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Human Sex Differences: Distributions Overlap but the Tails Sometimes Tell a Tale

Nicholas Pound & Michael E. Price

Department of Psychology, Brunel University, Uxbridge, United Kingdom

Correspondence: Nicholas Pound, Department of Psychology, School of Social Sciences, Brunel University, Uxbridge, UB8 3PH, UK. E-mail: nicholas.pound@brunel.ac.uk

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We agree with Stewart-Williams and Thomas (this issue) that the males-compete/females-choose (MCFC) model does seem to be accepted by some academics and members of the public as an accurate portrayal of either (a) human sex differences, or at least (b) the way in which these differences are conceptualized by evolutionary psychologists. Moreover, we also agree that both kinds of acceptance are problematic, because both are based on inaccuracies: The strict MCFC model is a misleading caricature of human sex differences that describes neither the reality of these differences nor the way in which these differences are regarded by most evolutionary psychologists carrying out primary research in this area. The large majority of such researchers assume that behaviors that are inconsistent with a strict MCFC model—such as strong male mating preferences, intrasexual female mating competition, adaptations for pair bonding, and (under many conditions) high male parental investment—are all fundamental aspects of human nature. The target article serves as a helpful corrective for anyone inclined toward believing otherwise.

In light of this, the target article has also led us to wonder, who is responsible for propagating the MCFC caricature of human sex differences, and the view that most evolutionary psychologists accept this caricature? Is it the fault of the scientists who are doing primary empirical research on sex differences, or are others responsible—such as other academics reviewing and discussing this research, members of the press, and popular science writers—for mischaracterizing this research for whatever reason? We believe that all of the above may be at fault to some extent, but here we focus mainly on how the manner in which results are reported by evolutionary psychologists might interact with people's tendencies to see the world in categorical terms so that, although scientists often report mean differences between the sexes on psychological or behavioral dimensions, these results often end up being perceived as categorical distinctions such as “females do A and males do B.”

Measuring and Reporting the Size of Sex Differences

In the target article the authors examine effect sizes for various sex differences that are widely referred to and discussed in the evolutionary psychological literature. And to examine the association between the actual size of reported differences and how they are presented to, and perceived by, a wider audience, they use a sex difference that people have an intuitive understanding of as a reference point: the sex difference in human height. As the authors suggest, this can help a general audience understand the everyday significance of reported differences. However, we feel a more systematic approach is really needed if sex differences in different domains are to be used to test hypotheses about ancestral selection pressures, human mating systems, and species-typical psychological design. Subjective

interpretation of effect sizes and ad hoc comparisons with other species cannot really substitute for formal meta-analytic and phylogenetic comparative methods, which are essential if firm conclusions are to be drawn regarding ancestral mating systems.

In general, although their approach could have been more systematic, the authors' focus on mean differences and effect sizes is entirely appropriate. Furthermore, in places, the authors rightly criticize the practice of using large differences in the preponderance of males and females seen within the tails of distributions to give the impression of huge sex differences when in fact the male and female distributions on the dimension concerned largely overlap and the mean difference between the sexes is actually extremely small. However, in some cases distribution tails can provide valuable information concerning the prevalence of certain behaviors in the population at large, which may otherwise be difficult to assess. Daly and Wilson's work using homicide rates as an "assay" of levels of interpersonal conflict in the wider population provides a compelling illustration of this point (e.g., Daly & Wilson, 1988a, 1988b).

In the target article, when it comes to the issue of sex differences in risk taking, by discounting the significance of substantial differences in the numbers of males and females seen in the distribution tail (i.e., premature deaths), the authors are urging us to discount perhaps one of the best available assays of risk taking behavior. Social desirability and other considerations make self-report measures of risk taking less than satisfactory—but some fatal illnesses and injuries can be considered to represent the "tip of the iceberg" of risky behaviors occurring throughout the population and consequently can provide an unbiased assay of the extent to which males and females engage in dangerous activities. The authors write, "Although there is a large difference in the number of men than women who die young, the sex difference in risk proneness for the vast majority of men and women is nowhere near as large" (p. 161). However, the claim regarding the sex difference for the "vast majority" of the population is not directly supported by the preceding observation. If engaging in a particular behavior carries the same risk of death for males and females, then the sex difference in mortality attributable to that behavior will likely be a good predictor of the frequency with which males and females engage in that behavior more generally.

To illustrate the point, for deaths due to "motor vehicle accidents" in the United States during the period 1999 to 2007, the average annual rates were 21.4 and 9.4 per 100,000 males and females, respectively (National Center for Health Statistics, 2010). The vast majority of journeys end safely, 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,

and not just about those unfortunate enough to be involved in a journey with a fatal outcome. In fact it likely underestimates sex differences in behavior in and around motor vehicles, as many female fatalities will arise in contexts where a male's driving behavior is a causal factor (e.g., female passengers, occupants of other cars, cyclists, pedestrians). Males need not necessarily be driving more dangerously to produce this pattern, they may just be driving more, but either way the sex difference in the distribution tail provides an assay of sex differences in behavior more generally.

Continuing with the issue of mortality, this is perhaps one area where public perceptions underestimate rather than overestimate the personal or everyday significance of sex differences, a situation that arises from focusing on one particular type of mean difference—the difference in life expectancy. When people hear that, for example, in the United States in 2007, life expectancy (at birth) was 75.4 years for males and 80.4 years for females (Arias, 2011), the 6% difference may not sound particularly significant for an individual. However, sex differences in life expectancy arise from substantial sex differences in age-specific all-cause mortality throughout life which peak in early adulthood with males aged 15 to 24 being 2.8 times more likely to die than females (National Center for Health Statistics, 2010) and fall slowly only with age (2.2 for 25–34-year-olds, 1.7 for 35–44, 1.7 for 45–54). A 6% difference in life expectancy might not sound particularly worrisome for a 20-year-old man, but being told he has a 170% greater chance of dying in the following year than a female of the same age might give him a different perspective on the nature of the sex difference in mortality. Moreover, for middle-aged men it is rather sobering (or perhaps not!) to consider the fact that by making lifestyle changes (e.g., diet, exercise) it would be difficult to achieve reductions in age-specific mortality that approach those that could be obtained by simply not being male.

Quantitative versus Qualitative Sex Differences

The target article correctly highlights the problem that research reporting mean differences in propensity to engage in particular behaviors can often be incorrectly interpreted as implying that one sex engages in a particular behavior, whereas the other does not. However, the authors do not explore some of the underlying biological mechanisms that mean that, even in domains where between-sex differences are quite substantial, these differences are likely to be quantitative differences on continuous dimensions rather than absolute categorical differences between sexes. The fact that many differences are quantitative differences in the probability of expressing, frequency of engaging in, duration of time spent carrying out, and/or ability to perform particular behaviors, rather than qualitative

distinctions between behaviors performed exclusively by one sex or the other, should not be surprising given the primary mechanisms responsible for sexual differentiation of the brain and behavior in mammals. Notwithstanding some intriguing recent findings of direct genetic effects on neural proliferation and sensitivity to hormones (see McCarthy & Arnold, 2011, for a review), there is abundant evidence that mammalian sex differences in brain and behavior depend largely on differential exposure to hormones, primarily testosterone, during critical periods of early development (for reviews, see, e.g., Arnold, 2009; Breedlove, 1994; Breedlove, Cook, & Jordan, 1999). The general picture that has emerged from empirical work carried out in various species following Phoenix, Goy, Gerall, and Young's (1959) pioneering study with guinea pigs is that the expression of many sex-typical behaviors in adulthood can vary continuously in a dose-dependent manner according to the degree of exposure (or lack of exposure) to the organizing effects of steroid hormones during critical periods of early development.

It nevertheless should be borne in mind that small differences between the sexes when measured on a particular dimension can conceal underlying differences in cognition that are more qualitative in nature. Take, for example, the well-documented sex difference in mental rotation ability that is observed across age ranges (Maylor et al., 2007) and across cultures (Silverman, Choi, & Peters, 2007; Silverman, Phillips, & Silverman, 1996). Using data from 40 countries, Silverman et al. (2007) found an effect size of $d = 0.48$ with males outperforming females on tests of three-dimensional mental rotation ability, whereas other meta-analyses have reported higher effect sizes using less culturally diverse samples— $d = 0.73$ (Linn & Petersen, 1985), $d = 0.56$ (Voyer, Voyer, & Bryden, 1995). Mental rotation test scores can be based on response accuracy, but mental rotation test speed is also often used as a dependent variable. In either case, references to sex differences in this domain often assume that males and females are attempting to do the same thing but that males are doing it, on average, faster and/or more accurately. However, different participants may use different solution strategies to complete mental rotation tasks and these distinct strategies may depend on fundamentally different cognitive mechanisms but yield only small differences in overall task performance.

Mental rotation tests are most typically thought of as testing the ability to mentally rotate a visual representation of an object in the mind, but participants may alternatively use analytic or verbal feature comparison approaches that depend on reasoning rather than mental manipulation of objects (e.g., Burin, Delgado, & Prieto, 2000; Gieser, Lehmann, & Eid, 2006; Schultz, 1991). In theory, relatively small sex differences in mean accuracy or

response time could arise from males and females preferentially using these fundamentally different strategies to solve the same problem. There is some evidence that males are more likely to use a nonverbal rotating strategy and that females are more likely to use an analytic approach (Gieser et al., 2006; Peters et al., 1995; Raabe, Hoger, & Delius, 2006), but other researchers have found no association between sex and strategy choice (Burin et al., 2000; Schultz, 1991). However, whether there is a sex difference in strategy choice for this particular task the point remains valid in principle—small quantitative differences between individuals or sexes on a particular continuous dimension may arise as a result of substantial qualitative differences in the cognitive processes underlying performance of the behavior or task in question.

Objective Measurement Versus Subjective Assessment of Sex Differences

Whatever the true magnitude of sex differences when quantified objectively, and the extent to which academics, popular science writers and the media might exaggerate or caricature these differences, there remains an additional factor influencing how they are perceived that is not dealt with directly by the authors of the target article. This is the human tendency to categorize the world (see, e.g., Anderson, 1991; Goldstone & Hendrickson, 2009; Rosch, Mervis, Gray, Johnson, & Boyes-Braem, 1976), which may lead people, academics and lay individuals alike, to view variation along behavioral and psychological dimensions as discontinuous and sex differences as qualitative. For any kind of difference between any two entities to be perceived, there must be a mechanism of perception that is sensitive enough to detect the difference, and the more sensitive this mechanism, the larger the difference will seem to the perceiver. Therefore, if the issue of whether a species' sex differences are "large" or "small" is considered as a matter of within-species subjective assessment (as opposed to a matter that can be resolved only via some kind of comparative analysis, such as cross-species comparison of morphological or behavioural differences), then we must consider how members of that species tend to perceive sex differences.

Consider human facial differences, that is, differences between individuals in the overall shape of, and configuration of features within, their faces. If we ask whether these facial differences tend to be large or small, it becomes clear that the answer depends on whether one possesses a cognitive system that is specialized for detecting them. Those with impairments to this system (i.e., individuals with certain forms of prosopagnosia) perceive these differences to be so slight as to make it impossible for them to recognize even close friends and family members based on facial appearance alone, whereas those who possess this system (i.e., normally developing human beings) perceive these differences to be so

large as to make facial recognition easy and instantaneous (Duchaine & Nakayama, 2006). Why are normally developing people equipped with a cognitive system designed to detect “small” facial differences? Probably because the system is a biological adaptation (Duchaine, Cosmides, & Tooby, 2001); ancestral humans needed to tell apart their kin, mates, friends, and enemies. The facial recognition system may inflate the subjective size of facial differences because the ability to tell faces apart is an extremely useful skill to have in negotiating one’s social world.

We are not claiming that people are necessarily biologically adapted to subjectively exaggerate the perceived size of sex differences reported by evolutionary psychologists, in the manner that they do with facial differences experienced in day-to-day life. However, if the general principle of “notice differences more when it seems more useful to do so” applies to the perception of sex differences, as it does to face recognition, then this may help explain why many people (both within and outside of academia) seem inclined to perceive human sex differences as large, even when these differences are, by more objective/comparative standards, actually quite small. In most human societies, men and women are constantly interacting, depending on one another, and impacting one another’s welfare. In such social landscapes, men and women must continuously attempt to read each other’s minds, understand each other’s motives, and predict each other’s actions. “Small” differences may become magnified in these contexts, because an understanding of differences may be more useful than an understanding of similarities for trying to predict behavior that would otherwise be unexpected or surprising (i.e., behavior that varies from that which one would expect from one’s self, or from a member of one’s own sex). Any cognitive process that subjectively emphasized sex differences may thus have some social utility to the user. However, such a process could also lead to stereotyping (Macrae, Milne, & Bodenhausen, 1994), with its myriad potentially negative effects, which may include increased intellectual receptivity to models such as MCFC, which misleadingly caricature sex differences. 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.

Sex Differences in Psychology versus Sex Differences in Observed Behavior

As noted previously, small differences in behavior on particular dimensions may conceal quite substantial differences in the fundamental psychological processes underlying the expression of the behavior in question. This is a concern when it comes to assessing the significance of sex differences in performance on laboratory tasks, but it is also a concern

when trying to interpret patterns of behavior observed in contemporary societies and/or thought to have been typical of ancestral human communities. In the target article the authors take issue with the claim that humans are a polygynous species by noting that although a large majority of societies are classified as polygynous (i.e., are characterized by some degree of polygynous mating), even within these societies, most marriages tend to be monogamous. In about three fourths of “polygynous” societies, Stewart-Williams and Thomas note, fewer than 20% of men are actually married polygynously. We agree that researchers should refrain from claiming that humans are primarily a polygynous species and believe that human mating is best conceptualized as strategically pluralistic (Gangestad & Simpson, 2000). That is, no single mating strategy was always most adaptive under all ancestral conditions, and humans have facultative adaptations for a variety of short-term and long-term strategies. Stewart-Williams and Thomas suggest that the theories of strategic pluralism and mutual mate choice (MMC) are not necessarily incompatible, and we agree. However, at some points they do seem to lean in the direction of suggesting that human nature is “mostly” monogamous, for example, when they state that “[the theory of strategic pluralism] does not undermine our position, as our position requires only that, among our varied mating options, pair bonding has tended to predominate” and “our evolved sexual nature has been shaped more by pair bonding than by harem polygyny” (p. 150).

We do not dispute that monogamy has been the most common type of long-term relationship in human evolutionary history, or that people have adaptations for pair bonding. However, neither of these points implies much of anything about the extent to which people could also be adapted for nonmonogamous mating strategies. In considering the extent to which men are adapted for polygynous mating, for example, we must consider not just how common polygyny was ancestrally but also how elusive it was. There is evidence to suggest that polygyny is relatively rare in small-scale societies not because men in these societies do not strive for it but because it is so difficult to achieve. It is difficult because usually only very high status men can attract multiple wives and support large polygynous families, and because already-married women often object to their husbands taking additional wives (Chagnon, 1979; Lee, 1993; Marlowe, 2004). The elusiveness of polygyny, however, should not necessarily make men less inclined to aspire to it; even in hunter gatherer societies with very low rates of polygyny such as the Ju/'hoansi and Hadza (4–5% of men married polygynously), 65% of men approve of a man having two wives (Marlowe, 2004) and it is “the express desire of men to take a second wife” (Lee, 1993, p. 85).

It is plausible that the ancestral elusiveness of polygyny could actually have made men more adapted, rather than less adapted, to strive for it. The strength of an evolved desire to achieve any goal may relate positively to both the fitness value and the elusiveness of the goal in ancestral environments; when a reproductive reward is harder to achieve, greater motivation is required in order to achieve it. Thus, in theory, 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 (Nesse & Williams, 1994). Given that polygyny was both elusive and reproductively rewarding to ancestral men, they may have evolved strong desires to achieve it. These desires could be facultative and less likely to be expressed in environments in which there are higher social penalties for pursuing polygyny (e.g., societies with socially-imposed monogamy; Alexander, 1987). Nevertheless, it is likely that contrary to what Stewart-Williams and Thomas suggest, the proliferation of polygyny that is observed when ecological and social restrictions against it are lifted—such as the large harems maintained by despots in all the world’s earliest civilizations (Betzig, 1986)—is evidence of something more than just the idiosyncratic preferences of men who are unusually high in “polygynous inclination.”

The MCFC versus MMC Dichotomy

The authors, with some justification, argue that evolutionary psychology could benefit from a stronger focus on MMC as a model for human mating systems.

They note that human males appear to be somewhat unusual among mammals in that they exhibit strong mate preferences for various anatomical and psychological traits in females. However, in doing so the authors do not fully acknowledge how these very findings demonstrate the extent to which MMC is already a major focus within evolutionary psychology. A quick search of Scopus for records featuring “evolutionary psychology” reveals 293 results also containing “female attractiveness” and only 142 containing “male attractiveness.” Other noteworthy counts within those results featuring the term “evolutionary psychology” include “female mate choice” ($n = 94$), “male mate choice” ($n = 48$), “female facial attractiveness” ($n = 110$), “male facial attractiveness” ($n = 224$), “waist-hip-ratio” or “WHR” ($n = 159$), and 34 results for “mutual mate choice” itself. Obviously this is a cursory and incomplete review of the state of research in the field, but it does seem to indicate that there is in fact a rather substantial amount of research being conducted on male mating preferences by researchers in a field which, the authors of the target article

argue, is focusing too much on the MCFC model and consequently neglecting the role of mutual choice.

The fact that studies of male mating preferences have proceeded in parallel with investigations of female choice demonstrates that evolutionary psychologists have long acknowledged the role of mutual choice in our species, if only implicitly at times. Across the animal kingdom, however, mutual choice is rather rare but is to be expected in species where the benefits of biparental care are high (Kokko & Johnstone, 2002). From this perspective, MMC in humans is perhaps not so surprising. However, although sexual monomorphism may be associated with mutual choice, there are problems with simply inferring MMC from lack of pronounced dimorphisms. Intense male–male competition does not always lead to the evolution of substantial size dimorphism (Dunham & Rudolf, 2009) with both genetic constraints (Lande, 1980) and socioecological mechanisms (Lawler, 1980) being able to hinder its development. Moreover, it is also difficult to exclude a role for mutual choice even in extremely dimorphic species, because male mate choice may be cryptic (Kokko & Johnstone, 2002).

One possible way to reconcile the abundant evidence for intense intrasexual competition between human males in some domains (MCFC) with the accumulating evidence that males do have strong mate preferences and therefore participate in mutual choice (MMC) is to consider the two processes as somewhat complementary. Kokko and Johnstone (2002) have shown that as one sex becomes choosier, this tends to lead to the evolution of reduced choosiness in the other. This is because, for example, increased female choosiness leads to a reduction in the average mating rate for males, so they can no longer afford to be so choosy themselves. However, where female choice is consistent (i.e., males are chosen on the basis of consensually desired characteristics), then selection can favor the retention of male choosiness because the reduction in mating rate does not affect the most highly desired males to the same extent as less desired males. Consequently, if females consistently prefer males who demonstrate success in intrasexual competition (e.g., through acquisition of social status and wealth), male choosiness might still be maintained because of the benefits that preferred males can derive from being choosy, even if it might not seem to be beneficial to less successful males. This might be particularly likely to be the case where male preferences can be facultative in their expression, and there is recent evidence suggesting that men possessing desirable characteristics are more choosy (Burriss, Welling, & Puts, 2011; Price, Pound, Dunn, Hopkins, & Kang, 2013). So instead of thinking of humans as being either an MCFC or MMC species, a better conceptualization might be MCFC > MMC.

That is, males compete, in various domains both behavioural and physical, for the opportunity to participate in mutual mate choice with females.

Concluding Remarks

In light of some of the issues highlighted by Stewart-Williams and Thomas, and others that we have just discussed, 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. However, despite the best intentions it may be difficult to discourage certain ways of thinking about sex differences. In their target article, Stewart-Williams and Thomas do not address the role of media coverage in creating and propagating caricatured and inaccurate views of research from an evolutionary perspective—not just research on sex differences in particular, but also on human psychology and behavior more generally. Media coverage of evolutionary psychology, in the United Kingdom at least, appears to differ in important ways from coverage of research in other areas of science. A quantitative analysis of U.K. print media coverage of evolutionary psychology during the 1990s carried out by Cassidy (2005) found that, in contrast to findings from other areas of science, coverage of evolutionary psychology was most frequently found in “features” material in newspaper supplements, or in opinion columns, rather than receiving coverage as news or in science supplements. Moreover, only a small proportion of the coverage was being written by specialist science journalists. With cognitive biases toward categorization, it is difficult enough for scientists to avoid thinking of small mean differences on continuous dimensions as implying categorical distinctions between male and female behavior. Once research findings have then been redescribed by nonspecialist journalists and then reinterpreted by a lay audience—both with similar biases—it isn’t hard to see how a small difference on a continuous measure could become more widely perceived as “females do A and males do B.”

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