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When is an orgasm just an orgasm?

Elizabeth Lloyd's *The Case of the Female Orgasm:*

Bias in the Science of Evolution

Despite the many sidelong glances I attracted while reading a book with the words *Female Orgasm* emblazoned on its cover, I found it hard to put this book down once I started reading. As a detailed and informative account of recent research on the subject, Lloyd's valuable contribution makes for a stimulating read. However, for reasons that I outline below, I remain unconvinced of her central thesis that evolutionary accounts of the female orgasm have been *unjustifiably* biased in favor of adaptationist explanations. My central conclusion is that the verdict is very much still out, and given the difficulties involved in obtaining good data on the subject, things will remain this way for some time. Thus, one might question whether this subject matter even has the potential to provide a representative case study of bias in evolutionary science.

Ever since Gould and Lewontin first characterized the adaptationist program as a "Panglossian paradigm" there has been a question about whether this methodology is intrinsically biased. Adaptationists begin with the working assumption that the traits they investigate have some adaptive function or other. This assumption is especially warranted in cases where a trait shows a high degree of complexity or when it is

associated with activities that impact fitness, like mating, the rearing of offspring, or foraging. Successive adaptive hypotheses are then generated and put to the test. “Only after all attempts to do so have failed is [the adaptationist] justified in explaining the [trait] as a product of chance” or as the product of some alternative evolutionary process besides selection (Mayr, 1983: 326). But critics of this methodology object that since adaptive hypotheses are so easy to generate but so difficult to test, viable alternative explanations are systematically ignored. “The problem,” according to Lloyd, “is that this seems to lead in the direction of a methodological rule that *all* traits should be considered adaptations at the end of the analysis. It’s a matter of not taking no for an answer” (Lloyd, 2005: 231 *original emphasis*). Does this criticism accurately portray the way adaptationists generally proceed? Is this research strategy inherently biased against alternative, non-adaptive explanations? Or can this methodology be used to identify non-adaptations when “at the end of the analysis” all of the viable adaptive hypotheses have been discounted?

Lloyd’s case study of adaptationist treatments of the female orgasm aims to shed light on these questions. Her argumentative strategy begins with a review of the relevant sexology literature, with particular emphasis on the apparent variability in the rates with which women achieve orgasm during intercourse. Lloyd proceeds to argue that all of the twenty or so existing adaptationist hypotheses fail to take this and other relevant facts into account. Instead, she claims, these hypotheses often rest on flimsy, methodologically suspect findings. According to Lloyd the only viable hypothesis capable of explaining the available sexology data is Donald Symons’ developmental byproduct account. On this view, female orgasms are like male nipples: although they

can occasionally be put to various, even stimulating applications they do not impact the fitness of their bearers. The alleged reason why female orgasms and male nipples persist in the population is because they are developmentally linked to traits that are under strong directional selection in the opposite sex. Thus, according to Lloyd, a female orgasm is just an orgasm. Not an adaptation. And the fact that most evolutionists have been slow to accept this hypothesis reflects, she claims, a twofold bias. First, evolutionists who research this phenomenon are in the grip of an adaptationist bias, favoring adaptive hypotheses over non-functional alternatives even when the latter enjoy greater empirical support. Second, these researchers are in the grip of an androcentric bias, taking the male orgasm as the exemplar they make a variety of unsupported assumptions about the frequency of orgasm among women and its relationship to intercourse. In what follows I focus on the first of these two alleged biases.

To begin with, I see no basis for contesting Symons' general proposal that the physical structures that enable some females to achieve orgasm share an early developmental trajectory with the ones that produce a similar response in most males. Lloyd makes a connection between such developmental explanations and what Darwin called the "correlation of characters", and she categorizes them as "nonadaptive" or "alternative historical accounts" (Lloyd, 2005: 13-14). However, it bears mention that developmental explanations are *not* alternatives to adaptationist explanations. As Tinbergen noted, these two levels of explanation are in fact complementary. Thus, (to take a far fetched example) if it turns out that male nipples are a secondary adaptation for enhancing a male's bilateral symmetry and, therefore, his physical attractiveness to females, this would not undermine the developmental claim that this trait is linked to one

that serves quite a distinct function in women. Thus, the viability of Symons' developmental hypothesis has little bearing on whether we should accept an adaptationist explanation for the female orgasm. The burden of Lloyd's argument is to show that none of the available adaptationist hypotheses are defensible.

One of the cornerstones of her argument is the alleged fact that orgasm rates during intercourse are highly variable among women. Lloyd assumes that if selection has acted on this trait then (as with men) the vast majority of women should show a fairly uniform tendency to orgasm with intercourse. But since female orgasm rates are (supposedly) all over the map, Lloyd argues that this trait is not an adaptation.

In a moment I will object that the most viable adaptationist explanation for the female orgasm does not, in fact, predict that women will show phenotypic similarity in this trait. Before doing so, however, let us take a page from Lloyd's own book and question the evidential basis for her empirical claim.

To establish the distribution of the rates of female orgasm with intercourse, Lloyd engages in a sort of informal meta-analysis of 32 studies conducted between 1929 and 1995. For each study Lloyd provides the percentages of respondents who claimed, for example, that they experience orgasm "always", "almost always" "sometimes" or "never". As Lloyd points out these studies are strikingly discordant with one another. From her table (see pages 28-34) we find that the proportion of women who claimed to experience orgasm with intercourse "usually" or "always" ranges somewhere between 70% and 30 %. Similarly, the proportion of respondents who claimed to experience orgasm "rarely" or "never" ranges between 60% and 7%. This variability alone should give us pause. As Lloyd notes, the way subjects respond to intimate questions about

their sex lives can be influenced by their social mores or by their levels of comfort in discussing such taboo subjects with a stranger. Thus, results of the interview studies are almost surely biased. Somewhat oddly however, Lloyd argues that the likely bias in these studies actually works in her favor. She suggests that due to the “enormous social pressure” on women to experience orgasm with intercourse, “the surveys are most likely to yield higher rates of orgasm than actually exist” (Lloyd, 2005: 42). This would support her claim that the female orgasm is only loosely tied to reproduction. What Lloyd apparently fails to consider however is that some women might feel uncomfortable reporting a high rate of orgasm in a face-to-face exchange with an unfamiliar male (just imagine saying in your best internal Marilyn Monroe voice: “oh yes sir, I orgasm *all the time* when having intercourse). It seems entirely possible that women are under-reporting their propensity for orgasm due to the sexual dynamics of the interview situation. The conservative conclusion to draw here is that we simply do not know (see Ericksen, 1998, for the various pitfalls associated with the interview technique in sexology research).

An even bigger problem is that only *two* of the available studies employed random sampling techniques. In some cases surveys were conducted in sex clinics on subjects trying to overcome their sexual problems. On these grounds alone some would conclude that it is impossible to draw inferences about the general population. To make matters worse, one of the two genuinely random studies (Laumann et. al., 1994) appears not to even address the *distribution* of orgasm rates with intercourse. Turning again to Lloyd’s table, we find only one entry beside this study indicating that 28.6% of women *always* orgasm with intercourse – nothing is said of the other categories. This leaves us

with just a *single study* (Stanley, 1995) that employed a sampling technique appropriate to address the question in which Lloyd is interested.

Another serious problem with any attempt to summarize these results is that there is no standard metric for comparing the data. What counts as “almost always” in one study might be considered “sometimes” by another researcher. Moreover, some studies lump their results into as few as two categories, while others break their results down into as many as seventeen. Despite these obstacles Lloyd forges ahead with an estimate of the distribution of orgasm rates, which she relies on throughout the remainder of her book. By comparing *different subsets* of the original sample of 32 cases (she doesn’t say which studies she chooses for each statistic, only that in each case it is a subset of the whole) Lloyd calculates that approximately 25% of women orgasm “always” with intercourse, 55% orgasm “more than half the time”, 23% orgasm “sometimes” 33% orgasm “rarely or never” and 5-10% never have an orgasm at all. However, given the uncertainty surrounding these calculations and the questionable compatibility of the studies she summarizes over (not to mention their often flawed sampling techniques) Lloyd’s estimate should be treated with a high degree of skepticism, to say the least.

Although apparently aware of the potential flaws in her meta-analysis Lloyd skirts over these methodological issues rather quickly: “Although there are problems with the methodology used in sex research, any evolutionary account must be compatible with such findings, because they are the only scientific results available” (Lloyd, 2005: 14). Whether these results can, however, be considered *scientific* is very much up for debate. At the very least, I would have liked to see Lloyd come up with a more tentative measure of the variability in female orgasm rates. A range of values for each of the five categories

she mentions - as opposed to such definitive percentages - would have been a more accurate representation of the highly variable data she summarizes

Of course, Lloyd might respond that the lack of good data has not prevented numerous other researchers from developing and defending adaptationist hypotheses about female orgasm. And often these hypotheses overlook the available data altogether. In this respect, perhaps Lloyd's attempt to grapple with the available evidence should be considered an improvement over the majority of the existing literature. But I would be more sympathetic with Lloyd's noble efforts if she were not so imbalanced in her scrutiny of the studies that fail to support her preferred hypothesis. For example, in a nation-wide mail survey on women's sexual behaviour, Baker and Bellis (1993) found that as many as 84% of experienced women enjoy orgasm with intercourse. This study is arguably methodologically superior to many of the ones that appear on Lloyd's A-list: there was no interview procedure, the sample consisted of three and a half thousand respondents, and was drawn from consumers of a magazine geared exclusively towards women. Yet Lloyd objects that "there is an acute difficulty in considering their survey results to be representative of *anything*, given their methodology, the apparently low response rate, and the *likelihood of a skewed sample*" (Lloyd, 2005: 200, *my italics*). This hardly seems fair. As we have seen, the same criticisms could be leveled at studies which Lloyd finds perfectly acceptable.

But enough stone throwing. Let us suppose for argument's sake that Lloyd's proposed distribution for the rates of female orgasm with intercourse is roughly accurate. What implications might this have for adaptationist explanations? As Lloyd argues, this finding would apparently undermine any hypothesis that presupposes strong directional

selection on this trait. I now want to argue however that a subset of the available adaptationist hypotheses – sperm competition accounts – predict a high degree of variability in female orgasm rates.

According to sperm competition hypotheses, the function of the female orgasm is to facilitate conception when a woman is copulating with a high-quality male. The most interesting version of this hypothesis situates the female orgasm as a key move in an evolutionary arms race between the sexes. In many socially monogamous species it is fairly common for females to engage in some strategic mating outside the pair bond. This occurs while the female continues to receive resources from a primary male. Suggested adaptive benefits of this strategy include: reproducing with genetically superior males, obtaining additional material resources, avoiding the threat of infanticide or hostility, or increasing the genetic variability of her offspring. If humans and their ancestors have engaged in this strategy for some time (for evidence of this behaviour in Macaques see Troisi & Carosi, 1998), and if the female orgasm contributes to the likelihood of conception (an important assumption), then selection would presumably favor females who orgasm either when they are paired with a high quality male or when they engage in extra-pair copulation with one. This propensity would show up at the population level in the form of a highly variable orgasm rate, which is just what the available empirical evidence indicates.

Lloyd offers a variety of objections to this line of reasoning. The central focus of her attack is the “upsuck hypothesis” (as it is affectionately known) or the assumption that female orgasm increases the chance of conception. Lloyd’s critique of this assumption is quite detailed and in most cases insightful. She shows that the upsuck

hypothesis has been supported with only weak empirical evidence using questionable measures of flowback and small sample sizes. This is not surprising given the sensitive and intrusive nature of the requisite experiments. Nonetheless, Lloyd's contention is that we should be cautious in placing too much stock in sperm competition accounts until the link between female orgasm and conception is better established, and this is a point well taken.

Where I part company with Lloyd is in her suggestion that the distribution of female orgasm rates with copulation undermines the sperm competition hypothesis. She maintains that,

This mechanism seems to rely on the existence of variability in the female's response to intercourse depending on the quality of the male. But what of the majority of females, who either always have orgasm with intercourse, or who never or rarely have orgasm with intercourse? It seems that the hypothesis by which female orgasm is an adaptation does not apply to them... If orgasm were really selected as an indicator of comparative male quality, why wouldn't all women be such that they sometimes have orgasm with intercourse and sometimes do not?" (Lloyd, 2005: 212).

As I understand it the argument goes like this: If there has been strong, directional selection pressure acting on the female orgasm to facilitate facultative polyandry, then most women should have the capacity to experience orgasm with intercourse and they should report intermediate orgasm rates. However, the available data suggests that as much as a third of all women never or rarely experience orgasm with intercourse. And a

significant proportion of women always do. Therefore, it is unlikely that the female orgasm is an adaptation for facilitating facultative polyandry.

An obvious reply to the fact that some women always experience orgasm with intercourse is that they are paired with high quality mates. Thornhill and colleagues (1995) provide some indirect evidence to this effect. But let's set this possibility aside for the moment. A more serious potential threat to sperm competition hypotheses is that as much as one third of women (if you believe Lloyd's figures) are incapable of achieving orgasm with intercourse. If directional selection has been acting on this trait, Lloyd asks, shouldn't it be more prevalent?

But why assume that the selection pressure on this trait has been directional? Facultative polyandry is a reproductive strategy that is very much at odds with the genetic interests of a female's primary partner. As this trait increases in frequency in the population there is increased selection pressure on males to adopt counter strategies. For example, males ought to become more vigilant or prone to withhold resources at the slightest whiff of cuckoldry. The rise of these defensive strategies would in turn generate an opposing selection pressure against females who are facultatively polyandrous, causing the frequency of this trait to decrease. However, once the frequency of this trait has dropped below a certain point, there would be less pressure on males to be vigilant, and the trait would once again increase in the population. In short, the evolutionary expectation is that such "socially hostile" traits will undergo regular fluctuations as a result of *frequency dependent* selection. If this process has been occurring, a haphazard sample of the general population at different time intervals would show a high degree of

variability in the rates of orgasm with intercourse. So the data that Lloyd cites does not rule this hypothesis out.

Studies on rates of facultative polyandry in birds shed further light on these issues. Although this trait is almost certainly under selection pressure, there is considerable variation in the rates of polyandry both among populations within the same species as well as within particular populations at different times. For example, in willow warblers one genetic study reported 0% extra pair offspring while another reported as many as 50%; and in redwing blackbirds the percentage of broods with extra-pair offspring varied between 17 and 35 percent over a five year period (Petrle & Lempenaers, 1998). One of the factors thought to influence variation in this trait is whether females are seeking “good genes” as opposed to simply maximizing the genetic diversity of their offspring. If the benefits to a female come in the form of good genes, the selective advantage of this strategy will depend on the degree of genetic variability among males in the population. Genetic variability tends to increase with population size. Thus, larger populations will tend to have higher rates of facultative polyandry than smaller sized populations. This generalization might extend to human societies. In large, industrialized societies where there is a greater amount of genetic diversity one would expect the rates of facultative polyandry to be higher than in smaller societies, and the corresponding selection on facultative orgasm with copulation should increase. Of course, one must allow sufficient time in order for the effects of such selection to become detectable at the population level. However, a careful study of how the fluctuations in both extra-pair copulation rates as well as females’ propensity to experience orgasm with intercourse seems like the next logical step in testing these hypotheses.

Where does this leave us with respect to Lloyd's contention that an unwarranted adaptationist bias has influenced evolutionists to overlook the best supported explanation of the female orgasm? The first thing to note is that however well supported it may be, the developmental linkage hypothesis is not in itself an alternative to the available adaptationist hypotheses. To show that the female orgasm is most likely not an adaptation would require good evidence contradicting the upsuck hypothesis which is altogether absent. Nor does the scientifically questionable data that Lloyd cites on the variable rates of orgasm with intercourse contradict sperm competition hypotheses (even when we lower our standards of scientific evidence beyond a level that Lloyd herself finds acceptable in certain other contexts). In sum, although my negative commentary fails to do justice to what is undeniably a fascinating and informative book, I cannot help but question Lloyd's choice of subject matter. If one is interested in determining whether the adaptationist program is inherently biased, then surely there are topics for which more reliable (but perhaps less interesting) data are available.

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