

Book Review

Women's Evolutionary Enigmas

A review of David P. Barash and Judith Eve Lipton, *How Women Got Their Curves and Other Just-So Stories*. Columbia University Press: New York, 2009, 210 pp., US\$29.95, ISBN 00978-0-231-14664-7 (hardcover).

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Just-so stories can be delightful. Some are written for children, such as Rudyard Kipling's imaginative accounts of how the camel got his hump, or the elephant his trunk. Some are written for adults. Few of the latter match the humor and wit demonstrated by Don Symons in his 1983 exposition of the Flying On Air Theory (FLOAT), which proposes an aerial phase in human evolution. According to FLOAT, many features of human anatomy and psyche evolved as specialized flight adaptations, with bipedalism being a landing device, breasts serving as handholds for infant riders, and the penis reflecting the male's role as a parachute for infants learning to fly. Flying may have become dysfunctional, Symons suggests, owing to "the extremes of sexual inventiveness possible to advanced, airborne hominids, resulting in collisions, entanglements, and other copulatory aerial disasters" (p. 28). FLOAT is not lacking in corroborating evidence: just think of the Judeo-Christian story of the "fall" of man, for instance, or the continuing appeal of stewardesses on the male imagination.

Most just-so stories are, however, not meant to be funny at all. In *The Naked Ape*, Desmond Morris (1967) explained female breasts as visual reminders of the buttocks, which in the course of human evolution would have encouraged face-to-face mating, which in turn would have promoted pair bonding. He was not kidding. Neither was Elaine Morgan (1982) when she claimed that humans evolved as aquatic apes, among whom breasts served as flotation devices for babies. These are just-so stories as commonly perceived: empty evolutionary tales. They figure, along with similar tales as well as more convincing accounts, in *How Women Got Their Curves and Other Just-So Stories*, the latest book by the evolutionary biologist David Barash and the psychiatrist Judith Eve Lipton. The book's title might be potentially misleading, for it suggests a negative assessment of evolutionary explanations. Nothing could be further from the authors' minds: they want to upgrade the epithet "just-so story," by pronouncing it to be the cradle of science. Scientific inquiry, they contend, generally begins as pure speculation and, hence, as a just-so story. Ideally this leads to fact-based research, in which case the narrative stops being a just-so story, and becomes science. This attempt at cutting the ground from under the critics' feet only works, I think, if one is willing to redefine just-so story-spinning as mere

heuristics – in which case we are no longer talking about just-so stories. Essential to just-so stories is that they are presented as full-blown explanations, in spite of their lack of supportive data and empirical validation.

Maybe, however, the title just serves to encourage a broad audience to crack this book. It has, after all, a playful and accessible ring to it, and as such it covers the authors' approach perfectly. Indeed, *How Women Got Their Curves* is, for the most part, a pleasure to read. It is engagingly written, and its authors are brimming with enthusiasm about their subject matter: the human female body and its unsolved evolutionary mysteries. Why do women menstruate? Why do they have breasts and other curves? Why do they conceal their ovulation and their time of maximum fertility? Why do they experience orgasm and undergo menopause? Barash and Lipton pleasantly chat away about the various hypotheses that have been proposed by evolutionary scientists, including themselves, to explain these traits, weighing the evidentiary pros and cons, and pointing out which scenarios they deem more plausible than others.

Inevitably, not everyone will share their assessment of the relative merits of each account. A case can be made, for instance, that the finding that estrus was not lost in the course of human evolution, and that ovulation in women is not so concealed after all, should have warranted more attention (see Thornhill and Gangestad, 2008). It might also be argued that Barash and Lipton take deceptive signaling-accounts of female curvaceousness too seriously. On these accounts, prominent nonlactating breasts and other female fatty deposits may well have started off as a way of storing calories important for pregnancy and lactation, which would have attracted male interest, but they evolved into deceptive signals, luring males into assuming more female competence in lactating and giving birth than was the case. According to the “provisioning hypothesis,” for instance – which is assumed by Barash and Lipton to be their own finding, but see Miller (1995) for a nearly identical hypothesis –, breasts are lactation mimics. Ancestral men may have been predisposed to provide women with enlarged breasts with extra food, for originally these would have signaled lactation (or pregnancy). Men would have found indicators of successful lactation attractive, for they promised well-fed babies. Well-provisioned women would have been more successful, which set the stage for women deceiving men into believing that they were lactating, by evolving large permanent breasts. As argued by Thornhill and Gangestad (2008), however, deceptive signaling theories of breasts and other female ornaments are unlikely to be correct, for empirical as well as theoretical reasons. For one, female ornaments decline in attractiveness with age and parity, as is to be expected if they are honest signals of reproductive value. Also, they reflect estrogen levels and hence, fertility levels. There is, moreover, suggestive evidence that individual variation in attractiveness covaries with condition and general health. On a more theoretical level, ancestral males who were able to discern true quality in women would have been favored by selection over males who were fooled by false signals. Deception can occur, but a system has to be largely honest in order to be evolutionarily stable.

These are only minor quibbles. My one serious critique of this book concerns the authors' departure from the usual standards of scientific discussion in the chapter on the female orgasm.

In their defense of an adaptive account of this trait, they resort to fallacious arguments in order to discredit Symons's 1979 byproduct hypothesis, which explains female orgasmic capacity as an embryological byproduct of selection on the male orgasm. The reader is misled into believing that if the female orgasm is a byproduct, it is nothing but an incomplete, irrelevant trait, and that the byproduct hypothesis reduces women to mere tag-alongs to men. She is taught that evolutionists are usually only feigning to take nonadaptive accounts of traits seriously, and that it is okay to conflate scientific explanations and moral and political pronouncements. It is suggested to her that advocates of a byproduct view fit in one of four categories: those who are only seeking to "demonstrate their scientific bona fides" (p. 126), those who have been misled, sexists, and feminists (who might adhere to the byproduct hypothesis because it more readily separates the significance of female orgasm from heterosexual intercourse). She risks coming away with the impression that maybe an evolutionary approach is not that scientific after all, if its practitioners so easily flaunt the principles of a scientific attitude, such as avoiding a priori conclusions and upholding a spirit of disinterested discussion.

Why do Barash and Lipton resort to false arguments on those pages? If you have got a good case, you don't need to do this. This seems exactly to be the problem: such a case is missing for adaptive accounts of women's orgasm, as has been convincingly demonstrated by Elisabeth Lloyd in her 2005 book *The Case of the Female Orgasm*. (It is, of course, Lloyd whom the authors are referring to when mentioning feminists driven by their own agenda, despite any indication that she proceeded in a non-even-handed way. The insinuation seems to be a residue of Barash's 2005 vitriolic attack on Lloyd's book.) Based on meticulous research, Lloyd demonstrated that the 20 adaptive explanations of the female orgasm that have been advanced, are seriously flawed. She made a persuasive case that a bias toward adaptive explanations has lead evolutionists to uncritically accept deficient evidence and to unduly neglect Symons's byproduct view, whereas this account best fits the evidence. As has been pointed out by Puts and Dawood (2006), Lloyd's definition of an adaptation is misguided, because it assumes that adaptations increase current fitness. This, however, does not make her critiques any less pertinent: in nearly all of the 20 adaptive accounts of the female orgasm, researchers misrepresented the evidence, made unsupported assumptions about the female sexual response, and/or based themselves on studies that are riddled with methodological problems. It raises the question, Lloyd notes, "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" (2005, p. 243).

Many examples of byproduct explanations of complex human traits or behaviors can be given, such as Palmer (1991) on rape, Daly and Wilson (1988) on murder, and Pinker (2002) on art. Nonadaptive explanations clearly are *not* being considered as inferior by many prominent scholars. Yet Barash and Lipton confirm Lloyd's impression with a vengeance. They unabashedly look down upon nonadaptive accounts of the traits studied by evolutionists and generalize this attitude to the entire evolutionary community. Nonadaptive options are described as a "mantra" that researchers "routinely pay lip service to," but "in fact they don't usually take

these options very seriously” (pp. 125-126). The authors hereby ignore the basic principle of adaptationism: that it is a method, not a preconceived scheme of things. It seeks to find out which traits have been selected and, if selected, whether they have been selected directly (adaptations) or indirectly (byproducts). It does not make any a priori assumptions (Thornhill and Gangestad, 2008). They also ignore Williams’s dictum that adaptation is a special and onerous concept that should be used only when it really is necessary (Williams, 1966). In the case of the female orgasm, invoking adaptation is far from necessary. Barash and Lipton’s claim that the trait “shows every sign of having been structured and fine-tuned by evolution” (p. 127) is emphatically incorrect. Adaptive accounts of the female orgasm face substantial evidential hurdles, as expounded by Symons (1979) and Lloyd (2005). A major one of these is the unreliability and inefficiency with which women achieve orgasm by intercourse. Copulation reliably yields orgasm for only about 25% of women, and one third rarely or never have orgasm this way. The high variability within women of the occurrence of coital orgasm, together with the high variability concerning the ease with which various women experience orgasm at all, with between 5% to 10% never experiencing it no matter what method is used, strongly suggests that the trait has been under little or no selection pressure. Consistent with a byproduct account, Wallen and Lloyd (2008) found that variability in clitoral length is three times as great as variability in its male homologue, penile length. Knowing that variation in clitoral structures correlates substantially with women’s orgasms, this finding again strongly suggests that selection has not operated to shape female genital anatomy to promote orgasm.

Regarding intercourse’s inefficiency at inducing orgasm in women, Barash and Lipton write that “evolution isn’t always maximally efficient,” and that it “has to work with what is available, not with what might be optimal” (p. 129). It is a long stretch, however, from the actual distribution of female coital orgasm and female orgasmic capacity to an optimality scenario. If, to borrow an example from Symons (1980), egg retrieval among greylag geese would occur as sporadically and irregularly as women’s orgasms, it is most unlikely that it would be considered an adaptation. Indeed, if evolution’s lack of maximal efficiency would be a legitimate argument for regarding traits as adaptations, the whole living world might be seen as evincing nothing but adaptive bliss.

Barash and Lipton’s favored adaptive hypothesis does not work either. It holds that women’s orgasm is *designed* to be hard to get, because it is a mate-selecting device: a conditional response to high-quality males (NOTE 1). As argued by Lloyd (2005, 2006), this account cannot explain those women either always having orgasm with intercourse, or rarely or never having orgasm with intercourse. Under this proposed hypothesis, we would expect to find all women to sometimes have orgasm with intercourse and sometimes not – something which is definitely not the case. The distribution of female orgasmic capacity (by any means), moreover, is nearly flat, with a slight peak at the no-orgasmic end. In Lloyd’s words, “the incompatibility of the flat distribution curve with the mate-choice hypothesis is fatal” (2006, p. 606).

The mate-selection hypothesis, moreover, depends on the assumption that female orgasm promotes fertilization by sucking in sperm; an assumption that does not have a shred of evidence

to it (Lloyd, 2005; Wallen, 2006). Based on this upsuck theory and the mate-choice hypothesis, Thornhill, Gangestad, and Comer (1995) hypothesized and purportedly demonstrated that women are more likely to experience orgasm with more symmetric (and hence genetically superior) men than with less symmetric men. As argued by Lloyd (2005), however, there are considerable statistical problems with this paper. Montgomerie and Bullock (1999) failed to replicate this purported association. Shackelford et al. (2000) did provide a replication, using several design variations. A problem with this study, however – as with most studies of female coital orgasm, such as Thornhill, Gangestad, and Comer (1995), is that participants were not asked how orgasm with intercourse was achieved: was extra manual stimulation required or not? This qualification would seem to be crucial. The hypothesis that female orgasm evolved as a facultative response to genetic quality in males implies, after all, the female having an orgasm due to the mechanics of intercourse, not to extra manual stimulation either by herself or her partner. In this respect the Shackelford et al. study is even more problematic than the Thornhill et al. study, for participants were asked whether they had an orgasm “just before, just after, or during” the last time they had intercourse with their partner. As the definition of coital orgasm implies that it is achieved *during* coitus, not before or after it, what Shackelford et al. have studied is female orgasm during sex, not during intercourse. Their finding that it is achieved more often with more attractive males might have an alternative explanation. Physically attractive women have a strong preference for facial masculinity and facial symmetry in a long-term partner (Little et al., 2001; Penton-Voak et al., 2003). Hence, attractive men will probably have attractive women as long-term mates. Women who perceive themselves as attractive report greater comfort with sex, more sexual activity, and more orgasm (Ackard, Kearney-Cooke, and Peterson, 2000).

In sum, Barash and Lipton have little reason to treat the byproduct account of the female orgasm as condescendingly as they do. It has much more to it than a mere “patina of bio-logic” (p. 132). Recently Symons (2008) proposed two more compelling arguments against the view that women’s orgasm serves to promote conception by enhancing sperm retention. Firstly, it would be exceedingly strange if a series of muscle contractions in a female body that mimic the muscle contractions of ejaculation in a male body would turn out to be well designed to achieve an entirely different goal: that of retaining sperm. Secondly, if women were more likely to conceive if ejaculation occurs close to the time of their orgasm, this would imply intense selection pressures on male anatomy, physiology, and psychology to have his and her orgasm co-occur. Again, evidence that selection has acted on men in this way is lacking entirely.

My complaints regarding the chapter on the female orgasm notwithstanding, I liked this book, and I would recommend it to anyone interested in the mysterious nature of women’s biology. It is highly informative, written with humor and passion, and it gives the reader a good idea of the diversity of theories that have been proposed to explain that “riddle wrapped in a mystery inside an enigma:” the female body.

Notes

1. According to Barash and Lipton, this hypothesis was first suggested by Barash in *The Whisperings Within* (1979). This reference is incorrect. The book develops the hypothesis that the female orgasm evolved to keep the male nearby, not that it is a mate-selecting device.

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