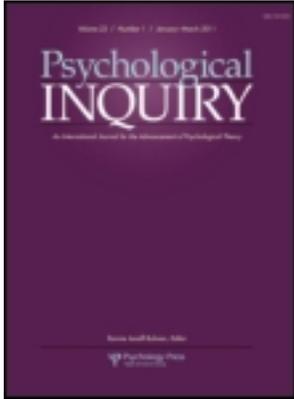


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Psychological Inquiry: An International Journal for the Advancement of Psychological Theory

Publication details, including instructions for authors and subscription information:

<http://www.tandfonline.com/loi/hpli20>

The Science of Human Mating Strategies: An Historical Perspective

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Published online: 02 Sep 2013.

To cite this article: David M. Buss (2013) The Science of Human Mating Strategies: An Historical Perspective, *Psychological Inquiry: An International Journal for the Advancement of Psychological Theory*, 24:3, 171-177, DOI: [10.1080/1047840X.2013.819552](https://doi.org/10.1080/1047840X.2013.819552)

To link to this article: <http://dx.doi.org/10.1080/1047840X.2013.819552>

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The Science of Human Mating Strategies: An Historical Perspective

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There is much that is admirable, scientifically accurate, and useful in “The Ape That Thought It Was a Peacock.” It summarizes, albeit selectively, much empirical evidence that has cumulated about human mating strategies. It deftly dismisses blank slate theories. It correctly points out that social role theory (in its various new or old labels) is woefully inadequate, cannot explain the now massive panoply of empirical findings, and should properly be consigned to a historical footnote (see also Miller, this issue). It argues that men and women both have evolved mate preferences and engage in mutual mate choice, and that both sexes engage in intrasexual competition—precisely the theoretical stance my colleagues and I have long maintained, as the authors correctly acknowledge (e.g., Buss, 1985, 1988a, 1988b, 1989b; Buss & Dedden, 1990; Buss & Schmitt, 1993; Buss & Shackelford, 1997; Schmitt & Buss, 1996). Because most of this review of the science of human mating is accurate, it is perhaps useful to pan back and provide a historical perspective on the study of human mating. Subsequently, I note the few quibbles I have with the target article, most of which are matters of labeling and emphasis, and suggest a few directions for the future science of human mating.

A Brief History of the Science of Human Mating Strategies

Prior to the work of evolutionary psychologists in the mid- to late 1980s and ensuing decades, the study of mating was a marginal and little-studied topic in the field of psychology. There were a few studies by social psychologists. The pioneering work by Ellen Berscheid and Elaine Hatfield was extremely important and broke some new ground in the 1970s. However, that work dealt mostly with initial attraction to strangers in laboratory settings, not to the range of human mating strategies that are now well documented. Moreover, that early work was entirely uninformed by an evolutionary perspective. It is possible that Darwin’s theories of natural and sexual selection were unknown to psychologists. Indeed, to this day scholars can receive a Ph.D. in psychology in every major university in America without ever taking a single course in biology. This rather unfortunate state of affairs left and

leaves most of psychology disconnected with the rest of the life sciences and remains to this day one of the reasons for so many persistent misunderstandings about evolutionary theory (Confer, Easton, et al., 2010). At any rate, evolutionary perspectives failed to inform any of the little empirical work on human mating that was conducted in psychology prior to the 1980s.

Theories of human mating, to the small extent that they existed, were extraordinarily simple and simplistic. They typically invoked single variables—*similarity theory* in which people were said to be attracted to those like themselves; *complementarity theory*, in which people were said to be attracted to opposites; *equity theory*, in which people were said to be attracted to those in which the benefit:cost ratio they received was analogous to the benefit:cost ratio they gave; and of course *Freudian theory*, in which people were said to be attracted to those who resembled their opposite-sex parent (see Buss & Schmitt, 1993, for a brief review of those extant theories).

In addition to their simplicity, each of these theories was content free and lacked a coherent functional foundation. Equity theory, for example, did not specify which dimensions were relevant for equitable evaluation. Does a good marble collection count the same as intelligence, attractiveness, or a good job in the benefit column? Is the cost of a potential mate with a child by someone else equivalent to the cost of a potential mate with an irritating habit of leaving the cap off of the toothpaste tube? By failing to specify the content of what counts as a cost and benefit, equity theory sans an evolutionary perspective foundered on its inability to generate an elegant array of crisply testable empirical predictions (it is now making a comeback of sorts, informed by an evolutionary perspective, under the assortative mate-value hypothesis that infuses evolutionarily relevant content dimensions).

Complementarity theory likewise did not specify on which dimensions it was important to be dissimilar—color preferences, income, attractiveness, personality, sense of humor? Perhaps because the theories failed to specify content, none contained any premises or predictions about sex differences, a rather striking omission in hindsight, but it was fully in keeping the then-prevalent blank-slate assumptions about male and female minds. Moreover, all theories treated

mating as a unitary phenomenon, failing to distinguish between qualitatively different sorts of mating relationships, such as a brief opportunistic copulation, a casual fling, a serious affair, or a committed long-term marriage.

Absent entirely from the then-theories and empirical research were answers to the following questions, about which we now know a considerable amount:

1. Do men and women differ in the value they place on different qualities in a long-term mate, and if so, were these sex differences limited to the United States or to Western cultures, or were they universal across cultures (Buss, 1989b)?
2. Do mate preferences vary depending on the type of mate sought, such as a short-term casual sex partner versus a long-term committed mate (Buss & Schmitt, 1993; Kenrick, Sadalla, Groth, & Trost, 1990; Li & Kenrick, 2006)?
3. Do mate preferences vary as a function of theoretically relevant social and ecological variables, such as cultural norms about premarital sex, operational sex ratio, or parasite prevalence (Buss, 1989b; Gangestad & Buss, 1993; Gangestad, Haselton, & Buss, 2006)?
4. Do women's mate preferences vary as a function of their ovulation cycle (Larson, Pillsworth, & Haselton, 2012)?
5. Do *expressed* mate preferences correspond to actual behavioral measures of *revealed* mate preferences (Hitsch, Hortaçsu, & Ariely, 2010)?
6. To what degree, and in which contexts, do mate preferences influence *actual mating and marital decisions* (Buss, 2003; Li et al., in press)?
7. Does one's personal *mate value* influence an individual's ability to translate their mate preferences into their actual mating decisions (Buss, 2003)?
8. Do women and men adjust their mate preferences up or down depending on their own mate value (Buss & Shackelford, 2008)?
9. Do *mate value discrepancies* within romantic relationships predict sexual infidelity and relationship dissolution (Buss & Shackelford, 1997)?
10. To what degree do cultural institutions, such as arranged marriages, limit the ability of individuals to seek the potential mates they desire (Buss, 2003)?
11. Can the mate preferences of one sex be used to predict the content of *attraction tactics* used by the opposite sex (Buss, 1988a; Schmitt & Buss, 1996)?
12. Can the mate preferences of one sex be used to predict the ways in which women and men *derogate their mating competitors* through the verbal slings and arrows of gossip (Buss & Dedden, 1990; Schmitt & Buss, 1996), and are there sex differences in the qualities of mating rivals that produce emotional distress (Buss, Shackelford, Choe, Bunk, & Dijkstra, 2000)?
13. Can the mate preferences of one sex be used to predict the *mate poaching tactics* used by the opposite sex (Schmitt & Buss, 2001)?
14. Can the mate preferences of one sex influence *behavioral tactics used for mate retention* in long-term dating relationships and actual marriages (Buss, 1988b; Buss & Shackelford, 1997)?
15. Do sex differences in mate preferences predict sex differences in *causes of divorce* across cultures (Betzig, 1989)?
16. Are *standards of beauty* arbitrary and infinitely culturally variable, as mainstream psychologists long assumed, or do women and men have evolved standards of attractiveness that are universal across cultures (Sugiyama, 2005)?
17. Do men and women have *different standards of beauty*, such as prioritizing facial versus body cues, depending on whether they are seeking a long-term or short-term mate (Confer, Perilloux, & Buss, 2010).
18. Do men and women get into predictable forms of *conflict* due to conflicting mating strategies (Buss, 1989a, 1996; Haselton, Buss, Oubaid, & Angleitner, 2005)?
19. Do men and women attempt to *deceive* potential mates in ways predictable from their mating strategies (Haselton et al., 2005)?
20. Do women and men experience predictable forms of *sexual regret* from missed sexual opportunities and retrospective errors in sexual choice (Galperin et al., in press)?

There now exists a formidable body of empirical evidence on all these questions (Buss, 2012). But it did not exist at all prior to the work of evolutionary psychologists publishing from 1985 to the present. Indeed, one could make a good case for the study of human mating strategies being one of the great "success stories" of psychology. Within the mating domain, evolutionary perspectives have guided researchers to important phenomena previously overlooked and entirely unexamined (e.g., mate retention, mate poaching), led to empirical predictions that have been tested now in hundreds of studies, and strikingly illuminated a domain of central importance to human psychology that was virtually ignored previously. It is important to note that the work on human mating strategies has a cumulative quality, with new research building on older research and adding additional layers of complexity to scientific knowledge—something rare in a field that is often filled with atheoretical effects, counterintuitive findings that sometimes become "classics" despite the fact that other scientists cannot replicate them (see Jussim, 2012, for a partial list), countless ungrounded minitheories,

faddish phenomena that come and go, and a striking lack of cumulative scientific progress.

As in all areas of exciting scientific exploration, there has been and will be much disagreement, competing hypotheses, and different interpretations of the empirical evidence. These are signs of a vibrant and healthy science at the cutting edge. To take one example, there exist at least three competing hypotheses about mate preferences shifts at ovulation, a theoretical controversy that is far from being scientifically resolved (Buss & Shackelford, 2008; Gangestad & Thornhill, 2008; Roney, Simmons, & Gray, 2011). As different evolutionary hypotheses get pitted against each other in empirical tests, the weight of the empirical evidence will adjudicate among the competing hypotheses. I anticipate that the science of human mating will become increasingly deeper and more nuanced in the coming years.

New discoveries about the evolution of human mating continue to be published at a pace that reveals that what we now know, important though it may be, is likely to be small compared to what will eventually be known. Human mating psychology may be one of the most complex domains of psychological functioning in the human mind, and there are good theoretical reasons for this expectation. Nothing is closer to the “engine” of the evolutionary process—differential reproductive success—so mating psychology has been a massive ‘target’ of the selective process.

The bright side is that research on human mating strategies is cumulative. New research builds on the old. New methods overcome the limitations of previous methods. Increasingly nuanced findings reveal more and more complexities of human mating psychology. Which raises the question, What are the most fruitful theoretical and empirical directions ahead in human mating research? Answers to that question require addressing the two quibbles with the target article—one dealing with labels and one dealing with emphasis.

On the Utility of Furnishing a Name that Correctly Characterizes Human Mating Strategies

The target article contrasts two labels to capture the essence of human mating—the males compete/females choose (MCFC) model and the mutual mate choice (MMC) model. I agree with much of the author’s critique of the MCFC model. As noted earlier, my research team and many other research teams around the world have theorized and extensively empirically studied (a) male mate choice as well as female mate choice, and (b) female intrasexual competition as well as male intrasexual competition. There is no question that theories that ignore mutual mate choice, mate preferences by both sexes, and strategies of mate com-

petition by both sexes are woefully inadequate and incomplete.

The authors of the target article prefer the label of *mutual mate choice* to capture human mating strategies. But I suggest that labels can be misleading, and this one is no exception. Yes, humans certainly engage in MMC, and that is crucially important. But does that label accurately capture or oversimplify the known complexities of human mating? I suggest that it oversimplifies, and I outline my rationale, but the key point is that labels can have multiple effects. Proper labels can help guide researchers to important domains of inquiry. But some labels can have a constraining effect, leading researchers to ignore complexities not captured by the label.

There can be little doubt that the authors are correct that the evolution of long-term pair-bonded mating is one of the main mating strategies of humans and that both men and women have evolved a rich psychology devoted to long-term mating, including *the emotion of love* (Buss, 1987; Fisher, 1994; Frank, 1988; Jankowiak, 1997). And they are also correct that this sets us apart from many other species because it is so rare. Indeed, it is so rare that unusual and remarkable selective pressures must have existed for humans that were absent in most primates and most mammals. But it is also true that long-term committed mating is not the exclusive mating strategy of humans, as the authors acknowledge, although perhaps they minimize its importance.

Humans engage in short-term opportunistic mating, for example, in the form of mate poaching (Schmitt & Buss, 2001; Schmitt, Timmermans, Van Overwalle, & Vanhooymissen, 2004). Short-term mating may be more prevalent in modern times, with people living in large cities with thousands of potential mates living in relative anonymity—conditions that would not have characterized the small-group living of our ancestors.

Nonetheless, the ethnographic evidence is replete with evidence of short-term mating, mate poaching, and extramarital affairs. These have been documented in the Ache of Paraguay, where affairs are common and “marriages” sometimes last for just a few days (Hill & Hurtado, 1996). They have been documented among the !Kung San (Shostack, 2009), among the Yanomamo (Chagnon, 1983), among the Amazonians of the rain forest (Gregor, 1987), and among the Tiwi of Australia (Hart & Pilling, 1960), to name just a few. They have even been documented among the Samoan islanders by Margaret Mead, who described in detail jealous violence that erupts as a consequence of sexual infidelity and sexual mate poaching (Mead, 2001).

Now one could argue that short-term mateships, infidelities, and extramarital affairs can also be subsumed by the label of mutual mate choice, as most of these other forms of mating are freely chosen by women and men who select each other. But restricting mutual mate

choice to long-term mating doesn't lead researchers to explore these other important strategies of human mating.

Moreover, it is well documented that men and women engage in *deception* in their mating strategies. This occurs in studies of online dating that reveal that men exaggerate their income and deceptively report their height to be a few inches more than it actually is (Toma, Hancock, & Ellison, 2008). It has been documented in studies in which women and men report about their actual experiences of being deceived (Haselton et al., 2005). For example, men sometimes feign commitment, exaggerate the depth of their feelings, and lie about love in order to secure short-term mating. Women sometimes lead men to believe that sex is forthcoming in order to secure resources or other benefits, but then fail to deliver on the implied promises. And men and women differ in the emotional upset and distress they experience when they discover that they have been deceived in these sex-differentiated ways (Buss, 1989a; Haselton et al., 2005).

Of course, one could argue that the label of MMC can subsume these mating strategies, as both men and women are still choosing in some sense. But the label does not naturally lead researchers to investigate these interesting and important mating strategies.

Finally, not all matings are freely chosen. The authors note one example, that of arranged marriages, and they correctly point out that individuals sometimes manage to influence their own matings even under these constrained conditions. They sometimes attempt to manipulate their parents, sometimes refuse to mate with the person their parents select, and sometimes elope with the one they truly love (Buss, 2003). Nonetheless, the fact that parents exert influence to varying degrees over the mate choices of their sons and daughters, even in modern Western societies (Perilloux, Fleischman, & Buss, 2011), again points to important strategies of human mating that the label MMC does not naturally guide researchers to discover.

Rape is another form of mating that is not captured by the label of MMC, as it bypasses female choice. Some have proposed that human males have evolved rape adaptations, whereas others have proposed that humans do not have rape adaptations, and that rape instead is a by-product of other evolved mechanisms (Thornhill & Palmer, 2001). I concur with Symons (1979) in his conclusion, "I do not believe that available data are even close to sufficient to warrant the conclusion that rape is a facultative adaptation in the human male" (p. 284). Nonetheless, absence of evidence is not evidence of absence, and it remains an open question. Whatever the ultimate explanation for rape turns out to be, there can be no doubt that it occurs at nontrivial frequencies, inflicts massive costs on victims, and bypasses female mate choice (Perilloux, Duntley, & Buss, 2012).

In short, the authors of the target article prefer the label of MMC and emphasize long-term committed mating, and that's perfectly fine. MMC, male and female mate preferences, female and male mate competition, and long-term committed mating are, and have been, central to work on human mating strategies for the past few decades. I prefer a broader label, such as human mating strategies or sexual strategies theory, phrases that are admittedly more diffuse but nonetheless do not preclude the multiple mating strategies known to exist in some cases, and hypothesized to exist in other cases—mating phenomena not well captured by the MMC label.

Are Sex Differences in Short-Term Mating and Desire for Sexual Variety Exaggerated?

A second quibble I have with the target article centers on the importance of sex differences in adaptations for short-term mating. The authors correctly note that abundant evidence supports the hypothesis that these sex differences exist and are substantial in effect sizes by the normal standards psychologists use to evaluate effect sizes. The target article marshals arguments for why these sex differences have been exaggerated by some, should not be regarded as "large," and why they might not be terribly important.

The authors are undoubtedly correct that popular media depictions often seize on the "men are from Mars, women from Venus" metaphor and sometimes portray the sexes as more different than they are. Qualifiers about effect sizes and overlap of distributions get lost in media depictions. And it's possible that some evolutionary psychologists, me included, may have contributed to these exaggerations (although I appreciate that the authors do not attribute such exaggerations to me). Fair enough.

Two points can be made about sex differences in short-term mating adaptations. The first is one of historical context. When evolutionary psychologists began to study these mating phenomena, they were going against an extremely strong mainstream grain in the social sciences that held that men and women were psychologically monomorphic. Any differences were believed to be due to dressing girls in pink and boys in blue, giving girls Barbie dolls and boys toy guns and masculine trucks (see, e.g., Block, 1973, and the television documentary *The Pinks and the Blues*, Block, 1981). In that historical context, it was indeed big news that mainstream social scientists were wrong. Indeed, findings of large sex differences in short-term mating adaptations were met with great skepticism and disbelief. Even to this day, some psychologists continue to maintain that women and men are psychologically identical, except by virtue of the "roles" to which they are "assigned," and differ solely in anatomy. To an

evolutionary psychologist, the notion that sexual selection would favor sex differences in anatomy, morphology, and reproductive biology with absolutely no attendant psychological, behavioral, or strategic sex differences would be baffling beyond belief and constitute the sole species on the planet for which such a strange disconnect occurred. As the target article appropriately states, comparative species analysis shows that such views strain scientific credulity to the maximum.

So emphasizing the sex differences in that historical context made excellent sense. And historical context is important. By way of analogy, G. C. Williams (1966) in his now classic book *Adaptation and Natural Selection*, spent what seems now to be an inordinate amount of space devoted to refuting the then-prevalent notions of “group selection.” But in that scientific context, his emphasis was essential, and his book had the salutary effect of promoting a major scientific shift in thinking in evolutionary biology. Were he writing that book into today’s scientific context, he once told me, such attention to debunking erroneous notions of group selection would simply not be warranted, because few scientists now adhere to these outmoded theories for reasons well-articulated recently by Pinker (2012).

The key is to simply be scientifically accurate—describe effect sizes, unpack the importance of those effect sizes, explore their ramifications, describe distribution overlap, and importantly focus on within-sex differences as well as between-sex differences. The field of evolutionary psychology, indeed the field of evolutionary biology, is beginning to move in that direction (e.g., see the many chapters in the recent edited volume by Buss & Hawley, 2011).

The second point worth pausing on is the issue of the “importance” of sex differences in human mating strategies, and this brings me to the crux of my disagreement in emphasis in the target article. Many metrics can be applied to evaluating scientific importance. One is certainly effect size. As the target article correctly notes, the effect sizes for sex differences in short-term mating adaptations are properly considered “large” by conventional standards in psychological science. Whereas typical effect sizes in psychology hover around .20 or .30, the magnitude of sex differences in the psychology of short-term mating are strikingly large—.74, .80, and in some cases more than 1.00 (the mean differences between the sexes ranging from three fourths of a standard deviation to more than a full standard deviation).

The fact that effect sizes for sex differences in some aspects of human mating psychology are so large, far exceeding typical effect sizes in psychology, should not be trivialized. Indeed, to my knowledge, there does not exist a rival to biological sex in predictive value when it comes to understanding the psychology of human mating.

Regardless, I do not believe that “importance” should be solely gauged by effect size. As Rosenthal and colleagues have elegantly demonstrated, even “small” effect sizes can have enormous real-world consequences (Rosenthal, Rosnow, & Rubin, 1999). A small effect size in the effectiveness of two different medications can make a difference of thousands of lives saved. Moreover, fitness consequences should figure centrally into judgments of “importance.” Consider the sex differences in who commits murder. The fitness consequences for homicide victims are enormous for victims, their mates, their children, and their extended kin (Buss, 2005; Duntley, 2005). From an evolutionary perspective, it’s really bad to get killed.

So what is the importance of the sex differences in short-term mating adaptations? It’s certainly true, as the target article suggests, that men’s greater desire for sexual variety results in only a minority of men patronizing prostitutes. But let’s consider adding a range of effects produced by these sex differences. Consider sex trafficking. It’s a low base-rate event. The effects, however, are devastating for the thousands of women across the globe who are victims of sex trafficking. Sex trafficking destroys lives. It undermines self-esteem, perhaps permanently. It acts as a conduit for numerous sexually transmitted diseases. It destroys entire families. And it exists primarily due to men’s sexual psychology.

Undoubtedly, prostitution and sex trafficking are extreme examples. But sex differences in short-term mating psychology produce many other effects—consequences that have arguably broader consequences for larger numbers of people. One example is sexual deception. Men more than women deceive members of the opposite sex by exaggerating the depth of their commitment, feelings, love, and long-term intentions in order to gain short-term sexual access (Buss, 2003; Haselton et al., 2005). Women, much more than men, report feeling “used” and “degraded” when they discover that the man they consented to having sex with was not at all interested in pursuing a long-term mate-ship (Meston & Buss, 2007, 2009). The growing body of research on the aftermath of “hookups” and “friends with benefits” discovers similar effects—women more than men feel psychologically used and degraded.

Then there is the related and growing scientific literature on sexual regret (Galperin et al., in press; Roese et al., 2006). Among other findings, 34% of women, but only 15% of men, reported regretting a one-night sexual encounter they experienced (Galperin et al., in press). The same study found that 37% of women, but only 15% of men, ruefully regretted having sex with someone who led them to believe they were interested in a relationship but in fact turned out to be only interested in having sex (Galperin et al. in press).

Consider the sex differences in sexual misperception—the tendency of males to overinfer

sexual interest in a woman based on minimal cues such as a smile, a touch on the arm, or mere friendliness (Haselton & Buss, 2000; Perilloux, Easton, & Buss, 2012). It is likely an adaptation to short-term mating, given the asymmetric reproductive costs to men of inferential errors associated with missing out on a potential sexual opportunity versus mistakenly inferring sexual interest. Although the sexual misperception bias may not be large in effect size (effect sizes vary across methods and studies), it plausibly exposes women to multiple costs ranging from unwanted sexual advances to sexual harassment, and may even contribute to acts of rape (Perilloux et al., 2012).

The key point is that the importance of a sex difference is properly gauged not solely by the magnitude of effect. Other metrics of “importance” include fitness consequences, physical costs, and psychological costs. In the case of sex differences in short-term mating psychology, these include sexual acts regretted and the costs produced by unwanted sexual advances, sexual harassment, stalking, and sexual assault—phenomena known to produce fear, anxiety, depression, and traumatic stress.

These are merely a few examples of the real-world consequences of evolved sex differences in short-term mating psychology. Many more could be added and elaborated upon, such as the marriages destroyed by sexual infidelity, the effects of sex-related causes of divorce on women and men, the often devastating consequences for children who get raised by single mothers or abused by steparents, and the spread of sexually transmitted diseases.

At a minimum, I think it prudent not to conclude that sex differences in sexual psychology are unimportant. By many metrics, from fitness consequences to disease spread to psychological damage, I think a good case that they qualify as “quite important.”

Acknowledgment

I thank Dan Conroy-Beam for helpful suggestions on an earlier draft of this article.

Note

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