female centered does not make *it* wrong. Moreover, sexual intercourse is no more male-centered than it is female-centered, although it is, I admit, heterosexually centered. There is indeed a reproductive bias to evolutionary biology generally, and my advice is that anyone who finds this objectionable should find a different line of work.

"There are elements of androcentrism," writes Lloyd, "and heterosexual bias operating in procreative focus as it applies to female orgasm, because procreative focus concentrates only on the kind of sex that is reliably associated with male reproductive success: intercourse" (pg. 234). This raises nonsense to new levels. Is male reproductive success somehow more tied to sexual intercourse than is female reproductive success? When last I checked, direct (Darwinian) reproductive success – for females no less than males – requires one or more acts of sexual intercourse, involving at least one male and one female, and as a result of which males and females experience precisely equal reproductive success. The variance between males and females may well differ; the reproductive success of the average female in any sexually reproducing diploid population is exactly equal to that of the average male.

I first became aware of *The Case of the Female Orgasm* when I received a phone call from a reporter asking for "my side" of "the controversy." I didn't know that I had a side. Turns out Dr. Lloyd had unearthed – and misrepresented – part of a single paragraph in a chapter I had co-written with my former graduate student, William Bernds (Bernds and Barash, 1979). This manuscript was not concerned with

female orgasms; rather, it sought to identify some of the factors responsible for “early termination of parental investment in mammals, including humans.” In it, we felt obliged to mention, in passing, the curious fact that Kinsey’s team as well as Masters and Johnson had commented on a possible correlation between “spontaneous abortion,” the topic we were treating, and female orgasm. This is hardly a theory for the evolution of female orgasm, especially not for ardent adaptationists, and we did not present it as such. indeed, the fact that Lloyd resurrected it in her book only to poke fun would be, in itself, no more than silly.

But this trivial episode may hide more than meets the eye, bespeaking how desperate Lloyd is to find straw men to dispute. Rhetorically, it might work; scientifically, it stinks. Interestingly, this is a practice with which Stephen Gould – of whom Lloyd is an acolyte – was not unfamiliar: trolling for targets of opportunity in the scientific literature, which, once suitably distorted, appear to strengthen one’s case.

On a similar note, after describing and criticizing Baker and Bellis (1993), Lloyd observes parenthetically “There is an unanswered question about how the Baker and Bellis paper ever got published in *Animal Behaviour*, the flagship journal in the field” (pg. 232). Firstly, as someone who – at least in the past – published quite often in *Animal Behaviour*, and has often served as an editorial consultant, I can’t imagine that anyone other than Dr. Lloyd considers it the “flagship journal” in human sociobiology, evolutionary psychology, or human reproductive biology, given that *Animal Behaviour* virtually never publishes articles concerned with *Homo sapiens*. This leads to the “unanswered question” whether Lloyd has ever read this journal that she ostensibly so reveres. More troubling is the snide tone of her remark itself. What, precisely, is she saying? That she posed this question (how did Baker and Bellis ever get published) to the editors of *Animal Behaviour*, which went unanswered? That the question arose in her mind, merely to languish unanswered? That it ought to arise in ours? That there is a conspiracy to publish articles with which Dr. Lloyd disagrees, or of which she disapproves? Readers beware: the evolutionists are coming, the evolutionists are coming!

Since she raised the matter, one might ask the same “unanswered question” concerning Lloyd’s own book. How did it ever get published by one of the flagship publishers of academic and serious trade books? After all, *The Case of the Female Orgasm* merely recycles Symons’ already familiar hypothesis, casts aspersions (some of them *ad hominem*) on legitimate researchers, misrepresents or misconstrues important aspects of evolutionary biology, and is poorly written to boot.

Is there anything worthwhile to be gleaned from *The Case of the Female Orgasm*? Actually, yes. The bibliography is extensive. I am grateful, as well, to Dr. Lloyd for pointing out that the “data” for “uterine upsuck” secondary to female orgasm as a possible contributor to fertilization (Fox, Wolff and Baker, 1970) derive from just one woman. And similarly, for pointing out that the Baker and Bellis (1993) data set on “flowback” is highly skewed and inadequate. But don’t take Lloyd as a statistical adviser: she is so desperate to critique Baker and Bellis that she comes

up with the unique and stunning claim that nonparametric statistics require larger sample sizes than do their parametric counterparts.

Lloyd is also effective in pointing out that even nonadaptive, by-product explanations are legitimately “evolutionary,” despite which they are the Rodney Dangerfields of biology: they don’t get any respect. But even here, she falters, following her now-familiar procedure of creating a straw man by exaggerating and misrepresenting the adaptationist approach, so as to seem oh-so-reasonable in disputing it. Thus, Lloyd claims that according to adaptationist thinking, “no byproduct explanation should *ever* be accepted” (pg. 252, emphasis in original). But no biologist, to my knowledge – even the most adaptationist-addled – disputes the byproduct interpretation for the evolution of male nipples, a perspective that Lloyd has repeatedly invoked in her own book!

I suggest that such explanations are – as well they should be – treated essentially as null hypotheses, against which adaptive interpretations are assessed. In this regard, the Symons’ by-product hypothesis remains, even now, a viable possibility. Konrad Lorenz once remarked that every good scientist should discard at least one cherished hypothesis every day before breakfast. Although I don’t recall him following his own advice, and although I strongly disagree with much – indeed, nearly everything – in Lloyd’s treatment of adaptationism versus byproduct-ism, her warning to evolutionists about the legitimacy of the latter is well taken: we must ask ourselves “whether anything more than lip service is being paid to the foundational assumption from evolutionary biology that alternative, nonadaptive explanations are part of the toolkit of evolutionary theory” (243).

As to the future, I place myself unabashedly in the Panglossian, ardent adaptationist, natural selection-besotted camp. Why? Because it works. As to Lloyd’s camp, she complains about a lack of evidence that “links female orgasms to either improved fertility or to increased birth rates or reproductive rates,” after which she notes “Without it, those who take an adaptationist line are relying on a future promise of such evidence being produced “ (pg. 222). Just so. That is what a research program is: efforts based on the future promise of results. If the promise is not fulfilled, one looks elsewhere ... maybe even to the by-product hypothesis. The final arbiter is fidelity to objective, empirically demonstrable findings. Lloyd, by contrast, reveals much of her agenda when she acknowledges that her sympathy with the “by-product” theory arises because it is the “account with the closest ties to the feminist value of separating definitions of women – including women’s sexuality – from women’s reproductive functions” (pg. 237).

Rather than “continuing to fight for definitions of women that are not based on their reproductive roles” (pg. 237) – Lloyd’s admitted bias – an adaptationist approach would indeed take a close look at the possible “reproductive role” of female orgasm ... not to define or constrain women, or to “privilege” heterosexuality, but for the old-fashioned reason that people do science: to learn something that we might not otherwise know. Thus, I suggest that my fellow adaptationists might profitably devote attention to the following questions (some of which have already been

broached, but all of which warrant more research): Is there evidence that female orgasms correlate with a particular suite of circumstances? With any particular characteristics of a female's partners? Is there any correlation between female orgasm and fertilization? Between female orgasm and successful termination of pregnancy? Between female orgasm and fitness – inclusive no less than Darwinian? What is the role, if any, of female orgasm when it comes to mate selection? What about oxytocin and its male analog, vasopressin? And – perhaps most important – how can we encourage people to prioritize empirical research and legitimate theory-building over the back-biting, ideological excess, and the nonsense of social constructivism so regrettably manifested in *The Case of the Female Orgasm*?

References

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