

ELISABETH A. LLOYD

PRE-THEORETICAL ASSUMPTIONS IN  
EVOLUTIONARY EXPLANATIONS OF  
FEMALE SEXUALITY

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My contribution to this Symposium focuses on the links between sexuality and reproduction from the evolutionary point of view.<sup>1</sup> The relation between women's sexuality and reproduction is particularly important because of a vital intersection between politics and biology — feminists have noticed, for more than a century, that women's identity is often defined in terms of her reproductive capacity. More recently, in the second wave of the feminist movement in the United States, debates about women's identity have explicitly included sexuality; much feminist argument in the late 1960's and early 1970's involved an attempt to separate out an autonomous female sexuality from women's reproductive functions.

It is especially relevant, then, to examine biological arguments, particularly evolutionary arguments, to see what they say about *whether* and *how* women's sexuality is related to reproduction. We shall find that many evolutionary arguments seem to support the direct linking of female sexuality and reproduction. Yet I will argue that this support is not well-grounded. In fact, I think evolutionary explanations of female sexuality exemplify how social beliefs and social agendas can influence very *basic* biological explanations of fundamental physiological processes. In this paper, I shall spend some time spelling out a few examples in which assumptions about the close link between reproduction and sexuality yield misleading results, then I shall conclude with a discussion of the consequences of this case study for issues in the philosophy of science.

The fundamental problem is that it is simply *assumed* that every aspect of female sexuality should be explained in terms of reproductive functions. But there is quite a bit of biological evidence that this is an empirically incorrect assumption to make. This raises the question of

why autonomous female sexuality, distinct from reproductive functions, got left out of these explanations. I shall ultimately conclude that social context is playing a large and unacknowledged role in the practice of this science.

Perhaps the notion of the potential independence of female sexuality and reproduction may be unclear: I suggest thinking in terms of two distinct models, one in which all basic aspects of sexuality are *explained* in terms of reproduction, and the other in which sexuality is seen as an autonomous set of functions and activities, which are only *partially* explained in terms of reproductive functions. The difference may seem minor, but the two models have significantly disparate consequences when used in scientific explanation.

Let us begin with a classic and wide-spread model representing the hormonal determination of sexual behavior. In this model, female animals are only willing to have sexual intercourse when they are fertile — their sexual interest and activity are completely hormonally controlled. Typical and familiar examples of this type of set-up include rats, dogs, and cats. When these animals are in estrus, they are willing and eager to mate, otherwise not. Technically, estrus is defined hormonally — that is, estrus is a particular phase of the menstrual cycle, in which the animal is fertile, and certain hormone measures are very high. This model embodies a *very tight* link between sexuality and reproduction: female sexuality functions completely in the service of reproduction.

Some interesting problems arise, however, in the application of this hormonally deterministic picture to human and non-human primate behavior.

First of all, although estrus is biologically defined as a hormonal state, it is very common for estrus to be defined *operationally* as the period in which “the female is willing to participate in sex”. In one species, the bonobos, this behavioral definition led to the comic conclusion that this species is in estrus 57–86% of the time.<sup>2</sup> Notice that identifying estrus in this manner amounts to an *enforcement* of the belief that sexual behavior is tightly linked to reproduction. It becomes impossible even to *ask* whether these primates have an active sexual interest outside of their peak hormonal periods.

It turns out that when independent studies are made, sexual activity is not confined to the fertile phase for a number of non-human

primates, including rhesus monkeys, several species of baboons, and common chimpanzees.<sup>3</sup> Social factors such as partner preferences can be as influential as hormonal factors in regulating sexual behavior in several of these species.<sup>4</sup>

Female homosexual activity provides a good test for the assumed dependence of female sexuality on hormonal status. In addition, homosexual behaviors are clearly independent of reproduction *per se*, and might be interpreted as an indicator of an autonomous female sexuality. It turns out that female homosexual activities, which are widely observed in non-human primates, seem to be *independent* of the hormonal status of the participants. This independence has led some researchers to ignore such behaviors, or to declare that they are not, in fact, sexual. For example, pygmy chimpanzee females are commonly observed engaging in “genito-genital rubbing” (called “GG rubbing”) in which two females hold each other and “swing their hips laterally while keeping the front tips of vulvae, where the clitorises protrude, in touch with each other.”<sup>5</sup> Kano argues that this behavior is not sexual, because non-human primates can only be “sexual” during estrus; the fact that pygmy chimps engage in GG rubbing outside of estrus, claims Kano, itself “suggests that this behavior does not occur exclusively in a sexual context, but has some other social significance.”<sup>6</sup> Generally, some caution about the interpretation of apparently sexual behaviors is appropriate; the misunderstanding of many dominance behaviors as sexual ones plagued primatology in its first decades. At stake in this case, however, is the very *possibility* of hormonally independent female sexuality. The issue was resolved in 1984, when Mori, using a detailed study of statistical relations among behaviors, concluded that GG rubbing was, in fact, sexual behavior, since the same cluster of behavior surrounded both mating and GG rubbing.<sup>7</sup>

A more blatant example of researcher bias typing reproduction and female sexuality tightly together appears in an experiment being done on female orgasm in stump-tail macaques. The original studies on female macaque orgasm, completed in the 1970’s, documented female orgasm in the context of female homosexual mounting — that is, one female mounts another female, and stimulates herself to orgasm.<sup>8</sup> One very interesting result of these studies was the finding that the mounting, orgasmic female was *never* in estrus when these orgasms occurred. This

is a provocative result for several reasons. First, according to the hormonal determinism model, female macaques are not supposed to be interested in any sexual activity outside of estrus; Second, these same female macaques *never* evidenced any sign of orgasm when they were participating in heterosexual coitus. A later study of the same species documented the same basic patterns, with the exception that four out of ten females in the group seemed, occasionally, to have orgasm during heterosexual coitus.<sup>9</sup>

I was surprised, therefore, when I spoke with a researcher who was working on the evolution of female orgasm in stump-tail macaques.<sup>10</sup> He described his experimental set-up to me with some enthusiasm: the females are radio-wired to record orgasmic muscle contractions and increased heart rate, etc. This sounds like the ideal experiment, because it can record the sex lives of the females mechanically, without needing a human observer. In fact, the project had been funded by the NIH, and had presumably gone through the outside referee and panel reviews necessary for funding. But then the researcher described to me the clever way he had set up his equipment to record the female orgasms — he wired up the heart rate of the *male* macaques as the signal to start recording the *female* orgasms. When I pointed out that the vast majority of female stump-tail orgasms occurred during sex among the females alone, he replied that yes, he knew that, but he was only interested in the *important* orgasms.

Obviously, this is a very unfortunate case. But it is not an isolated incident. Observations, measurements, interpretations, and experimental design are all affected by the background assumptions of the scientists. There is a pervasive and undefended assumption that female sexuality in non-human primates is tightly linked to reproduction. I would like now to explore briefly the situation regarding human beings.

#### HUMAN CASES

In most of the literature on the evolution of human sexuality, much attention is paid to the distinct attributes of human beings. The continual sexual “receptivity” of the human female is contrasted with the (supposed) strict hormonal restrictions on sexual activity in non-human animals. Human beings are supposed to be uniquely adapted to be

sexually free from hormonal dictates, the possessors of a separate and self-constructed sexuality. When it comes to evolutionary explanations of women's sexuality, though, the tight connection between reproduction and sexuality remains firmly in place.

To continue with the hormonal theme, we can begin by looking at beliefs about the distribution of female sexual interest during the menstrual cycle. Many researchers, in evolutionary biology, behavior, and physiology, have *deduced* that it must be the case in human females that peak sexual interest and desire occur at the same time as peak fertility. This conclusion is a simple extension of the hormonal determinism model from mice and dogs. While this may have the ring of a reasonable assumption, it is not supported by the clinical literature. Kinsey, for example, found that 59% of his female sample experienced patterns of fluctuation in their sexual desire during their cycle — but only 11% experience a peak of sexual desire in mid-cycle, when they are most likely to be fertile.<sup>11</sup> More recently, Singer and Singer, in a survey of studies, found that only 6–8% of women experience an increase in sexual desire around the time of ovulation. Most studies found peaks of sexual desire right before and after menstruation, when the woman is almost invariably infertile.<sup>12</sup>

Hence, the majority of evidence supports a picture in which female sexual interest and activity is clearly *decoupled* from her reproductive state. Sexual interest and motivation is highest when the woman is least likely to conceive. Unfortunately, a number of researchers working in the area of the evolution of sexuality have not taken this on board, and continue to assert that peak sexual desire *must* be around the time of ovulation — otherwise it would not make any sense.

This “making sense” is precisely what I'm interested in. According to these researchers, female sexuality doesn't *make sense* unless it is in the service of reproduction. There is no scientific defense offered for this assumption. A similar assumption is also present in the evolutionary explanations offered for female orgasm.

I have examined thirteen stories for the evolution of human female orgasm, and all except one of these stories assume that orgasm is an evolutionary adaptation. That is, they assume that orgasm conferred a *direct selective* advantage on its possessors, and that is how it came to be prevalent among women. The most common general formula for

explaining the evolution of human female orgasm is through the pair-bond. Here, the pair-bond means more-or-less monogamous heterosexual coupling, and it is argued that such coupling increases the potential reproductive success of both parties through mutual cooperation and assistance with rearing offspring. The idea is that the male and the female in the pair bond provide mutual support to one another, and assist each other in rearing offspring, and that offspring raised under these conditions will tend themselves to have higher survival and reproductive success than those raised under other circumstances.

Hence, pair-bonding is seen as an adaptation in the evolutionary sense — it exists *because* it confers better chances of surviving and reproducing to those who display the trait. Under the assumption that pair-bonds are adaptive, frequent intercourse is also seen as adaptive, since it helps “cement the pair bond”. And this is where orgasm comes in. Orgasm evolved, according to these pair-bond theorists, because it gave the female a reward and motivation to engage in frequent intercourse, which is itself adaptive, because it helps cement the pair bond. A number of different theorists have developed permutations of this basic story, but it remains the most widely accepted evolutionary story for female orgasm.<sup>13</sup>

Now, there is a glaring problem with this story — It assumes that intercourse is reliably connected to orgasm in females. All of the available clinical studies on women’s sexual response indicate that this is a problematic assumption. Somewhere between 20–35% of women always or almost always experience orgasm with unassisted intercourse.<sup>14</sup> I should add that this figure is supported by what cross-cultural information exists.<sup>15</sup> This figure is very low, and it is especially striking given that somewhere around 90% of women do experience orgasm. Furthermore, about 30% of women *never* have orgasm with intercourse — this figure is taken from a population of women who do have regular intercourse, and of whom almost all are orgasmic.<sup>16</sup> What this means is that *not* to have orgasm from intercourse is the experience of the majority of women the majority of the time. Not to put too fine a point on it, if orgasm is an adaptation which is a reward for engaging in frequent intercourse, it does not seem to work very well.

Obviously, this observation does not rule out the possibility that there is some selective advantage to female orgasm, but the salient

point is that *none of these pair bond theorists even address this problem*, which I call the orgasm-intercourse discrepancy. Rather they simply assume that when intercourse occurs, so does orgasm.<sup>17</sup>

In general, the association of intercourse with orgasm is relatively unproblematic among males. Hence, what is being assumed here is that female sexual response is like male sexual response to the same situation. There is little or no awareness, among the pair-bond theorists, of the orgasm-intercourse discrepancy, in spite of the fact that they cite or refer to the very studies which document this fact, including Kinsey's 1953 report on women's sexual response.

There is one obvious and understandable reason for this slip. They are, after all, trying to explain orgasm through evolutionary theory, which involves showing that the trait gave a reproductive advantage to its owner. It's easy to see how the equation of reproduction through intercourse and orgasm went by unnoticed. Nevertheless, this case does illustrate the main thesis, that female sexuality is unquestioningly equated with reproduction, and with the sort of sex that leads to reproduction.

There is another intriguing line of argument for the adaptive value of female orgasm, which was first published by Desmond Morris in 1967, though Shirley Strum tells me that Sherwood Washburn was teaching this in his classes at Berkeley earlier. Morris claimed that orgasm had a special function related to bipedalism (that is, walking on our hind legs), because it would increase chances of fertilization. Here again we have the direct link between female sexuality and reproduction.

It does this in a rather special way that applies only to our own peculiar species. To understand this, we must look back at our primate relatives. When a female monkey has been inseminated by a male, she can wander away without any fear of losing the seminal fluid that now lies in the innermost part of her vaginal tract. She walks on all fours. The angle of her vaginal passage is still more or less horizontal. If a female of our own species were so unmoved by the experience of copulation that she too was likely to get up and wander off immediately afterwards, the situation would be different, for she walks bipedally and the angle of her vaginal passage during normal locomotion is almost vertical. Under the simple influence of gravity the seminal fluid would flow back down the vaginal tract and much of it would be lost. There is therefore . . . a great advantage in any reaction that tends to keep the female horizontal when the male ejaculates and stops copulation. The violent response of female orgasm, leaving the female sexually satiated and exhausted, has precisely this effect.<sup>18</sup>

Morris' view is in turn based on his understanding of physiological

response — he says earlier . . . “after both partners have experienced orgasm [in intercourse] there normally follows a considerable period of exhaustion, relaxation, rest and frequently sleep.”<sup>19</sup> Similarly, he claims, “once the climax has been reached, all the [physiological] changes noted are rapidly reversed and the resting, post-sexual individual quickly returns to the normal quiescent physiological state.”<sup>20</sup>

Now let us refer to the clinical sex literature, which is cited by Morris and by others. According to this literature, the tendencies to states of sleepiness and exhaustion following orgasm, are, in fact, true for men but not for women. Regarding Morris’s claim that the physiological changes are “rapidly reversed”, this is also true for men but not for women — women return to the plateau phase of sexual excitement, and not to the original unexcited phase, as men do. This was one of the most noted conclusions of Masters and Johnson, whose picture of sexual response was enthusiastically adopted by Morris — but, it seems, only in part.<sup>21</sup>

In fact, Masters and Johnson publicized an interesting and important difference between men’s and women’s sexuality, and that is the capacity of many women to have more than one orgasm without a significant break. 47% of the women in Hite’s survey did not feel that a single orgasm was always satisfying to them, and many women wanted more, some as many as 15–25. If, at this point, you are concerned about Hite’s bad reputation as a statistician and researcher, I’d like to point out that many of Hite’s findings in that first study, published as the Hite report, were consistent with Kinsey’s figures, and the Kinsey reports are considered, to this day, and in spite of any problems they might have, to be the best general studies ever done on the topic of women’s sexuality.<sup>22</sup> Masters and Johnson contrast the ability of many women to have five or six orgasms within a matter of minutes with the adult male’s usual inability to have more than one orgasm in a short period.<sup>23</sup> This female ability is linked to the fact that, following orgasm, women do not return to the pre-aroused state, as men do, but instead to the plateau phase of excitement.

Hence, Morris’s story is in trouble. He claims that the physiological changes are rapidly reversed for women as well as for men. He also neglects the sizable percentage of women who are not satisfied by a single orgasm. Given the documented tendency in men to sleep and



exhaustion following a single orgasm, it's not at all clear that a female desire to have orgasm wouldn't have exactly the opposite effect from that described by Morris — perhaps the woman would jump right up and cruise for a little more action at precisely the time when the sperm are most likely to leak out.

Actually, another serious problem with this story was recently pointed out by Shirley Strum, an expert on baboon behavior.<sup>24</sup> Supposedly, the selection pressure shaping female sexual response here is the potential loss of sperm that is threatened because human beings walk on two legs, and because the vaginal position is thus changed from horizontal to almost vertical. One would think, then, that our relatives walking on four legs would be protected against this occurrence, for anatomical reasons. But Strum says that immediately following intercourse, female baboons like to go off and *sit down* for ten or fifteen minutes. When they get up, she says, they inevitably leave a visible puddle of semen on the ground. Perhaps, then, the loss of semen is not the serious evolutionary challenge that Desmond Morris and others take it to be.

#### SUMMARY

I claim that social agendas appear in these stories through the obliteration of any female sexual response that is independent from her function as a reproducer. Autonomous, distinct female sexual response just disappears.

In these explanations women are presumed to have orgasms nearly always with intercourse, as men do. Women are presumed to return to the resting state following orgasm, as men do. One could object that Morris is a relatively easy target, so I will offer the following tidbit in defense of my analysis. Gordon Gallup and Susan Suarez published, in 1983, a technical discussion on optimal reproductive strategies for bipedalism, and took up Morris' anti-gravity line of argument. They argue that orgasm would be adaptive because it would keep the woman lying down, and hence keep the semen from escaping. In the context of these paragraphs on female orgasm, they state, "it is widely acknowledged that intercourse frequently acts as a mild sedative. The average individual requires about five minutes of repose before returning to a

normal state after orgasm.”<sup>25</sup> The scientific reference they offer for this particular generalization is Kinsey 1948, which is, in fact, exclusively on *male* sexual response. In other words, this “average individual” which figures in their story about female orgasm, is, in fact, explicitly male.

#### AN ALTERNATIVE EXPLANATION

Donald Symons, in his book *The Evolution of Human Sexuality* (1979), argues that female orgasm is not an adaptation. He develops a story parallel to the one about male nipples — female orgasm exists because orgasm is strongly selected in males, and because of their common embryological form, women are born with the potential for having orgasms, too.<sup>26</sup> Part of the story, then, is that orgasm is strongly selected in males; this is fairly plausible, since it is difficult for male mammals to reproduce without ejaculation, which requires a reflex response in certain muscles. These muscles are, in fact, the same (homologous) muscles that are involved in female orgasm. It is also significant that the intervals between contractions in orgasm is 4/5 of a second in both men and women. This is considered evidence that orgasm is a reflex with the same developmental origin in both sexes.

One of the consequences of Symons’ theory is that it would be expected that similar stimulation of the clitoris and penis would be required to achieve the same reaction or reflex response. This similarity shows especially in the figures on masturbation. Only 1.5% of women masturbate by vaginal entry, which provides stimulation similar to the act of intercourse; the rest do so by direct or indirect stimulation of the clitoris itself.<sup>27</sup> Also, on the developmental theory, one would *not* expect similar reactions to intercourse, given the differences in stimulation of the homologous organs.

Finally, this theory is also supported by the evidence of orgasm in non-human primates. The observed orgasms occur almost exclusively when the female monkeys are themselves mounting other monkeys, and not during copulation. On the non-adaptive view of orgasm, this is almost to be expected. There, female orgasm is defined as a potential, which, if the female gets the right sort and amount of stimulation, is activated. Hence, it is not at all surprising that this does not occur often during copulation, which in these monkeys includes very little, if any,

stimulation of the clitoris, but occurs rather with analogous stimulation of the homologous organs that they get in mounting.

Symons' proposal, which I found very powerful and plausible, has been sharply criticized by a number of feminists. For instance, a leading feminist sociobiologist, Sarah Blaffer Hrdy, claims that this non-adaptive explanation is dismissive of female sexuality.<sup>28</sup> Similarly, Mina Caulfield accuses Symons of denying the "significance of female pleasure."<sup>29</sup>

I view these criticisms as misguided, because they are based on the assumption that *only* adaptive explanations can provide for the significance of a trait. But why should we believe this? Musical and singing ability are not adaptations, but they are very important to human culture and human life. One must have adopted the idea, not merely that 'what is natural is good', but further, that 'only what is adaptive is good.' The evolutionarily derivative role of female orgasm implies absolutely nothing about its importance unless you are a committed adaptationist. Finally, I wonder why these feminists are so eager to get orgasm defined as an adaptation — several of the serious evidential problems with evolutionary explanations about female orgasm arose, I have argued, from making an easy connection between sexuality and reproduction.

I would like to just mention a possible alternative interpretation. The conclusion that orgasm is not an adaptation *could* be interpreted as emancipatory. After all, the message here is that orgasm is a freebie. It can be used in any way that people want; there is no 'natural' restriction on female sexual activities, nor is there any scientific ground for such a notion. Under the developmental view, the constraints are loosened on possible explanations about women's sexuality that are consistent with accepted clinical conclusions and with evolutionary theory. Hence, the realm formerly belonging exclusively to reproductive drive would now be open to much, much more.

## DISCUSSION

I would like to draw two conclusions.

First, I believe that prior assumptions have more influence in these areas of science than is commonly acknowledged in the usual philo-

sophical and scientific pictures of scientific theorizing and testing. In the cases examined here, science is not very separate from the social and cultural context. Rather, social assumptions and prior commitments of the scientists play a major role in the practice of science itself, at many levels — experimental design, data collection, predictions, hypothesis formulation, and the evaluation of explanations.

To understand this area of scientific practice, we need a view of science that is more sophisticated, one that has more moving parts, than the pictures typically presented by philosophers of science. Under the usual approaches, science is seen as involving relations purely between theory and data, or between theory, data, and explainer. But this is not enough. We need a way to recognize and analyze the vital role of pre-theoretical beliefs and categories in *all* stages of scientific research.

One might object that the subject matter of this part of science makes social influence inevitable, and that one would not expect this same level of cultural bias in other scientific contexts. That's probably right. But we do not need to show social forces at work in every possible case of scientific inquiry in order to insist on having a theory of science with enough flexibility to work in many areas. The cases I have presented here are definitely 'science', with plenty of funding, backing, authority, influence, and prestige. Philosophers who insist on a *pure* view of science, based on isolated and idealized examples of physics, are voting themselves out of the action. There are very interesting and important things going on in other areas as well, as the cases I have outlined above attest. Developing a view of science which can account for these other fields is vital.

My suggestion does *not* involve commitment to a relativist position. In a complete analysis of evolutionary explanations of human sexuality, I would adopt Helen Longino's general approach, in which she characterizes objectivity in science as resulting from the critical interaction of different groups and individuals with different social and cultural assumptions and different stakes. Under this view, the irreducibility of the social components of the scientific situation is accounted for — these social assumptions are, in fact, an essential part of the picture of scientific practice.

At any rate, I take it that the cases I have described above violate our common philosophical understandings of how we arrive at scien-

tific beliefs, how knowledge is created, and how science works. If philosophers go the route of labelling as 'science' *only* that which obeys the demands of current philosophy, we will end up discussing only some parts of physics and maybe some math. Meanwhile, what about the rest of science — biology, social sciences, anthropology, psychology, biochemistry? I suggest adopting and developing recent contextualist and feminist views of science, which take explicit account of pre-theoretical assumptions and preconceptions, and their social origins.

This case involving female sexuality is very interesting because there are *two* very strong forces working to put sex and reproduction together. Adaptationism, within biology, promotes the easy linking of all sexual activity with reproduction success, the measure of relative fitness. Secondly, the long social tradition of *defining* women in terms of their sexual and reproductive functions alone also tends to link sexuality and reproduction more tightly than the evidence indicates.

The long struggle by various women's movements to separate sex and reproduction seems to have had very little effect on the practice of the science we have examined in this paper. This is especially ironic, because politically, ever since the late Nineteenth Century, scientific views about gender differences and the biology of women have been the single most powerful political tool against the women's movements. My second and more controversial conclusion is that current 'purist' philosophy of science actually *contributes to* that political power 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. Science would then lose at least *some* independent authority in the political arena. Judging by the scientific work that I have discussed in this paper, I think that would be a good thing.

#### NOTES

<sup>1</sup> This paper contains the text of an invited lecture delivered at the Symposium, Sex and Reproduction, Pacific Division APA, 1992. Further documentation and discussion will be found in *All About Eve: Bias in Evolutionary Explanations of Women's Sexuality* (Harvard University Press, forthcoming).

- <sup>2</sup> Kano, T. (1982) "The social group of Pygmy Chimpanzees of Wamba," *Primates* 23(2), 171–188.
- <sup>3</sup> Hafez, E. S. E. (1971) "Reproductive cycles," in *Comparative Reproduction of Non-human Primates*, ed. E. S. E. Hafez. Springfield, IL: Charles C. Thomas.
- <sup>4</sup> Wolfe, L. (1979) "Behavioral patterns of estrous females of the Arachiyama West troop of Japanese Macaques (*Macaca fuscata*)," *Primates* 20(4), 525–534.
- <sup>5</sup> Koruda, S. (1980) "Social behavior of the Pygmy Chimpanzees," *Primates* 21(2), 181–197. Quote from p. 189.
- <sup>6</sup> Kano, T. (1980) "Special behavior of wild Pygmy Chimpanzees (*Pan paniscus*) of Wambe: A preliminary report," *Journal of Human Evolution* 9, 243–260. Quote from p. 243.
- <sup>7</sup> Mori, A. (1984) "An ethological study of Pygmy Chimpanzees in Wambe Zaire: A comparison with Chimpanzees," *Primates* 25(3), 255–278.
- <sup>8</sup> Chevalier-Skolnikoff, S. (1974) "Male-female, female-female, and male-male sexual behavior in the Stumptail Monkey, with special attention to the female orgasm," *Archives of Sexual Behavior* 3(2), 95–116; (1976) "Homosexual behavior in a laboratory group of Stumptail monkeys (*Macaca arctoides*): Forms, contexts, and possible social functions," *Archives of Sexual Behavior* 5(6), 511–527.
- <sup>9</sup> Goldfoot, D., J. Westerborg-van Loon, W. Groeneveld, and A. Koos Slob (1980) "Behavioral and physiological evidence of sexual climax in the female stump-tailed macaque (*Macaca arctoides*)," *Science* 208, 1477–1479.
- <sup>10</sup> The identity of this researcher is not included for publication. The information stated here was obtained through personal communication.
- <sup>11</sup> Kinsey, A. C. et al. (1953) *Sexual Behavior in the Human Female*. Philadelphia: W.B. Saunders.
- <sup>12</sup> Singer, I. and J. Singer (1972) "Periodicity of sexual desire in relation to time of ovulation in women," *Journal of Biosocial Science* 4, 471–481.
- <sup>13</sup> Morris' work has been criticized by later researchers as being methodologically flawed (e.g., Wilson (1975) *Sociobiology*, Harvard UP; Crook (1972) "Sexual selection, dimorphism, and social organization in the primates," in *Sexual Selection and the Descent of Man*, ed. B. Campbell. Chicago: Aldine) but it is still widely cited, and its basic premises are accepted or slightly modified in other respected accounts such as: F. Beach (1973) "Human sexuality and evolution," in *Advances in Behavioral Biology*, eds. Wm. Montaga and Wm. Sadler. NY: Plenum Press, pp. 333–365; G. Pugh (1977) *Biological Origins of Human Values*, NY: Basic Books; Crook (1972); and B. Campbell (1967) *Human Evolution: An Introduction to Man's Adaptations*. Chicago: Aldine.
- <sup>14</sup> Hite, S. (1976) *The Hite Report*, NY: Macmillan; Kinsey, A. et al. op cit.
- <sup>15</sup> See, e.g., Davenport, W. (1977) "Sex in cross-cultural perspective," in *Human Sexuality in Four Perspectives*, ed. F. Beach. Johns Hopkins U. Press, pp. 115–163.
- <sup>16</sup> Hite, S. op cit.
- <sup>17</sup> Typically, in evolutionary explanations, if a trait is taken to have evolved as an adaptation, yet is rarely used in the adaptive context, some explanation of the details of the selection pressure or the extreme adaptive value of the trait is offered.
- <sup>18</sup> Morris, D. (1967) *The Naked Ape*. London: Jonathan Cape, p. 79.
- <sup>19</sup> *Ibid.*, p. 55.
- <sup>20</sup> *Ibid.*, p. 59.
- <sup>21</sup> Masters, W. H. and V. Johnson (1966) *Human Sexual Response*. Boston: Little, Brown.
- <sup>22</sup> Kinsey (1953, pp. 375–376); Hite (1976, p. 417); Masters and Johnson (1966, p. 65).
- <sup>23</sup> Masters, W. H. and V. Johnson (1961) "Orgasm, anatomy of the female," in *Encyclopedia of Sexual Behavior*, Vol. II, eds. A. Ellis and A. Abar-banal. New York: Hawthorn, p. 792.

<sup>24</sup> Personal Communication.

<sup>25</sup> Gallup and Suarez (1983) "Optimal reproductive strategies for bipedalism," *Journal of Human Evolution* 12, 195.

<sup>26</sup> This argument is spelled out in more detail by Stephen Jay Gould, in an essay that was based on my research and arguments ("Freudian Slip," *Natural History*, Feb. 1987, pp. 14–21).

<sup>27</sup> Kinsey, 1953; Hite (1976), pp. 410–411.

<sup>28</sup> Hrdy, S. B. (1981) *The Woman That Never Evolved*. Cambridge, MA: Harvard University Press, p. 165.

<sup>29</sup> Caulfield, M. D. (1985) "Sexuality in human evolution: What is 'natural' in sex?" *Feminist Studies* 11(2), 343–363.

*Department of Philosophy*  
*University of California, Berkeley*  
*Berkeley, CA 94720*  
*USA*