

## Evolutionary Psychology

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### Book Review

#### The Human That Never Evolved

A review of Christopher Ryan and Cacilda Jethá, *Sex at Dawn: How We Mate, Why We Stray, and What It Means for Modern Relationships*. Harper Perennial: New York, 2010, 402 pp., US\$15.99, ISBN #978-0-06-170781-0 (paperback).

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The latest in a long tradition of scholarship critiquing what is seen as the persistent and pertinacious inadequacies of widely held evolutionary perspectives on human sexuality (e.g., Hrdy, 1980; Sherfey, 1972; Small 1993), *Sex at Dawn*, by Christopher Ryan and Cacilda Jethá, is certainly one of the most ambitious. Opting out of the formal academic style of writing, the book makes for an entertaining read, accessible to the lay reader not intimately familiar with modern evolutionary theory. Indeed, its popular appeal is revealed by its (paperback edition) front cover boast of inclusion on the *New York Times* bestseller list. While numerous reviews have been presented in newspapers, magazines, and websites, I have failed to find one review in an academic journal or by an evolutionary scientist (those who might be expected to give the most informed type of assessment of content). The public—in many cases unfortunately, but understandably—is largely educated in science through popular expositions such as this, and therefore it is crucial that researchers in the pertinent fields not ignore such publications or shirk from weighing in on the issues. In this review, I address what I see as biased reporting of data, theoretical and evidentiary shortcomings, and problematic assumptions misleadingly put forth as well-supported hypotheses contained in *Sex at Dawn*. Spatial constraints prevent comprehensive evaluation of the numerous topics touched on in the book. Thus, I limit discussion to a few issues I see as especially meriting attention.

*Sex at Dawn* sets out to topple what the authors call the “standard narrative of human sexual evolution” (p. 7), with its emphasis on the centrality of pair-bonding and monogamous mating, and fundamental conflicts of reproductive interests in the evolution of human sociality and sexuality. They see the mainstream evolutionary perspective as deeply flawed, as well as politicized, arguing that “It hides the truth of human sexuality behind a fig leaf of anachronistic Victorian discretion repackaged as science” (p. 35). The standard narrative, Ryan and Jethá argue, is merely the projection of contemporary arrangements back into the ancestral past, a tendency they refer to as “Flintstonization” (p. 32). Not only are current mainstream views on human sexual evolution theoretically and

factually wrong and political, “but *destructive*, sustaining a false sense of what it means to be a human” (p. 33, emphasis added), adding misery to the lives of those who buy into the myth, but struggle to live up to it. Now those are some bold claims! And bold claims demand strong evidence. I examine a sampling of what they proffer shortly.

The standard narrative of male preferences for youth and fidelity; contingency of parental investment on paternity certainty; female preferences for status, wealth, and willingness to invest; and the establishment of exclusive long-term reproductive bonds are seen by Ryan and Jethá not “as elements of human nature so much as adaptations to social conditions—many of which were introduced with the advent of agriculture no more than ten thousand years ago” (p. 8). Thus, monogamous pair-bonding is a response to environmental novelty largely created by an agricultural mode of subsistence, and “promiscuous impulses remain our biological baseline, our reference point” (p. 46).

Opening their book by pointing out the everyday evidence of a sort of sexual identity crisis of modern Western society, Ryan and Jethá endeavor to explain our painfully conflicted sexual existence. Their diagnosis in a nutshell: the advent of agriculture and its socioecological consequences. But this is getting too far ahead of things, and I should examine some elements of their case from the ground up.

The introduction and the first three chapters outline their main claims (introduction), give the reader a brief lesson on cultural relativity (chapter one), discuss Darwin’s views of sexual selection and the Victorian social context of early theories of human evolution (chapter two), and sketch out evolutionary psychology and the “standard narrative” as they see it (chapter three).

In chapter four, Ryan and Jethá begin to lay the foundation of their alternative narrative of human sexual evolution by rejecting the chimpanzee model, and, taking a lead from Lewis Henry Morgan’s speculations on the nature of primitive human sexuality (pp. 41-44), embracing the bonobo model as representative of early hominin sociosexuality. The basis for this assignment lay in the many similarities they see between human and bonobo sociosexual behavior and ontogeny, which differ for chimpanzees, including: sex across the menstrual cycle and during pregnancy and lactation, the comparatively slow rate of infant development, the return and rapid recommencement of sexual behavior of females after parturition, varied copulatory positions, eye gazing and kissing during copulation, a more forwardly oriented vulva, food sharing associated with sexual activity, homo- and heterosexual activity, sex for non-reproductive ends, and possibly genital-genital rubbing (pp. 77-78). Brushed aside from their comparisons of humans, chimps, and bonobos include sex-based hierarchies, sex-biased cooperation and coalitions, and intergroup hostility, for which humans have more in common with chimps than bonobos (although they argue that intergroup aggression among chimps are the result of provisioning and other human disruptions). Weakening their own argument, Ryan and Jethá point out later in the book that rape, infanticide, war, and murder have never been witnessed among bonobos (p. 187). Yet, all of these behaviors occur among humans.

In adopting the bonobo model of ancestral human sociosexuality, Ryan and Jethá would seem to be placing promiscuous sexuality deep in the roots of human phylogeny. This, however, is complicated by the fact that before introducing the hypothesis of a bonobo-like ancestry, they assert that “a few million years ago, our ancient ancestors

(*Homo erectus*) shifted from a gorilla-like mating system where an alpha male fought to win and maintain a harem of females to one in which most males had sexual access to females” (p. 11). Thus, according to the arguments put forth by Ryan and Jethá, any similarities between humans and bonobos are (re?)convergent adaptations to ecological pressures unspecified by the authors. At any rate, while it locates their arguments on a shaky and debatable foundation, the bonobo paradigm should not be dismissed out of hand, and its merit should be judged on the evidence mustered for it, and the utility made of it in formulating explanations. In my assessment, their evidence is weak, and their use of it is limited to occasional restatement of a bonobo-like past for humans.

The bonobo view of ancestral humans leads Ryan and Jethá to the assertion of two points: That war was rare among ancestral foragers, and that ancestral human societies were characterized by several ongoing, nonexclusive mating relationships wherein paternity certainty was a nonissue. I will address these issues in turn, although not in the sequence which they are presented in the book.

According to Ryan and Jethá, intergroup aggression was rare among ancestral foragers simply because there was nothing worth fighting over. “[H]ierarchical, aggressive, and territorial behavior is of recent origin for our species. It is...an adaptation to the social world that arose with agriculture.” (p. 76). They argue that unconcentrated dispersal of reliable food sources ruled out conflict over these resources, and low population density meant that territoriality was not a concern—this despite abundant (unmentioned) ethnographic evidence of territoriality and territorial intergroup aggression among hunter-gatherer populations (see, e.g., Ember, 1978; Keeley, 1996; Manson and Wrangham, 1991; Wilson, 1980, pp. 107-109; Wrangham and Peterson, 1996). Lastly, they permit the possibility that intergroup conflict may have been over women, but state that “this claim presumes that population growth was important to foragers and that women were commodities to be fought over...” (p. 76). This statement ignores the evidence from several foraging societies in which women are a cause of intergroup aggression (see Manson and Wrangham, 1991). Furthermore, in addition to being group-selectionist, it presumes that humans are motivated by ultimate considerations.

In a section titled “Professor Pinker, Red in Tooth and Claw,” the authors single out a lecture given by Steven Pinker in which he presents data on male warfare-related mortality showing much higher percentage of male deaths due to lethal intergroup aggression among traditional societies as compared to 20<sup>th</sup> century US and Europe. Ryan and Jethá reject Pinker’s figures as representative of ancestral male war-related death because six of the seven societies included are not properly classified as “immediate-return” foragers, but rather as horticulturalists; four of the societies are from densely populated Papua New Guinea; and debate about the typicality and male warfare mortality rate among the Australian society included (the Gebusi) (pp. 184-185).

In light of the authors’ critique, one would expect the presentation of some evidence supporting their claims as to the paucity of intergroup aggression among contemporary and ancestral foragers. Alas, the only evidence offered is a handful of quotes from authors in agreement with their position, DNA analyses indicating low population densities in the Pleistocene, and the scarcity of skeletal indications of interpersonal violence in the archaeological record (pp. 190-193). That, and the complaint that bonobos are given short

shrift in published accounts of the origin of human warfare (p. 186). Ryan and Jethá draw attention to the correlation between population density and warfare, and implicate agriculture and its concomitants as a major factor in the prevalence of war in post-Pleistocene human history. There is widespread agreement that agriculture-related population density seems to have exacerbated the frequency and intensity of war post-Pleistocene. However, the authors make a weak case for arguing that it was a rare and unimportant selective pressure in human evolution. Curiously omitted from their review is the fact that the !Kung, referred to by Thomas (1959) as the “harmless people,” engaged in lethal intergroup raiding (Eibl-Eibesfeldt, 1989), and had a homicide rate rivaling that of the “most violent American urban ghettos” (Daly and Wilson, 1988, p. 291).

The portrait that Ryan and Jethá paint of the ancestral forager—small, nomadic, isolated, egalitarian, peaceful groups with no territorial agenda, “whose life was solely made up of cooperation with scarcely any competitiveness, aggression, or nastiness of any kind” (Alexander, 1987, p. 127)—is what Alexander has referred to as the “central myth of modern anthropology” (1987, p. 127). The myth is the assumption that contemporary foragers inhabit the same regions and display the same behavior as ancestral foragers. Modern-day foraging populations reside where they do largely because that is where they have been driven by their more powerful neighbors. And inhabiting impoverished habitats, population densities are kept very low. As Alexander argues, the modern foraging lifestyle “is a way of life that is stable because it has so far been *forced* on those who follow it” (ibid.). If Alexander is correct, this calls into question just what can be gleaned about the past from foragers today.

I turn now to the next, and most important, aspect of Ryan and Jethá’s narrative: the nature of ancestral human sexuality. As mentioned above, the adoption of the bonobo model lays the foundation for their conception of ancestral mating as promiscuous and fluid (p. 45), and males unconcerned with paternity (p. 15, 104). Flowing from the bonobo model combined with the ethics of generosity and reciprocation described as vitally important to “fiercely egalitarian” modern foragers, is their hypothesis that “*Socio-Erotic Exchanges* (S.E.Ex. for short) strengthen the bonds among individuals in small-scale nomadic societies (and, apparently other highly interdependent groups), forming a crucial, durable web of affection, affiliation, and mutual obligation” (p. 94). In addition, they go on to note that “Without frequent S.E.Ex., it’s doubtful that foraging bands could have maintained social equilibrium and fecundity over the millennia” (p. 94) Let us look at the evidence they bring to bear on the argument.

The first piece of evidence they present in Chapter six is the existence of cultures that practice ‘partible paternity,’ the idea that a child can have more than one genitor (see Beckerman and Valentine, 2002). In a cursory and selective treatment of the literature, Ryan and Jethá portray sexuality and the relations between males and females in partible paternity societies as carefree, unencumbered by the jealousy and other difficulties and conflicts that attend more restrictive cultural mores. Promiscuous sex creates and promotes webs of affection and affiliation. Further, the institution of partible paternity means that male parental care is diffused, resources are distributed among a wider social network, and children benefit from the investment of multiple fathers (but see Ales, 2002; Crocker, 2002; Peluso and Boster, 2002). In sum, “Belief in partible paternity spreads fatherly feelings

throughout the group” (p. 107). What the authors fail to mention is that male sexual jealousy and sexual conflict are not absent from even the most sexually liberal of partible paternity societies (see, e.g., Crocker and Crocker, 2004; Kensinger, 2002). Crocker and Crocker (2004, p. 111) tell us that the Canela, one of the most promiscuous cultures known, “believe that husbands have to be taught not to be jealous of their wives.” For their part, young Canela women are taught by their kin to be accepting of obligatory sequential sex rituals (Crocker and Crocker, 2004, p. 112). The concept of partible paternity manifests itself in different ways in different cultures. For example, where post-marital residence is virilocal, and/or where descent is patrilineal, infidelity is sanctioned, and restrictions exist as to which males may share paternity (Ales, 2002; Chernela, 2002; Valentine, 2002). Even where male-biased residence and kinship do not obtain, men and women are rarely given free reign to their sexuality, and paternity is never a matter of unimportance (see contributions in Beckerman and Valentine, 2002).

Although they are mentioned only in passing in their discussion of sexually liberal societies, it’s worth pointing out that among the Trobriand Islanders, where Malinowski (1929) tells us that men are thought to play no role in procreation, male sexual jealousy is not lacking, to say the least. Also, the equivalent word for ‘promiscuous’ “is perjorative...and generally applied to women” (Symons, 1979, p. 229).

It is interesting indeed that Ryan and Jethá approvingly cite some horticultural societies (all partible paternity cultures in South America; the Trobriand Islanders; Tahitians; Mohave) as affirming evidence of the sexually promiscuous nature of humans, and a purported lack of universal concern over paternity, while at the same time rejecting other horticultural societies as representative of ancestral humans in their discussion of warfare on the grounds that they are not foragers. They are attempting to have their cake and eat it, too. If socioecological conditions among horticulturalists render them not appropriately comparable to foragers in the context of war, then a case could likely be made that there is something about the socioecology that renders it inappropriate to extrapolate from horticultural to forager sexuality. I do not intend to belabor this point further, many readers can probably easily think of a few candidate variables, but it deserves recognition.

The presentation of the ethnographic evidence comes to a climax with their discussion of the Musuo (Na) of China and the sexual autonomy apparently conferred in the multiple and transient *açia* relationships of men and women (pp. 126-131). This matrilineal society is presented, in part, as a refutation of Symons’ (1979) doubts as to the existence of a society where paternity certainty is so low that a man is typically more related to his sister’s than to his wife’s offspring. The Musuo may very well be such a case, we don’t know. However, Musuo “walking marriages” (*açia*) are only one form of sexual relationship in this culture, with the other two involving long-term co-habitation of husbands and wives (Blumenfield, 2009). It also bears mentioning that the primary ethnographer describes the Musuo nobility as having traditionally practiced a bilateral system of descent with wealth and status being transmitted from father to son (Hua, 2001)—hardly a situation where paternity certainty would have been a nonissue. Both of these observations were left out of Ryan and Jethá’s discussion. The agricultural mode of subsistence, and the historical socially stratified feudalism, raise doubts as to the ancestral

representativeness of the Musuo.

In fact, of all the societies they offer as supporting evidence of a human nature of promiscuous sexuality, only one can truly be considered a foraging population: the Inuit, and it is unfortunate that Ryan and Jethá give only a brief anecdotal nod to the Inuit practice of spouse exchange, leaving out the fact that “Among the North Alaskan Eskimo, wife exchanges were arranged between the husbands, and the wives were not consulted” (Symons, 1979, p. 246).

Despite their proclamation that with the dawn of agriculture and the derivative notion of private property, “*for the first time in the history of our species, paternity became a crucial concern*” (p. 15, emphasis in original), and their description of an “anthropological record so rich with examples of societies where biological paternity is of little or no importance” (p.15), the survey of the ethnographic record given by Ryan and Lethá does not lend itself readily to these suggestions. It fares little better in providing support for a promiscuous human nature.

It appears that men everywhere take a proprietary attitude toward female sexuality and strive to monopolize the reproductive resources of their mates (Wilson and Daly, 1992). Cross-culturally, adultery (particularly female infidelity) is the most common cause of divorce (Betzig, 1989). Sexual jealousy is the predominant precipitating factor in lethal and nonlethal violence against women (Daly and Wilson, 1988; Wilson and Daly, 1996), and competition among men over women or the resources needed to attract them has been the cause of much bloodshed in our species. These facts simply are not compatible with the narrative put forth in *Sex at Dawn*.

But so much for ethnography. The authors have another source of evidence: anatomy and psychology.

The first bit of phenotypic evidence of a history of human promiscuity introduced is sexual dimorphism in body size (pp. 216-219). Pointing out that body-size dimorphism is correlated with male competition for females, and that humans exhibit modest body-size dimorphism, Ryan and Jethá surmise that this indicates reduced competition between ancestral males for mates. Dismissing the “standard narrative” view that the reduction of dimorphism tracks the evolutionary transition from polygyny to monogamy, they argue, based on the mating systems of chimps and bonobos, and our phylogenetic relationship with these apes, that the reduction of male-male competition was borne in upon an increase in female promiscuity. Reduced dimorphism is also taken as evidence against the claim that “humans are naturally polygynous harem-builders” (p. 217), as the means to support multiple wives and children “*simply did not exist before agriculture*” (p. 217, emphasis in original). In regard to the arguments put forth by Ryan and Jethá, two things are worth considering. First, as Puts (2010) points out, human sex differences in size are affected by the unique trait of female fat stores. When fat-free muscle is considered, differences between male and female muscle mass is similar to that of gorillas. Second, if human males are not inherently inclined to polygyny, how are we to explain the fact that at least some degree of polygyny is to be found among the majority of extant foraging societies (Marlowe, 2003)?

Another piece of evidence given for a history of promiscuity is human male testicular size (chapter 16: The Truest Measure of a Man). As noted by Ryan and Jethá, the

volume of human testicles relative to body mass is intermediate between gorillas and chimps, and this has been used to argue both sides of the debate over human promiscuity. Recognizing that human testes size doesn't implicate the levels of bonobo-like promiscuity they see as characteristic of ancestral humans, they resort to the hypothesis that human testes might have been shrinking since the end of the Pleistocene (more so in some racial and ethnic groups than in others) as a result of the increased monandry accompanying an agricultural mode of subsistence (pp. 226-227, 240-241). Fair enough, but this is a very difficult hypothesis to test. Luckily, Ryan and Jethá claim that it has already been confirmed! Referring to a paper by Wyckoff, Wang and Wu (2000) reporting that certain genes involved in sperm and seminal fluid production in the lineages of humans, chimps, and bonobos appear to have undergone quite rapid evolution changes. Ryan and Jethá remark that this study by Wyckoff and colleagues "confirm a prediction made by Roger Short...[that] 'Testis size might be expected to respond rapidly to selection pressures'" (p. 227). But nowhere in the Wyckoff et al. article do the authors mention anything about genes influencing testes size.

Regarding females, many key features of women's sexuality are given rather cursory treatment and not analyzed as to how they fit in with the evolutionary scenario presented in *Sex at Dawn*. For example, regarding concealed estrus, they restrict discussion to the pair-bonding hypothesis as presented by Helen Fisher (p. 59), and the paternity confusion hypothesis of Sarah Hrdy (pp. 59-60). Ryan and Jethá favor neither of these hypotheses, and they leave the matter at that. On human female orgasm, they point out that female orgasmic behavior has been observed in some primate species with multimale-multifemale mating systems, whereas the monogamous gibbon female does not exhibit such behavior. Following this, Symons' (1979) by-product argument is summarized, and a few pages later, changes in vaginal acidity associated with orgasm are suggested as a possible mechanism of sire choice (pp. 266-267). In their discussion of the unique trait of perennially enlarged human female breasts, they favor some version of a fertility signaling hypothesis, likening them to the sexual swellings of nonhuman primates. Human breasts, like bonobo sexual swellings, do not change much over the course of the ovulatory cycle (p. 261). Thus, the human breast is seen as evidence of a bonobo-like past. (See Marlowe, 1998 for an alternative hypothesis).

Chapter 20 (On Mona Lisa's Mind) addresses the work of Meredith Chivers and others showing the greater "erotic plasticity" of women compared to men. The reason for the exposition of this body of research seems to be to show the complex and contextualized nature of female sexuality that is not always revealed by responses to questionnaire items and behavior in the laboratory; that the view of female sexuality contained in the "standard narrative" is overly-narrow, and an artifact of the way and place it has been studied (see, for example, p. 143). Reporting on the discrepancy between subjectively claimed arousal and plethysmographically measured arousal found by Chivers, Ryan and Jethá state that "it could well be that the price of women's greater erotic flexibility is more difficulty in knowing—and, depending on what cultural restrictions may be involved, in accepting—what they're feeling." (pp. 273-274). Thus, females get aroused by all sorts of things (including watching bonobo sex), they just don't know it, nor might they want to accept it. In response to this, we can ask the question of what plays a larger role in determining actual

female behavior: conscious sexual desire, or genital blood pressure?

Greater female than male sexual plasticity had been known about long before Chivers' studies. Symons' (1979, pp. 312-313) suggested that "the enormous range of sexual variation observed among females may be primarily an artifact of artificial, postagricultural habitats." If female sexuality among our foraging ancestors had always been, to some degree, constrained by males, the range of variability and potentials of female sexuality would have been buffered to an extent from selection. Here we have two opposing views on the effects of modern society on female sexual plasticity. Symons argues that modern arrangements lift some of the constraints to reveal hidden potentials, while Ryan and Jethá argue that it imposes constraints that hide potentials. One step toward a resolution is to consider how constrained or liberated a typical female university student in Western society is compared to her counterpart in a traditional foraging society? Ryan and Jethá would have us believe it is the latter that is the less constrained, yet provide no evidence that this is so. The prevalence of arranged marriage and prescribed marriage partners among foragers suggest that it is not so (see Flinn and Low, 1986; Walker, Hill, Flinn, and Ellsworth, 2011).

Scattered throughout *Sex at Dawn* are references to mate preference shifts and other cycle-related behavioral changes associated with human estrus. None of the recent evidence on estrus adaptations, however, suggests a history of promiscuity (defined by Ryan and Jethá as a number of ongoing, nonexclusive sexual relationships). Rather, their manifestations are highly contextualized and specific—estrus is not a generalized increase in sexual interest or desire. Indeed, females are choosiest when most fertile (Gangestad and Thornhill, 2008). In a thorough review of the available evidence, Thornhill and Gangestad (2008) conclude that the primary selective pressures favoring such female estrus adaptations were pair-bonding and dependence on male provisioning. Estrus adaptations motivate sex outside the pair-bond only when it could pay-off genetically (i.e., when a genetically superior male has been targeted, when the risk of detection is low, and when conception is possible).

Much of the remaining material put forth as support for a promiscuous past concerns male motivation for sexual variety (see especially pp. 288-298)—hardly an indication of ancestral promiscuity. As Trivers (1972) and others (e.g., Symons, 1979) have argued, a male desire for sexual variety is expected to be favored by selection even within a monogamous mating system. Because the minimum possible investment in reproduction is so much lower for males than females, any inclination to be on the lookout for opportunities for the attainment of sexual variety and low-cost sexual encounters can bring large reproductive pay-offs to men and will be favorably selected.

The evidence contained in modern human male and female phenotypes that has been, and continues to be, amassed, reveal an evolutionary history that was characterized by a not insignificant amount of non-monogamous sexual behavior. To be sure, I doubt that any serious evolutionary scientist, even the most ardent supporter of the "standard narrative," would argue that humans evolved in a milieu of perfectly monogamous pair-bonds. This is patently not the case, and it defies evolutionary logic to assume so. Men and women are genetic competitors with different available routes to reproductive success (another lamentable element of the "standard narrative," e.g., p. 49, 58, 270). There is

considerable cross-cultural variation concerning such things as extramarital sex, premarital sexual freedom, strength of marital bonds, and degree of female reproductive autonomy. And sperm competition likely posed a selective pressure on ancestral males (Shackelford, Pound, and Goetz, 2005). But to argue that the evidence points to the level of promiscuity argued by Ryan and Jethá is to turn a blind eye to disconfirming, inconvenient facts, while indulging in quite a bit of fantasizing. If promiscuity even slightly approaching bonobo levels were characteristic of (post-*Homo erectus*) ancestral sexuality, there would be much more evidence for it than *Sex at Dawn* manages to drum up. Ryan and Jethá conjure up a phantom of human nature that vanishes in the face of scrutiny—a naïve vision of a human that never evolved.

Monogamous pair-bonding is a strained compromise between male and female; perhaps not the best of all possible worlds, but one in which most of our ancestors evidently found themselves. It is in this sociosexual context that selection shaped who we are today. It is true, as *Sex at Dawn* points out, that monogamy is difficult in modern society, but doubtful that this is because we are promiscuous at heart (this may apply to the behavior of most women more than the desire of most men), shackled by the trappings of a postagricultural dilemma of our own devices, unable to return to the ancestral days of sexual communism.

Is this book likely to open the eyes of scientists and make them realize that the emperor has, for so long, not been wearing any clothes? Will it initiate a major revision of perspective and research on the evolution of human sexuality among scientists? The answer to both is “no.” But, as mentioned at the beginning of this review, books like *Sex at Dawn* inform the wider public of the goings-on in academia. In this case, a distorted portrayal of current theory and evidence on evolved human sexuality is presented, and for this reason it deserves more attention from those on the inside.

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