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Sexual Strategies: A Journey Into Controversy

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When I began my scientific exploration of human sexual strategies in 1981, I sensed that the work would be controversial. Mix the ingredients of sex, evolution, and sex differences, and emotions start to run high. Humans don't seem especially well designed for dispassionate intellectual discourse about any of these topics when they are considered alone, much less when they are combined. When people universally feel so strongly about a topic, however, and when scientific findings are greeted with fear and loathing, it's a pretty sure sign that one is on to something important.

During my undergraduate and graduate training in psychology, from 1971 to 1981, there was no such thing as evolutionary psychology, no one studied human mating strategies, and sex differences were believed to be trivial or nonexistent. The dominant theories of the field emphasized social learning, socialization, and arbitrary social roles—all precursors of social constructionism. If there was a nature to humans, according to mainstream assumptions, it was that humans had no fundamental nature. People were plastic, formless, passive receptacles whose adult form was achieved solely by input that occurred during development—input from media, parents, teachers, peers, and the dominant interests of those in power. Aggression, I was told, was learned and therefore could be unlearned. Whether participants in psychological studies were male or female was not even reported in many published articles, until the American Psychological Association added a rule sometime in the 1970s to require it. Even those reporting the sex composition of their participants rarely analyzed their data to see whether men and women might differ. Those few differences that were so obvious that they could not be swept under the rug—differences in physical aggression and in spatial rotation ability—were attributed to socialization and to arbitrary "gender schemas" that could be eliminated and replaced with alternative gender-free schemas. One of my graduate school mentors, Dr. Jeanne Block, was featured in a TV documentary called *The Pinks and the Blues* (Nova Science Programming, 1980). Her theory, published in *American Psychologist* (Block, 1973), was a prototype of the dominantly held views of psychology—sex differences derived from sex role socialization. Dressing girls in pink and boys in blue and giving boys baseball bats and girls Barbie dolls exemplify this hypothesized process. I cannot say that there was ever a time when I found these mainstream theories compelling, but I was in a tiny minority.

The Genesis of an Idea

Evolutionary theory was the first intellectual idea that mesmerized me. My exposure to it came from a freshman college geology class. It had never occurred to me before then that there were theories designed to explain things as vast and complex as the origins of life and the existence of the component parts of all living things.

As an undergraduate, I had a rebellious streak that persists in muted form today. Rattling the cages of my more conventional teachers was a favorite pastime. In this context, as a junior, I took a course in 1975 from David Hovland, an assistant professor at the University of Texas and the son of the famous social psychologist Carl Hovland. He encouraged creativity, so I decided to go out on a limb. I wrote a term paper entitled "Dominance and Access to Women," in which I advanced the thesis that the central motivation behind men's quest for status, the reason men struggle and claw their way up the social hierarchy, was to gain sexual access to women. The paper was based on a naive understanding of evolution, a superficial reading of the primate literature, and a fascination with strange non-Western cultures such as the Tiwi tribe of northern Australia who practice an unusual form of polygyny. At the time, I did not really believe that the central thesis of the paper was correct. To my astonishment, Dr. Hovland liked the paper and it generated tremendous interest from other students when he asked me to present it to the rest of the class.

From then until my PhD from the University of California at Berkeley in 1981, my interests in evolution and mating remained largely submerged. I continued to read books about evolution in my spare time and even conducted one study in graduate school to see whether those who scored high on a dominance scale did in fact have a larger number of sex partners (I never got around to analyzing the data). However, my interest in evolution and human nature grew in graduate school. The Berkeley faculty were tolerant enough to permit me to choose "evolution and personality" as one of three topics for my qualifying examinations for PhD candidacy. But none of the faculty knew much about evolutionary biology, and I ended up choosing a more mainstream topic for my dissertation. Evolution remained an avocation, something I pursued when not "working."

Arrival at Harvard

My first position after the PhD in 1981 was as assistant professor at Harvard University, and I experienced the bliss of total freedom of intellectual pursuit for the first time. The first class I taught was Human Motivation, a course I took over from David McClelland. I decided to use evolution as the overarching theme for the course. The main focus of my research remained mainstream, however, and I was able to "make my bones" with publications in journals such as the *Journal of Personality and Social Psychology* and *Psychological Review* without ever mentioning evolution. Perhaps this early mainstream success afforded me the latitude to take chances. I think the main impetus for what appeared to others as a radical shift in my research came from pure intellectually driven curiosity. I had been reading about the evolutionary theories of Robert Trivers and others, began reading the fascinating book by Don Symons (1979) called *The Evolution of Human Sexuality*, and started to teach the exciting ideas contained in these works to my classes. It occurred to me that some of the evolutionary ideas could actually be tested with humans—specifically, Trivers's (1972) theory of parental investment and sexual selection, from which predictions about psychological sex differences could be derived.

At the time, I happened to be designing my first large-scale study of married couples for purposes entirely unrelated to evolution or mating (I sought a way around the limitations of self-report data and so wanted to obtain the additional data source of spousal reports). I decided to include a 76-item measure of mate preferences that had been developed by a former mentor, Dr. Harrison Gough of the University of California at Berkeley, but never formally published. I wrote down my predictions in advance, based on a straightforward derivation from the ideas of the great thinkers George Williams, Robert Trivers, and Don Symons. When the results rolled off from the computer printout, I felt captivated—the sex differences in mate preferences emerged exactly as predicted. Although I had published some 20 articles already and had plenty of success in pursuing the more mainstream paths, this was the first time in my scientific career that I felt like I was dealing with powerful predictive hypotheses anchored in a solid theoretical foundation.

I did not publish these results. Findings of sex differences in one sample of 200 Cambridge residents hardly seemed like compelling support for the evolutionarily derived theories. Perhaps these sex differences were culture specific. In 1983, I pursued two courses of action. First, I sought out competing explanations for the findings. I talked to perhaps 2 dozen scientists of all stripes—psychologists, sociologists, anthropologists, and biologists. I showed them my findings and asked them how they would explain them. Then I asked them to make a prediction: Would the sex differences be found universally across cultures? I will not divulge their names, although many are well-known. Almost all offered explanations of this sort: These sex differences are best explained by socialization, culture-specific social roles, sex-role socialization, or the economic powerlessness of women. Almost all these scientists predicted that sex differences would not be found across cultures. Some proposed that they might be found in Western cultures, or capitalist cultures, but surely not in non-Western cultures, and surely not in traditional or tribal cultures (these were then called "primitive" cultures). The sex differences in mate preferences—the greater premium placed by women in cues to resource acquisition (e.g., financial prospects, social status, older age) and the greater premium placed by men on cues to fertility (youth, physical attractiveness), they argued, were unique to America, capitalism, or particular cultural contexts. I wish that I had requested these scientists to sign their names to their predictions, which I scrupulously wrote down. It is a small irony that evolutionary hypotheses are often unjustly criticized as being post hoc stories; I found out subsequently that this accusation is far truer of socialization and social role explanations, from which clear predictions are almost never derived, yet all findings can somehow be "explained" post hoc. At any rate, being trained by my Berkeley mentors that the hard hand of empirical data is the final arbiter of scientific theories, I set out to see which predictions would be confirmed and which disconfirmed.

The International Mate Selection Project: The 37-Cultures Study

The International Mate Selection Project (IMSP) started out small, for I had no funds except for a few thousand dollars of seed money that Harvard granted to its assistant professors on arrival. Most of these moneys had been spent paying for the Cambridge couple study, which required paying each couple for their participation. Harvard had no "subject pool," so free study participants were nonexistent. I had to rely on friends, squash partners, colleagues, and a team of extraordinary Harvard undergraduates who, for reasons that baffled my fellow professors, remained ferociously devoted to my lab (among them were Mike Barnes, Dolly Higgins, Mary Gomes, Karen Lauterbach, Sara Oppenheim, and

Neils Waller, most of whom subsequently earned their PhDs at places such as Yale University, Stanford University, the University of Michigan, and the University of Minnesota, and all of whom achieved various combinations of professional and reproductive success).

My first cross-cultural opportunity came from a colleague in Germany, Professor Alois Angleitner, who invited me to a personality conference in Bielefeld in 1984. In preparation, I enlisted a squash partner, who happened to be a professional translator, exceptionally fluent in both in German and English. For several weeks running, after our squash match, he and I poured over successive revisions of the translation of two key instruments, discussing the subtle nuances of each word. Two weeks after my return from Germany, a package arrived, courtesy of Dr. Angleitner, containing the data of 400 German participants who completed the translated instruments. Thus, in a flourish of excitement, the IMSP was launched. The relative ease of securing a German sample deluded me into believing that I could secure samples from other countries with comparable effort.

Anyone who has done cross-cultural work knows well the enormous difficulties involved in translation, back translation, and resolving discrepancies between translation and back translation. Some languages require different words for the male and female forms. Some languages contain no exact equivalent to the English terms. Sometimes, collaborators get creative and insert additional items, thus jeopardizing the parallel structures of the instruments. Data collection in foreign countries requires compromises in testing conditions and the nature of samples. Many collaborators performed heroic and outstanding feats. My Venezuelan collaborator obtained random stratified samples from every fifth house in neighborhoods differing in socioeconomic status. My Brazilian collaborator gathered data from six different cities, including Curitiba, São Paulo, Santa Catarina, Brasília, and Rio de Janeiro. My Israeli collaborators managed to secure respectably sized samples of both Jewish and Palestinian inhabitants. And a brave graduate student collected a small, but invaluable, sample from Tehran, Iran.

I was especially eager to obtain data in non-Western cultures, and in these we sometimes encountered unexpected difficulties. My collaborator from South Africa braved physical danger to collect a Zulu sample. Some Zulu women were reluctant to divulge their mate preferences, expressing a fear that such knowledge might give Zulu males some advantage in mating and promote sexual deception. China, in 1984, was closed to outsiders, and information flow was policed scrupulously. Through a series of connections, my collaborator was able to secure a sample of 500 individuals from four disparate locations within mainland China, laboriously transcribe the responses into special code, and essentially smuggle out the results in the form of a letter that miraculously made it to my office in William James Hall. My Nigerian colleague wished to know whether I sought mate preferences for a man's first wife, second wife, or third wife. We had to modify the Nigerian instrument to reflect that fact that it is a legally polygynous culture (three cultures in the IMSP practiced legal polygyny).

In one then-communist country, my collaborator's first reply to my invitation to join the project was: "Dear Dr. Buss, I'm delighted to work with you, and will begin data collection right away." Several months later, I received another letter: "Dear Dr. Buss, I have collected data on the female sample, and am now commencing data collection on the male sample." Another 6 months passed: "Dear Dr. Buss, I am very sorry, but I have been *unable* to collect any data." I found out later that the central government had gotten wind of the project and terminated their country's participation. Data from that country were never obtained.

It took 4 years and the unpaid dedication of 50 research collaborators from around the world to secure 10,047 participants from 37 cultures within 33 countries located on 6 continents and five islands. I reasoned that the effort was worthwhile. The results would subject the evolutionary hypotheses to the first truly rigorous empirical test. Of all possible patterns of results, nearly all could falsify the hypotheses; only one pattern of results could confirm them. I knew when I launched the project that no matter how much data I collected, there would always be someone who would say something such as this: "Well, that's all fine and good, but have you studied the Bongo Bongos [a fictitious group] from northern south Zafariland? I hear they do it all differently there." However, I collected enough data from enough different places over a 4-year span to warrant taking stock. Indeed, data of this cross-cultural scope far exceeded, and continue to exceed, most tests of psychological hypotheses, which, as is well-known, typically use only American college students. And this point, I was blessed with the supreme dedication of University of Michigan student Armen Asherian, who managed the monstrous data set as well as the team of half a dozen assistants required to process the data.

In retrospect, doing this study—indeed, embarking on empirical tests of evolutionary psychological hypotheses—was a risky, perhaps foolhardy thing to do. I was an assistant professor without tenure. I knew the findings had the potential to upset people. I had a thriving career doing more mainstream psychological research. However, I had no choice—I realized how important mating was, how inadequate mainstream theories of mating were, and how little was known empirically about human mating. Nothing fascinated me more. Indeed, mating was then, and remains now, a topic that captivates me more than any other. I couldn't not study human mating.

Taking the Plunge—Deeper Into Mating

While all these data were being collected, a number of other factors conspired to propel me to delve deeper into mating. One signal event occurred when Bob Sternberg invited me to give a talk to the Yale psychology department, and I will always be grateful to Bob for giving a young, virtually unknown assistant professor this wonderful opportunity. Instead of giving the usual talk on my mainstream research, I decided to take a chance—I gave my first colloquium on human mating. Unbeknownst to me, the editor of the prestigious journal *American Scientist* was in the audience. After I returned to Harvard, I received a letter from him inviting me to write an article for *American Scientist* on human mating. It was the publication of that article in 1985 that led to many of the professional research collaborators who eventually became part of the IMSP. The Harvard name probably didn't hurt either.

To prevent making an utter fool of myself by revealing my actual ignorance about the topic, I set out to educate myself and read virtually every empirical and theoretical article that had ever been published on the topic of human mating. It began to dawn on me that, simply put, there were no good theories of human mating out there. In fact, all existing theories were extraordinarily simplistic, positing single and simple-minded mechanisms—that people seek "equity" or "similarity" or "one's opposite-sex parent" when seeking a mate. No theory of mating described why humans would be motivated in these directions. None contained any clauses or provisions for sex differences in desires, preferences, or strategies; indeed, none even mentioned sex differences, presumably because men and women were thought to be psychologically identical. Furthermore, all extant theories focused on marriage, ignoring the many other forms of mating such as short-term mating, extrapair sex, serial mating, and mixed mating strategies. At the same time, I educated myself about mating in other species—from scorpion flies to pigeons to peacocks to primates. I devoured the theoretical work on mating in evolutionary biology. And although my publishing productivity suffered during this period of intellectual retreat, I had an intuitive sense that I was onto something important.

What happened next is the sort of creative intellectual flowering that I have only experienced a few times. I began to see that mating was not some isolated topic of mild cocktail party amusement, separated from other areas of psychology. I began to believe, perhaps delusionally, that mating was the center of the psychological universe. This may not seem as wild as it sounds, for it has a compelling evolutionary rationale. The evolutionary process, contrary to the surface understandings held by many, is not centrally about survival. It is about reproduction or, more precisely, differential reproduction caused by heritable differences in design. In fact, the only things that can evolve by natural selection are those things that contribute to reproductive success, either directly (through personal reproduction) or indirectly (through enhanced reproduction of one's genetic relatives). And nothing lies closer to differential reproduction, the engine of evolution, than mating. Those who fail at mating lack descendants. Each one of us is an evolutionary success story—the end product of thousands of generations of ancestors, each of whom succeeded in the complex tasks of successfully choosing a mate, attracting a mate, mating with a mate, and ensuring that the products of those matings produced offspring who themselves would succeed in mating. Many social phenomena—scaling social hierarchies, forming friendships, maintaining coalitions, detecting cheaters—are ultimately tributary to success in mating (Buss, 2003).

Thus, the topic of our desires in mating—what I had begun to study with the IMSP—was merely the beginning, but it was an extremely important beginning. In my view, desires lie at the foundation of human mating. Desires determine the people to whom we are attracted, as well as those from whom we are repulsed. Fulfilling the desires of another is the key to successful mate attraction. Violation of desire is key to conflict between the sexes. Competition, conflict, harmony, and happiness can all be predicted, in part, from deep knowledge of what people desire. Therefore, I launched projects on the tactics that people use to attract a mate, which have great relevance to many forms of intrasexual competition (Buss, 1988a), and this led to studies of how people use verbal tactics to derogate their competitors (Buss & Dedden, 1990). I studied "love acts," testing hypotheses about the evolutionary biology of love (Buss, 1988c). I launched studies of how people do "mate guarding," or perform tactics to retain their mates and fend off rivals (Buss, 1988b; Buss & Shackelford, 1997). I tested novel hypotheses about conflict between the sexes in the mating arena (Buss, 1989a, 1991), which led to studies on jealousy and eventually homicide (Buss, 2000; Buss, Larsen, Westen, & Semmelroth, 1992). These were a few of the many fruits of this personal intellectual flowering. To this date, I have only been able to test a small fraction of that early explosion of ideas about human mating. Nonetheless, I felt like I was onto something important, had some powerful theories that were guiding my research, and was in fact discovering new things about human mating that no one had previously discovered.

I think I was fortunate to get offered a tenured position as associate professor at the University of Michigan in 1985, 4 years out of my PhD, well before the fruits of the mating work hit print. Although I had published my first evolutionary article in *American Psy-*

chologist already (Buss, 1984), it was not at all clear, either to me or to anyone else, that my entire career would get devoted to helping to establish the theoretical and empirical foundations for the field of evolutionary psychology. Had Michigan known this, I am not sure they would have hired me.

Although my first publication on human mating came out in *American Scientist* in 1985 (Buss, 1985), it was not until the1989 publication of the 37-culture study in *Behavioral and Brain Sciences* (*BBS*; Buss, 1989b) that reactions grew intense.

Reactions to the 37-Culture Study of Universal Mate Preferences

One sort of reaction was foreshadowed when I gave a talk to the University of Michigan sociology department. A woman approached me afterward and suggested that I suppress the data. She thought that it would disturb women to know that men placed a premium on physical attractiveness and on youth—things about which there was little women could do (subsequent research has confirmed that these things are perceived as relatively "uncontrollable," despite the multibillion dollar cosmetic industry that caters to attempts to modify appearance).

Reactions to the *BBS* article, however, were extremely varied. Some reactions were purely hostile—some people hated the findings and hated the author who discovered and documented them. I may be naive, but it continues to surprise me when academic folks get mad at the author of a study when they don't like the findings. Some acted as though I were personally to blame for the fact that men worldwide place a premium on youth and beauty. Some acted as though I were personally insulting them when the study revealed that women value a man's ambition, social status, and economic resources. In the course of my mating research, I've discovered a number of things that I personally find disturbing, so in a sense, I can empathize when people get upset about my work. But why shoot the messenger? One good outcome from this sort of reaction was that I honed my writing and speaking skills so that I could communicate in a way that largely diffused emotional reactions before they occurred. Indeed, over the past 10 years, receptiveness to this line of work has grown tremendously.

I benefited in 1989 from having the thoughts of 30 *BBS* commentaries on the article, feedback of university students who took my classes, and reactions from audiences from dozens of subsequent invited talks. I began to receive letters and e-mails from all over the world—one of the joyous side benefits of doing cross-cultural research.

Aside from the overt hostility, reactions fell into several clusters: (a) It is true, and the evolutionary explanation is compelling; (b) it is not true; we do not believe the results; and (c) it is true, but anyone could have predicted it. Fortunately, reactions falling into the first category have mushroomed. Mating research has flourished, and evolutionary psychology has emerged as a vibrant and thriving discipline.

However, in 1989, some people refused to believe in the universality of the findings, claiming that there were as yet unstudied cultures, not included among the 37, where everything was different. The past 14 years of research have failed to yield any evidence of such cultures. In fact, the sex differences found in the 37-culture study have subsequently been replicated in dozens of additional cultures. To my knowledge, there has not been a single instance of failure to replicate—an achievement rarely attained in social science.

The third class of reactions, as noted earlier, was "It is true, but anyone could have predicted it." Such is the power of the hindsight bias, which is why I wish I had asked people to sign their predictions in advance. In fact, all but the evolutionarily informed scientists whom I interviewed prior to conducting the 37-culture study predicted that the sex differences would not be found universally. The most frequently nominated "alternative theory" proposed after the fact to explain the results turned out to be none other than some variant of the structural powerlessness hypothesis, first articulated by Buss and Barnes (1986). According to this hypothesis, women are forced to value resources more than men in a mate because men control all the resources, women are excluded, and so marrying a mate with resources is the only avenue available to them. Buss and Barnes furthermore specified a number of empirical predictions that followed from the logic of this hypothesis, such as (a) that women who have more personal resources will value them less than women without personal resources and (b) that in cultures with greater economic equality, the sex differences in mate preferences on these dimensions would diminish in magnitude.

It is an interesting irony that recent theorists, such as "social role" and "socialization" theorists, have advanced the structural powerlessness hypothesis and variants of it without citing Buss and Barnes (1986) as the first articulation of it and apparently without realizing that the author they criticized has in fact published empirical tests of this alternative hypothesis (e.g., Buss, 1989c). It is also noteworthy that authors who had been publishing "social role" and "socialization" explanations for many years prior to 1989, in some cases entire books, had totally ignored men's and women's mating strategies but then, after the findings were in, claimed that their theories could somehow in retrospect "explain" the universal sex differences.

Although there is nothing logically wrong with the structural powerlessness hypothesis and its more recent social role–socialization–social construction variants, aside from the fact that it fails to explain causal origins of power, it does not happen to have much empirical support. Women residing in cultures that are more economically egalitarian, such as Sweden, do not differ in their mate preferences compared with women who reside in cultures with great economic disparity between the sexes, such as Japan (Buss, 1989c). The magnitude of sex differences does not diminish in economically egalitarian cultures compared with those with greater disparities. And perhaps most damning, women within cultures who earn more money tend to place more, not less, value on economic resources in a mate, flatly contradicting the structural powerlessness hypothesis—a result confirmed by a half dozen independent investigators (see Buss, 2004, for summaries).

Moreover, these "structural" or "role" explanations simply cannot explain the other findings, such as why men would place such a high premium on youth and beauty. Social role theorists appear to have abandoned this large explanatory problem entirely, and they effectively sweep these troublesome findings under the rug by ignoring them or relegating them to footnotes.

Some have tried to explain men's preferences for young women as a reflection of power—they suggest that younger women are "easier to control." However, Kenrick and his colleagues have effectively falsified this hypothesis (Kenrick, Keefe, Gabrielidis, & Cornelius, 1996). They examined mate preferences of men varying in age and found that teenage males actually prefer women a few years older than they are. For example, 15-year-old males are most attracted to women who are 17 years old, despite the fact that these older women show no interest in them, the 15-year-old males are not "reinforced" by attempting to attract them, and these 17-year-old women are certainly not more easily controllable by younger adolescent males!

I have no doubt that people will continue to struggle with these findings and will continue to come up with alternative explanations for them. That is the nature of science. However, at this point in the history of science, I would venture the following conclusions and predictions: (a) Only the hypotheses anchored in evolutionary logic succeeded in predicting these sex differences in advance of their universal discovery across cultures; (b) alternative theories that attempt to explain the sex differences, including the one articulated by this author in the form of structural powerlessness, have all failed miserably in actual empirical tests; (c) the basic sex differences in mate preferences found in the 37-culture study have been replicated subsequently dozens of times by independent investigators across many additional cultures, with not a single exception, to my knowledge; (d) these sex differences are extremely important for many aspects of social interaction—social competition, hierarchy negotiation, gossip, mate attraction tactics, derogation of competitor tactics, strategies to retain mates, causes of divorce, and many forms of conflict between the sexes (Buss, 2003); and (e) in the history of psychological science, these pillars, knowledge about fundamental desires of men and women in their search of mates, will stand the test of time.

Sexual Strategies Theory: A Menu of Mating Mechanisms

The 37-culture study was essentially my first attempt to test evolution-based hypotheses. I did not know what to expect going into the study, because no one had ever done a study of its kind and scope, and if the results had come out differently, it is likely that I would have abandoned evolutionary work entirely. Few choose to devote a career to a theoretical edifice that yields specific predictions that are falsified by empirical data (on the other hand, many seem to hold onto theories that are so vague that they fail to generate falsifiable predictions). I was all too cognizant of the limitations of the 37-culture study—it dealt only with long-term mate preferences, or what people desire in a spouse or committed partner. Other dimensions and complexities of human mating begged for exploration, and I launched dozens of projects to explore some of them. One of the most important directions was an analysis of the temporal dimension of mating, anchored by the somewhat arbitrary labels of *long-term mating* and *short-term mating*. David Schmitt and I used this as one of the pillars of "Sexual Strategies Theory," which was published in *Psychological Review* (Buss & Schmitt, 1993).

Prior theories of mating, as noted earlier, had severe limitations. First, they typically focused on a single mating motive such as "similarity" or "equity," ignoring the possibility that humans have a complex menu of mating motives. Second, they failed to explain why humans would be motivated in these ways to begin with; thus, the theories seemed arbitrary, lacking any strong conceptual foundation. Third, the theories were so general that no specific predictions could be derived from them. Fourth, each theory assumed that men and women were identical in their mating motives, so no sex-differentiated predictions could be derived from them. Fifth, previous theories of mating were context blind, positing the same mating tendencies regardless of circumstances.

Sexual strategies theory was a first pass at a theory that attempted to rectify these crucial omissions by articulating a selective rationale for the origins of the mating strategies that men and women exhibit and some of the psychological mechanisms that underlie those strategies. A core premise of the theory is that human mating is inherently strategic—that humans possess mating adaptations that have been "designed" by selection to solve specific mating "problems." Our use of the term *strategy* was meant to convey the goal-directed and problem-solving nature of human mating and carries no implication that strategies are consciously planned or articulated.

It was the first theory of mating to posit that men and women both have distinct short-term and long-term mating strategies. One of the discouraging reactions to sexual strategies theory is the degree to which some critics have been wildly inaccurate in depicting it. A full 25% of the theory was devoted to women's short-term mating, the evolutionary rationale behind women's short-term mating, empirical predictions about women's short-term mating, and empirical tests of these predictions about women's short-term mating. The abstract notes that "*Both* [italics added] men *and* [italics added] women are hypothesized to have evolved distinct psychological mechanisms that underlie short-term and long-term mating" (Buss & Schmitt, 1993, p. 204). An entire table was devoted to outlining the adaptive problems women and men confront in each of these mating contexts. Given our explicitness on this issue, when a critic describes the theory as proposing that "men are promiscuous, women are monogamous," one can only wonder about the person's scholarship, training, or eyesight. Attacking straw persons in academia is common, but this is nothing short of absurd. I will not embarrass specific authors or editors, whose scholarship may be described generously as cavalier, but suffice it to say that these gross errors have appeared in psychology journals that are otherwise quite reputable. I've found that some journal editors have an uncommonly low threshold for accepting articles critical of evolutionary work on humans, no matter how shoddy the scholarship, arguments, or evidence. Frankly, it is an embarrassment for the field of psychological science.

The 1993 version of sexual strategies theory outlined nine fundamental hypotheses and 22 empirical predictions that were derived from them. These empirical predictions have been robustly confirmed now by literally hundreds of empirical studies by hundreds of independent investigators. Indeed, sexual strategies theory spawned a sort of cottage industry of mating research, and it is now covered with greater or lesser degrees of accuracy in most introductory textbooks.

My current appraisal of the 1993 sexual strategies theory is that it was incomplete in many ways but nonetheless was markedly better than any theory of mating that had preceded it. Readers with a historical bent or mere curiosity might pick up any social psychology text or handbook in the 1980s or earlier to see the state of theories at the time. Sexual strategies theory was never advanced as a complete and finished theory of human mating. It is a working draft of a theory pending the addition of important complexities. Thus, it is worth noting what I think some of its inadequacies were in the 1993 version.

First, the theory downplayed the role of "gene quality" in mate selection. Although Schmitt and I discussed the problem of gene quality, subsequent work in the field has shown gene quality to be of far greater importance than we realized, particularly in women's short-term mate selections (the work of Steve Gangestad, Randy Thornhill, and others has been critical in this domain; for a recent summary, see Buss, 2003). Second, our 1993 formulation (Buss & Schmitt, 1993) provided insufficient attention to individual differences within sex—a limitation that we noted at the time, although at the time we offered a page of possibilities for predicting principled within-sex variation. Third, by focusing so heavily on sex differences, the theory slighted the many ways in which men's and women's mating strategies share commonalities. Many of these limitations have been rectified in various ways over the past decade, and our scientific understanding of human mating is vastly more complex and sophisticated than it was a decade ago (see Buss, 2003).

Foundations and Future of Evolutionary Psychology

All of my work on human mating strategies was part of a broader vision—to establish the foundations for a new science of the mind called evolutionary psychology (Buss, 1984, 1995, 2004). In this quest, I have been fortunate to be part of a larger scientific movement. Charles Darwin was the first evolutionary psychologist, for he noted the following at the end of his classic 1859 treatise, *On the Origins of Species*: "In the distant future I see open fields for more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation" (Darwin, 1859, p. 389). It is an honor to have contributed in some small measure to the fulfillment of Darwin's prophesy—the quest to discover where, as human beings, we came from, who we are, and the mechanisms of mind that define what it means to be human.

Note

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