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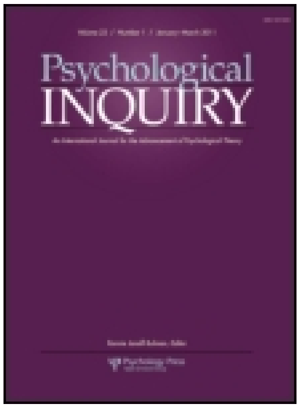

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The Ape That Kicked the Hornet's Nest: Response to Commentaries on "The Ape That Thought It Was a Peacock"

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REPLY

The Ape That Kicked the Hornet's Nest: Response to Commentaries on "The Ape That Thought It Was a Peacock"

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We respond to the commentaries on our target article, "The Ape That Thought It Was a Peacock." We start with specific issues raised by the article. These relate to the magnitude of human sex differences; the evolution and relative importance of pair bonding, paternal care, and polygyny in our species; and the distinction between the males-compete/females-choose (MCFC) model of human sexual psychology and the mutual mate choice (MMC) model. We then evaluate two competing theories of human sex differences and similarities: Social Role Theory and Attachment Fertility Theory. We conclude with some thoughts about how to present and teach evolutionary psychological research and theories without conveying an exaggerated impression of the scale of human sex differences.

We were truly gratified by the quantity and quality of the thoughtful responses to our target article. All the commentaries raised important issues, and we enjoyed mentally jousting with them all. In the following pages, we report on some of the outcomes of this mental jousting. First, though, we briefly recap the thesis of the article in light of the commentaries. The thesis boils down to five key claims:

1. Humans are a relatively monomorphic animal. Certainly, we exhibit some degree of dimorphism; it is, after all, easy to tell men apart from women (Doug Kenrick's son, 2013, as cited in Kenrick, this issue). And certainly we are more dimorphic for some traits than for others (Kenrick, this issue; Wood & Eagly, this issue). However, humans are less like polygynous peacocks or deer than we are like socially monogamous foxes and robins (Gray, this issue), or cooperatively breeding tamarins and marmosets (Snowdon, this issue).
2. Our low general dimorphism is a consequence of high levels of pair bonding and male parental care in our ancestral past. This is not to say that humans are adapted solely for long-term pair bonding. Like dunnocks,[1] human sexual psychology is compatible with a range of reproductive arrangements, including monogamy, polygyny, and promiscuity (Roberts & Havlíček, this issue). However, pair bonding and biparental care were important enough throughout human evolution to lead to a very general decline in psychological dimorphism.
3. Contrary to the view that humans are relatively monomorphic, evolutionary psychology (EP) sometimes gives the impression that there are extremely large sex differences in our species. It does this in two main ways. First, there are verbal statements in the EP literature suggestive of large differences, but for which the research actually shows rather modest differences. Second, there is research that appears to show very large differences, but which on closer inspection probably overstates these differences (e.g., Clark & Hatfield's [1989] "Would you go to bed with me?" study; the sexual predilections of despotic leaders; see Betzig, 1986, this issue).
4. The exaggeration of sex differences in EP is not the product of a hidden agenda or the deliberate cherry-picking of results (see Pham, Shackelford, & Jeffery, this issue). Indeed, it is probably not the product of any single cause (Buss, this issue; G. F. Miller, this issue; Pound & Price, this issue). However, one contributing factor might be EP's historical commitment to what we call the males-compete/females choose (MCFC) model of sexual

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[1] Dunnocks (also known as hedge sparrows) are LBBs—Little Brown Birds—found throughout Europe and Asia. They are also common in New Zealand.

selection—a model that emphasizes sexual dimorphism, female mate choice, and male competition for mates.

5. The MCFC model is not the only model employed in EP. It coexists with what we call the mutual mate choice (MMC) model. This model emphasizes mutual courtship and the centrality of pair bonding and biparental care in our species. There is a great deal of research in EP on these MMC phenomena. However, there is *also* a tendency to present humans as a highly dimorphic MCFC species. Although some commentators suggest that the MCFC and MMC models are both correct and that humans facultatively switch between the two (Pham et al., this issue; Roberts & Havlíček, this issue), we argue that the models describe two different animals, only one of which is us (see also Eastwick, this issue; Miller, this issue).

We were heartened to see that several prominent evolutionary psychologists were receptive to these claims, including some of the claims that were somewhat critical of EP. Regarding the exaggeration of sex differences, Buss (this issue) noted that "it's possible that some evolutionary psychologists, myself included, may have contributed to these exaggerations (although I appreciate that the authors do not attribute such exaggerations to me)" (p. 174). Regarding the presence of the MCFC model in EP, Campbell (this issue) recounted how her research on women's competition for mates initially met with a chilly reception, as a result of the MCFC assumption that:

> Women's greater parental investment makes them a limiting resource for male reproductive success and so men must compete, often ferociously, for sexual access. Women have no reason to compete since any man will be only too happy to sexually oblige them. (p. 178)

Also regarding the MCFC model, G. F. Miller (this issue) noted that, "In my dissertation, I advocated an MCFC 'runaway brain' model of human mental evolution. . . . So, for a while in the early 1990s, I was indeed 'an ape that thought it was a peacock'" (p. 207). Only later did he make the arduous journey to the MMC position. (We made the same journey, but by that time we were lucky enough to have G. F. Miller's, 2000, *Mating Mind* as one of our road maps.) Finally, several commentators provided additional evidence for our claims regarding mutual mate choice and reduced dimorphism (Eastwick; Gray; Snowdon), and several provided additional examples of the exaggeration of sex differences in EP (Eastwick; Harris; Snowdon).

But of course it wasn't all agreement and harmony. Various commentaries raised objections to the different aspects of our thesis. Indeed, some of the commentaries were rather heated, leading us to wonder whether we should have written on a less controversial topic, such as religion or politics. Each critical commentary pushed us to ask ourselves: "Are we right? Is our position defensible?" In every case, though, we ultimately decided that the position *is* defensible, albeit with some provisos and concessions which we will discuss. Needless to say, we cannot comment on every point raised. However, in the following pages, we address some key areas of disagreement and also attempt to clear up some misunderstandings of our position. Our response is divided into three main parts. First, we deal with some specific issues raised by the target article. These relate to the magnitude of human sex differences; the evolution and relative importance of pair bonding, male parental care, and polygyny; and the MCFC versus MMC dichotomy. Second, we look at two alternative theories of sex differences, namely, Social Role Theory and Attachment Fertility Theory. For those who enjoy a good academic row, this section and the associated commentaries may be of interest (see L. C. Miller, Christensen, Pedersen, Putcha-Bhagavatula, & Appleby, this issue; Wood & Eagly, this issue.) Finally, we conclude with reflections on how to present and teach EP research in a way that does not convey an exaggerated impression of the magnitude of human sex differences.

Specific Issues Raised by the Target Article

Cutting Sex Differences Down to Size

> We need a new scale of standardized effect sizes. d = 0.1 'crap' 0.2 'rubbish' 0.3 'meh' 0.4 'not bad' . . . and so up to d = 2.0 'well I'll be . . . ' —Tweeted by Neuroskeptic (8 June, 2013)

Love it or hate it, research on sex differences inevitably attracts attention. Of all the thousands of studies conducted by evolutionary psychologists over the decades, it is the literature on sex differences that defines EP for laypeople and for most psychologists. In the target article, we provided two general arguments for viewing sex differences as more modest than evolutionary psychologists sometimes suggest. The arguments went unnamed in the article, but here we will call them the *effect size argument* and the *tail-of-the-distribution argument*. Below we consider each in turn, and ask whether the arguments can withstand the various criticisms raised against them by the commentators.

The Effect Size Argument

One of the central arguments in the target article concerned effect sizes for human sex differences.

Evolutionary psychologists commonly argue that, although most psychological sex differences are fairly modest, those related to mating and reproduction are often very large (Buss & Schmitt, 2011). One of the largest relates to men's and women's willingness to engage in casual sex, a trait sometimes known as *sociosexuality* (SO). The effect size for the SO sex difference averages around $d = .8$ (Lippa, 2009; Schmitt, 2005). This is conventionally described as a large effect (Cohen, 1988). The question we asked in the target article, however, was, Is this really such a big difference? To answer this question, we suggested comparing the SO difference to the sex difference in height. The height difference is generally viewed as rather modest, especially when humans are placed in a lineup with other mammals. But here's the key point: According to one estimate, the effect size for the height difference is around $d = 1.63$ (Lippa, 2009). This means that the sex difference in SO is only around *half the size* of the modest sex difference in height. This seems to suggest that the SO difference—one of the largest sex differences psychologists have discovered—is not really so large after all. In addition to height, we proposed comparing the SO difference to the size difference in a species universally viewed as monomorphic: the lars gibbon. The gibbon size difference is around $d = .8$ (calculated from data in Schultz, 1941, as reported by Geissmann, 1993). This means that the SO difference is only as large as the size difference in the monomorphic gibbon. This suggests that the difference falls within a range that can reasonably be considered monomorphic. Certainly, it dwarfs most other human sex differences. But this may just be because we're a relatively monomorphic animal.

In the original article, we extended this line of reasoning only to SO. However, it is much more broadly applicable. The vast majority of sex differences fall within the monomorphic range (Hyde, 2005). This includes the MCFC-type differences emphasized in EP, such as sex differences in number of sexual partners desired ($d = .4$ to $.5$; Schmitt & 118 Members of the International Sexuality Description Project, 2003), sex differences in the seeking of short-term mates ($d = .31$–$.67$; Schmitt & 118 Members of the International Sexuality Description Project, 2003), and sex differences in overall choosiness regarding short-term mates ($d = .5$ for sexual relations; $d = .76$ for one-night stands; calculated from data in Kenrick, Groth, Trost, & Sadalla, 1993, Table 3). Evolutionary psychologists often describe these differences as "profound," "stark," or "fundamental" (e.g., Buss, 2003, pp. 256, 259; A. P. C. Davies & Shackelford, 2008; Schmitt & 118 Members of the International Sexuality Description Project, 2003, pp. 87, 95). In light of the effect size argument, we might conclude that these are overstatements (or alternatively, that the gibbon size difference is profound and the human height difference doubly so). Note that natural selection *can* produce strongly dimorphic psychological differences; for example, the sex difference in whether one is primarily attracted to men or women is around $d = 3.99$ (Lippa, 2000). However, this is a conspicuous exception to the rule. Overall, compared to most mammals, humans are relatively monomorphic in their sexual psychologies.

So, that's the effect size argument. It is fair to say that the argument met with mixed fortunes among the commentators. Some liked it (e.g., Harris); others were less enamored (e.g., Campbell, Kenrick, Pham et al.). One recurring criticism was that the human height difference is an arbitrary yardstick by which to judge the magnitude of other sex differences. As Campbell (this issue) observed, as soon as we start making such judgments, "we enter a relativistic debate as to whether the glass is half full or half empty, reminiscent of Hyde's (2005) gender similarities hypothesis" (p. 181). Although we agree that there is always some degree of arbitrariness in this kind of exercise, we suggest that it is still informative, and moreover that height is a particularly useful yardstick to use. Unlike sex differences in psychology, or in physical traits like strength, height is readily observable. As such, people have well-informed intuitions about the magnitude of the sex difference and the variance within each sex (Pound & Price, this issue). In addition, there is general agreement that the sex difference in height in humans is small or moderate compared to that found in most dimorphic animals. Thus, rather than pushing us into a relativistic debate, height provides a way to escape that debate. Campbell does make the point that, if we had chosen another yardstick—the sex difference in body mass, fat mass, or upper body strength, for instance—the psychological differences would have appeared even smaller by comparison, thereby strengthening our case (see Lassek & Gaulin, 2009). We suggest, however, that using a more modest difference as our yardstick has advantages of its own. Specifically, it allows us to argue that not only are the psychological sex differences much smaller than the very largest physical differences, they're smaller even than the modest sex difference in height.

Of course, one could always argue that any attempt to evaluate whether a given effect is small or large, monomorphic or dimorphic, is still ultimately arbitrary, and thus should be excluded from the realm of scientific discourse. Pham and colleagues bite the bullet and take this tack, suggesting that it is "meaningless *scientifically*" (p. 222) to assign any labels to any effect sizes. It is meaningless for the same reason that it would be meaningless to assign the label "hot" to the sun—after all, relative to much hotter stars, the sun is rather cool. Although strictly speaking this is true, we are not convinced that the point is decisive. Note, to begin with, that it is a rather extreme way to get around the effect size argument. It immediately invalidates every claim in the EP literature—and indeed in all of psychology—that a given effect is large, or profound, or fundamental, or anything else. More important, though, it does not undermine our basic argument. Even if we abandon conventional labels such as small, medium, and large, we still want some way to get a handle on the relative magnitude of any difference. The comparison with height gives us that. Labels or no labels, it tells us something important that the sex difference in SO is only half the magnitude of the sex difference in height. It also tells us something important that the SO difference is around the same magnitude as the sex difference in size in gibbons, and less than *one fourth* the magnitude of the sex difference in attraction to women versus men.

A very different criticism of the effect size argument came from Kenrick (this issue). In his commentary, he presented a nice *reductio ad absurdum* of our position:

> Yes, an effect size less than 1.0 does allow for considerable overlap in the distributions of two groups, but if we started treating every effect less than 1.0 as unworthy of our attention, I would have an easy time doing the next revision on my social psychology textbook: I could simply throw out most of the classic experimental and correlational findings in the field, including the bulk of findings on cultural influences, hormonal influences, personality effects, persuasion techniques, obedience to authority, conformity, and on and on. Indeed, using the 1.0 effect size standard, general psychology textbooks could also be, as a class, reduced to very short pamphlets. (p. 203)

This is a great riposte, but it's not, we think, fatal to our position. After all, we're not arguing that small effects are unworthy of research attention or that they should be barred from the textbooks; we're arguing that small effects shouldn't be described as large effects. Kenrick's comment does raise an important issue, though. In arguing that psychological sex differences in humans are generally small, we don't mean to imply that they are smaller or less important than other findings in psychology. As Eagly (1995) observed, sex differences fall within the same range of magnitudes as most research findings: The vast majority are small or medium and only a handful are large. However, the conclusion to draw from this is not that sex differences must therefore be important. The conclusion to draw is that *most* effects in psychology are relatively *un*important. That is, most variables, considered in isolation, have relatively little impact on behavior. This doesn't mean that Kenrick should stop writing his social psychology textbook or that we should all abandon psychology and become plumbers instead. The small magnitude of most effects in psychology is itself a discovery of psychology. One might argue, in fact, that it is one of the great metadiscoveries of the field. Most variables have little impact, and thus most of the phenomena studied by psychologists are products of a multiplicity of variables. Sex differences are no exception to this rule.

The Tail-of-the-Distribution Argument

The second argument for downplaying sex differences applies to data taken from the tail of the distribution. As a general rule, such data magnify any group differences found at the mean. So, for instance, if we look at the extremes of height in a population, we find a very high ratio of men to women—at 6 feet tall, for instance, the ratio is around 2000:1 (Pinker, 2005). However, as we move closer and closer to the average height, the ratio of men to women gets closer and closer to 1:1. There is still a mean difference, of course, but it's much smaller than the difference at the tail. This implies that if data from the tail are extrapolated to the general population, we are likely to form an exaggerated impression of the typical difference. In the target article, we applied this logic to several lines of evidence in EP, namely, the utilization of prostitutes as an index of the sex difference in interest in casual sex (Symons, 1979), the harem-holding habits of despotic leaders as an index of the sex difference in polygamous inclinations (Betzig, 1986, this issue), homicide rates as an index of the sex difference in intrasexual competitiveness (Daly & Wilson, 2001b), and rates of premature death as an index of the sex difference in risk taking (Kruger & Nesse, 2006). In her commentary, Harris (this issue) provided an additional example: homicide statistics and other extreme behavioral syndromes as an index of the sex difference in sexual jealousy. To be clear, our argument isn't simply that the "average man manages to grow old without killing another person, killing himself, or being married to more than one woman (at the same time)" (Kenrick, this issue, p. 202). The argument is that those few who do engage in these behaviors come from the extreme of the distribution of traits such as aggression or polygamous inclination, and the sex ratio at the tail exaggerates the difference in the wider population. The issue isn't how many men engage in these extreme behaviors but what the sex differences at the tail say about the species as a whole.

There were no direct attacks on the underlying logic of the tail-of-the-distribution argument, but several commentators had misgivings. Pound and Price (this issue) noted that, although it is rare for people to accumulate harems as large as villages, to murder their fellow human beings, or to die in multicar pileups, the sex differences seen in these various domains still tell us something about sex differences in general. Specifically, they tell us that, on average, human males are more polygamous, more intrasexually competitive, and more risk-prone than human females. If we were to throw these data away, note Pound and Price, we'd be losing some of the best data we've got.

Self-report measures are prone to social desirability bias, and this compromises their accuracy. In contrast, homicide statistics, road fatality statistics, and other data from the tail are highly accurate and provide an assay of sex differences that are otherwise very hard to measure (see, e.g., Daly & Wilson, 1988).

We agree that sex differences at the tail of the distribution tell us something about the differences in the rest of the population. However, for the reasons outlined, they probably exaggerate that something. The tails sometimes tell a tale, as Pound and Price put it, but to some extent it's a tall tale: an embellished account of the facts. As far as we can tell, none of the examples Pound and Price discuss escape the logic of the tail-of-the-distribution argument. Take road fatalities, for example. Between 1999 and 2007, men in the United States were 2.3 times more likely than women to die in a car accident. "The vast majority of journeys end safely," Pound and Price observe, "but we would argue that this 2.3 fold difference likely tells us something about the activities and behavior of men and women in the population at large" (p. 225). We agree; it tells us that, on average, men are more risk prone than women. But the average man is probably not *2.3 times* more risk prone than the average woman. Although anyone can get unlucky and die in a car accident, people who are atypically high in risk-proneness are at an increased risk. They will therefore be disproportionately represented in the road fatality statistics. Because these individuals come from the extreme of the distribution for risk-proneness, the ratio of men to women will be higher than it is in the general population. Thus, although the road fatality statistics do tell us something about the sex difference in risk proneness, if we use them to estimate the difference in the general population, our estimate will be an overestimate. Consistent with this suggestion, questionnaire and lab studies looking at less extreme forms of risk taking in more typical samples tend to find a smaller gender gap (Byrnes, Miller, & Schafer, 1999; Wood & Eagly, 2012, this issue). Now, does this mean that we should discard data from the tail of the distribution? Definitely not! As Pound and Price note, these data have been extremely useful in testing EP hypotheses. Daly and Wilson's (1988) homicide research in particular is some of the finest research in EP. Thus, our point is not that we should never use such data; it is that when we do, we need to take the tail-of-the-distribution phenomenon into account before we draw conclusions about the magnitude of sex differences closer to the mean (i.e., for most people).

Buss (this issue) and Kenrick (this issue) raised a rather different concern. They pointed out that even if the mean sex differences in traits such as aggression and short-term mate seeking are relatively modest, and even if extreme manifestations of these traits—such as homicide and sex trafficking—are relatively rare, men are still massively overrepresented in these activities. Furthermore, despite their rarity, homicide and sex trafficking create huge amounts of suffering. Thus, the sex differences have real-world consequences and these consequences really matter (Kenrick, this issue). Our response is to say only that we completely agree. We don't deny for a minute that homicide and sex trafficking matter or that the sex differences uncovered by evolutionary psychologists explain men's overrepresentation in these arenas. Our only claim is that the sex differences found for these extreme behaviors are not representative of the differences in traits like aggression and short-term mate seeking in the species as a whole.

What Have We Got Against Sex Differences, Anyway?

At the end of his commentary, Kenrick poses two questions for us:

> - Why do they want to sweep the differences under the rug?
> - What makes it better to call humans monomorphic than to acknowledge, and attempt to understand, when and why we are dimorphic? (pp. 205–206)

Good questions! To begin with, notice that the first question is premised on the assumption that our overall argument is false. Given that we don't share that assumption, here's how the question sounds to us. Imagine that you overheard a colleague announce that men are 4 times larger than women. You point out that although there is certainly a sex difference in size in our species, to claim that men are 4 times larger than women is to exaggerate that difference. Your colleague then turns to you and asks, "Why do you want to sweep the difference under the rug?" Your answer, of course, is that you *don't* want to; you just don't agree that men are 4 times larger. And that's our answer to Kenrick's first question. We would only add that we're not just concerned about evolved sex differences being overstated; we're also concerned about them being understated (see the later sections on Social Role Theory and Attachment Fertility Theory). As for the second question—why it might be better to call humans monomorphic than to acknowledge when and why we are (somewhat) dimorphic—well, we don't agree that we have to choose between these options. We can do both. We can acknowledge that human beings exhibit varying degrees of dimorphism across different traits, while at the same time acknowledging that most of this variation falls within the monomorphic range. In other words, we can locate humans on a rough dimorphism–monomorphism continuum without losing any information regarding precise levels of dimorphism for particular traits. Of course, this answer is premised on the assumption that our overall argument is correct. We hope, though, that this section has helped to make the case that it is.

The Three P's: Pair Bonding, Paternal Care, and Polygyny

> The intimate male–female relationship . . . which zoologists have dubbed a "pair bond," is bred into our bones. I believe this is what sets us apart from the apes more than anything else.
>
> —Frans de Waal (2005, p. 114)

> The majority of husbands remind me of an orangutan trying to play the violin.
>
> —Honoré de Balzac.

If it is accepted that human sex differences are generally rather modest, the next question is why this might be. The answer we presented in the target article is that humans evolved to exhibit high levels of pair bonding and biparental care, and that this substantially reduced the sex difference in reproductive variability in our species, leading to a general decline in dimorphism (see also Gray, this issue). None of the commentators denied that pair bonding is our most common mating arrangement, and several agreed that it is (Pham et al.; Pound & Price; Roberts & Havlíček). Nevertheless, there were several objections to the details of our presentation. These centered on two main issues: First, the evolution of pair bonding in the hominin lineage, and second, the relative importance of pair bonding versus polygyny in shaping the human psyche. We will address each of these in turn.

Pair Bonding and Paternal Care: The Plot Thickens

There are many theories about the factors leading to the evolution of pair bonding and paternal care in the human line (e.g., Chapais, 2008; Lovejoy, 1981; Winking & Gurven, 2011). In the target article, we argued for a version of *the provisioning hypothesis*. Specifically, we argued that pair bonding evolved primarily to facilitate biparental care and that paternal care was evolutionarily advantageous to men primarily because it increased the survival and reproductive success of their offspring (see also L. C. Miller et al., this issue). Most male mammals do not form pair bonds or care for offspring, but human males evolved to do both because of the extreme dependency of our young. Several commentators challenged this view (Campbell; Roberts & Havlíček). Roberts and Havlíček, for instance, discussed research indicating that the primary function of pair bonding for men might instead have been mate guarding (i.e., keeping other men away from the woman and vice versa). They and Campbell also suggested that pair bonding and paternal care may have evolved not because paternal care enhanced offspring survival but in response to a female preference for investing males (see also Campbell's sexual conflict argument; Campbell, this issue). We did acknowledge in the article that pair bonding may have been favored in part as a male mate guarding strategy (p. 146), and that paternal care may have been favored in part by female choice (p. 152). However, the commentators are correct in suggesting that we gave paternal care and its impact on offspring survival the starring roles in our evolutionary drama, and thus there is indeed a case for us to answer. The provisioning hypothesis has a long history in biological anthropology (e.g., Lovejoy, 1981). As the conventional view in the area, it might seem dated or even a little old-fashioned. We argue, however, that this old-fashioned view stacks up well against newer ideas.

Pair Bonding as a Mate Guarding Strategy

Consider, first, the idea that pair bonding is primarily about mate guarding rather than paternal care—or at any rate, that its original function was mate guarding (e.g., Chapais, 2008). We see three problems with this idea. First, it raises the question: What is the evolutionary advantage for the woman? If the man did not invest, directly or indirectly, in the woman or her offspring, it is not clear that the woman's fitness would have been enhanced by the pair bond. But if the woman's fitness was not enhanced, why would women evolve the psychological disposition to fall in love, experience jealousy, and engage in mate guarding of men? The mate guarding hypothesis provides a stronger explanation for men's pair bonding psychology than it does for women's.

Second, the mate guarding hypothesis does not explain why pair bonding evolved in our species but not in any other Great Ape or in most mammals. Why are we such an exception to the mammalian rule? The provisioning hypothesis has a ready answer: We evolved to form pair bonds because of the costliness of our offspring and the consequent need for allomaternal care. The mate guarding hypothesis, in contrast, is forced into the awkward position of saying that, although humans are the only Great Ape that exhibits pair bonding, and although we are also the only Great Ape possessing a conspicuous trait that could explain this fact (i.e., the costliness of our offspring), the former actually has nothing to do with the latter. This is logically possible, but it seems unlikely. More plausibly, the evolution of human pair bonding is related primarily to the care of our costly, big-brained young.

A third and final argument relates to men's paternal psychology. Roberts and Havlíček (this issue) pointed out that "numerous mammalian species which frequently form pair bonds do not show paternal care" (see also Emlen & Oring, 1977). This is certainly true. However, one reason to think that humans are *not* one of these species is that humans *do* show paternal care (Marlowe, 2000). As detailed in the article, there are strong reasons to think that the male psyche is specifically adapted to bond with and care for children (pp. 146–148). If pair bonding were *wholly* a matter of mate guarding, we would not expect this. Thus, all the evidence for men's evolved paternal psychology counts as evidence against the idea that pair bonding is wholly a matter of mate guarding.

The Evolution of Paternal Care Through Female Choice

None of the above arguments is applicable to the second alternative explanation for pair bonding and paternal care: the female choice hypothesis (Campbell, this issue; Roberts & Havlíček, this issue; see also Buss & Schmitt, 1993). According to this hypothesis, women evolved to mate preferentially with men willing to invest in them and their offspring. This in turn created a strong selection pressure on men, leading to the evolution of pair bonding and paternal care. Thus, pair bonding and paternal care evolved in much the same way as did the peacock's tail: through the mating decisions of females. Unlike the mate guarding hypothesis, the female choice hypothesis provides clear benefits of pair bonding for both women and men. For women, the main advantage is a spare pair of hands to help provide for her and her young (Campbell, this issue). For men, in contrast, the main advantage comes from gaining exclusive (or near-exclusive) sexual access to the woman. In the absence of a female preference for investing males, men's fitness might have been better served by seeking multiple mates than by pair bonding and caring for offspring (Winking & Gurven, 2011). Given the female preference, however, the latter strategy might have yielded a greater fitness increment. Importantly, according to the female choice hypothesis, this is because paternally inclined men increased their chances of siring their mate's future children, *not* because paternal care selectively boosted the survival and reproductive success of their *own* children.

It is certainly plausible that female choice could shape paternal tendencies. Mathematical models indicate that if females mate preferentially with parental males, male parental care can evolve even when paternity probability is low (Alonzo, 2012). But although female choice might have played some part in the evolution of paternal care, it seems unlikely that it was the sole driver. The reason is simple: If the only reproductive payoff for paternally-investing men was sexual access to the woman, it would make no difference to men's fitness whether the children they cared for were their own or someone else's. As long as they had a reasonable chance of siring their mate's next child, paternal care would have achieved its goal. However, if that were the case, there would be no reason to expect men to invest more in genetic offspring than in step-offspring. In other words, there would be no reason to expect the Cinderella effect (Daly & Wilson, 1988, 2001a; Marlowe, 1999). Thus, all the evidence for the Cinderella effect counts as evidence against the hypothesis that paternal care is solely a matter of meeting female preferences and obtaining sexual access. More than that, it tells us that men's investment in their genetic progeny must be an essential part of the evolutionary story.

Problems With the Provisioning Hypothesis

We've pointed out some of the challenges facing the mate guarding and female choice hypotheses. As several commentators observed, though, our preferred hypothesis faces some challenges of its own (Campbell; Roberts & Havlíček). One of the strongest comes from an ambitious cross-cultural study by Sear and Mace (2008). This involved a detailed review of 45 natural-fertility societies: societies that had not yet undergone the demographic transition. Sear and Mace were interested in children's survival rates, and whether the presence of a given category of family member (e.g., mother, father, grandparent, sibling) was associated with better survival. The results were striking. In every society for which there was evidence (28 out of 28), the presence of the mother increased the chances that her offspring would survive. The presence of the father, on the other hand, had a much less consistent effect. Father presence was associated with increased offspring survival in only 32% of societies (7 out of 22).[2] Grandmothers and older siblings were associated with increased survival in a greater number of societies than were fathers. Sear and Mace do not deny that fathers sometimes invest in their young. They argue, however, that this is not as consistently important or universal as many assume it is. Mothers always receive allomaternal support from *someone*, but it's not always the father (see also Hrdy, 2009).

We have some sympathy for this view; as we mentioned in the target article, paternal investment is facultative rather than obligate, and this probably accounts for some of the variability in paternal care across cultures. We also agree that fathers are not the only allomaternal helpers (see Snowdon, this issue). Nonetheless, we suspect that Sear and Mace's (2008) findings underestimate the importance and impact of paternal care. Indeed, it would be surprising if they did not; after all, men in most societies engage in paternal care

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[2] Note that if we consider only the studies that Sear and Mace judged to be statistically valid, the percentage of societies in which father presence was associated with offspring survival rises to 47% (7 of 15; see Sear & Mace, 2008, Table 3).

(Marlowe, 2000), and this would be rather puzzling if paternal care usually had no effect. One issue with the study, which we touched on in the article, is that child survival is an extremely exacting measure of investment, and one that is blind to some of the subtler ways that paternal care could enhance the father's fitness. This includes decreasing the woman's interbirth interval, and thereby increasing the number of offspring the man can sire with her (Campbell, this issue). A second issue, also mentioned in the article, is that most of the societies Sear and Mace surveyed were agricultural societies, whereas for most of our evolutionary history, humans lived as hunter-gatherers. The fact that, in many agricultural societies, children do just as well without an investing father does not imply that this was necessarily the case among Pleistocene hunter-gatherers (Draper & Harpending, 1987). Children don't need even a *mother* to survive in modern welfare states, but we could not infer from this that children did just as well without a mother in premodern societies. In short, it is not clear that Sear and Mace's findings reflect the typical conditions in which humans evolved, and thus it is not clear that the findings reflect the typical rates or impact of paternal care throughout most of our evolutionary history.

Even if we ignore these concerns, however, there is still a genuine question about how to interpret the findings. Sear and Mace (2008) seem to assume that increased offspring survival tells us who's providing evolutionarily significant help: If the presence of the father increases child survival, fathers are helping; if it doesn't, they're not. We're not so certain. Consider, first, the fact that, in many of the societies surveyed by Sear and Mace, there was little or no mother effect after the child reached the age of 2—that is, young children were just as likely to survive with or without a mother. Does this mean that mothers in these societies made no contribution to their children's survival after their second birthdays? It seems unlikely. Two-year olds cannot feed themselves or keep themselves safe. Certainly, these responsibilities do not fall solely to the mother; however, in most traditional societies, the mother takes the leading role (Wood & Eagly, 2002). Why, then, does the mother effect evaporate after children hit 2? One possibility is that, if the mother dies, other family members often take over and help keep the child alive (Sear & Mace, 2008). This might sometimes mask the mother effect. However, the absence of a mother effect would not then imply that *living* mothers are *not* helping keep their children alive. Presumably, they are. The same argument applies to fathers. If a father dies (or absconds or otherwise vanishes), other family members may take over his parental duties, and this may often mask the father effect. But the absence of a father effect would not then imply that living fathers make no difference to their children's survival. They may well make a difference, but in their absence, others may step in and make the same difference. If *all* the child's allomaternal helpers vanished, then the contribution of the father might show up more clearly.

Certainly, mothers are more reliably associated with offspring survival than fathers. But in our view, this implies only that mothers have a consistently *bigger* impact on children's welfare than fathers. It does not imply that fathers rarely have any impact at all. Thus Sear and Mace's (2008) study does not undermine the claim that paternal care is a central part of humanity's reproductive toolkit, or the claim that it was selected because it boosted the survival and reproductive success of the father's genetic progeny. These hypotheses are still live options.

Is Polygyny the Royal Road to Reproductive Success for Men?

There is one more matter to consider: the relative importance of pair bonding versus polygyny in shaping men and women's sexual psychologies. In the target article, we argued that, although pair bonding is not human beings' only mating system, it is our *primary* system. The pair bond is the most common context for sex and reproduction in our species, and quite possibly has been for much of our evolutionary history (Stewart-Williams & Thomas, this issue, pp. 145–148, 150–151). As such, this mode of mating has left a deep imprint in our evolved nature. Some of the commentators suggested that, in making this argument, we might have overstated the importance of pair bonding in our ancestral past, and correspondingly understated the importance of polygynous or promiscuous mating. Roberts and Havlíček (this issue) point out that, although social monogamy was indeed more common than polygyny, polygyny was still a common-enough feature of the human mating landscape that it too left an imprint in our evolved nature. Furthermore, although polygynous mating was infrequent, this does not imply that men had no interest in it; instead, most men's polygynous ambitions may simply have been thwarted by socio-ecological constraints (see also Kenrick, this issue). In a similar vein, Pound and Price (this issue) note that, although opportunities for polygynous mating were usually rare, the reproductive benefits of polygyny were so great for genes located in male bodies that the male mind might still have evolved to take advantage of those opportunities, if and when they did arise. As a result, men may harbor strong polygamous desires—much stronger than women's—even if these desires are frustrated for most men throughout most of their lives (Symons, 1979). Pound and Price put it well:

> because fat, salt, and sugar were nutritionally valuable yet elusive in ancestral environments, people tend to express unhealthily strong appetites for these nutrients in environments in which they are abundant. . . . Given

> that polygyny was both elusive and reproductively rewarding to ancestral men, they may have evolved strong desires to achieve it. (p. 227)

We don't have any major quarrels with any of these arguments. We agree with the general point that ancestral humans engaged in nontrivial levels of polygyny, and that this left an imprint in our evolved nature, just as social monogamy did (Stewart-Williams & Thomas, this issue, p. 150). We also agree that certain selection pressures would have pushed our species toward polygyny. To balance out the picture, though, we would like to add that there would *also* have been selection pressures pushing in the opposite direction. So, for instance, if we are right in thinking that ancestral men were often able to enhance their lifetime fitness by forming pair bonds and contributing to the care of their offspring, this would have selected against a strongly polygynous or promiscuous male psychology. It would have made it somewhat less profitable, evolutionarily speaking, for most of our male ancestors to devote too much time or energy to pursuing new wives or short-term flings. This is not to say that natural selection has eliminated all polygynous inclinations from men's minds; we know that it has not. However, the commentators focused on the benefits of polygynous mating for men but not on the costs, which could foster an inflated expectation of the magnitude of the sex difference in polygamous inclinations.[3] We are happy to concede that polygyny may have been more important than we implied in the target article. Nonetheless, for the reasons given, we wouldn't be too surprised if we got it about right.

The Notorious MCFC versus MMC Dichotomy

> Men . . . are like verbal peacocks, using language to impress women during courtship . . . women need good verbal skills in order to choose a good mate by making an accurate judgement of a potential partner's ability.
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> —Workman and Reader (2008, p. 291)

> The only model that makes sense for humans and for other cooperatively breeding species is the Mutual Mate Choice model.
>
> —Snowdon (this issue, pp. 239–240)

Next on the agenda is our claim that there are two contradictory conceptions of human sexual psychology inhabiting the EP literature—the MCFC and MMC models—and that the MCFC model has somewhat distorted the evolutionary psychologists' picture of our species. Of all the claims in the target article, this was the one that most divided the commentators (see, e.g., Buss; Eastwick; G. F. Miller; Pham et al.; Pound & Price; Roberts & Havlíček). Among those who took issue with it, the criticisms revolved around two main points. The first was our assertion that there is a contradiction in the EP literature between the MCFC and MMC models. The second was the (supposed) implication that EP has entirely neglected core components of the MMC model, including male mate choice, pair bonding, and paternal care.

Is There a Contradiction in the EP Literature?

We begin with the idea that there is a contradiction in EP between the MCFC and MMC models. The contradiction, in our view, concerns claims about overall levels of dimorphism in our species. On the one hand, humans are presented as (primarily) a pair bonding, biparental species with mutual mate choice—which seems to imply low overall dimorphism. On the other hand, humans are presented as a species with high dimorphism—which is hard to reconcile with the idea that we are primarily a pair bonding, biparental species with mutual mate choice. Several commentators denied that there is any real contradiction here (Pham et al.; Roberts & Havlíček). They agreed that there is material in the EP literature that fits our description of the MCFC model, and material that fits our description of the MMC model. However, they argued that EP provides a way to reconcile these apparently competing claims: Both are true; sometimes we are an MCFC species and sometimes we are an MMC species. According to theories such as Sexual Strategies Theory (Buss & Schmitt, 1993) and Strategic Pluralism Theory (Gangestad & Simpson, 2000), humans facultatively adjust their mating behavior in response to their circumstances (Betzig, this issue; Pham et al., this issue; Pound & Price, this issue; Roberts & Havlíček, this issue). Sometimes, we form pair bonds; sometimes, we engage in short-term mating. In the first case, we tend to exhibit low dimorphism: Both sexes are choosy about their mates and compete for the best mates available, and both invest in any offspring (MMC model). In the second case, we exhibit higher dimorphism: Women are much choosier than men about their mates, and men engage in vigorous competition to secure as many sexual conquests as possible (MCFC model). Thus, EP can account for all the claims made about our species, and there is no contradiction in the literature.

Does this move work? First, let us say that there is some merit in the suggestion. Humans are clearly more dimorphic for some traits than for others, and as we mentioned in the article, sex differences related to early courtship and short-term mating tend to be larger than those related to later courtship and long-term mating

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[3] That said, we do take Campbell's point that frequency-dependent selection could produce a population consisting for the most part of relatively monogamous men, but with a subpopulation of rogues who cheat the system (see Bailey, Kirk, Zhu, Dunne, & Martin, 2000).

(Buss & Schmitt, 1993; Kenrick et al., 1993; Kenrick, Sadalla, Groth, & Trost, 1990; Li & Kenrick, 2006). Thus, one might argue, as Pham et al. (this issue) did, that "*some elements* of the MCFC model *sometimes* manifest in human behavior and psychology" (p. 222; see also Roberts & Havlíček, this issue).

Ultimately, though, we don't think the proposed solution dissolves the contradiction. In our view, pair bonding and biparental care, although not our only mating options, were sufficiently important in our ancestral past to lead to a *very general* decline in psychological dimorphism. For most psychological traits—including those related to sex and reproduction—the differences are remarkably small. In other words, although we sometimes exhibit trends in an MCFC direction, these trends are weak. It is true that the sex difference in choosiness is greater in a short-term than a long-term context (Pham et al., this issue). However, as noted previously, even in a short-term context, the difference is still modest (see, e.g., Finkel & Eastwick, 2009; Kenrick et al., 1993). Likewise, even for SO—one of the largest and most theoretically significant sex differences found in our species (Lippa, 2009; Schmitt, 2005)—the effect size is only around half that of the human height difference. Thus, although there is variation across traits in the exact levels of dimorphism, almost all this variation clusters together near the monomorphic end of the spectrum (see *The Effect Size Argument*, pp. 249–251). This is consistent with the claim that pair bonding and biparental care produced a general reduction in psychological dimorphism. If we accept this claim, then there is immediately a contradiction in EP between the pair bonding literature and the literature suggesting high dimorphism.

Of course, this is precisely the claim that Pham et al. (this issue) and Roberts and Havlíček (this issue) tacitly reject. They argue that there is no contradiction in the EP literature, which would seem to imply that we *cannot* classify humans in terms of their overall level of dimorphism (see also Kenrick, this issue). We cannot say that humans tend toward monomorphism; we're monomorphic in some circumstances but dimorphic in others. However, this proposal deserves closer scrutiny. For most species, we are happy to talk about overall levels of dimorphism. Gibbons and penguins are monomorphic; gorillas and peacocks are highly dimorphic. Admittedly, gibbons and penguins are dimorphic when it comes to their reproductive anatomy, and gorillas and peacocks are monomorphic when it comes to perceptual capabilities. Nonetheless, it makes sense to say that, overall, the former are monomorphic and the latter dimorphic. On the face of it, it seems implausible that humans would be an exception to this rule—that we, unlike most animals, would be both dimorphic and monomorphic, with no general tendency in one direction or the other. It seems especially implausible given that, when we look at the evidence, most of it suggests that there *is* a general tendency in one direction: Humans tend to have relatively low dimorphism—again, even for traits like SO.

We do take Roberts and Havlíček's point that human beings, like dunnocks, exhibit a fair amount of variability in their mating habits; indeed, we have made the same comparison elsewhere (Stewart-Williams, 2005). But this variability does not undermine our position. Neither the MCFC nor the MMC model posits a single human mating strategy. Both acknowledge the variability in men and women's mating behavior; they simply construe it in different ways. So, for instance, pair bonding is part of the MCFC model but is a "second-best strategy" for men who fail to mate polygynously; polygyny is part of the MMC model but is less central than EP typically suggests (see Eastwick, this issue). Thus, we disagree with Roberts and Havlíček's suggestion that our article attempts "to define a species-typical model based on one component of evolved nature, while ignoring other components" (p. 233). On a related note, we have some misgivings about how Roberts and Havlíček interpret the variability of mating patterns in both dunnocks and humans (see also Pham et al., this issue; Pound & Price, this issue). According to N. B. Davies (1985, 1989), much of the variability seen in dunnocks is the result of relatively invariant male and female preferences interacting with different ecological conditions (e.g., the local sex ratio, the distribution of food), rather than facultative switching from one distinct sexual psychology to another. The same may apply to humans. Thus, although humans are somewhat more dimorphic for preferences and motivations related to short-term than long-term mating, it might be overstating the case to suggest that we facultatively switch between a gibbon-like sexual psychology (MMC) and a peacock-like sexual psychology (MCFC; see Eastwick, 2009, this issue, for a perceptive discussion of the short-term/long-term distinction). Instead, different circumstances may simply give people more or less scope to act on particular preferences and motivations. In any case, the fact that humans exhibit some flexibility in their mating behavior does not change the fact that the psychological sex differences in our species generally fall within the monomorphic range, even in a short-term context.

Our conclusions, then, are as follows: (1) it is meaningful to make statements about overall levels of dimorphism in our species, (2) humans are relatively monomorphic for most psychological traits, and (3) there is thus a genuine conflict in EP between the literature suggesting low dimorphism and that suggesting high dimorphism. It is reasonable to think that the MMC and MCFC models represent distinct visions of human nature, and that the apparent contradiction between them cannot be shooed away simply by arguing that both are preprogrammed modes of operation of the human animal.

Does EP Neglect Mutual Mate Choice?

Another point of contention relates to our suggestion that the MCFC model has resulted in a somewhat distorted EP picture of human sexual psychology and has sometimes led EP to minimize the importance of mutual mate choice. This suggestion drew fire from a number of commentators. Pham et al. (this issue) were concerned that the target article might persuade nonexperts that EP has neglected pair bonding and paternal care, and has propagated an exclusively MCFC vision of the human species. To demonstrate the falsity of this view, they discussed various lines of EP research that directly examine or tacitly assume the importance of pair bonding and paternal care. (We thought it was a nice touch that they included several studies on which one of the present authors [SSW] was a collaborator!) Likewise, Pound and Price (this issue) argued that the MMC model "is already a major focus within evolutionary psychology," noting, for instance, that "there is in fact a rather substantial amount of research being conducted on male mating preferences" (p. 228). Finally, Kenrick (this issue) pointed out that he and other evolutionary psychologists have taken high male parental investment into account in their research and theorizing for several decades (see, e.g., Buss, this issue; Kenrick et al., 1990).

We certainly hope that readers don't get the impression that EP has ignored male mate choice, pair bonding, or paternal care, and perhaps we could have been clearer about this. In our defense, though, we did stress early in the piece, and at several points thereafter, that EP *does* conduct research on all aspects of the MMC model. We even wrote that "EP has led the field in documenting these aspects of human sexual psychology" (p. 152). Our claim, remember, is not that male choice, pair bonding, and paternal care are absent from the literature but that there is an unnoticed contradiction in EP between the MMC and MCFC models. We could not claim such a contradiction unless we recognized that EP studies elements of the MMC model such as pair bonding and paternal care. And not only did we recognize it; we cited a great deal of EP research on these topics (see *The Evolution of Mutual Mate Choice*, pp. 144–152). However, we *also* cited a great deal of EP research reflecting the influence of the MCFC model. The final section of the article was devoted entirely to this task (see *The MCFC Model in Evolutionary Psychology*, pp. 152–162). The fact that we were able to draw on the EP literature in both the MMC *and* MCFC sections neatly sums up our position: There is plenty of research in EP premised on the assumption of mutual mate choice, but there is also plenty of research giving the distinct impression that MCFC is the primary dynamic in human mating. Arguably, the least ambiguous examples of the latter are studies that treat courtship behaviors like humor, language, and creative intelligence as if they were designed solely for men to attract women and not also the other way around (see the quote at the start of the section). This represents a strong MCFC interpretation that is erroneous even in a short-term mating context (G. F. Miller, this issue). But the MCFC bias also leaks out in subtler ways, as we hope we showed.

We share Pham et al.'s concerns about the misrepresentation of EP. However, this is not the only thing to be concerned about. If we are correct in thinking that EP has overemphasized human sex differences, this too is cause for concern and something worth pointing out. Of course, it is also worth pointing out when scholars *under*emphasize the sex differences. With that in mind, we turn now to the next major section of the article.

Competing Theories

In this section, we consider two competitors to the evolutionary psychological account of human sex differences. These are Wood and Eagly's Social Role Theory and L. C. Miller et al.'s Attachment Fertility Theory. We start with Social Role Theory.

Social Role Theory

> Stewart-Williams and Thomas's mischaracterization of our theory as invoking nurture alone apparently arises from their inability to reason about nature and nurture as interacting influences.
>
> —Wood and Eagly (this issue, p. 243)

The gloves came off with Wood and Eagly's commentary! Wood and Eagly are the originators of a theory of sex differences that stands in opposition to EP. This is known variously as Social Role Theory, the biosocial extension of Social Role Theory, and, more recently, biosocial construction theory. We called it Social Role Theory (SRT) in the target article, and will continue to do so here. According to SRT, the contemporary division of labor in a society, and the associated social roles, are the primary cause of the psychological sex differences and similarities in our species. Ancient selection pressures have little or nothing to do with it. In our article, we provided several arguments against this view. Wood and Eagly's response was to declare that we misunderstood their theory and to argue that, contrary to our conclusion, SRT can explain the full panoply of human sex differences. We disagree on both of these points.

Is SRT a blank slate theory? To begin with, Wood and Eagly (this issue) suggested that we misrepresented SRT by lumping it together with blank slate, Nurture-Only theories of human psychology. They reject such a designation, arguing that SRT is instead a nature–nurture interactionist theory that "integrates the broad classes of biological and social-cultural factors"

(p. 241). EP, in contrast—in their estimation—adheres to a one-sided, Nature-Only view of sex differences. (Ironically, evolutionary psychologists also describe *their* approach as a nature–nurture interactionist theory and claim that *their* competitors adhere to a one-sided, *Nurture*-Only view; see, e.g., Tooby & Cosmides, 1992.)

Is SRT a blank slate theory? Our answer is: yes and no. On the one hand, Wood and Eagly incorporate some evolved sex differences into their model. As we noted in the article, however, these are confined almost entirely to *physical* differences—the differences in height, strength, speed, and the ability to bear and nurse offspring. According to SRT, these physical differences help to determine the allocation of men and women to different social roles, and these social roles then shape people's psychologies. In this way, evolved physical sex differences indirectly help shape the psychological sex differences we observe in the everyday world. Based on these ideas, many have concluded that SRT acknowledges evolved sex differences in men and women's bodies but retains a blank slate conception of the human mind. Is this interpretation correct? Again, the answer is yes and no. Wood and Eagly accept that the mind has some built-in structure, including the neural and hormonal systems underlying "basic perceptual, sensory, and motivational systems" (p. 241). As such, they don't posit a blank slate mind in the sense of a mind devoid of any native structure (see also Eagly & Wood, 1999, p. 409). But for present purposes, the question is, Do they accept that there are any inherited psychological sex differences? If not, they maintain a blank slate view *with respect to sex differences*.

The overwhelming impression one gets from their work is that that is exactly the view they maintain. So, for instance, in a recent, detailed summation of their theory, Wood and Eagly (2012) presented a figure outlining the key features of SRT (p. 58). This included two broad sets of influences shaping the division of labor and thus the social roles of a society: (1) cultural, economic, and ecological factors, and (2) evolved physical sex differences. These were the only factors they thought important enough to include. Certainly, if you look at the small print, Wood and Eagly don't entirely rule out inherited psychological differences. They note, for instance, that differences in early temperament might help shape the division of labor, perhaps because they make it easier to socialize boys versus girls into certain roles (Wood & Eagly, 2012). But as they note in their commentary, they reserve judgment on both the origins and implications of temperamental differences, and in their final assessment, "any inherited psychological sex differences are not of overriding importance" (p. 243; see also Eagly & Wood, 1999, p. 414). We conclude, therefore, that when it comes to the ultimate origins of sex differences, SRT must be classed either as a blank slate theory or as a virtually-blank-slate theory. If we're wrong about this, and Wood and Eagly accept that there are *important inherited psychological sex differences* in our species, they should directly and unequivocally say so.

Until that time, we reject the claim that we misrepresented SRT. We also reject the claim that SRT represents a genuinely integrative biosocial theory of human sex differences. Granted, Wood and Eagly include evolved physical differences in their model, and they describe how neurohormonal systems might be recruited to enact social roles. But that's all the *bio* they include in their supposedly *biosocial* theory. There is certainly no *evolutionary* bio. A truly integrative model of sex differences would weave together sociobiological theories and sociocultural theories. SRT simply rejects the sociobiological theories. Indeed, to a large extent, the theory is a reaction against those theories—an attempt to frame an alternative explanation for the findings of EP (see, e.g., Wood & Eagly, 2012, p. 102).[4] It seems reasonable to conclude, then, that Wood and Eagly's biosocial theory is essentially a traditional social theory disguised as a biosocial theory.

Our arguments against SRT. Wood and Eagly devote a significant chunk of their commentary to rebutting our arguments against SRT. Before determining whether their efforts succeed, let us make clear our position on the evolutionary versus social causes of sex differences in our species. Wood and Eagly assume that we hold a Nature-Only view. This can be seen in the title of their commentary: "Biology or Culture Alone Cannot Account for Human Sex Differences and Similarities." Our response to this statement is: Of course! Although we think that the evolutionary contributions are sometimes more influential, and although we focused on these in the article, we do not deny the importance of social forces or cultural evolution (see, e.g., Stewart-Williams, in press-a, in press-b). We do not think that "inherited biological factors thoroughly constrain sociocultural influences on men and women," as Wood and Eagly charge (p. 242). On the contrary, we imagine it would be possible in principle to set up a society in which men and women were identical on average, or in which the normal sex differences were reversed, or in which the normal sex differences were strongly exaggerated. (Indeed, the latter can arguably be observed already in strongly patriarchal societies.) We suggest, however, that it would take extreme social interventions to create and maintain societies like

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[4] To be fair, in *some* cases these explanations are plausible. Consider, for instance, the fact that, on average, women rate earning capacity as more important in a mate than do men (Buss, 1989). Eagly and Wood (1999) showed that this gender difference is larger in nations in which women have less independent access to wealth, suggesting that the difference owes less to evolution than to economics.

these, because they would go so strongly against the grain of human nature. In the absence of continuing social pressure, these societies would drift back to the usual pattern of modest sex differences. That, in a nutshell, is how we conceptualize the interaction of nature and nurture. Consistent with this view, our claim in the article was not that social roles play no part in determining sex differences, but rather that they could not be the *complete explanation* for the differences found in human beings.

We provided several arguments in support of this conclusion. First, we argued that certain sex differences found in our species—the differences in size, physical aggression, sex drive, interest in multiple sexual partners, parental inclinations, timing of puberty, and life expectancy—are found in a very wide range of species, and thus that the primary explanation for these differences in humans (including the psychological differences) is likely to be evolutionary forces common to many species rather than social forces unique to our own. Wood and Eagly basically ignored this argument. They did argue that size dimorphism alone is not diagnostic of a species' evolved mating pattern, but we never suggested that it was. Our argument concerned the cross-species trend across multiple traits and its implications for the origins of a specified set of psychological sex differences seen in human beings. Given that sociobiological theories offer an explanation for the cross-species trend, whereas SRT does not, it would be rash to accept Wood and Eagly's conclusion that sociobiological theories are "outmoded" or "outdated."

Wood and Eagly did address our next argument, but they badly mangled it. The argument was this: Humans evolved from an earlier species possessing inherited sex differences in traits like aggression, parental inclination, and sex drive. SRT maintains that humans no longer possess inherited sex differences in these or other traits. This implies that, somewhere in our evolutionary past, those inherited differences must have been eliminated by natural selection, which in turn implies that they must have become maladaptive—that is, that they must have been selected against, relative to a more flexible, less sexually differentiated psychology. Wood and Eagly mounted a very poor response to this argument. They first observed that they never said that the sex differences became maladaptive or were removed. And that's true! We weren't summarizing their theory at that point; we were identifying an *implication* of their theory that they have not adequately dealt with. Wood and Eagly then denied that natural selection removed any adaptations (e.g., neurohormonal systems) shared with other animals. But we never claimed that SRT implies that natural selection removed any *adaptations*. We claimed that it implies that natural selection removed any *inherited psychological sex differences*. This would almost certainly *not* have involved removing adaptations or neurohormonal systems shared with other animals. Most likely, it would have involved tinkering with the neurohormonal systems directly, or it would have involved the evolution of regulation by higher order systems. Either way, SRT seems to imply that natural selection must have nullified any preexisting, inherited sex differences in traits like aggression, parental inclination, and sex drive.

As we see it, there are two ways that Wood and Eagly can respond to this argument. First, they can deny that natural selection removed any inherited psychological differences—in which case, they agree with us that those differences are still found in humans. Alternatively, they can deny that humans possess inherited psychological sex differences—in which case, they are committed to the view that natural selection removed the differences (and, to reiterate our earlier conclusion, they would then hold a blank slate view of human sex differences). If they take the latter option (as we think they must if they wish to preserve SRT), they then need to explain why it happened. Why were these differences suddenly selected against? It is not enough to engage in vague hand waving about cumulative culture and human adaptedness "not primarily to particular environmental features, but to variation itself" (Wood & Eagly, this issue, p. 241). We agree that no other animal possesses the kind of cumulative culture found in our species. But many animals—from chimpanzees to capuchin monkeys to sperm whales—engage in complex social learning despite still possessing inherited sex differences. These things are not mutually exclusive. And certainly the dual-inheritance/gene-culture coevolutionary theorists that Wood and Eagly align themselves with—Boyd and Richerson, Henrich, Laland, Mesoudi—do not deny that humans possess evolved psychological sex differences. For all these reasons, we stand by our initial claim that Wood and Eagly have yet to explain why natural selection might have wiped out the male–female differences found in our prehuman ancestors. In other words, they have yet to square SRT with modern evolutionary theory.

Other problems with SRT. Wood and Eagly's commentary helped crystallize various other misgivings we have about their theory. First, although we agree that a society's roles and rules have some impact on the sex differences found in that society, we suggest that their impact is considerably stronger for overt behavior than for internal psychology. So, for instance, in nations where women but not men can face execution for adultery or premarital sex, there is likely to be a large sex difference in these behaviors—not because women have radically different sexual desires in those nations, but because people generally inhibit behavior that is likely to get them killed. Social roles no doubt influence our desires as well; however, their impact on desires is presumably weaker than their impact on behavior. Why else would women's sexual behavior be so strictly policed in these societies? More generally, although people usually channel their behavior into the framework of the local social roles, these roles may fit their native psychologies to a greater or lesser degree.

Second, within the framework of SRT, the causal arrow only ever points in one direction: from social roles to psychology. Wood and Eagly (this issue) suggest, for instance, that the "division of labor yields the familiar psychologies of women and men" (p. 242) but not that the influence might also go the other way. This, we suggest, is a rather extreme view. It seems much more plausible to us that it's a two-way street—that social roles influence psychology but that psychology also influences social roles. We can frame this in terms of cultural evolution: Just as our tools evolved culturally to fit our hands, so too our social roles evolved culturally to fit persisting aspects of the human mind. Roles that jar too violently with human nature are unlikely to persist for long, at least without the application of significant social force. If this is correct, it raises the possibility that some social roles might have evolved culturally to fit traits that, although found in both sexes, are more common in one than the other. This is emphatically *not* to say that there are some male roles and some female roles. But it is to suggest that there might be some social roles that suit more men than women, and others that suit more women than men—not just because of evolved physical differences but because of evolved psychological differences as well. To be clear, we are not proposing that socialization has no part to play in fitting people to the social roles of their society. Our proposal is much more modest: that socialization is not the *only* contributing factor.

Third, Wood and Eagly attribute a huge amount of power to social roles. Social roles are not merely one influence among many; they are the primary determinant of sex differences and similarities in our species (Wood & Eagly, 2012). But various lines of evidence point to the falsity of this conclusion. This includes research on people who are born as members of one sex but who are raised as members of the other. Such cases effectively pit biology against social roles. Our reading of the literature is that the outcome is often a compromise, and that biology sometimes even trumps social influences. Consider, for example, the famous—and very sad—case of David Reimer (Colapinto, 2006; Diamond & Sigmundson, 1997). As an infant, Reimer's penis was mutilated in a botched circumcision. Under the guidance of sexologist John Money, it was decided that he should be surgically castrated and raised as a girl. This was based on the assumption that sex role socialization is all-important, and thus that, with the appropriate socialization (plus a steady diet of female hormones), Reimer would turn out just like any other girl. Early reports suggested that he was indeed a normal, well-adjusted girl, just as SRT might predict (Money, 1975). For several decades, the case was hailed as proof positive of the nurture theory. It was eventually revealed, though, that Reimer had been a deeply unhappy child, who constantly complained that he felt like a boy. As a preschooler, he ripped off his dresses and refused to play with dolls, opting instead for guns and other stereotypically boy toys. At school, he got into fist fights and insisted on standing while urinating. Finally, at the age of 14, his parents told him the full story and Reimer started living as a male. He stopped taking the hormones, had an artificial penis constructed, and eventually married a woman. Sadly, the story ends with Reimer's suicide at age 38.

Now this is a single case, and there is some debate about what exactly we can conclude from it (see, e.g., Jordan-Young, 2010). But other cases point in the same direction. This includes several dozen cases of otherwise-normal genetic males born with genital anomalies (e.g., micropenises), who were surgically reassigned as female while still infants. The psychiatrist William Reiner (2004; Reiner & Gearhart, 2004) reported that, despite being raised as girls, by adulthood a majority of these individuals identified as males. Furthermore, virtually all of them—including those who identified as females—had typically masculine interests and preferences (see also Bradley, Oliver, Chernick, & Zucker, 1998). These cases, like the Reimer case, challenge the idea that social roles are the *primary* determinant of human sex differences. Evolution matters too.

(Note that these cases also challenge Wood and Eagly's conception of the relationship between nature and nurture. In their view, "nature and nurture are not competing influences but instead are interacting forces" (p. 242). The Reimer case and its kin suggest that this is false: Nature and nurture may often work together harmoniously, but they can also sometimes come into conflict.)

Are human sex differences generally small? Wood and Eagly (this issue) rejected our suggestion that psychological sex differences in *Homo sapiens* are generally rather modest and that EP sometimes exaggerates those differences. They begin by stating that we misconstrued the usual EP position on the issue, which is that these differences are found only in domains where men and women faced recurrently different adaptive challenges (Buss's well-known metatheory of sex differences; Buss, 1995). We were surprised by this argument. First, we were surprised that anyone who read the article could take the impression that, in our opinion, EP claims that sex differences are uniformly large across all domains. Aside from anything else, we explicitly discussed the EP finding that the differences tend to be larger in a short-term than a long-term mating context (pp. 151, 157). More to the point, though, the fact that EP does not claim large sex differences across the board does not imply in any way that evolutionary psychologists never exaggerate the magnitude of *particular* sex differences. This argument can be safely ignored.

Next, Wood and Eagly argued that, contrary to our view that sex differences are generally quite modest, the differences are actually highly variable. We were a little surprised by this argument too. As noted, Eagly (1995) has written elsewhere that most sex differences (and indeed most effects in psychology) are small or medium in size and that only a few are large—which is essentially our position (see also Hyde, 2005). Still, perhaps Wood and Eagly object to our further suggestion that even some of the larger differences (such as the sex difference in SO) are not so large after all, and that most differences fall within a range that can be described as monomorphic. In their commentary, they provide an example of the variability of sex differences, so let's see whether it contradicts our position. The example concerns sex differences in different forms of risk taking (Byrnes et al., 1999). For social risk taking, they note, the sexes barely differ. In contrast, for risky games involving physical skills, there is a sex difference with an effect size of $d = .43$: a medium effect according to Cohen (1988). Finally, when it comes to life-threatening acts of heroism, there is a very large difference: Around 91% of the recipients of Carnegie Hero medals are men (Becker & Eagly, 2004; see also Johnson, 1996).

Does this variability undermine our claim that sex differences tend to be modest? It does not. First, although a d value of .43 constitutes a medium effect according to Cohen's arbitrary standard, it is only around one fourth the magnitude of the modest human sex difference in height and only around half the magnitude of the sex difference in size in the monomorphic gibbon. It therefore fits easily with our generalization that most human sex differences fall within the monomorphic range (see *The Effect Size Argument*, pp. 249–251). As for the Carnegie Hero medalists, it is probable that these individuals come disproportionately from the extreme right-hand tail of the distribution for risk taking. As we noted several times in the target article, sex differences are much larger at the tail than they are nearer the mean, and thus data derived from the tail give an inflated impression of the difference in the species as a whole (see *The Tail-of-the-Distribution Argument*, pp. 251–252). Thus, Wood and Eagly's example is perfectly consistent with the view that our hominin ancestors underwent a general decline in psychological dimorphism, and thus that most psychological sex differences are fairly modest.

Can SRT explain variability in sex differences? Wood and Eagly argue, quite rightly, that any complete theory of sex differences must explain the variability we see in the magnitude of these differences across cultures and historical epochs. SRT, they suggest, does exactly that. According to the theory, sex differences and similarities are products of the social roles of the society in which they're found. The more differentiated these roles are by sex, the larger the differences will be. This leads to two sets of predictions. First, in nations with high levels of gender equity, most sex differences will be smaller than they are in nations with low levels of gender equity. Second, as women's status increased in the West over the last century or so, sex differences will have shrunk. Wood and Eagly (2012, this issue) summarized various lines of evidence consistent with these predictions.

Once again, we accept that social roles have an impact on the minds of men and women, and thus on the magnitude of any sex differences. We are not convinced, however, that SRT provides a complete and comprehensive explanation for the pattern of similarities and differences across cultures and times. As we noted in the target article, there is evidence that some sex differences are actually *larger* in societies with greater gender equity, rather than smaller (Schmitt, 2012; although see Harris, this issue; Wood & Eagly, 2012, this issue). Even leaving this aside, though, the evidence that Wood and Eagly cite does not definitively settle the question in favor of SRT. Consider, for instance, the fact that, as women's status has increased in the Western world, women have become more agentic and sexually adventurous, and thus that the sex differences in these domains have become smaller (Peterson & Hyde, 2010; Wood & Eagly, 2012). This is consistent both with a pure SRT explanation *and* with an EP/SRT hybrid explanation. The EP/SRT hybrid explanation (which we favor) would be that women in the West 50 or 100 years ago were less agentic and less sexually adventurous *because their natural tendencies were being suppressed*. As society has become freer, these tendencies have emerged to a greater extent, and thus women are now more similar to men in these traits. The pure SRT explanation would be that women today and women 50 or 100 years ago were simply socialized into different roles and that neither role went against the grain any more or any less than the other—after all, within the framework of SRT, there *is* no grain; all the normal-range variation that we see is attributable to the social roles of the period. As we've made clear, the first option seems much more plausible to us. However, the point of the example is to show that evidence of smaller sex differences in more gender-equitable societies or historical epochs does not provide unambiguous support for SRT, as it fails to rule out significant evolved psychological differences.

In sum, Wood and Eagly (this issue) argued that we misrepresented their position; proffer an outdated, one-sided view of sex differences; and fail to explain important data. We argue that this has things precisely back to front: *They* misrepresented *our* position; they proffer an outdated, one-sided view of sex differences;

and they fail to explain important data. Our final verdict on SRT is that, for some traits, social roles may exert a significant influence on the magnitude of sex differences, especially in overt behavior. However, social roles are merely one influence among many, and SRT cannot replace EP as an explanation for the sex differences we share with other animals.

Attachment Fertility Theory

Another challenge to our EP perspective came from L. C. Miller et al. (this issue). The topic of their piece was Attachment Fertility Theory (AFT; L. C. Miller & Fishkin, 1997; L. C. Miller, Putcha-Bhagavatula, & Pedersen, 2002; Pedersen, Putcha-Bhagavatula, & Miller, 2011). As the name suggests, AFT is a theory of human mating psychology based principally on attachment theory. It has several things in common with our approach. Specifically, it stresses the centrality of pair bonding and biparental care, and it argues that EP sometimes overstates the sex differences in human sexuality. As will soon become clear, however, there are also fundamental differences between the two approaches, and we argue that, on balance, AFT is not a credible theory. In this section, we address several issues raised by Miller et al.'s commentary, including the claim that we misconstrued their theory, that there are negligible or no evolved sex differences in short-term mating inclinations, and that recent research on the sexual practices of men who have sex with men provides unique support for AFT. Before going any further, though, we need to address a peculiar allegation in Miller et al.'s commentary, namely, that we took an argument from one of their publications but failed to give them proper credit for it.

Credit where credit's due? In the target article, we summarized several lines of physiological evidence suggesting that human beings are not an exclusively promiscuous species, and that we probably had comparatively low levels of sperm competition throughout our evolutionary history (for reviews, see Dixson, 1998; Gray & Anderson, 2010). In an earlier article, L. C. Miller et al. (2002) surveyed some of the same research and reached a similar (though not identical) conclusion. In that article, they provided some useful data on relative testis size in humans and various other primates. We used those data in our article, and cited their article accordingly (along with three other AFT articles). Apparently, though, Miller and colleagues think they deserve more credit than this. In their commentary, they say the following:

> Stewart-Williams and Thomas's position and review covers very similar ground; in fact, they use the calculations for relative testicle size (we computed) that were part of our earlier review (the reader will note the citation for that—but not the argument—in the footnotes). (p. 214)

This has the clear implication that we simply rewrote their summary of the research and cribbed their argument. This allegation is false. First, we actually made a rather different argument than Miller et al. Our argument was that, although humans are adapted to engage in some short-term mating, the physiological evidence shows that this is not our *primary* evolved mating pattern. They, in contrast, argued that humans are not adapted for short-term mating at all—that according to primatologists, we are "classified as long-term and not short-term maters" (L. C. Miller et al., this issue, p. 213). Second, the basic argument is not original with L. C. Miller et al. (2002). Evolutionary biologists have debated for many decades whether human physiological features place us among primates with a long-term or a short-term mating system (Dixson, 1998; Gomendio, Harcourt, & Roldán, 1998; Harcourt, Harvey, Larson, & Short, 1981; Short, 1979). L. C. Miller et al. (2002) simply provided a brief summary (three paragraphs) of one position in this debate (pp. 88–89). This does not make the argument theirs. Third, we dispute the claim that we covered "very similar ground" to them, at least in any sense that implies wrongdoing. Admittedly, we and they both cite Dixson (1998) and Nunn, Gittleman, and Antonovics (2000). But that's not because we regurgitated Miller et al.'s three-paragraph overview; it's because we and they were working from the same primary literature, and these are seminal publications in the area. Note also that we cited many studies that Miller et al. did not cite (e.g., Gomendio et al., 1998; G. F. Miller, Tybur, & Jordan, 2007; Short, 1979; Thornhill & Gangestad, 2008). There's a simple explanation for the fact that we didn't credit Miller with the argument: We didn't get the argument from Miller. The allegation is false.

Has AFT said it all before? With that out of the way, let's move on to other matters. A common theme in Miller et al.'s commentary is that AFT has already staked out the position we argued for in the target article. The authors note, for instance, that "We find ourselves in general agreement with the above basic arguments. But, then again, as just suggested, many of these and/or related arguments were made earlier in AFT" (p. 212). We agree that there are several areas of overlap between our work and theirs. For instance, AFT proposes, as do we, that human beings evolved to engage in high levels of pair bonding and biparental care and that we are less sexually dimorphic than EP sometimes implies. We suggest, however, that the overlap exists almost entirely at this very general level of analysis. When we look at the nitty-gritty of the two approaches, there are large and important differences between them.

One difference concerns the theoretical basis of the two positions. AFT's approach to sex differences is grounded in attachment theory (see L. C. Miller et al., 2002, p. 89). Ours, in contrast, is grounded in Trivers's parental investment theory. Although these two theories are potentially reconcilable (Eastwick, this issue), AFT theorists have not attempted to broker a reconciliation. Indeed, they appear to eschew parental investment theory, suggesting (falsely, in our view) that "Primatologists have argued that Trivers's theory does not apply well to primates" (L. C. Miller et al., 2002, p. 89). Thus, the theoretical underpinnings of the two approaches are fundamentally different.

A second difference, which we discuss in more detail later, relates to AFT's suggestion that short-term mating is a by-product of the human pair bonding system rather than an adaptation in and of itself (as EP holds). As L. C. Miller, Pedersen, and Putcha-Bhagavatula (2005) put it, "AFT argues for universal, sex-similar, evolved mechanisms leading up to and affording pair-bonding. These could also quite naturally . . . produce short-term and other types of dating *as by-products*" (p. 290, emphasis added). This view of short-term mating is arguably what distinguishes AFT most clearly from other evolutionary theories of human sexuality. After all, most other theories argue, like AFT, that human beings have evolved to form long-term pair bonds (see Schmitt, Shackelford, & Buss, 2001, p. 231). It is AFT's stance on *short*-term mating that differentiates it from other evolutionary approaches, our own included.

Finally, although both we and Miller et al. suggest that sex differences in sexuality are smaller than EP often implies, our evidence and arguments for this conclusion are largely distinct from theirs (see, e.g., the effect size and tail-of-the-distribution arguments; our dissection of particular studies such as Clark and Hatfield, 1989). Moreover, AFT takes the whole line of argument much further than we do, positing not merely modest sex differences but negligible or even no differences. As we discuss below, this is no longer a credible position. In sum, L. C. Miller et al. (this issue) suggest that we "make arguments very much like those made previously by AFT" (p. 219). However, they overstate the similarities between our approach and theirs, and thus they overstate the extent to which they have already put forward the position we defend.

If we misrepresented AFT, so did its authors. Next, Miller et al. argue that we misrepresented their theory in a number of ways. They quote us as saying, for instance, that according to AFT, "long-term pair bonding is our solitary evolved mating pattern" and that "short-term mating is merely a non-adaptive or maladaptive by-product of these mechanisms operating in evolutionarily-novel conditions" (p. 151). They agree with none of this. They argue, for instance, that contrary to the view that long-term pair bonding is our solitary evolved mating pattern, AFT can explain "not only pair-bonding as a mating outcome, but every other type of mating outcome" (p. 217), including various forms of short-term mating. They also point out that they never suggested that short-term mating is nonadaptive or maladaptive. On the contrary, they maintain that it can sometimes be adaptive for individuals, perhaps functioning as a "secondary alternative adaptive strategy" (p. 215).

We have considered these objections carefully, and for the most part we stand by our initial description. To some extent, we and Miller et al. may be talking at cross-purposes. So, for instance, when we suggested that, according to AFT, long-term mating is humans' solitary evolved mating pattern, we were not suggesting that AFT had no explanation for short-term mating. Indeed, we *outlined* their explanation in the target article. Our point was that, according to AFT, short-term mating is a *by-product*, rather than an adaptation in and of itself, and thus that long-term mating is the one mating outcome that the human attachment system has been *specifically designed* to produce (see, e.g., L. C. Miller & Fishkin, 1997, pp. 202, 228; L. C. Miller et al., 2005, p. 290). As for our suggestion that, according to AFT, short-term mating is nonadaptive or maladaptive, that is a simple implication of the claim that it is a by-product rather than an adaptation. AFT theorists might not use the terms "non-adaptive" or "maladaptive" themselves; however, that is what is implied by their use of the evolutionary biologists' term "by-product."

But what about the claim that, rather than being maladaptive or nonadaptive, short-term mating can actually be an adaptive alternative strategy for some individuals? This, we suggest, points to an area of confusion within AFT. First, it is not clear that the adaptive-strategy hypothesis is consistent with AFT's claim that short-term mating is "a 'fallout' of a failure to interface with human's adapted for social environment (e.g., responsive paternal and maternal caregivers)" (L. C. Miller & Fishkin, 1997, p. 228). More important, though, it is not exactly clear what Miller and colleagues mean by "adaptive." Do they mean that short-term mating is adaptive in the strict technical sense used by biologists—in other words, that engaging in short-term mating led to higher average number of grandchildren in ancestral environments? Or do they just mean that it is adaptive in the everyday sense of the word—for example, emotionally or psychologically beneficial? If they mean the latter, then that does not contradict our assertion that, within the AFT framework, short-term mating is a nonadaptive or maladaptive by-product; a trait can be adaptive in the everyday sense without being a biological adaptation. However, if they mean the former—that short-term mating is a distinct secondary strategy crafted by natural selection—then that would contradict *their* claim that short-term mating is a by-product: an "emergent outcome" of an attachment system designed, ideally, to produce long-term relationships. It would imply that humans, rather than being long-term maters (as AFT has long claimed), instead have a variable mating system incorporating both long-term and short-term mating—the standard EP position (Buss & Schmitt, 1993). Thus, assuming that Miller et al. are using the terms "adaptive" and "by-product" correctly, there is a deep contradiction within AFT.

Are there sex differences in short-term mating? As noted, one of the key distinctions between AFT and EP relates to their stance on short-term mating. AFT claims (or usually claims) that short-term mating is a by-product of sex-similar long-term attachment mechanisms. It is not an adaptation in and of itself but instead "falls out" of the "system of mechanisms leading up to, supporting, and enabling" pair-bonding (L. C. Miller et al., this issue, p. 215). In the target article, we laid out an argument against this view. It goes like this. There is good evidence that the sex difference in short-term mate seeking is larger than the difference in long-term seeking (Buss & Schmitt, 1993; Kenrick et al., 1993). This pattern is even reported in at least one AFT publication (L. C. Miller & Fishkin, 1997, p. 225, Figure 8.4). But the pattern poses a unique difficulty for AFT. If short-term mating is merely a by-product of sex-similar attachment systems, why is the sex difference in short-term mating larger than the difference in long-term mating? AFT has a hard time answering this question. EP, in contrast, explains it effortlessly in terms of average discrepancies in parental investment in our ancestral past. This, we argue, provides a good reason to favor EP over AFT.

L. C. Miller et al.'s (this issue) main response to this challenge was to deny that there is anything to explain: The difference isn't really there. "AFT," they note, "argues that there is not credible evidence that humans evolved sex-distinct short-term mating mechanisms" (p. 214). If by "sex-distinct" mechanisms, they mean two separate mechanisms, one found in men and one found in women, then we agree—and so would all competent evolutionary psychologists. But if they're also denying that there is an *average* difference in interest in casual sex, then we strongly disagree—and so does almost all the research on this topic. There is now a substantial body of work demonstrating this sex difference in diverse samples across multiple nations (e.g., Buss & Schmitt, 1993; Li & Kenrick, 2006; Schmitt, 2005; Schmitt & 118 Members of the International Sexuality Description Project, 2003). Given the imposing weight of this research, and its consistency with the pattern observed elsewhere in the animal kingdom, we would need a very strong reason indeed to reject this finding.

Miller et al. cite various studies they suggest provide just such a reason. This includes their own research on the topic (L. C. Miller & Fishkin, 1997; Pedersen, Miller, Putcha-Bhagavatula, & Yang, 2002; Pedersen et al., 2011). Most of this is aimed at debunking Buss and Schmitt's (1993) number-of-partners-desired measure (i.e., "How many different partners would you ideally like to have sexual intercourse with in the next 1 month, 1 year, 30 years, etc.?") Miller and colleagues argue that the data from this measure are positively skewed, and thus that the appropriate index of central tendency is the median rather than the mean. When comparing medians, however, there are typically no differences in the number of sexual partners desired by women versus men—at least when using the "appropriate" statistical analyses (L. C. Miller & Fishkin, 1997; Pedersen et al., 2002; Pedersen et al., 2011). There are several things to say about this. First, the number-of-partners-desired measure is only one measure among many examining men and women's propensity to engage in casual sex and seek sexual variety. Some of these measures, such as the Sociosexual Orientation Inventory–Revised, produce data that are not especially skewed, but nonetheless detect the usual sex difference (Penke & Asendorpf, 2008). Second, Schmitt and 118 Members of the International Sexuality Description Project (2003) conducted a large-scale, cross-national study using the number-of-partners-desired measure. Whereas AFT studies typically involve several hundred American undergraduates, this study included more than 16,000 participants from more than 50 nations. To deal with the potential problems associated with the use of means, Schmitt and colleagues analyzed the data using various nonparametric tests. As hypothesized, they found the standard sex difference in every one of their 10 major world regions.

Overall, attempts by Miller and others to make the sex difference in short-term mating go away have been unsuccessful (see also Conley, Moors, Matsick, Ziegler, & Valentine, 2011, and response by Schmitt et al., 2012). One might argue that their continued efforts to do so make them examples of what Kenrick (this issue) called "the social science equivalent of climate-change deniers" (p. 204).

Do MSM seeking UAI support AFT over EP? Finally, L. C. Miller et al. (this issue) presented new data which they suggest support AFT over its rivals. The data came from a large sample of men who have sex with men (MSM), all of whom were currently in long-term relationships. There were two key findings: First, men who were less emotionally close to their primary partners had more casual sex partners; and second, these men engaged in higher rates of unprotected anal intercourse (UAI). According to Miller et al., this supports their view that humans are long-term pair bonders and that short-term mating occurs mainly in the context of faltering pair bonds.

The findings are interesting and potentially very useful, and we agree that they are consistent with AFT. However, we don't believe that they are *uniquely* consistent with AFT. They fit equally well with an EP/MMC perspective. In his fascinating commentary, Eastwick (this issue) discusses research on the *derogation of alternatives* (see, e.g., Maner, Rouby, & Gonzaga, 2008; Simpson, Gangestad, & Lerma, 1990). This research shows that when people are involved in a strong pair-bonded relationship, they often find other potential romantic partners less alluring. This applies not only to women but to men as well. According to Eastwick, the pattern suggests that men's short-term mating psychology is often deactivated in the context of a strong pair bond, which he observes fits well with the MMC model. The data that Miller and colleagues present documents a related phenomenon: MSM involved in strong pair bonds have fewer extracurricular partners. This is consistent with the AFT view that short-term mating is a by-product. However, it is equally consistent with the view that short-term mating is an adaptation, but one that recedes into the background when people are involved in a long-term relationship. Thus, Miller et al.'s data do not support AFT over other theories of human mating psychology. Taken together with the fact that AFT struggles to explain well-documented sex differences in sexual psychology, we suggest that this theory should probably be given little credence.

On the Teaching of Evolutionary Psychology

> At this basic pedagogical level, [Stewart-Williams and Thomas] are correct that EP over-emphasizes MCFC logic and sex differences.
>
> —G. F. Miller (this issue, p. 207).

> Scientists carrying out primary empirical research—and other academics reviewing and discussing this work—should probably strive for greater discipline when describing sex differences, particularly in communication with lay audiences.
>
> —Pound and Price (this issue, p. 228).

Before wrapping up, we would like to say a few words about something close to our hearts: the teaching of EP. In the target article, we argued that the data gathered by evolutionary psychologists generally show small sex differences but that the verbal descriptions of these data often suggest that the differences are large. By the time the research bubbles up to the level of popular discourse, the imagined differences have grown larger still. This happens for a variety of reasons. As several commentators noted, people have a natural tendency to polarize the differences: to view a difference in two shades of gray as a black-and-white difference (Kenrick; Pound & Price). On top of that, the media often pounces on any sex differences that EP happens to uncover, and qualifications, nuances, and sex similarities are invariably lost in the process (Buss; Kenrick; Pham et al.; Pound & Price). This all raises the question of how we can present and teach evolutionary psychological theories and findings in a way that minimizes the misunderstandings. Pound and Price made one proposal in their commentary; they suggested that "One helpful antidote to these effects may be, as suggested in the target article, comparative analyses which demonstrate that many human sex differences are in fact relatively small, compared to other species and to our own perceptions and expectations" (p. 227). Below, we suggest three additional tactics for presenting EP research on sex differences in a way we hope lessens the chances of inflating these differences in the minds of students, nonspecialist audiences, and perhaps even specialist audiences as well.

Avoiding Focusing Excessively on Differences

Imagine that a zoologist from Mars was sent to Earth to study elephants, and that it had never seen one before. Its initial observation upon seeing a herd of elephants for the first time would presumably *not* be: "Wow! On average, the males are somewhat larger than the females!" It would be: "Wow! Those are large animals!" A follow-up observation would be the average sex difference in size. However, this would be a qualification to the initial observation—a peripheral rather than a central claim about the morphology of elephants. If, in its subsequent report, the Martian zoologist began by highlighting the sex difference and barely mentioned that elephants are, first and foremost, large animals, we should not be surprised if other Martians got the wrong idea.

The distinction between central and peripheral claims is applicable to many aspects of human sexuality. Consider, for instance, the emotion of jealousy. Evolutionary psychologists place a strong emphasis on sex differences in this domain. The standard claim is that men are more worried by a partner's sexual infidelity than emotional infidelity, whereas women are more worried by a partner's emotional infidelity (e.g., Buunk, Angleitner, Oubaid, & Buss, 1996, p. 139). As Harris (this issue) observed, however, such claims overstate the actual differences. In studies employing a forced-choice decision—for example, "Would you be more upset by a partner's sexual infidelity or emotional infidelity?"—men are usually quite evenly split in which option they choose (see, e.g., Buss, Larsen, Westen, & Semmelroth, 1992; Buss, Shackelford, & Kirkpatrick, 1999; Kuhle, 2011). Furthermore, despite any small average sex differences in this area

(Pietrzak, Laird, Stevens, & Thompson, 2002; Sagarin et al., 2012), the overwhelming trend is that most men and most women are extremely upset by both sexual *and* emotional infidelity (Lishner, Nguyen, Stocks, & Zillmer, 2008). This suggests that the central EP claim regarding jealousy should be "Human beings evolved to experience jealousy in romantic relationships" rather than "Men and women evolved different patterns of jealousy." The latter statement is true but should be considered a qualification to the former: a peripheral rather than a central claim. To stress the sex difference alone would be like observing that male elephants are bigger than females while steadfastly neglecting to mention that all adult elephants are large compared to most terrestrial animals. It would almost certainly foster an inaccurate view.

A similar analysis applies within the realm of mate preferences. Several commentators pointed out that sex differences in human mate preferences are generally quite small (Eastwick, this issue; Eastwick, Luchies, Finkel, & Hunt, in press; Snowdon, this issue). Consider, for instance, the preference for physical attractiveness in a mate. In his international study of mate preferences, Buss et al. (1990) had respondents rate the importance of a mate's looks on a 0-to-3 scale, with the anchors *irrelevant*, *desirable*, *important*, and *indispensible*. Collapsing across nations, the average for both sexes fell between *desirable* and *important*. The male average was close to *important*, whereas the female average was right in the middle, leaving less than half a point difference between the averages for each sex. Likewise, a multinational survey of more than 200,000 men and women revealed only a moderate sex difference in the preference for physical attractiveness and a small-to-moderate difference in the preference for facial attractiveness (Lippa, 2007). It seems, then, that the sex difference in the preference for good looks is rather modest (although see Kenrick, this issue; Li, Bailey, & Kenrick, 2002). As such, the central claim in EP should probably be "Human beings evolved to put a fair amount of weight on good looks in a mate" rather than "Men evolved to put more weight on good looks than women." Again, the latter statement is true but potentially misleading. This sounds like a contradiction, but it is not; the statement is misleading *if* it is given undue weight.

In addition to distinguishing central and peripheral claims, it should always be emphasized that there are important areas where sex differences in sexual psychology are trivial or nonexistent. So, for instance, of the 18 traits that Buss and colleagues (1990) examined in their cross-national investigation of mate preferences, the traits that people considered most important in a mate were mutual love, dependability, emotional stability/maturity, pleasing disposition, good health, education/intelligence, sociability, and desire for home and children. Most of these showed no consistent sex differences. Similarly, the top-ranked traits in Lippa's (2007) study included intelligence, humor, honesty, and kindness, and the sex differences for these traits were small or negligible (see also Snowdon, this issue). In other words, when it comes to the traits we consider most important in a long-term mate, human beings are largely monomorphic. This is one of the most significant findings of these studies; however, it is easily overlooked when the discussion becomes fixated on traits that people consider less important but where sex differences are found. By shining a spotlight on these traits, we may create an inaccurate picture of our species, *even though the differences are real*. Our picture of human nature may be built on a foundation of exceptions to the rule. The rule—the fact that males and females in our species are surprisingly similar in many ways—may be relegated to the background. By taking genuine differences and then exaggerating their importance, our picture of our evolved nature may become a caricature: It may contain a recognizable grain of truth but distort its object.

Presentation Strategies

Other practices could have the same effect. For instance, a number of textbooks in EP divide long-term mating preferences into women's preferences and men's preferences (Buss, 2003, 2012; Gaulin & McBurney, 2001). A natural interpretation would be that women's preferences are those that most women possess but that most men do not, or that are important to most women but not to most men. This is not the case, however; women's preferences are simply those for which the average score for women is higher than the average score for men, and vice versa. In some cases, the trait in question may be reasonably important to most men and most women (e.g., good looks); in other cases, it may not be particularly important to either (e.g., wealth for many people in the Western world; Lippa, 2007). Taken together with the fact that the differences are generally rather modest, and the overlap between the sexes substantial, the practice of dividing mate preferences into men's and women's is likely to mislead. Indeed, given how prone people are to stereotyping and thinking in dichotomous terms, it would be surprising if it did *not* mislead people (see Kenrick, this issue; Pound & Price, this issue).

A related point is that, in the teaching and reporting of findings in EP, there is often a tendency to start by emphasizing the sex differences (perhaps because these are more interesting and attention grabbing than the similarities) and only later to mention that these differences are generally quite small. This sequencing of the material might foster an inflated impression of the differences. To give a concrete example, the authors of a widely used EP textbook noted that "Human mate choice decisions reflect the essential differences between males and females in terms of maximizing fitness; females typically concentrate on the rearing component of reproductive effort while males concentrate on the mating component" (Barrett, Dunbar, & Lycett, 2002, p. 136). They did then qualify this statement by noting that the differences are smaller in our species than in most, as a result of male parental care. However, they issued this brief qualification only after first fixing the dichotomous vision of the sexes in the readers' minds. A better pedagogical strategy might be to reverse the order of presentation: to start with the fact that, as a species with biparental care, we are relatively monomorphic in the allocation of mating versus parenting effort, and then to qualify *this* statement by noting that, because there are average sex differences in parental investment and reproductive rates, men exhibit a somewhat higher average level of mating effort.

Keeping Sight of Within-Sex Variation

Last but not least, it is important not to lose sight of individual differences. A danger in emphasizing mean values for each sex is that these values may be projected onto all or most normally developing men and women. The mean may be treated as a description of the typical group member, despite the fact that the majority of individuals fall above or below it. Psychologists do make some effort to stress that means cannot be attributed to all members of any group, as evidenced by the fact that we often append the phrase "on average" to our descriptions of mean differences. But is this enough? Consider again the robust sex difference in willingness to engage in casual sex: The mean SO score for men is higher than that for women (Lippa, 2009; Schmitt, 2005). What does this tell us, though, about *individual* men and women? It clearly does not tell us that all men are interested in casual sex and that all women are not. However, given the degree of overlap between the male and female distributions (Simpson & Gangestad, 1991), it also does not tell us that *a large majority* of men are more interested in casual sex than a large majority of women. That is, it is not accurate to say even that "men are *typically* more interested in casual sex than women, but there are of course exceptions." Here is what the data that the means are drawn from actually tell us:

> Men and women can be found at virtually every level of interest in casual sex. At the right-hand tail of the distribution, only a small number of people are strongly interested in casual sex; however, of these people, more are men than women. At the left-hand tail, only a small number of people are strongly *dis*interested in casual sex; however, of these people, more are women than men. Most people—men *and* women—fall somewhere in between. If you were to choose one man and one woman at random, it would be somewhat more likely that the man would have higher SO. However, you wouldn't want to bet your life savings on it. Around a third of the time—i.e., closer to 50% than to 0%—the woman would have higher SO. (Stewart-Williams & Thomas, this issue)

If this is not what springs immediately to mind as soon as the words "on average" are appended to a description of mean differences, then the words "on average" have not rectified the damage done by the use of means to describe populations of varied individuals. This is not to suggest, of course, that means should never be used. The point is simply that we should not lose sight of within-group variability and the fact that male and female distributions almost always overlap for psychological traits. This is especially important when addressing less statistically savvy audiences. Such audiences could perhaps be encouraged to think of two normal distributions, one representing males and the other females. Instead of imagining that natural selection creates two distinct psychological types—a male type and a female type, described by the mean values for each group—they could be encouraged to imagine that natural selection pushes the male and female distributions closer together or further apart. This simple expedient may help people to visualize the effects of natural selection on average sex differences without at the same time losing sight of the variation within each sex. Admittedly, this formulation glosses over many complications, including the fact that the distribution for males is often wider and flatter than that for females (Archer & Mehdikhani, 2003; Harris, this issue), the fact that the distributions for many traits are highly skewed (Campbell, this issue; Pedersen et al., 2002), and the fact that some portion of the variation might itself be explicable in terms of natural selection (Buss & Greiling, 1999; Campbell, this issue). Nevertheless, it is good place to start, and it may help us avoid some of the more egregious misunderstandings of what the data collected by evolutionary psychologists are telling us about ourselves and our species.

Conclusion

One last thought. In their commentary, Pham and colleagues suggested that the target article represents an unfair and inappropriate attack on EP. We hope we have shown that it represents instead a reasonable difference of opinion *within* EP. We grant, though, that in one sense, the charge of unfairness may be true. Evolutionary psychologists are not unique in making unduly strong claims about their findings. No doubt, researchers in all fields are prone to this. Why, then, pick on EP, rather than any other field or on psychology in general? The reason is that, in our view, EP represents a Great Leap Forward for psychological science. Like researchers in every field, evolutionary psychologists sometimes push their theoretical framework too far. However, evolutionary psychologists are genuinely onto something. A lot of people have noticed this and are paying attention. Thus, it's important to get it right. Now, maybe EP already has it right, and any criticism of the field is inappropriate, misguided, and unfair. But maybe not. Either way, though, perhaps we can agree that everyone involved in this debate has the same motivation: to try to make our picture of our evolved nature as accurate as possible.

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Note

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