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ORIGINAL ARTICLE

Shifts in Masculinity Preferences Across the Menstrual Cycle: Still Not There

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Abstract Harris (2011) failed to find support for the popular hypothesis that women are attracted to masculine-faced men when conception is likely but attracted to feminine-faced men during other menstrual cycle phases. In response, DeBruine et al. (2010) wrote a commentary criticizing Harris theoretical analysis and data (e.g., sample age). The current paper addresses those criticisms with new data analysis, additional literature review, and logical arguments. Harris' results are not attributable to her sample's age; no preference shift was found for the subsample of women under 30 years old and no hint of an interaction existed between participant age group and menstrual cycle phase. This work also revisits the questionable assumptions inherent in the cycle shift hypothesis and reviews literature that suggests such assumptions are not tenable.

Keywords Attractiveness · Evolutionary theory · Menstrual cycle shifts · Masculinity preferences · Ovulatory cycle · Infidelity

Introduction

One evolutionary psychology hypothesis that has received enormous attention in both popular press and academic outlets proposes that women's preference for different types of men changes across the menstrual cycle (e.g., Penton-Voak et al. 1999; Penton-Voak and Perrett 2000; Little et al. 2002). Advocates of this view posit that women are wired up to engage in infidelity with particular types of men—for example, those high in masculinity, symmetry, and testosterone—and that such infidelities primarily take place during the fertile phase of the menstrual cycle. The current paper focuses particularly on one instantiation of this menstrual cycle shift hypothesis, namely, that women's attraction to men with masculine faces changes across the menstrual cycle.

According to the cycle shift hypothesis, women have an evolved psychological mechanism that leads them to find masculinized faces more attractive during the phase of peak fertility but to find feminized faces more attractive during the rest of the menstrual cycle (e.g., Penton-Voak et al. 1999; Penton-Voak and Perrett 2000). The theory offered by evolutionary psychologists working in this area to account for purported menstrual cycle effects is that such preference shifts evolved because of some type of fitness trade-off between choosing mates with more masculinized faces relative to those with more feminized faces (e.g., Penton-Voak et al. 1999; Little et al. 2002). In particular, they postulate that masculine-faced men have better genes, but also have other characteristics that would make them less suitable as permanent mates and caregivers (e.g., they are perceived as less cooperative, warm, and honest; Perrett et al. 1998). In contrast, men with more feminized faces, according to Penton-Voak and colleagues, purportedly have worse genes but make better full-time partners and parents. Therefore, to maximize inclusive fitness, women should pair-bond with more feminine-faced men and thus reap the benefits of having them as permanent partners, but should seek to mate with masculine-faced men when conception is likely in order to obtain the best genes for their offspring.

In sum, the menstrual cycle shift hypothesis offered by Penton-Voak and colleagues assumes not only that women's preferences for different types of men shifts across the menstrual cycle, but also that differences in male traits such as facial masculinity/femininity reflect differences in fitness. Logically, there are also several other assumptions inherent

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in the menstrual cycle shift hypothesis that must be true in order for this view to make sense. These will be analyzed in detail later in the paper.

Harris (2011) Findings

Harris (2011) presented a new empirical study that examined the purported effect of menstrual cycle on masculine face preferences, which appeared in the special issue of *Sex Roles* on Feminist Appraisals of Evolutionary Psychology (2011). In this work (Harris 2011), new data and theoretical analysis were presented that cast doubt on the robustness of a shift in women's preference for masculinized male faces. A large sample, primarily from North America, evaluated facial attractiveness of Caucasian and Asian men in photos that had been altered to vary on degree of masculinization-feminization. The stimuli were the same as those used in the highly cited paper by Penton-Voak et al. (1999), which had reported an effect of menstrual cycle on facial preferences. In Harris (2011), attractiveness judgments by 258 target subjects (women who were premenopausal, not pregnant, not taking oral contraceptives, and reported regular menstrual cycles) revealed no support whatsoever for the view that women who are in the peak fertility phase of their menstrual cycle show preferences for more masculine faces. Several different calculations for peak fertility phase were used based on different methods presented in the literature. None revealed support for the shift hypothesis. In fact, when fertility phase was calculated using the method by Penton-Voak and Perrett (2000), a significant effect in the *opposite* direction was found for the Caucasian stimuli; women who were in a high-fertility phase preferred *less* masculine men than women in a low-fertility phase.

DeBruine et al. (2010) Commentary

In response to Harris (2011), a group of researchers (DeBruine et al. 2010) wrote a commentary that appeared in *Evolutionary Psychology*; these authors include Penton-Voak, Perrett, Jones, and others who have previously published papers on menstrual cycle preference shifts (many of which are discussed elsewhere in the current paper). The DeBruine et al. paper criticizes several aspects of Harris' data and analysis. Their arguments take several forms including 1) claiming that while some studies such as mine (see also Peters et al. 2009) fail to find an effect of menstrual cycle on masculine face preferences, the larger literature as a whole shows robust evidence of such effects; 2) proposing that characteristics of my sample (e.g., age) may be responsible for my failure to replicate the menstrual cycle shift effect; and 3) debating my critical analysis of the various assumptions inherent in the cycle shift hypothesis, citing additional studies that purportedly bolster some of these claims. The present paper addresses each of these criticisms in turn beginning with focusing on the specific data that support or refute the cycle shift hypothesis, including presenting new analyses of the Harris (2011) data that should silence some of the concerns expressed by DeBruine et al. (2010). I then turn to the larger theoretical issues, laying out clearly the more general assumptions inherent in the menstrual faces shift hypothesis and critically analyzing the purported evidence offered by DeBruine and colleagues for these assumptions. I will show that the evidence to date is even stronger against the menstrual cycle hypothesis than it was at the time that the Harris (2011) article was published.

Further Evidence Against Cycle Preference Shifts

New Analyses of Harris (2011) Data

DeBruine et al. (2010) suggest that my data may have failed to reveal an effect of menstrual cycle on masculinity preferences because more than half of my sample was 30 years of age or older (with roughly a quarter being in their 40s). A priori there is nothing in the shift hypothesis that should suggest that the effect would be limited to women under 30 years of age. In fact, older subjects were included in one of the original and highly-cited papers of menstrual cycle effects on masculinity preferences coauthored by Penton-Voak and Perrett (2000), both of whom are authors on the DeBruine et al. (2010) paper. The mean age of the Penton-Voak and Perrett (2000) sample was 30.7 years with an age range of 14–50. These sample demographics appear similar to those of the target subjects in Harris (2011), who had a mean age of 33.3 years and range of 19–51 years. Of note though, Harris had considerably more power (n=258) to detect possible effects than Penton-Voak and Perrett (n=139). Furthermore, all of the subjects in Harris (2011) were premenopausal and reported having regular menstrual cycles. Nonetheless, DeBruine et al. (2010) suggest that most previous research has primarily been on women under 30 years of age and propose that the older women in my sample might have problems with anovulatory menstrual cycles or other hormonal changes, which could potentially lead to misclassification. Their particular concern is that perhaps some of these older women who were classified as high fertility might not actually be in a high-fertility phase. They suggest that younger women in my sample might show a shift in preferences.

To assess this criticism, I reran analyses of the Harris (2011) dataset focusing only on women who were under 30 years of age. Following the procedures of Penton-Voak and Perrett (2000), 1) menstrual cycle phase was determined

by self-report measures; 2) the high conception risk group included women who were 6–14 days post onset of last menses; 3) the low conception group consisted of women who were 0–5 and 15–28 days post onset of last menses; and 4) women who had cycles longer than 28 days were excluded. These new analyses revealed no evidence of a menstrual cycle preference shift in this younger sample. In contrast to the proposal of DeBruine et al. (2010), younger women did not prefer more masculine faces when they were in a high-fertility risk phase relative to a low-fertility risk phase: $t(82)=-1.16$, $p=.25$ for Caucasian stimuli; $t(82)=.23$, $p=.82$ for Asian stimuli. In fact, as seen in row 1 of Table 1, mean attractiveness preferences for the Caucasian face stimuli were in the opposite direction of the shift hypothesis (as they were for the sample as a whole—see also Harris 2011).

Below I conduct further analyses of the original Harris dataset. These analyses, which directly compare responses of the younger women ($n=84$) to those of the older women ($n=164$), further argue against DeBruine and colleagues' proposition that a failure to find a relationship between masculinity preferences and peak fertility phase was due to including older women in my original sample. Two separate 2 (Fertility Phase: high vs. low) by 2 (Age: under 30 vs. 30 and older) ANOVAs were performed on masculinity preferences (one for preferences of Caucasian face stimuli and the other for preferences of Asian face stimuli). Neither analysis revealed any hint of an interaction between fertility phase and age on desire for masculinity; for Caucasian faces, $F(1, 244)=.001$, $p=.973$; for Asian faces $F(1, 244)=.001$, $p=.996$. Means are presented in Table 1. Thus, it is not the case that younger women prefer masculine faces during peak fertility but older women do not as proposed by DeBruine et al. (2010).

Table 1 Average (standard deviation) facial preference for women by age, fertility risk, and target ethnicity

Age Group	Caucasian Stimuli		
	Fertility Status		
	Low Fertility Risk	High Fertility Risk	Mean
Under 30	3.32 (1.20)	3.68 (1.16)	3.40 (1.19)
Over 30	3.23 (1.25)	3.61 (1.20)	3.37 (1.24)
Mean	3.27 (1.23)[a]	3.63 (1.18)[b]	
	Asian Stimuli		
	Fertility Status		
	Low Fertility Risk	High Fertility Risk	Mean
Under 30	2.82 (1.27)	2.74 (1.37)	2.80 (1.29)[c]
Over 30	3.36 (1.36)	3.28 (1.40)	3.33 (1.37)[d]
Mean	3.15 (1.35)	3.15 (1.41)	

Higher numbers represent greater attraction to more feminized faces. (Scale ranges from preference of 1 = 40 % masculinized face, 2 = 20 % masculinized face, 3 = non-altered face, 4 = 20 % feminized, 5 = 40 % feminized)

The two main effects that were significant are noted with differing superscripts: 1) For Caucasian male stimuli, the high fertility group preferred greater feminization than the low fertility group ($p=.05$, denoted by a and b); 2) For Asian male stimuli, younger women preferred more masculinity relative to older women ($p=.009$ denoted by c and d). No other main effects or interactions were statistically significant

Consistent with what was reported in Harris (2011), there was a main effect of cycle phase on masculinity preferences for Caucasian faces, but this was in the *opposite* direction of the cycle hypothesis, $F(1, 244)=3.87$, $p=.05$. The sample as a whole preferred more feminized Caucasian male faces during high fertility phases relative to low fertility phases (see Table 1 for means). There was no main effect of cycle phase for masculinity preferences of Asian faces: $F(1, 244)=.15$, $p=.70$. These new analyses revealed an effect of age such that, relative to younger women (M=2.80, SD=1.29), older women (M=3.33, SD=1.37) preferred more femininity in Asian faces, $F(1, 244)=6.89$, $p=.009$, but this age effect was not found for Caucasian face preferences, $F(1, 244)=.20$, $p=.65$.

In sum, there is no support for DeBruine and colleagues' (2010) suggestion that the use of older women in my sample prevented an effect of menstrual cycle from being revealed. When responses from women under 30 were analyzed, there was no evidence that younger women showed a preference shift during peak fertility phase. Furthermore, when younger and older women's preferences were directly compared, there was no hint of an interaction between age and menstrual cycle phase on masculinity preferences.

Recent Meta-analysis: No Robust Effect of Menstrual Cycle on Masculinity Preferences

Harris (2011) specifically examined preferences for *facial* masculinity, as Penton-Voak and colleagues had done in the seminal work in this area, but failed to find any effect. DeBruine et al. (2010) try to revive support for the menstrual cycle shift hypothesis by claiming that studies that measure masculinity in other ways (e.g., voice pitch, body shape) show strong support for increased masculinity preferences during peak fertility. However, as described below, a recent meta-analytical review by Wood et al. (2012) of this literature provides compelling evidence that there is no robust effect of menstrual cycle on masculinity preferences.

DeBruine and colleagues (2010) also express concerns that Harris (2011) did not employ hormonal assays to determine ovulation status or assess non-oral hormonal birth control usage. I agree that both of these would have strengthened my study, although the hormonal assay would not have been plausible with a large demographically diverse internet sample. However, I am puzzled that DeBruine and associates (2010) are willing to argue that not doing so renders my study problematic. In fact, the vast majority of

studies in this area do not take either of these steps, including the studies that I was attempting to replicate by Penton-Voak's group (Penton-Voak et al. 1999; Penton-Voak and Perrett 2000). Importantly, Wood and colleagues' meta-analysis (2012), discussed next, suggests that menstrual cycle effects do not robustly appear even when these additional precautions are taken into account.

Wood et al. (2012) examined women's preference for a number of characteristics that are purportedly influenced by phase of menstrual cycle including preferences not only for masculinity, but also dominance, physical symmetry, health, kindness, and testosterone. Importantly, Wood's group was careful to include both published and unpublished studies, obtained through a variety of avenues (conference abstracts, requests to listserves including those of evolutionary psychology societies). Furthermore, to assess whether unpublished studies had inferior methods than published studies, Wood and colleagues examined several measures. They found no evidence that unpublished studies provided less valid tests of menstrual cycle effects; unpublished work did not use less accurate indicators of menstrual cycle (hormonal assessments were used just as often), did not assess attractiveness with less appropriate dependent measures (general attractiveness as opposed to sexiness), or focus on inappropriate relationship contexts (long-term over short term). The Wood et al. (2010) analysis resulted in 15 studies that specifically assessed preferences for masculinity, making this the most comprehensive literature review to date. Meta-analysis of these studies failed to find any overall effect of menstrual cycle on masculinity preferences. (The case for purported menstrual cycle effects on preferences for most other characteristics, such as dominance and testosterone, did not fare any better.)

Several additional analyses by Wood et al. (2012) provide compelling evidence against other criticisms raised by DeBruine et al. (2010) of Harris (2011). Consistent with my analyses above of age, Wood and colleagues found no evidence to suggest that excluding older women would help reveal menstrual cycle effects. Moreover, contrary to DeBruine and colleagues' claims, Wood et al. (2012) did not find any stronger effects when 1) within-subjects designs were used, 2) menstrual cycle phase was confirmed with hormonal assays, and/or 3) more restrictive criteria for hormonal contraceptive use were employed. Thus, DeBruine and colleagues' suggestion that Harris (2011) failed to find menstrual cycle shifts due to design or sample characteristics is not supported by meta-analyses of the literature as a whole (Wood et al. 2012).

Why, then, did there appear to be an effect of menstrual cycle on preferences of mate characteristics such as masculinity if there is no such effect? One likely contributor, as discussed by Wood et al. (2012), is publication bias. Failures to replicate a given finding usually remain unpublished and therefore a review that is restricted to the published literature can present a biased picture of reality (normally biased toward implying the existence of effects that are not corroborated). A second likely cause of a high number of false alarms in this literature (also noted by Wood et al. 2012) may be due to what Simmons et al. (2011) have referred to as researcher degrees of freedom, where researchers increase their likelihood of getting significant effects by exploring different subsets of their data, different covariates, and different combinations or transformations of measures. Such elasticity in data-analyses can lead investigators inadvertently to capitalize on chance and result in false-positive findings that occur at rates far in excess of the nominal 5 % rate.

A close examination of the literature on purported menstrual cycle changes raises hints that the kinds of practices described by Simmons et al. (2011) may possibly be at work in this literature. Take for example another paper by the DeBruine group, Jones et al. (2005), which is heavily relied on in their commentary. DeBruine and her coauthors (2010) argue that this paper replicates the masculine face preference shift effect and does so "with a sample over 2.5 times larger than Harris' sample". This claim is made not only in the body of the DeBruine et al. (2010) paper but also in its abstract. What DeBruine failed to mention is that while 656 women were recruited in Study 2 of Jones et al., half (328) of these women were excluded from the key analyses that compared low- vs. high-fertility groups! The women who were excluded by Jones et al. (2005) were not thrown out for obviously justifiable reasons (such as pregnancy, hormonal contraception use, etc.). Instead any women whose predicted onset of next menstrual cycle was in 0–4 days, 13–14 days, and 23 or more days were simply excluded from the analyses. As far as I can tell, such extreme exclusions are not consistent with other studies in this literature, including other work by these very same authors as described below.

Concerns about experimenter degrees of freedom and Type 1 error are heightened when one notes that there are similar vagaries in how peak fertility is defined across several studies authored by an overlapping group of collaborators, many of whom co-authored the DeBruine et al. piece (Jones et al. 2005; Penton-Voak and Perrett 2000; Penton-Voak et al. 1999). To determine peak fertility, Penton-Voak and Perrett (2000) used a "forward calculation" (day 6–14 after onset of previous menses). Jones et al. (Study 2, 2005) used a "backward calculation" (15–22 days prior to predicted onset of next menses). Penton-Voak et al. (1999) apparently used a combination "backward and forward calculation" (end of previous menses to 14 days prior to next menses). Thus, across these studies, the practice for determining the peak fertility phase varies not only by which specific days are included but also by the total number of days included (ranging from 6 to 9 days). Each of these criteria for counting a woman as ovulating could be defensible. However,

the authors of these studies provide no rationale for having deviated from their earlier choices. The choice to use one rather than another with any given dataset can easily affect the size and even potentially the direction of effects.

These differences across studies are even more disconcerting when one takes into account the striking inconsistencies in what constitutes the non-fertile comparison group, as detailed above. Furthermore, the range of days that comprises the nonfertile groups varies greatly from 8 to 19 days. Even within the same paper, menstrual cycle is sometimes analyzed differently across studies. Take for example, Jones et al. (2005). As noted previously, Study 2 did make direct comparisons between what they defined as a high vs. low fertility group (after excluding half the sample). However, such analyses were missing from Study 1 of this same paper (i.e., no direct comparison of a low vs. high fertility group was reported). Instead, estimates of estrogen and progesterone levels, based on women's self-reported cycles, were used as covariates in analyses to assess menstrual cycle effects in Study 1. While Study 2 also included some covariate analyses, from what I can ascertain even these were analyzed differently across the two studies (i.e., Study 1 appeared to use a forward calculation and Study 2, a backward calculation to determine estrogen and progesterone levels).

As Simmons et al. (2011) point out, the false alarm rate is dramatically changed when investigators explore data by considering a range of different definitions. Such variability can readily produce spurious effects and raises the chances of a false alarm well above the 5 % that is generally assumed to occur in published research. The reader might wonder whether such changes in how menstrual cycle is calculated might really have such profound effects. The fact that it can is illustrated in the Harris (2011) study. When peak fertility was calculated using the days specified by Penton-Voak and Perrett (2000) a significant effect was found ($p<.03$, although in the opposite direction of the cycle hypothesis; namely, women who found masculine faces more attractive were in a low fertility phase not a high fertility phase). However, when other fertility calculation methods were used, significant effects were not found (p was as high as .56). Furthermore, in their meta-analysis, Wood and colleagues (2012) found that the greater the number of days counted as being part of the peak fertility phase, the more likely a study was to report a significant effect of cycle phase on attractiveness judgments (range 3–15 days). Thus, as Wood et al. point out, less precise measurements reveal larger effects. This is more consistent with false alarms than real effects.

Theoretical Considerations

The previous analyses along with the recent meta-analysis by Wood et al. (2012) should put to rest the question of whether there is any empirical support for the hypothesis that women's masculinity preferences for men shift across the menstrual cycle. However, there are some additional theoretical issues and misconceptions inherent in the DeBruine et al. (2010) article that deserve discussion. For the moment, let us imagine that a cycle effect was to be found; would this mean that the adaptationist story (i.e., the theoretical analysis) put forward by Penton-Voak and colleagues (e.g., Little et al. 2002; Penton-Voak et al. 1999) would be on solid ground? While one cannot conclusively prove adaptationist analyses, one can assess their consistency with the full range of observations. In the case of menstrual cycle and masculinity preferences, not only would one need to find evidence that such preference shifts occur but one would also need to provide evidence for several other propositions that are inherent in the cycle shift hypothesis. If the data fail to support any one of these, the theory would need to be rejected. Some of these assumptions are clearly discussed in the writings of proponents of the cycle shift hypothesis, as described previously, while others are logically derived. DeBruine and colleagues (2010) contend that there is ample evidence for these various assumptions. Below I discuss each of these assumptions in turn, as well as the purported evidence for them. Table 2 also outlines these assumptions.

The Nature of Female Infidelity

At its core, the cycle shift hypothesis is about female infidelity and inherent in it are several key underlying assumptions about the nature of such infidelities (see also Harris 2011). For one, the theory requires that women in the ancestral environment engaged in infidelity at high rates, and reaped inclusive fitness benefit from doing so; otherwise the purported preference shift would not have become an adaptation. Proponents further argue that these extra-pair matings were short-term and specifically occurred during the follicular phase (e.g., Penton-Voak et al. 1999). Logically, all these assumptions would need to hold in order for the cycle shift hypothesis to make sense. This is because the cycle preference shift hypothesis presumes that women will be mating with different men when they are fertile (more masculine men) vs. not fertile (less masculine men). (Changes in attractiveness preferences need to translate into changes in behavior in order for such preferences to have emerged as evolutionary adaptations.) As discussed in Harris (2011), if women were engaging in sex with short-term partners for longer than the small window of time when conception was likely, then there would be no need for a preference shift during the menstrual cycle. To illustrate, imagine that a woman and her extra-pair partner carried on an affair for 6 months. The exact day in the menstrual cycle when the affair began would have no bearing on whether

Table 2 Assumptions inherent in the hypothesis that women prefer more masculinized men during the fertile phase of the menstrual cycle but prefer more feminized men during non-fertile phases of the menstrual cycle

Assumption 1	Nature of Female Infidelity—Requires three propositions be correct: a) high rates of infidelity in ancestral environment, b) extra-pair bonds were very short term (e.g., several days at most), c) occurred specifically during the follicular phase of the menstrual cycle
Assumption 2	Testosterone depresses immune functioning and therefore is a handicap
Assumption 3	Greater masculine facial features are an index of higher *current* levels of testosterone
Assumption 4	Masculine-faced men have better genes than feminine-faced men
Assumption 5	Other features of men with masculine features make them undesirable long-term mates. (Otherwise women would want them as primary mates.)

she was likely to conceive a child with this man, since they would be together through a number of fertile and infertile phases. Moreover, the man's masculinity or genetic quality would not be changing over this time period, so it seems to me that it makes little sense to propose that there would be any advantage in having a woman's preference for masculinity change over the course of her menstrual cycle. Therefore, if women were engaging in sex with their extra-pair mates for more than a few days, a shift in attraction would not be necessary.

Early work (e.g., Penton-Voak et al. 1999) on cycle shift preferences primarily relied on work by Bellis and Baker (1990) to support claims about the nature of women's extra-pair mating. Harris (2011) pointed out the lack of evidence for the idea that women are mating with different men at different points in their cycles. In fact, Bellis and Baker (1990) found that women tended to engage in double-mating, that is, had sex with both with their lovers and their primary mates at high conception phases—findings which Bellis and Baker use to support their proposition of sperm wars in humans. (See Harris 2011 for more detailed arguments).

In response to my arguments, DeBruine et al. (2010) claim that, "many other published studies present converging evidence that women's interest in extra-pair mating is greater as ovulation approaches than it is during other phases of the menstrual cycle" (p. 770). DeBruine et al. (2010) cite 11 studies, which they assert support claims ranging from that women report more frequent sexual fantasies about other men to that women are more receptive to men's courtship invitations. However, a close examination of such studies reveals that they fall far short of showing that women tend to prefer other men in preference to their own mates during peak fertility.

DeBruine et al. (2010) seem confused about what type of evidence would be required to provide evidence for the menstrual cycle shift hypothesis. First, they would need to show that women have a selective shift in interest in *extra-pair mating* during the high-fertility phase relative to low-fertility phase and that this greater interest in mating does not extend to their primary mates. It is not enough to simply cite work that shows that women generally feel more sexy or are more interested in or receptive to sexual advances by any man (mates included) during high-fertility phase. (After all, the cycle shift hypothesis is about women desiring men *other* than their primary mates when conception is likely.) Most of the papers cited by DeBruine et al. (7 out of 11) do not bear on women's desires or behaviors regarding infidelity because they do not assess anything related to extra-pair mating.

For example, DeBruine et al. (2010) cite Gueguen's work (2009a, b) in support of the idea that women engage in infidelity during peak fertility. It is unclear how they draw this conclusion. Gueguen (2009a) examined *single* women's willingness to dance with an attractive man in a nightclub. Women who were in relationships were excluded from the study. Clearly, such data cannot possibly speak to women's preferences for extra-pair mating. The other Gueguen paper (2009b) cited found that women accepted date invitations more often in the high-fertility phase but this was the case only for women not taking the pill. Although relationship status was not assessed, Gueguen's favored explanation for his findings was that women who were taking the pill were likely in relationships and therefore generally not interested in other men. Consistent with this interpretation is his finding that in each phase of the menstrual cycle, non-pill users accepted the date request more than pill users. (This would be in keeping with the general idea that sexual interest is greater in women during peak fertility times and if they have a mate they will engage in sex with him. If they do not have a mate, they will have greater interest in pursuing one.) Despite DeBruine and colleagues' claims, several of the other studies they cite also do not address women's interest in extra-pair bonding. Durante et al. (2011) report that women show a greater interest in sexy clothing and accessories when near ovulation but do not assess infidelity and desire for extra-pair mating. In fact, some of the findings in this work seem inconsistent with the theory underlying the cycle shift hypothesis: When women were primed with attractive men, the effect of menstrual cycle on preference for sexy clothes went away. Yet, the presence of attractive men would seem to be the very situation in which one should see menstrual cycle effects.

Other work cited by DeBruine et al. (2010) that does actually examine women in relationships also casts doubt on

the claimed link between peak fertility and infidelity. DeBruine et al. imply that wearing revealing clothes is a proxy for women wanting to have sex with other men. However, Haselton - Durante et al. (2008) found a significant interaction such that it was women *not in relationships* that wore more revealing clothing during high-fertility phases (relative to low-fertility phases), while those in relationships showed the opposite pattern. It is hard to see how DeBruine et al. (2010) felt able to conclude that this research supported their claim that women's interest in extra-pair mating is greater at ovulation. In short, most of the studies cited by DeBruine and colleagues regarding female infidelity either do not bear on the topic at all or in some cases provide tangential evidence *against* the assumptions required by the cycle shift hypothesis. The presentation of the literature in DeBruine et al. (2010) is likely to seriously mislead readers who have not examined the original papers.

Moreover, even the four papers cited by DeBruine et al. (2010) that *did* examine variables relevant to interest in infidelity tend to suffer from serious shortcomings that prevent the drawing of any strong conclusions. This is the case for the work by Gangestad and colleagues (Gangestad et al. 2002, 2005), which according to DeBruine and colleagues shows "women report more frequent sexual fantasies about men other than their primary partner near ovulation than at other times, while the reported frequency of sexual fantasies about their primary partner does not change" (p. 770). Several features of this work suggest it may not be wise to put too much stock in the findings.

Gangestad et al. (2002) had high exclusion rates, a small sample size, and did not even report mean values for key analyses. Specifically, through exclusions, an initial sample of 118 women not on hormonal birth control was reduced to only 51 women. Moreover, of these, only 31 were in relationships and only these subjects provided evidence about women's relative desires for other men vs. their own mates. It also is not clear that the data support the key assumption inherent in the cycle shift hypothesis, namely that women prefer other men more than their own mate during peak fertility. While no women in the study reported actually engaging in infidelity, Gangestad et al. (2002) reported two main effects; 1) women were *more sexually interested in their own mates than in other men across the cycle* and 2) sexual interest was higher during peak fertility. There was also a significant interaction—sexual interest in other men rose more than interest in own mate during peak fertility. However, neither means nor figures are presented for this interaction, making it hard to interpret the data (a cross-over interaction would be needed to support the theory). For example, what is reported is consistent with women preferring their own mates across all cycle phases but just showing some additional interest in other men during peak fertility phase.

This interpretation seems even more likely in light of other findings by these same authors (Gangestad et al. 2005), which DeBruine et al. (2010) cite. Women's attraction to their own mates far exceeded attraction to other men not only in the nonfertile phase (M=5.10 vs. M=1.31) but also in the fertile phase (M=5.13 vs. M=2.32)! Such a pattern would be inconsistent with the claim that women shift to preferring other men over their own mates during peak fertility. (Additional analyses suggest that this interacts with symmetry of mates, but as in Gangestad et al. 2002, means are not provided for this interaction.)

The other two studies that bear on the topic of women's possible desire for extra-pair mating are also problematic for the theory (Haselton and Gangestad 2006; Pillsworth and Haselton; 2006). Neither of these studies reveals a significant main effect of menstrual cycle phase on women's attraction to other men (the dependent measure with the greatest face validity). Significant results are only found in analyses that include additional "moderator" variables (e.g., a difference score between the primary mates' sexual vs. investment attractiveness) and/or involve dependent measures that do not directly assess a women's desire for other men (e.g., their ratings of their mate's solicitousness; or hypothetical interest in going to dance clubs or parties where men could be). Performing a number of analyses of this variety could easily increase the likelihood that effects are the results of Type 1 error (cf Simmons et al. 2011).

To be clear, I am not claiming that no finding in any study cited by DeBruine et al. (2010) is consistent with any part of the shift hypothesis. Some may be. However, what is certain is that many are not, and that DeBruine and colleagues copious citations are misleading, leaving the reader with a gross misimpression of the literature. This is particularly surprising given that some of the mischaracterized evidence actually involves work by coauthors of the DeBruine piece (e.g., Haselton - Durante et al. 2008).

Other Assumptions Inherent in the Cycle Shift Hypothesis

As detailed above, there is simply no adequate evidence to back up the idea that women's preferences for different types of men shift across the menstrual cycle. The evidence (or lack thereof) should be enough to lay to rest the theory of menstrual cycle preference shifts for masculinity. However, it is also worth briefly commenting on the status of the data for other assumptions inherent in the cycle shift hypothesis, which further highlights how shaky other aspects of the theory are. (See Harris 2011 for additional discussion of these points.)

Given the proposition that masculine faces indicate good genes, one might have expected that masculine faces would be associated with physiological benefits. Paradoxically, proponents of cycle shift hypothesis argue for something much more complicated, based on a controversial

hypothesis that has been proposed for mating preferences in nonhuman animals (e.g., Penton-Voak et al. 1999; Little et al. 2002). The immunocompetence handicap hypothesis (ICHH; Folstad and Karter 1992) proposes that testosterone, in addition to promoting secondary sexual characteristics, tends to suppress the immune system, rendering a male more vulnerable to parasitic illness. So why should females want high-testosterone males? Folstad and Karter (1992) suggested that by selecting a mate who appears to be thriving despite the immunological handicap posed by high testosterone levels, a female can obtain evidence of a genetic fitness sufficient to overcome the immunological handicap produced by the testosterone.

Several elements of this analysis are highly speculative, and are controversial even amongst biologists studying animal behavior. First, it is not clear that testosterone is associated with decreased immune functioning across studies. A meta-analysis of experiments manipulating testosterone in non-humans (Roberts et al. 2004), while finding support for the ICHH in reptiles, did not find general support for it in birds and mammals when non-independence of studies was taken into account (although some specific analyses did show some effects consistent with the hypothesis). Furthermore, Braude et al. (1999), reviewing various types of evidence, argued that there might not be a general dampening of the immune system even in the cases where fewer numbers of immune cells are found in animals after the administration of testosterone. Instead, testosterone might lead to a strategic *migration* of different kinds of immune cells from some body compartments to others—a sort of redeployment of immunity—rather than an overall suppression of immunity. The limited data from humans also do not provide compelling evidence that testosterone decreases immune functioning. While some studies report some association (e.g., Fimmel and Zouboulis 2005), insofar as I could ascertain, the study with the largest sample size (n= 4415 men) found no negative relationship between any immune measure and testosterone (Granger et al. 2000).

Second, even if it turns out that there is some immune functioning costs incurred by testosterone, evidence would still be needed that there is a connection between *current* levels of testosterone and masculine facial features in humans. While testosterone clearly plays some role in development of masculine facial features, particularly during adolescence, I suggest that it is a large leap to then assume that facial features formed earlier are indicative of a male's current testosterone level (e.g., van Bokhoven et al. 2006, found no correlation between testosterone levels of men at 21 years of age and their levels at 13 and 16 years of age). Current circulating levels of testosterone are important because that is what has been proposed to reduce immune functioning in the ICHH hypothesis (Folstad and Karter 1992). The literature bearing on this in humans is small and mixed. While some studies report a correlation between circulating testosterone levels and rated masculinity (e.g., Roney et al. 2006; Penton-Voak and Chen 2004), at least as many studies find no such relationship (e.g., Peters et al. 2008; Hönekopp et al. 2007; Neave et al. 2003)

Finally, the cycle shift hypothesis proposes that there be something about masculine-faced men that makes them poor mates (e.g., Little et al. 2002). Logically this is important, otherwise women should prefer masculine men as long-term mates as well as short-term mates. However, this assumption is highly speculative with little support offered for it. The primary data cited by cycle shift proponents (e.g., Penton-Voak and Perrett 2000) for this proposition consists of a judgment study by Perrett et al. (1998), in which faces that had increased masculinity were *judged* as having a higher ranking on dominance, but a lower ranking on warmth, emotionality, honest, cooperation, and parental quality. In short, evidence is lacking for the contention that more masculine men really are chosen less often as full-time mates. In fact, Wood et al. (2012) found that greater masculinity was judged as more attractive (but this preference did not change across menstrual cycle).

Concluding Remarks

In sum, findings from the literature as a whole do not provide evidence in support of the widely cited claim that women's preferences for masculinity in mates changes across the menstrual cycle. Moreover, a careful analysis of the complex set of assumptions implied by proponents of this account reveals a convoluted web of speculation. Examination of the evidence suggests that none of these assumptions currently rests on strong empirical ground. While one or two of these may ultimately be found to have support, the cycle shift hypothesis requires that each one be correct, which seems highly unlikely. The weakness of the adaptive theory is not so surprising in light of the finding that menstrual cycle phase does not in fact shift women's mating preferences in this domain.

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