

18. Lumsden and Wilson, *Promethean Fire*, 183.

19. See Ruse and Wilson, "Moral Philosophy," 183–185; for discussion of human incest avoidance, see Kitcher, "Developmental Decomposition," reprinted as chapter 14 in this book.

20. Ruse and Wilson, "Moral Philosophy," 192.

21. For reasons given in Kitcher, *Vaulting Ambition*, and Kitcher "Developmental Decomposition," the latter reprinted as chapter 14 in this book.

22. Wilson, "Relation of Science to Theology," 431.

## Pop Sociobiology Reborn

### The Evolutionary Psychology of Sex and Violence (2002)

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#### I. Introduction: A Dismal History

Here's a recipe for winning fame and fortune as an architect of the new-and-improved human sciences. First, make a bundle of claims to the effect that certain features are universal among human beings, or among human males, or among human females. Next, couple each claim with a story of how the pertinent features were advantageous for primitive hominids, or males, or females, as they faced whatever challenges you take to have been prevalent in some lightly sketched savannah environment. (Don't worry that your knowledge of past environments is rather thin—Be creative!) Finally, announce that each feature in the bundle has been shaped by natural selection, and so corresponds to something very deep in human nature (male human nature, female human nature), something that may be overlain with a veneer of culture but that molds our behavior and the forms of our societies. Accompany everything with hymns to the genius of Darwin, broadsides against "blank slate" views of the human mind, and vigorous denunciations of the lack of rigor and clarity that has hitherto reigned in the human sciences.

In the second half of the twentieth century, three major movements tried to follow this recipe. First came animal ethology with stirring yarns about naked apes and territorial imperatives. These stories were recast by the second wave, as human sociobiology drew more systematically on the resources of contemporary evolutionary theory. In the 1960s and 1970s, the integration of mathematical models with field observations enabled students of animal behavior to advance, support, and refine detailed theories about caste structure in social insects, copulation in dungflies, and the mating structures of red deer.<sup>1</sup> Successes like these inspired the ambitious to propose that kindred insights could be achieved with respect to our own species: they claimed that human beings are, by nature, xenophobic and "absurdly easy to indoctrinate," that human societies are inevitably stratified by relations of power and domination, that men are fated to be fickle and women to be coy, that human altruism is an illusion and that we can't hope to achieve genuine sexual equality.<sup>2</sup> Pop sociobiology was born.

By the mid-1980s, the movement had attracted a barrage of criticism. Skeptics pointed out that, by contrast with the careful studies of nonhuman animals, the suggestions about universals of human behavior (or male behavior, or female behavior) rested on anecdotal evidence. Furthermore, pop sociobiology contented itself with telling informal stories about advantages, instead of putting to work the mathematical tools of evolutionary theory, painstakingly deployed by workers on deer and dungflies. Careful work on the evolution of behavior had appreciated, from the beginning, the need to consider alternative hypotheses and to discriminate among them using data from evolutionary genetics, experiments, comparative observation, or mathematical modelling, but no such pains were taken by the leading proponents of pop sociobiology.<sup>3</sup> Nor was there, to begin with, any appreciation of the possibility that cultural transmission might affect the traits of human beings, and when, belatedly, pop sociobiology came to terms with this issue, its attempts to show that "the genes hold culture on a leash" depended on arbitrary assignments of values to crucial parameters.<sup>4</sup>

Yet perhaps the most important defect lay in the conclusions, often announced with commendable regret, that certain unpleasant features were so deeply ingrained in human nature as to be unmodifiable. Critics noted that such conclusions cannot validly be derived from the kinds of evolutionary scenarios presented.<sup>5</sup> The most those scenarios could reveal is that there are pieces of DNA that, in the particular environments encountered by our hominid ancestors, give rise to characteristics—competitiveness, coyness, xenophobia, whatever—that proved beneficial in those environments; the scenarios have no bearing on whether, under different regimes of development, those traits would be bound to arise (nor whether they would be advantageous in these rival circumstances).

Would-be Darwinian reformers of the human sciences adopted a strategy for coping with these criticisms. "Indeed," they explained, "some sociobiologists have made unwarranted claims; but our approach should not be dismissed; we are aware of the criticisms; we have made them ourselves; we are reformed; we have abandoned the idea that genes are destiny; we are evolutionary psychologists, who aim to use Darwinian insights to fathom human tendencies." Some of them continued to insist on the importance of the enterprise in indicating to us how we might amend unwanted forms of behavior. In the late 1980s, when evolutionary psychology kept its claims modest and its head down, charity commended giving the new movement the benefit of the doubt. But the publication of a rousing revival of the pop favorites of the past<sup>6</sup> made it apparent that the old mistakes haven't lost their allure. Evolutionary psychology turns out to be pop sociobiology with a fig leaf.

## 2. The Pop Sociobiology Revival: An Overview

We'll try to substantiate this last accusation by looking at two of the most prominent exhibits in the Pop Revival, David Buss's proposals about male and female sexual attractiveness and the hypotheses of Randy Thornhill and Craig Palmer<sup>7</sup> on

rape (Thornhill and Palmer draw on Buss's efforts, so our critique of Buss will extend to their program). First, however, we'll offer a more general view of the evolutionary psychology movement.

The principal advance evolutionary psychologists take themselves to have made consists in recognizing that natural selection doesn't shape human behavior directly, but rather the psychological mechanisms underlying behavior. Bad old pop sociobiology supposed that natural selection would favor males who were fickle and promiscuous. Thoroughly modern Darwinian analyses recognize the need to integrate biology with the right approach to psychology, to wit the view that the mind consists of lots of special-purpose devices (modules<sup>8</sup>) that prompt different forms of behavior. Evolutionary psychology reflects on the problems and challenges faced by our hominid ancestors, generating hypotheses about the kinds of psychological traits natural selection has bequeathed to us. These hypotheses are evaluated by collecting evidence from human subjects who report their feelings and preferences in actual or imagined situations, or by studying human behavior. Support for a psychological claim is supposed to come from juxtaposing contemporary data with an *independent* Darwinian expectation about what kinds of ancestral tendencies would have contributed to reproductive success.

If this is to be successful, then *both* the evidence collected and the Darwinian theorizing have to satisfy important constraints. Let's start with the evidence. Whether or not this consists of responses to questionnaires or statistical patterns of behavior, it will have probative force with respect to a hypothesis about a psychological mechanism only if that hypothesis can be integrated with other claims about the psychology of human subjects to generate expectations about what should be observed in the pertinent experimental or natural situations. When the mind is conceived as a bundle of psychological capacities and dispositions that interact with one another and that are causally affected by external cues, the psychological account has to tell us enough about the nature of the interactions and the responses to the cues so that we can derive specific claims about human actions. A claim about a single trait, in splendid isolation, leaves entirely open what sorts of behavior are to be expected—since the activity of other mechanisms could override, suppress, amplify, or redirect whatever tendency is hypothesized—and, in consequence, loose associations between hypothesized psychological tendencies and a pattern of behavior should impress nobody.

It would, of course, be unfair to ask any evolutionary psychologist to provide us with a complete, detailed psychology. Yet if the psychological account provided introduces a collection of capacities that might easily prompt an agent to incompatible forms of behavior—as for example when we're told that people are attracted to different characteristics that regularly turn up in different locations—then we can't tell much about what typical subjects will do. Consider preferences for various types of food. It's a familiar fact that someone's actual diet may not reflect her craving for a particular food, precisely because what she chooses to eat is a function of several underlying psychological dispositions. So we could "protect" an evolutionary story about universal gustatory yearnings by supposing that the underlying tendencies are inhibited by other mechanisms. Or, to put the point differently, the hypothesis that human beings have evolved to crave large hunks of red meat (say)

issues no definite predictions about the frequency of carnivorous displays in any human population.

Turning now to the specifically Darwinian part of the enterprise, we should recognize an important point often made by John Maynard Smith: model-building requires attention to the details, and mathematical modeling uncovers and refines hidden presuppositions. (Maynard Smith and W. D. Hamilton are pioneering figures in evolutionary theory, on whose work sociobiology has drawn; the illuminating work of people like Eric Charnov, Geoffrey Parker, Peter Harvey, John Krebs, and many others shows the salutary influence of Maynard Smith and Hamilton.) Mathematical models aren't always necessary in evolutionary work: sometimes alternative hypotheses can be screened out by considerations drawn from genetics, or careful experiments, or detailed cross-species comparisons. In human sociobiology, however, where rival hypotheses can easily be multiplied, where genetic ignorance is the order of the day, where many of the experiments that might clear up controversy are rightly forbidden as unethical, and where cross-specific comparisons are vulnerable to worries about salient differences, it's crucial that the proposals about histories of natural selection should be formulated clearly and precisely. Pop sociobiology often substituted casual stories about selective advantages for rigorous models of selective pressures. To do better, one must know enough about the alleged environment in which the selection process occurred to formulate defensible claims about reproductive costs and benefits.

In the human case (and, quite possibly, in investigations of other species) it's also important to recognize the possibility of cultural transmission. Since the important work of Robert Boyd and Peter Richerson,<sup>9</sup> everyone interested in Darwinizing the human sciences should have known that a population under the joint influence of natural selection and cultural transmission can exhibit characteristics different from those of a population under the influence of natural selection alone, and that the modes of cultural selection generating this type of deviation can themselves be sustained under natural selection.<sup>10</sup> Hence even when one works out the precise details of a hypothesis about the *natural* selection of some trait, it will always be pertinent to wonder if that characteristic would have emerged under the *joint* influence of natural selection and cultural transmission. In short, then, the models that reformed pop sociobiologists are going to use have to be *more* elaborate than those used by their counterparts pursuing nonhuman studies.

There are two theoretical points that add further difficulties to pursuing a serious Darwinian psychology. As many leading Darwinians have declared repeatedly, Darwin replaced the notion of a species as a *type* with an emphasis on intraspecific variability. Perhaps, then, evolutionary psychology's commitment to a universal human nature is suspect. Even though there are surely some traits that are found (almost) universally across our species, it's important not to suppose that universal fixation is the norm. One can't reply that natural selection is a homogenizing force, for, although there are some circumstances—when the underlying genetics is free from well-known complications and there's an optimal form of a particular trait—in which natural selection would be expected to make one variant virtually universal, the necessary hedges can't be disregarded. Sometimes the genetic details make it impossible that the optimal form of a trait should be fixed (a simple example

is when the optimal trait is coded by a heterozygote), and there are other instances in which natural selection is expected to generate a polymorphic equilibrium (a classic case is the Hawk-Dove polymorphism from elementary evolutionary game theory).

The idea of individually selected psychological capacities should also be carefully scrutinized. For all their shortcomings, earlier pop sociobiologists did recognize that evolution has something to do with genes, and they were frequently chastised for naïve assumptions that there were genes available to direct females to be coy or human beings in general to be xenophobic. The error, here, as we've already remarked, was to introduce a form of genetic determinism: if the underlying genotype generated the pertinent trait in the ancestral environment, then, it was assumed, it would yield the trait in all environments. Recent pop sociobiologists, by contrast, don't like to talk about genes. For all their reticence, however, they can't avoid advancing genetic hypotheses. After all, without a genetic basis for a trait—that is, a tendency for the underlying genotype to yield a particular phenotype *in the selective environment*—there can be no natural selection. To suppose that there's a naturally selected psychological mechanism for this or that—cheater detection, say, or directing young women to swoon at the prospect of powerful older men—is to claim that there's been genetic variation in some ancestral population pertinent to the propensity to perform such narrowly defined tasks. Although they don't say as much, they must think that there are two alleles—call them A and B—associated in the primeval environment (or range of environments), with a greater or lesser ability to carry out the appointed task (detect cheats or swoon appropriately).

Let's take a deep breath at this point. It's worth reminding ourselves of what genes do. Genes encode proteins. So A and B encode different proteins, and, on a simple version, it seems that evolutionary psychologists are committed to saying that these differences amount to solely and precisely a difference in cheat-spotting-acuity or swoonability. We're prepared to concede that differences in proteins might show up in alternative forms of neural chemistry, evident in psychological changes—it's not incredible that a modified neural receptor protein might make a mouse, or a human, more or less good at remembering things, or slower or faster to learn. What's highly implausible is that changing a protein could leave all our psychological tendencies untouched while fine-tuning the talent for cheat-spotting or weakness at the knees at the thought of a mate with status, power, and wealth. Until we are offered some plausible idea about mechanisms, we ought to dismiss these suggestions as vague speculation. The overreaching is hidden only because the latest Darwinizers have learned from the demise of old-style pop sociobiology: Be cagey about genetic hypothesizing!

This is surely simplistic, and evolutionary psychologists ought to repudiate the words we've put into their mouths. A better suggestion would be that the pertinent proteins have lots of different phenotypic consequences, but *the one that matters* concerns the narrowly specified psychological disposition (spotting cheats, swooning appropriately). The claim, then, is that the rival genotypes give rise to phenotypes that differ in lots of ways, but *only the evolutionary psychologist's favorite disposition* makes a serious difference to reproductive success—the rest is a wash.

The fitness contribution of the chosen trait swamps any correlated effects. But, lacking any hints about the underlying genotypes, how their differences might make neural—and therefore psychological—differences, and what impact such *overall* differences might have, there's just no reason to believe that claim. Why should a priori guesses about the nonexistence of correlations with selective significance serve as the basis for evolutionary analysis?

Let's put the point more positively. Forget the fine-grained psychological dispositions for the moment, and ask how natural selection might shape human psychology. Absent revolutionary proposals, the obvious answer is that different genotypes might encode proteins that participate differently in the reactions that underlie neural development, in the formation or pruning of synapses, in the sensitivity to various molecular signals, or in the speed of processes of transmission. It doesn't follow that selective modification of genotypes would affect all aspects of our psychology. But these considerations do suggest the real possibility that psychological phenomena are genetically linked in ways about which we're currently ignorant, so that a particular genetic modification would produce a spectrum of psychological responses, increasing some aspects of human performance and diminishing others. If so, then hunting for the ways in which selection has shaped such fine-grained psychological traits as a disposition to detect cheats is an unpromising strategy, and one can't do any serious Darwinian psychological analysis until there's much greater knowledge of the intricacies of neurodevelopment. Many evolutionary psychologists naively posit their favorite psychological atoms, each under individual selective control and thus each associated with some locus that affects nothing else. This is myth-making, not serious science.

We anticipate a response: "We have to start somewhere. Science must always begin from ignorance, so to demand knowledge at the beginning is antiscience."<sup>11</sup> We acknowledge that no investigation begins from complete knowledge; so much is truism. But well-planned investigations recognize which forms of current ignorance matter and endeavor to ameliorate them, rather than whistling away the complications and hoping that they won't prove significant.

Our review of general issues is intended to highlight the mistakes that attend the recent pop sociobiology of sex and violence. We now turn to the details.

### 3. Savannah Yearnings: A Romance

The sun is setting, casting a soft bronze glow on the meadow. You, Primeval Pru, realize that you face the hardest decision of your life as a hunter-gatherer: It is time to choose your man. Two stand before you. On the left is a younger man whose deep-set eyes are framed by rich black lashes. His body is unscarred, suggesting that he has not exerted himself much in close encounters with beast or man. But you find it hard to turn your gaze from his warm smile. On the right is an older, balding fellow with plain features and a commanding manner. He gestures to his impressive hut and his collection of animal skins. Whom should you pick?

David Buss knows. He has a theory of evolved mate selection in humans—his "Sexual Strategies Theory"—which informs us as to what Primeval Pru and her

contemporary descendants will do (or, more exactly, what Primeval Pru would have done if she has a lot of contemporary descendants). This "theory" is best conceived as an amalgam of claims about mate selection, all of which rely on the same few fundamental tenets. The basic principle from which Buss generates his conclusions (as Thornhill and Palmer after him) is that "the sexes will differ in precisely those domains in which women and men have faced different sorts of adaptive problems."<sup>12</sup> The pertinent evolutionary pressures are supposed to have operated during the "environment of evolutionary adaptedness" (EEA), apparently the Pleistocene, when our ancestors lived as hunter-gatherer groups.

Here's the story. Men's and women's roles in reproduction are asymmetrical in three different ways. First men, but not women, face "parental uncertainty." Second, women are fertile for a smaller portion of their lives than are men. Third, women invest considerably more in reproduction than do men. Following many other pop sociobiologists, Buss waxes lyrical about the contrast between the roughly 450 nutrient-loaded gametes that a woman will produce in a lifetime and the millions of tiny mobile gametes in a single male ejaculate (replenished, as he points out, at a rate of about twelve million an hour). After conception, a woman is also committed to nine months of pregnancy, and after birth, only she can lactate and thus provide milk for the offspring.

These asymmetries create three adaptive problems for men and women. Men will need to increase the probability of paternity and to identify female reproductive value (which peaks in a woman's mid-teens when she has all of her fertile years before her).<sup>13</sup> Women will need to find men who can provide them with resources and defend them and their children against predators and human aggressors. Natural selection will thus select for psychological dispositions that incline men to sexual jealousy, that will prompt them to take advantage of whatever opportunities they have for a quick copulation on the side, and that lead them to be attracted to women with the signs of peak reproductive value—full lips, clear eyes, lustrous hair, a bouncy gait (all these figure in Buss's catalogue, as does a waist-hip ratio of roughly 0.7). Similarly, selection will favor women whose psychological dispositions lead them to be attracted to older men (men with power and resources) and that make them less inclined to wander.

So much for the Darwinian "expectations." Now for the data. To his credit, Buss has carried out an extensive survey in which questionnaires were administered to members of 37 cultures in 33 countries. Besides asking for biographical information (age, sex, religion, etc.) the questionnaires contain queries about mate preferences, first in the form of open-ended questions and then by means of rating and ranking tasks. The open-ended part requires the subject to state the age at which he or she wishes to marry, the age difference the subject would prefer to exist between the subject and the subject's spouse, and the number of children desired. The second part of the first instrument requires respondents to rate 18 characteristics (such as earning capacity, ambition/industriousness, youth, physical attractiveness, and chastity) based on how "important or desirable" each would be in choosing a mate. The respondent must give a numerical rating on a scale from 0 to 3, ranging from "irrelevant or unimportant" (0) to indispensable (3). The second instrument asks subjects to rank 13 characteristics, based on their desirability

in a mate. Ten thousand and forty-seven (10,047) subjects were included in the study.<sup>14</sup>

Buss reports that the results accord with his Darwinian expectations. For 36 of 37 samples, there's a statistically significant difference showing that women rate "good financial prospect" higher than do men. In 29 of 37 samples, there's a statistically significant difference with respect to ambition/industriousness (women rating it more highly), and in 34 samples there's a statistically significant difference with respect to physical attractiveness (men rating it as more important). Averaged over all samples, women responded that they prefer men who are 3.42 years older than themselves, while men answered that they prefer women who are 2.66 years younger.<sup>15</sup>

Although his study is the centerpiece of his evidence, Buss defends his "Sexual Strategies Theory" with other considerations more squarely in the pop sociobiological tradition.

A comparison of the statistics derived from personal advertisements in newspapers reveals that a man's age has a strong effect on his preferences. As men get older, they prefer as mates women who are increasingly younger than they are. Men in their thirties prefer women who are roughly five years younger, whereas men in their fifties prefer women ten to twenty years younger. He also reminds us of the familiar male pride in "conquests" and "notches on the belt," which he views as signaling an adaptation to brief sexual encounters.<sup>16</sup> A favorite tale of the differences in "short-term mating strategies" stems from an experiment conducted on a college campus: an "attractive person" approaches a member of the opposite sex and issues a sexual invitation; 100 percent of the women declined, 75 percent of the men accepted.<sup>17</sup>

So there's a clear message for Primeval Pru. Avert your gaze. Forget that smile. Snuggle down with the animal skins.

We disagree. We don't think we know enough to offer Pru any advice at all. In line with the general conclusions drawn in the previous section, we find Buss's claims about the operation of selection naïve and his alleged empirical support questionable. Let's start with the data.

What exactly does Buss's questionnaire measure? Consider first the issue of whether the responses accord with respondents' preferences. Subjects may have beliefs about how they should respond to the questionnaire, or how those who distribute the questionnaire want them to respond. Although Buss notes that his research assistants did not know his hypotheses, any concordance between his predictions and the stereotypes prevalent in a culture will leave his results vulnerable to bias, whatever the ignorance of his subjects and those who administer the instruments. Furthermore, even if we neglect possibilities that responses will reflect widespread cultural values, Buss must assume that people have access to their own preferences. Interestingly, he emphasizes that "sexual strategies do not require conscious planning or awareness," so that his faith in the questionnaire has to rest on a nice distinction in typical human levels of awareness: we know our preferences but we don't recognize why we have them.<sup>18</sup> As we'll note shortly, inquiring what subjects would say in explaining their responses might well prove illuminating. An even more fundamental assumption is that there are such things as stable

preferences that endure beyond the situation of answering the questionnaire into the contexts in which people actually make their decisions. A significant tradition of psychological research—pioneered by Walter Mischel over a period of three decades—has produced convincing evidence that many personality traits are situation-specific, and recent data suggests that the same may apply to preferences.<sup>19</sup>

Yet even if we grant that Buss is measuring genuine stable preferences, uncontaminated by cultural norms, the most important question concerns the *content* of these preferences. The connection between "mate choice"—the topic of the various questions and tasks—and sexual attraction needs scrutiny. Choosing a mate typically means more than picking a sexual partner (or even a reproductive partner), and in many, if not all, of the cultures that figure in Buss's survey, the consequences of mate choice affect many dimensions of the parties' lives. Recall a point from the last section: actual behavior results from the interaction among psychological mechanisms. Assuming that there are such mechanisms, it's only the most simplistic psychology that takes mate choice to reflect the pure operation of the "sexual attraction" mechanism(s). Can we seriously believe that, in societies in which virtually all of a woman's aspirations will be affected by the economic status of the man she marries, the response to questions about "mates" will be unaffected by nonsexual considerations? Buss's brief attempt to confront one instance of this point—his discussion of the hypothesis that women like men with resources because they are cut off from acquiring such resources for themselves—fails to appreciate both the force and the scope of the challenge. Data indicating that successful women have a strong preference for men with resources do not forestall the obvious concern that such women can attain their nonsexual goals, in the kinds of societies in which they live, only by following the culturally approved course for their less fortunate sisters and cousins. Furthermore, the general point is that in all cases libido may run one way and socioeconomic considerations quite another. Indeed, Buss might have found this out had he probed why his respondents gave the answers they did, for their explanations might have shown the various life dimensions along which they viewed mate choice. Perhaps, as Mae West unfortunately did not say, sex has nothing to do with it.

The point we've been developing extends to a broader criticism of Buss's "theory" by exposing its psychological poverty. As we noted above, in any attempt to link hypothetical psychological traits to behavior—even to the relatively special behavior of filling out a questionnaire—one must know how the traits interact and how they are affected by environmental cues. Imagine Buss's hero, Savannah Sam, with wonderfully refined dispositions to react to waist-hip ratio, hair lustre, bounciness in gait, and so forth. If Primeval Pru sets all the sensitivities aquiver, then, provided that no nonsexual disposition interferes (a large assumption), we can expect Sam to court (if that's the right verb) Pru. Sam's alternatives are not likely to be Pru, on the one hand, and Geriatric Georgina on the other. Maybe one of the women Sam confronts is ahead on bounciness and fullness of lips, but another wins on hair lustre and waist-hip ratio. What should the poor lad do? Buss doesn't tell us what the mate choice should be, and this is typical of the looseness of the amalgam of claims he offers. You can predict just about anything you want to from

his hypotheses by adjusting the relative strength of the sexual attraction dispositions or by invoking interference from other parts of the psyche.

Does this matter? One might think that Buss has done enough by describing a bundle of psychological traits and that he can leave it to future researchers to decide how these traits interact to produce behavior. Recall, however, that the point of the enterprise was to connect human psychology with evolution under natural selection, and natural selection will presumably discriminate our primeval players on the basis of their behavior. Until we have some idea of how the traits posited will issue in behavior, we can't make any judgment about their selective impact.

The elasticity of the connection between claims and evidence can be illustrated by returning to the proposition-in-the-quad. On the face of it, there's a striking asymmetry in male and female responses to the opportunity for a spot of recreational sex. But what accounts for the difference? Just the firing of the "sexual attraction" disposition in the men and its inhibition in the women? We agree with Natalie Angier's suggestion that the evidence may have more to tell us about women's fears than about their sexual yearnings.<sup>20</sup> Depending on how you adjust the relative strengths of the "attraction disposition" and the "fear disposition" you can predict the data from any hypothesis you choose about asymmetries in male-female sexual desire. Buss's favorite has no special privilege.

Even though we think that Buss's arguments from the data he assembles have the flaws to which we've pointed, we see his search for empirical evidence as an improvement in the customs of pop sociobiology. We can't be so positive about his Darwinizing. Consider his claim that "over a one-year period, an ancestral man who managed to have short-term sexual encounters with dozens of women would likely have caused many pregnancies."<sup>21</sup> A little sober physiology will show that there's a 1 to 2 percent chance of producing offspring per copulation. If Savannah Sam manages one-shot sex with one hundred different women, he may produce two offspring. His enduring evolutionary contribution will, of course, depend on whether these children survive (with whose support, exactly?). Even though one might wonder just what the expected reproductive success might be, it's important to recall that significant evolutionary change can occur when selection pressures are very small (of the order of 0.001, for example). So Sam's modest chances may make a crucial difference.

At just this point, however, the EEA fades into a rosy blur. Sam is supposed to be competing with other aggressive males for the chance to copulate. Some of his female targets may have long-term mates, primed (we recall) to be on the watch for lowered paternity certainty. The females themselves (we remember) are supposed to be less-than-completely interested in casual sex, so Sam is going to have to do a fair bit of talking before they go off with him for a romp in the bushes (but stay tuned! late-breaking news from Thornhill and Palmer suggests that talk may not be needed!). So let's ask the obvious questions: How big is the population to which Sam belongs? To what extent is it possible for his rendezvous to go undetected by others? In what percentage of the pregnancies he brings about will the child receive biparental support? What's the chance of surviving to sexual maturity without biparental support? It may spoil the fun to raise these questions, but until

they've been answered there's no way of telling whether Sam's ventures in sperm-spreading will prove selectively advantageous (or disastrous). To put it bluntly, we have to do some delicate accounting to decide if the expected increase in reproductive success is outweighed by the expected effects on Sam of the reactions of those around him to his activities. Any serious exploration of the operations of natural selection must make definite assumptions about what strategies are available to the organisms involved and what ecological constraints affect the reproductive payoffs.

One fundamental oversight of many misadventures in pop sociobiology (and its recent offshoots) is their neglect of within-group differences in strategies. Back to Primeval Pru. If (as Buss and others suggest) ancestral societies were pyramidal with a few men in power and many more scrambling underneath, it's not entirely obvious that being attracted to the Big Man with the Resources is a good female strategy. Maybe there's too much competition there, and Pru would do better to latch on to Mid-Level Mel. (Similarly, if all the males are drooling over Pru, Sam may do better to respond to the maternal promise of Plain Jane across the watering hole.) Pru needs enough to support herself and the kids, but that doesn't mean she'll be at an advantage if she goes for power, age, and the big bucks. If she's good at spotting talent, then Energetic Ernie—nothing but promise but nothing but promise!—would be a better bet. These are only *possibilities*, but they are rival accounts of selection that must be explored, not simply neglected. We leave as exercises to the reader the construction of formal models that will yield any number of different "Darwinian expectations,"<sup>22</sup> although we're prepared to concede to Buss the banal point that in none of these will Pru find Doddering Dan the Deadbeat the lodestone of her life.

We'll close our critique of Buss by pointing out how his conclusions, allegedly generated from Darwinian analyses of life in the EEA are, in fact, used as premises in ameliorating his ignorance about ancestral environments and their demands. Consider the following claims that are typical of Buss's efforts in evolutionary analysis:

Women over evolutionary history could often garner far more resources for their children through a single spouse than through several temporary sex partners.<sup>23</sup>

A lone woman in ancestral environments may have been susceptible to food deprivation. She may also become a target for aggressive men.<sup>24</sup>

The second is cagey enough, but he quickly slides from the cautious "may" in order to argue that ancestral women would need the protection and support of mates. So in both instances we have definite pronouncements about the challenges of the EEA. Intriguing and informative pronouncements.

In fact, current researchers know very little about the EEA—or even whether there's some privileged time period on which we should concentrate in understanding the evolutionary origins of human psychological tendencies. Should we even be concerned with selection on our hunter-gatherer ancestors rather than considering primate evolution on the one hand, and more recent gene-culture coevolution on the other? But Buss has a simple way of overcoming his ignorance. Consider his defense of the idea that paternity uncertainty was a problem for ancestral men: "Behavioral, physiological, and psychological clues point powerfully to a



human evolutionary history in which paternity uncertainty was an adaptive problem for men."<sup>25</sup> So here's the argument. We know that current preferences and propensities are actually adaptations because we can identify them as selectively advantageous in the EEA. And we recognize the selective advantages by drawing conclusions about the EEA on the basis of our knowledge that those current preferences and propensities are really adaptations. The analysis is viciously circular.

#### 4. The Slaving Beast Within: A Gothic Novella

The most substantial part of Thornhill and Palmer's *A Natural History of Rape* is its second chapter, in which the authors draw on earlier pop sociobiological discussions of asymmetries in sexual strategies, particularly the work of David Buss. The authors aim to build on those discussions to advance an account of how natural selection underlies many aspects of rape. Thornhill and Palmer are particularly interested in three main points, advanced in the writings we've just reviewed. First, the appropriate female strategy is to be choosy about potential mates. Second, the appropriate male strategy is to try to copulate as much as possible. Third, males have been selected to worry about issues of paternity. From these three points, Thornhill and Palmer draw their central conclusions. Rape should be especially painful to females because their attempts to choose their mates have been subverted. Males should be more inclined to rape because they are primed to copulate even when females are not interested, and, of course, they should be especially tempted by those females who exhibit the signs of high reproductive value (the young with bouncy gait, lustrous hair, and so forth). Males have also evolved to be suspicious of female claims that they have been coerced into copulating (more specifically: men have evolved to suspect the claims made by their mates), and that is why rape laws have taken the historical forms that they have.

So there we have it. An explanation of the principal features of rape by applying sound Darwinian principles. Add on a denunciation of that feminist canard that rape isn't a sexual act—what nonsense!—and we're done.

Well, not quite. What exactly are the Darwinian explanations supposed to be? Let's begin with the fundamental phenomenon. Some men rape women, and sometimes, men rape other men. Why do these acts occur and why do they occur in the contexts they do with a certain distribution of types of victims? Critics of previous sociobiological stories about rape have pointed out that many instances of rape involve as victims girls who haven't yet reached menarche or women who are past menopause. Thornhill and Palmer reply that "younger women are greatly overrepresented and that girls and older women greatly underrepresented in the data on victims of rape."<sup>24</sup> Waiving some concerns that will occupy us later, we note that this evidence seems relevant only to the kinds of questions that occupy Buss: the most it can show is something about the women rapists find most attractive (and, of course, we don't think it shows much about that). The question has been subtly shifted. Given that some men rape—for whatever reasons—why do they tend to rape young women? Answer: men are more likely to be attracted to young women,

so whatever it is that impels them to sexual coercion, young women are more likely to be the victims.

We are concerned with two features of this answer. First, we want to note that there's a controversial assumption that the psychology of rape parallels that of consensual sex. The rapist's behavior is seen as the product of a disposition to be attracted toward certain kinds of people, whether or not they are willing, and a disposition to force sex on a particular occasion. There's an obvious alternative psychological hypothesis, one that not only corresponds to many people's introspective awareness but also seems to permeate the folk tales, poetry, dramas, and stories of almost every culture, that views reciprocity as a central feature of sexual attraction. If that alternative hypothesis is right, then the strategy of seeing the rapist as someone whose tendencies to sexual attraction are just like those of any one else of the same sex, with something extra added on, is misguided. We don't know that the hypothesis is true—indeed, we recommend psychological exploration of it—but we don't think it should simply be dismissed without careful consideration.

We'll spend more time on a second issue. In our view, the major question about rape concerns the causes of coercion. At risk of being pedantic, let's aim for maximal clarity on this point. Imagine two stylized situations. In the first, a man (Adam) is attracted to a woman (Eve) and makes her a sexual proposition. Eve demurely declines. Adam does not force her (he may try to persuade, but he doesn't coerce). In the second, another man (Tarquin) is attracted to a different woman (Lucretia). Like Eve, Lucretia says "No." Tarquin presses on and eventually forces Lucretia to couple with him. Surely the centerpiece of a Darwinian account of rape should not be a story (a bad story, we've argued) about why Eve and Lucretia are found attractive, but rather an explanation of the difference between Adam and Tarquin. What is it about Adam that makes him hold back when Tarquin uses force?

Thornhill and Palmer don't offer any clear answer to this question. Whether this is because they don't have the issues in focus or because they haven't made up their minds we don't presume to judge. They do tell their readers that there are two different ways to apply Darwinian ideas to the study of rape. The *direct* approach supposes that there are "psychological mechanisms designed specifically to influence males to rape in ways that would have produced a net reproductive benefit in the past" (p. 59). The *by-product* approach proposes that there are a number of psychological mechanisms that have been shaped by natural selection that sometimes combine to trigger an act of rape. In a version of this approach that the authors draw from Donald Symons,<sup>27</sup> the mechanisms hypothesized are "the human male's greater visual sexual arousal, greater autonomous sex drive, reduced ability to abstain from sexual activity, much greater desire for sexual variety per se, greater willingness to engage in impersonal sex, and less discriminating criteria for sexual partners."<sup>28</sup> For reasons we've offered in earlier sections, we doubt that these hypothetical characteristics have been targets of natural selection, but the example does have the virtue of exposing Thornhill and Palmer's intended contrast. On the by-product approach, there's no commitment to supposing that acts of rape enhance (or once enhanced) the reproductive success of the rapist. Maybe there are all these adapted psychological dispositions that sometimes combine in ways that are unfortunate for the rapist (as well as being terrible for the victim).

Thornhill and Palmer don't advance any definite hypotheses about the Adam/Tarquin difference. We'll try to do better. Start with the direct approach. There are two possibilities. Either the adaptation is almost universal among human males or it isn't. On the former assumption, the rape disposition is present in just about every human being with a Y chromosome, and the fact that a lot of men don't engage in rape must be explained by invoking some combination of contextual cues and the inhibiting activity of other psychological dispositions. Plainly there's not going to be a lot of direct data to support this hypothesis until we've been told a lot more about possible cues and interactions. But maybe we can get some clues by thinking about the past action of natural selection.

Here's the simplest story. Males have been programmed to rape when they have a chance for copulating with a potentially fertile female and they can get away with it. If there were genetic variation in some savannah population with respect to the disposition to use force, so that most of the male population never engaged in sexual coercion while occasional mutants would rape fertile females only under conditions in which they incurred no costs, then the mutants would have slightly higher expected reproductive success (alternatively, we might suppose a disposition to use force only when the expected costs are lower than the expected reproductive benefits). At this point, everything depends on the details. As we noted in the last section, the chance that a copulation will lead to a birth is 1–2 percent (a figure with which Thornhill and Palmer<sup>29</sup> seem to agree), and this figure has to be discounted by the chance that the child will be abandoned, die before attaining puberty, or simply be ill-prepared for a successful reproductive future. Equally, we need a sober evaluation of the potential costs of an act of rape. Under what conditions, if any, in the savannah environment, could a rapist be expected to recognize that the chances of physical injury from other hominids were sufficiently low that the small benefit of forcing a copulation outweighed the expected costs? Again, we leave to the reader the exercise of constructing formal models that show rampant rape, a low incidence of rape, or no possibilities for the aspiring rapist. Hint: it's simply a matter of adjusting group size, daily habits, social structures, and aggressive tendencies.

The natural selection of the rape disposition is, of course, mediated by that remarkable mutant genotype that expresses itself in just the tendency to coerce copulation in the face of female reluctance when the circumstances are right (or whose effects on fitness are only so mediated). We harbor doubts about that genotype just as we are doubtful that some (or all) of us carry a genotype that enabled our Pleistocene ancestors to stand firm and pick an extra berry or two just when a lion was sufficiently far off to let them garner a small nutritive benefit without cost.

As we acknowledged, the story we've been telling is the simplest version of the universal variant of the direct approach. One embarrassing feature of our tale is that it fails to account for the difference between Adam and Tarquin—there are many Adams who seem to pass up opportunities that Tarquins exploit. Plainly, we need some epicycles, another psychological disposition or two to explain Adam's undue reticence or Tarquin's lack of proper caution. We'll also have to face up to the fact that rape victims are sometimes young girls or older women, so there'll have to be

other causal factors that make the tendency to rape misfire. Of course, as we build these in, we'll have to be very careful that we don't subvert whatever story we've been telling about the advantages in the ancestral environment; it will, for example, be disastrous if the sources of inhibition or excitation might have led our ancestors to actions that incurred great risks of injury (like the mythical Pleistocene berry-picker who carries an instant too long).

Maybe we can do better by switching to the polymorphic variant of the direct approach. Now we suppose that some men develop the rape disposition and others don't. No problem now with explaining the difference between Adam and Tarquin: Tarquin has it, Adam doesn't. The challenge this time is to conjure up a plausible tale about the way in which natural selection on our ancestors produced this polymorphism. Here's one way to try. Suppose that all males share a conditional disposition: if one experiences one type of developmental environment the rape disposition develops, if one experiences a different type of developmental environment it doesn't. Back now to Savannah Sam, first bearer of the mutant allele associated with this conditional disposition. Sam is going to have to have some reproductive edge. If this fails to involve any act of rape on his part, then it's hard to see why the allele should persist in the population. But if Sam's Darwinian advantage is a consequence of his developing in the pertinent environment, acquiring the rape disposition, and going in for a rape or two, then it's hard to see why a *fixed* disposition to acquire the rape disposition, come what may, wouldn't have been equally good. Once again, we urge readers to be imaginative and to construct evolutionary models for their favorite outcomes.

Perhaps the indirect approach will fare better. Indeed, there's a reading of Thornhill and Palmer on which the indirect approach must succeed if the direct approach fails. For, unfortunately, rape happens. The people who commit rape belong to a species that has evolved under natural selection. So, when an act of rape occurs, some combination of psychological features that humans have evolved to have must combine with environmental stimuli to prompt it. A triumph for the Darwinian approach to the human sciences?

Not really. The interpretation we've offered is banal, and would go through equally well whatever human activity—chopstick use or needlepoint, say—we were to consider. If the indirect approach is to vindicate Thornhill and Palmer's advertisement that evolutionary theory will guide “the scientific study of life in general and of humans in particular to fruitful ends of deep knowledge,”<sup>30</sup> then it will have to provide something more substantive than the vacuous suggestion that human actions draw on evolved psychological mechanisms. Something more like the version Thornhill and Palmer reconstruct from Symons, perhaps.

Let's assume for the time being that the asymmetries celebrated by Symons, Buss, and Thornhill and Palmer are genuine: males are more inclined to want casual sex than females and so forth.<sup>31</sup> Somehow these differences are supposed to be parlayed into an account of why rape sometimes occurs. So far as we can tell, there's just one option that will serve Thornhill and Palmer's turn. From time to time some men get so overstimulated that they just can't hold back, even though what they go on to do may be maladaptive (as well, of course, as being traumatic for their victims).



It doesn't take much thought to see why so simple a proposal won't do. Without further elaborate psychological hypotheses, we have no reason to reject the apparent evidence that a fair number of men who are as sexually stimulated as those who rape manage to accept a woman's refusal. On the face of it, the difference between Adams and Tarquins isn't simply one of the strength of sexual desire. If Thornhill and Palmer want to argue that appearances are deceptive, then they have a lot of work to do—they would have to show that there is some psychological (or neurophysiological) measure of level of sexual arousal that distinguishes all the rapists from all those men who accept rejection.

So what exactly is the difference between those males who behave like Tarquin and those, equally ardent, who emulate Adam? The obvious suggestion is that there are inhibitory mechanisms whose strength varies between the cases. Can we find any Darwinian clues about what such mechanisms might be? Thornhill and Palmer seem to believe we can. They cite work by "the evolutionary psychologist Neil Malamuth" on reduced sexual restraint. Malamuth, and others, have found that certain kinds of developmental experiences are correlated with an apparent "sexual impulsiveness and risk taking." Apparently "reduced parental investment (resulting from poverty or the absence of the father)" leads to "a male's perception of rejection by potential mates." Allegedly, "men emerge from this background with a perception of reduced ability to invest in women, an expectation of brief sexual relationships with women, a reduced ability to form enduring relationships, a coercive sexual attitude toward women, and an acceptance of aggression as a tactic for obtaining desired goals."<sup>32</sup>

The Darwinian language in the passage from which we have quoted is entirely gratuitous. What the studies reveal is that boys who are brought up in poor environments without a father have a higher tendency to harbor certain attitudes toward women and toward sexual relationships, attitudes that increase the chances that they will force sex. There's no warrant whatsoever for suggesting that this has a lot to do with parental investment or the young men's investment in potential mates. You don't need an evolutionary perspective to discover these attitudes and you don't require an evolutionary perspective to interpret them. The basic point is that there do seem to be variations among males in the mechanisms that inhibit the expression of sexual desire in the face of female reluctance, and by standard psychological studies of rapists, one can find correlations between the relative strength of the inhibitory mechanisms and characteristics of the developmental environment.

Once we've come this far, it's not hard to see that the insistent Darwinizing is at best irrelevant and at worst an obstacle. The fundamental question concerns the complex of psychological attitudes that inhibit, or fail to inhibit, the forcing of sex. If we consider the entire spectrum of rapes, including the rape of children and post-menopausal women, which Thornhill and Palmer consistently downplay, we can reasonably conjecture that the rapist's attitude often fails to acknowledge the victim as a person and sometimes even embodies a deliberate intention to demonstrate that the victim is the object of hostility or contempt. Adam holds back, even in the grip of intense desire because he acknowledges Eve's right to say "No." Tarquin, by contrast, sees Lucretia as less than fully human, or wishes to show his dominance

of her, or intends that his rape will serve as an act of revenge. The critical task for a theory of rape is to be able to characterize these attitudes as precisely as possible and to understand how they come about. We are prepared to believe that poverty can breed frustration, that a father's absence and the lack of parental affection can engender tendencies to see others as utensils rather than people. Exploring these psychological issues and the causal relationships they involve is not advanced by the speculative invocations of Darwin that Thornhill and Palmer favor.

But wait! Don't Thornhill and Palmer have a reply to the charges we've leveled? After all, they devote an entire chapter to attacking "the social science explanation of rape," in which they consider, and take themselves to demolish, arguments to the effect that rape is about hostility, dominance, punishment, and the desire for control. Consider the following typical passage.

Brownmiller (1975) sees rape in large-scale war as stemming in part from the frenzied state of affairs and the great excitement of men who have just forcefully dominated the enemy. That hypothesis predicts that soldier rapists would be indiscriminate about the age of the victims. But they are not; they prefer young women.<sup>33</sup>

The second sentence we've quoted is, we believe, unwarranted. Brownmiller's position, as we would reconstruct it, can be developed as a pair of claims:

1. For whatever reasons (not necessarily the Darwinian tales Thornhill and Palmer borrow from Buss), men are typically more attracted to young women.
2. The coercive expression of sexual desire is the result of a failure in an inhibitory mechanism that can be caused by hostility toward the victim.

So Brownmiller (at least on our reconstruction) would predict *both* that the frequency of rape would be greater in a situation of war, in which soldiers express hostility towards the victims (and, very probably, their desire to show dominance), *and* that the distribution of rape victims would be skewed towards younger women.

The logical mistake evident here is common to T&P's other discussions of social scientific hypotheses about rape in general and of feminist proposals in particular. They claim that all kinds of confusions flow from viewing rape "as an act of violence."<sup>34</sup> But the confusions are all Thornhill and Palmer's. Rape is not just about violence; there's a difference between the rapist and the batterer. In our judgement, however, rape isn't just about sex either. If Thornhill and Palmer had seen clearly that they need to account for the difference between Adam and Tarquin, they'd have recognized that other psychological mechanisms and attitudes come into play and have appreciated the obvious possibility that, in most instances of rape, motives of aggression and dominance are also present. Further they might have seen that general characteristics of societies are pertinent to the attitudes that adult human beings have toward one another, and in particular to the attitudes that men have toward women. They might then have acknowledged that broad social tendencies can permeate psychological development and lead men to acknowledge women as full persons—or not. The feminist authors who have suggested that prevalent cultural images of women are relevant to how a woman's refusal is heard have a genuine point.<sup>35</sup>

We'll be completely explicit. When rape occurs, there's a sexual dimension to the event. When sexual intercourse is forced, there are typically nonsexual dimensions to the event. The attitudes that lead to the coercive sex often involve intentions to hurt, dominate, humiliate, and obtain revenge. Those attitudes are themselves often present because of a complex developmental history, one that may involve not just details of individual ontogenies (lack of parental affection, for example) but also more general cultural influences that lead men not to see women as full people (but, for example, as collections of salient body parts—genitals, breasts, buttocks, lustrous hair, full lips, and so on).

Let's sum up the discussion of this section. We've examined the two variants of the direct adaptation approach and found that the task of working out a coherent Darwinian model that will fit the evidence is, to say the least, challenging; the challenge is not taken up by Thornhill and Palmer. The by-product approach leads fairly quickly to the sensible proposal that rape occurs when certain inhibitory mechanisms are weakened. Despite their attempts to drag in Darwinian language, T&P fail to show how evolutionary psychology can illuminate the character of these inhibitory mechanisms. Further pursuit of the sensible proposal seems to require research in developmental psychology, and quite possibly elaborations of the social science hypotheses that Thornhill and Palmer deride.

We'll spare the reader an equally extensive treatment of Thornhill and Palmer's two other major claims, the thesis that rape is especially hurtful to women because it subverts their preferred mating strategy and the idea that rape laws reflect male concern with paternity certainty. The analysis of these proposals would proceed on similar lines. Once again, we'd ask just what the selective advantage of intense female pain is supposed to be. Is this a psychological adaptation shared with other primates, or is it part of a female tactic for reassuring Mr. Big Bucks with his refined paternity uncertainties? We'd invite consideration of the hypothesis that people have a general tendencies to feel hurt when they have been used and to expect tenderness and the expression of affection in sexual contact. Similarly, it would be appropriate to ask exactly why attitudes of suspicion toward female testimony are supposed to be adaptive, and to consider the precise costs and benefits of reacting to rape in different ways.

We have offered only hints. Any serious evolutionary account is going to have to advance definite claims about the character of the adaptation, the set of available strategies, and the environment in which selection is alleged to have taken place. This, of course, is what evolutionary theorists do. But Thornhill and Palmer do not live up to the standards of the discipline. Their identification of adaptations is entirely elusive, and there's not a shred of discussion of available strategies (let alone of potential genetic bases for them!) or of the environmental details.

These are harsh words, and we anticipate protests. Surely Thornhill and Palmer do appeal to broad and familiar features of evolution on sexual species, the sexual asymmetries, paternity worries, and so forth that they treat as cardinal dogmas of general evolutionary theory. Isn't it enough to rely on the work of others and to consider ways in which the challenges of natural and sexual selection might be met? No. To make progress in understanding the springs of human behavior, it's necessary to be far clearer about the nature of the selection pressures, the consequences

of the allegedly favored strategy and the possible rivals. Thornhill and Palmer tell us nothing specific about the problems that might be addressed by a tendency to rape or by a disposition to feel intense pain at being raped. All their readers get are vague gestures. Such insubstantial suggestions would not be taken seriously in other areas of evolutionary studies. Workers on social insects or sage grouse don't simply talk vaguely about the requirements of obtaining food or avoiding predators; they explore the ecological parameters they take to be significant; they engage in studies to discover the kinds of strategies their organisms can employ; they collect data on reproductive rates. We appreciate the difficulties of meeting such high standards in the study of our own species. But, when the gap between standards and practice is as vast as it is in this discussion of human rape, it's simply false advertising to claim to be in the same business.

## 5. Conclusion: In Defense of Irreverence

We believe that the studies we have reviewed are scientifically shoddy. But there's surely a fair amount of bad work in the world. Why should people become so upset with the evolutionary psychology of sex and violence, as practiced by Buss, Thornhill, and Palmer? We'll close with a brief attempt at explanation.

It's not incumbent on scientific researchers to offer policy suggestions, but some recent pop sociobiologists—including Thornhill and Palmer—have defended their proposals about human nature by declaring that they can help resolve urgent social issues. Even though we concede that they have good intentions, that they want to help decrease the incidence of rape, it's hard to avoid the judgment that Thornhill and Palmer's suggestions, where not banal, will do little good. Given the speculative character of their Darwinizing and the elusiveness of their proposals, even their inability to recognize crucial issues, policies influenced by their text might well make matters worse.

Consider, for example, their suggestions about educational programs. They begin with a program for boys, agreeing "with social scientists that males should be educated not to use force or the threat of force to obtain sex."<sup>36</sup> No problem so far, but we didn't need any Darwinizing to arrive at this judgment. Keen to show the fecundity of their ideas, Thornhill and Palmer continue with two disastrous further suggestions. First, they propose that educators should explain the differences between male and female sexuality. As we pointed out repeatedly in the last section, even granting the pop sociobiological claims about these differences, the crucial question is why some men (Adams) hold back from forcing women to their desires and others (Tarquins) don't. Any program based on stating "the evolutionary reasons why a young man can get an erection just by looking at a photo of a naked woman."<sup>37</sup> is pointing in the wrong direction and encouraging a view of the springs of rape that may encourage young men to downplay its importance ("Well, it's only human nature after all!"). The critical part of the education, as so many feminists and their social scientific allies have insisted, should be to teach young men that "No" means No, and to help them overcome the kinds of hostility, dominance, and desires for power that are so frequently part of the psychological cause of rape.

A misguided program for boys is bad enough. But Thornhill and Palmer also want a parallel program for girls, pointing out to them the True Nature of the Slavering Beasts with whom they are doomed to reproduce. Young women "should be made aware of the costs associated with attractiveness."<sup>38</sup> Not only is this vulnerable to just the criticisms we directed at the tutorial for boys, but its social consequence is likely to be a continued perception that women are partly responsible for rape ("She was asking for it").<sup>39</sup> Any sensible approach to rape education should be freed from suggestions of female responsibility or complicity, directed toward correcting a problem in male attitudes, clearly demarcated from the expression of some hypothetically universal male sexuality, and firmly linked to a failure in inhibiting mechanisms. Thornhill and Palmer seem to be suggesting an educational program that will reinforce attitudes that ought to be extinguished.

No wonder, then, that they arouse such ire. But we still have told only part of the story. If, as many scholars believe, individual ontogenies are affected by stereotypes in the broader culture, so that male views of women are sometimes shaped by a widespread tendency to reduce them to sexual playthings, then pop sociobiologists don't just ignore crucial causal factors. In their style of analysis, their tendentious talk of "reproductive potential," "investment," "paternity certainty," and so forth, they dehumanize the complex activity of human courtship, love and marriage, embodying in their prose just those images of women as bundles of sexually pertinent body parts—genitals, breasts, lustrous hair, and the rest—that are taken to contribute to the devaluation of women and the incidence of rape. Buss, Thornhill and Palmer and their colleagues give academic respectability to ways of regarding women and of viewing sexual relations that many people see as profoundly damaging, and they do so by using an idiom that portrays women as resources and sex as commerce.

There are self-pitying moments in *A Natural History of Rape* in which the authors wonder why their work inspires hostile reactions. No prizes for guessing their preferred explanation: they stand in a line of thinkers that extends back to Galileo, a line of fearless revolutionaries dedicated to science and truth. We offer a harsher alternative. They pretend to scientific rigor when they have none; they misunderstand the positions of those whom they lambast; they blunder into sensitive issues, self-righteously offering proposals that it's reasonable to fear will be counterproductive; and they employ language and images that reinforce just those social tendencies their opponents view as crucial factors in producing pain and humiliation for women.

Just as we think the comparison with Galileo inappropriate, we don't recommend that pop sociobiologists be shown the instruments of torture. We think instead that what Thornhill and Palmer, and others of their ilk, merit is a thorough irreverence, born of recognizing that the dignity of academic prose is not in order here. In short, the Bronx cheer.

#### Notes

We would like to thank Allan Gibbard for helpful conversations, although we are not persuaded by his more positive view of evolutionary psychology; we are also grateful to Patri-

cia Kitcher for some extremely constructive advice about an earlier draft. Jerry Coyne and Richard Lewontin supplied extensive written comments on the penultimate version and have helped us to improve it in a large number of ways; we are deeply indebted to them.

1. Respectively: G. Oster and E. O. Wilson, *Caste and Ecology in the Social Insects* (Princeton, N.J.: Princeton University Press, 1978); G. Parker, "Searching for Mates," in J. R. Krebs and N. Davies, eds., *Behavioral Ecology: An Evolutionary Approach* (Oxford: Blackwell, 1978); T. Clutton-Brock et al., *Red Deer* (Chicago: University of Chicago Press, 1981).

2. E. O. Wilson, *Sociobiology: The New Synthesis* (Cambridge, Mass.: Harvard University Press, 1975); Wilson, *On Human Nature* (Cambridge, Mass.: Harvard University Press, 1978); David Barash, *The Whisperings Within* (London: Penguin, 1979); Pierre van den Berghe, *Human Family Systems* (New York: Elsevier, 1979).

3. For critique, see Richard Lewontin, Steven Rose, and Leon Kamin, *Not in Our Genes* (New York: Pantheon, 1984); Philip Kitcher, *Vaulting Ambition: Sociobiology and the Quest for Human Nature* (Cambridge, Mass.: MIT Press, 1985).

4. Charles Lumsden and E. O. Wilson, *Genes, Minds, and Culture* (Cambridge, Mass.: Harvard University Press, 1981); John Maynard Smith and N. Warren, "Review of *Genes, Minds, and Culture*," *Evolution* 36 (1982): 620–627; Kitcher, *Vaulting Ambition*, chapter 10.

5. Lewontin et al., *Not in Our Genes*; Kitcher, *Vaulting Ambition*.

6. Randy Thornhill and Nancy Thornhill, "The Evolutionary Psychology of Men's Sexual Coercion," *Behavioral and Brain Sciences* 15 (1992): 365–375.

7. Randy Thornhill and Craig Palmer, *A Natural History of Rape* (Cambridge, Mass.: MIT Press, 2000).

8. A classic source of the modular approach to the mind is Jerry Fodor, *The Modularity of Mind* (Cambridge, Mass.: MIT Press, 1981). Whether Fodor would recognize the use that evolutionary psychologists make of his ideas is quite another matter. But many of the most influential writings in evolutionary psychology, particularly the articles of Leda Cosmides and John Tooby, do champion the Fodorian notion of module as an "informationally encapsulated psychological subsystem." The terminology is much less evident in the authors whose views we discuss here, although they share the common evolutionary psychological strategy of atomizing the mind into parts that are taken to be under independent selective control. We'll henceforth avoid the technical term *module*.

9. Robert Boyd and Peter Richerson, *Culture and the Evolutionary Process* (Chicago: University of Chicago Press, 1985).

10. In a rather uninformed discussion of culture and its impact on behavior, Thornhill and Palmer (see *Natural History of Rape*, 27) show that they do not really understand the work of Boyd and Richerson. They show a similar lack of comprehension in lumping the recent group selectionist proposals of Elliott Sober and David Sloan Wilson with older views that have been decisively discredited (*ibid.*, 6). It strikes us as odd that authors who are so keen to introduce an evolutionary perspective into the social sciences should be so superficially informed about theoretical issues pertaining to evolution.

11. See Thornhill and Thornhill, "Evolutionary Psychology," 405.

12. David Buss, "Psychological Sex Differences: Origins through Sexual Selection," *American Psychologist* 50 (1995): 164–168, at page 164.

13. David Buss, "Sex Differences in Human Mate Preferences: Evolutionary Hypotheses Tested in 37 Cultures," *Behavioral and Brain Sciences* 12 (1989): 1–49.

14. *Ibid.*

15. *Ibid.*

16. David Buss, *The Evolution of Desire* (New York: Basic Books, 1994), 52, 77.
17. David Buss, *Evolutionary Psychology: The New Science of the Mind* (Boston: Allyn and Bacon, 1999), 161. The observant reader will note that there's a slight problem in Buss's coopting this experiment for his own purposes, since the point of his investigations is to *discover* what kinds of people men and women find attractive. The experiment was, however, carried out (by Clarke and Hartfield) on the basis of a prior estimate of attractiveness. But we let this pass.
18. See Buss, "Mate Preferences Mechanisms: Consequences for Partner Choice and Intrasexual Competition," in J. Barkow et al., eds., *The Adapted Mind* (New York: Oxford University Press, 1992), 249–266, at p. 253.
19. Walter Mischel, *Personality and Assessment* (New York: Wiley, 1968); D. A. Moore, "Order Effects in Preference Judgments: Evidence for Context Dependence in the Generation of Preferences," *Organizational Behavior and Human Decision Processes* 78 (1999): 146–165.
20. Natalie Angier, *Woman: An Intimate Geography* (Boston: Houghton Mifflin, 1999).
21. Buss, *Evolutionary Psychology*, 162.
22. See Kitcher, *Vaulting Ambitions*, 170–171, for some straightforward ways of replacing casual speculations about sexual strategies with the kinds of models that are constructed in competent evolutionary studies.
23. Buss, *Evolution of Desire*, 23.
24. David Buss, "The Psychology of Human Mate Selection," in C. B. Crawford and D. L. Krebs, eds., *Handbook of Evolutionary Psychology* (Mahwah, N.J.: Erlbaum, 1998), 405–429, at page 416.
25. David Buss, "Paternity Uncertainty and the Complex Repertoire of Human Mating Strategies," *American Psychologist* 51 (1996): 161–162, at page 161.
26. Thornhill and Palmer, *Natural History of Rape*, 72, drawing on Randy Thornhill and Nancy Thornhill, "Human Rape: An Evolutionary Analysis," *Ethology and Sociobiology* 4 (1983): 137–173.
27. Donald Symons, *The Evolution of Human Sexuality* (New York: Oxford University Press, 1979), 264–267.
28. Thornhill and Palmer, *Natural History of Rape*, 62.
29. *Ibid.*, 100.
30. *Ibid.*, 3.
31. *Ibid.*, 62.
32. *Ibid.*, 68–69.
33. *Ibid.*, 134. Citation to Susan Brownmiller, *Against Our Will: Men, Women, and Rape* (New York: Simon and Schuster, 1975).
34. Thornhill and Palmer, *Natural History of Rape*, 136ff.
35. Perhaps there's a more charitable interpretation of Thornhill and Palmer, one that sees them as recognizing the fact that rape isn't only about sex or only about aggression (power, dominance, etc.). Perhaps Thornhill and Palmer and the feminists they criticize can agree on rejecting both polar positions (rape is a matter of sex alone, rape is a matter of aggression alone). We think that the constant emphasis on sexual strategies shaped by selection, and the failure to distinguish the question of explaining the characteristics of rape victims from the question of distinguishing between Adam and Tarquin makes any such interpretation unlikely. Authors with the more charitable interpretation clearly in view would have written a very different book.
36. Thornhill and Palmer, *Natural History of Rape*, 171.
37. *Ibid.*, 179.
38. *Ibid.*, 181.

39. As Dick Lewontin pointed out to us, this phrase needs careful consideration. Sometimes women do dress in ways that they hope will lead men to find them desirable. But surely these women do not want the male desires to lead to sexual coercion. Educational programs should surely be very clear about the difference between the desire to be desired and the desire to be attacked.