

# Evolution of Human Jealousy

## A Just-So Story or a Just-So Criticism?

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To operationalize the methodological assessment of evolutionary psychology, three requirements are proposed that, if satisfied, would show that a hypothesis is not a just-so story: (1) theoretical entrenchment (i.e., that the hypothesis under consideration is a consequence of a more fundamental theory that is empirically well-confirmed across a very wide range of phenomena), (2) predictive success (i.e., that the hypothesis generates concrete predictions that make it testable and eventually to a certain extent corroborated), and (3) failure of rival explanations (i.e., that crucial and successful predictions attributed to the hypothesis in question cannot be derived from alternative hypotheses). The author argues that the hypothesis about evolutionary sex differences in human jealousy satisfies all three requirements.

*Keywords:* evolutionary psychology; adaptationism; philosophy of science; testability

### THE METHODOLOGICAL SIN: MOLIÈRE OR VOLTAIRE?

A methodological transgression most often attributed to evolutionary psychology is that it is adaptationist, unfalsifiable, Panglossian, evidentially unfounded, and purely speculative. Is this true? I think that the question is hardly manageable in this form. A much more promising way to approach the issue is to narrow the focus significantly and evaluate some representative, more concrete, and more tractable hypotheses proposed by evolutionary psychologists. But

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even limiting our attention to, say, the evolutionary account of human emotions is still leaving us with a too broad topic for reaching a reasoned judgment about the enterprise. Those who try to oppose contemporary evolutionary theorizing at this level of generality typically rely on simplistic objections and hurried condemnations without much argumentative substance. Here is an extreme but certainly not an isolated example:

But most of what passes for evolutionary explanations of emotion in both psychology and philosophy these days are no more enlightening than Molière's famous explanation of the power of a sleeping potion. To show that something serves a purpose or a function says no more and no less about the evolutionary process than the crudest creationist or contingency theories. Evolution is the new magic wand, which with a wave changes something inexplicable into something only seemingly explained. As Nietzsche famously noted, we always prefer bad explanations to no explanations at all. (Solomon 1998, 5)

Apparently, the word has got around that evolutionary psychology is suffering from fatal methodological defects, and consequently it has become perfectly acceptable to dismiss the whole field with just a sneering quotation from Nietzsche or attribution of a ridiculous fallacy (*virtus emotiva?*).

But the charge of adaptationism and undisciplined speculation is raised against evolutionary theories of emotions also in a more serious way, and by people who certainly know what they are talking about. For instance, in his excellent book, *What Emotions Really Are*, Paul Griffiths says, "The historical turn in the study of adaptation promises to lift adaptive explanation above the provision of just-so stories. *But it has yet to be applied to adaptive studies of emotions*" (Griffiths 1997, 74, emphasis added)

Obviously, Griffiths—like so many other biologists, psychologists and philosophers of science—believes that the Darwinian theories of emotions (and especially of the so-called "higher cognitive emotions") that are currently on offer have not risen above the level of just-so stories. Before we agree, or disagree, with him we have to do two things. First, we have to see what he means by a "just-so story." And second, as already suggested, we have to sharpen our focus further and assess some typical and quite specific evolutionary hypotheses that have been proposed in the literature. If we want to exorcise the Panglossian devil, we will have first to find him in the details.<sup>1</sup>

### MAKING SENSE OF KIPLING'S EXPRESSION

What is the difference between an arbitrary narrative, or "just-so story," and a true explanation? For one thing, a good explanation shows that the explained phenomenon was to be expected. It predicts it or postdicts it. Of course, this is not enough. An additional requirement seems to be that the same form of explanation should *not* work equally well if the facts of the *explanandum* are imagined to be capriciously changed. Surely, the Newtonian account of planetary movements would be worthless as a scientific theory if invoking gravitation and the laws of motion would explain equally well, say, triangular orbits as elliptical ones.

It is exactly in this respect that, in Griffiths' opinion, the evolutionary psychology of emotions falters. He argues that its conceptual resources are so elastic that they can easily be stretched to account even for very different situations from the one that actually exists. And indeed, if a plausible evolutionary explanation is easy to concoct, whatever the data, the entire research program stands condemned: its indiscriminate "successfulness" makes it explanatorily vacuous.

Unfortunately, the main example that Griffiths gives to support his diagnosis is far from convincing. The hypothesis he criticizes is Donald Symons's (1992) evolutionary explanation of the fact that men are attracted to nubile young women. Symons suggests that this makes excellent adaptive sense, because men with that trait were likely to leave more offspring and reproductively outcompete others. Griffiths (1997) objects that an equally plausible just-so-story could be invented on the spot even if empirical data about male sexual preferences were different. He asks, what if it turned out that "more mature young women" were in fact more attractive? Wouldn't this kind of men's preference also make excellent adaptive sense because these women would have more skills, would have proven their fertility, and so on (Griffiths 1997, 110)?

Although Griffiths' (1997) story may initially sound like a plausible alternative scenario, a more careful look reveals a number of problems. To begin with, men's sexual desire is directed both toward so-called short-term mates and long-term mates. But in selecting short-term-mates, the postulated preference for more mature women with more skills would obviously pay no evolutionary dividends (because here the relation is *ex hypothesi* restricted just to sexual intercourse). With respect to long-term mates, however, it is again unclear what would be the fitness advantage of having a sexual desire that guides

men to systematically choose as monogamous partners for life those women who already passed the period of their highest expected reproductive value (and who may already have children of other males to take care of). Furthermore, the idea that older age is somehow connected with increased fertility is tossed in with no justification: the category “more mature young women” certainly does not include only those (or typically those) “who have proven their fertility.” All this indicates that what Griffiths has offered is merely a fanciful and half-baked explanation sketch that lands in a morass of complications before it could be compared against possible rivals. The purpose of his example was to illustrate the “unbearable lightness” of inventing *prima facie* plausible adaptationist stories but ironically it appears to have proved exactly the opposite.

All right, maybe this was a bad example. But it is still possible that Griffiths and other critics are right about the main thing: that typical scenarios of evolutionary psychology are only initially plausible and that on closer inspection they will turn out to be methodologically rotten or, to use Griffiths’ term, *profoundly unscientific*. To operationalize the question, I will propose three requirements that if satisfied, show that a hypothesis is *not* a just-so story:

- (a) theoretical entrenchment,
- (b) predictive success, and
- (c) the failure of rival explanations.

By “theoretical entrenchment” I mean that H (the hypothesis under consideration) is a consequence of a more fundamental theory that is empirically well-confirmed across a very wide range of phenomena. “Predictive success” means that H generates concrete predictions that make it testable and eventually, to a certain extent, corroborated. Finally, the “failure of rival explanations” tells us that H is explanatorily superior to its alternatives, in the sense that some of its crucial and successful predictions cannot be plausibly explained by these alternative hypotheses.<sup>2</sup> I hope that these three requirements capture essential characteristics of good scientific theories, and that they provide the much-needed measuring stick for assessing epistemological virtues and vices of evolutionary psychology. The only thing that now remains to be done before embarking upon the evaluative task is to choose a particular representative hypothesis that will be put under a magnifying glass. This is necessary because, as already said, discussing evolutionary psychology *tout court* would

produce just a patchwork of superficial judgments about different and often unrelated theoretical contexts. A good slogan here is: think globally, probe locally. Otherwise, if we paint with a too broad brush, there is a real danger that we will soon be lost in generalities, and that, so to speak, we will not see the trees for the forest.

### WHY JEALOUSY?

The best case for evolutionary psychology can be made with respect to those aspects of human mentality that have obvious links with increased fitness: fear, sexual desire, hunger, and so on. But neither of them would be a good test-case for our purposes. For, however positive and impressive the results of our analysis might be, it would be always possible to object that the favorable judgment is here simply due to a badly biased choice of precisely those examples that were specially amenable to evolutionary explanation in the first place. Therefore, it would carry much more weight if we took a harder chestnut—for instance a nonbasic, higher-cognitive emotion—and explore theoretical arguments that purportedly lead to its Darwinian roots. An apt choice would be human jealousy; or, more specifically, the claim that emphasized sexual jealousy, particularly characteristic of men, is an evolutionary adaptation. This claim recommends itself to our attention for two reasons. On one hand, a lot of work has been done on this topic in evolutionary psychology, and it is the kind of work that is widely regarded as truly representative of the whole research program. On the other hand, since the claim introduces a nontrivial and nonobvious difference between the sexes, and since the emotion in question is undoubtedly highly complex, no one can say that the trait carries the signs of its selective advantage on its sleeve. In other words, the claim is controversial enough, and if it can be shown to be a serious contender for truth, this should be seen as a significant theoretical accomplishment.

Let me stress again, however, that my main concern in this article is methodology, not truth. So with the claim about jealousy, too, I am more interested in what kind of story it is (a just-so story or a respectable science), and less in whether that hypothesis is acceptable, all things considered. A theory can be good methodologically, and still be wrong. Therefore, given my limited goal here, if the claim under consideration turns out to be vindicated by my analysis, this will only

mean that it has escaped the clutches of Dr. Pangloss, but not necessarily those of falsity.

### THEORETICAL ENTRENCHMENT

Is there a reason why we should expect sex differences in human jealousy? The affirmative answer to that question is often preliminarily derived from some very general Darwinian considerations. The usual starting point is Robert Trivers' theory of parental investment, a middle-level evolutionary theory that has ramified applications in many different parts of biology. Among other things, the theory says that organisms that systematically offer parental care to the offspring that is not their own are likely to go extinct fast: they will be superseded by more discriminating parents. Natural selection will favor organisms that tend to care for the young that are theirs, maybe not with complete certainty but at least with a sufficiently high degree of probability.

It is already at this stage that a conspicuous sex difference in humans emerges. Namely, maternity is easily established (because babies come out of mothers' bodies), whereas the question of paternity is always connected with some level of uncertainty. Given this asymmetry between the sexes, it seems that only men face the following evolutionary problem: since they are less than 100% sure about their parenthood, then in any given instance their help in raising the child may well not be, strictly speaking, a case of *parental* care at all, but rather a wasted effort (in terms of fitness) misdirected to another man's offspring. Clearly, paternal investment makes much more evolutionary sense if paternity uncertainty is relatively low. As a matter of fact, it does not only make more sense in the abstract: a connection between the two magnitudes has been richly documented in many different species. Figure 1 gives the graph representing, in a quite general way, what some biologists regard as the "most reasonable shape" for that relationship.

According to Figure 1, below a given threshold of paternity probability, investing in paternal care is no longer worthwhile. Under the circumstances, males would be better off channeling their energy toward other goals (e.g., additional mating efforts). Actually, zero paternal care is fairly common in the animal world. In mammals, for instance, males do not care for the young in more than 95% of species (Clutton-Brock 1991, 132). Even among primates paternal care is rela-

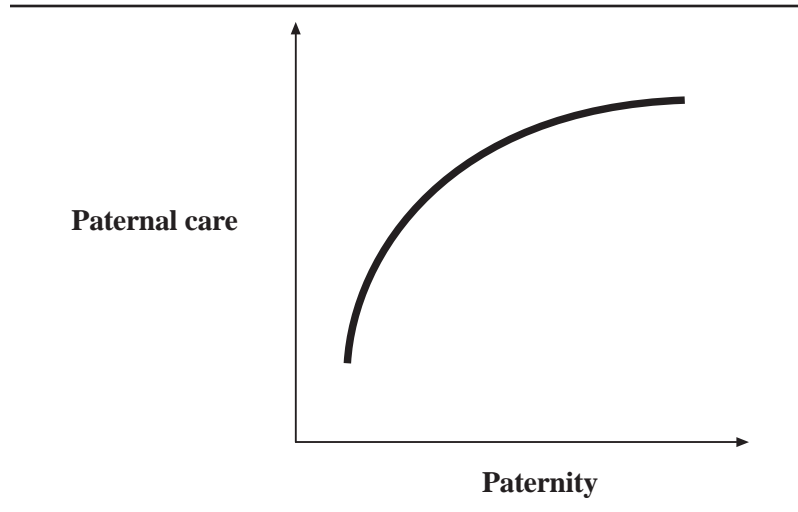


Figure 1: Paternity vs. Paternal Care (adapted from Wright 1998)

tively rare and atypical,<sup>3</sup> and most tellingly, in our closest phylogenetic relatives (chimpanzees and bonobos) males usually do not take much care of the offspring (Geary 2000, 55).

So, why do we have this trait then? This seems not to be well understood (see Geary 1998, 116-19; Mealey 2000, 293-94). According to one view, it is the slow growth of the human child, its vulnerability and long dependence on adult help that made the input from both parents essential for its well-being and survival. This constraint, it is argued, was probably one of the main factors that led to the establishment and spread of marriage, or to the emergence of social monogamy, a highly common type of social organization in many societies studied by anthropologists, and also believed to be distinctive of *Homo sapiens* in the so-called environment of evolutionary adaptedness.

Generally speaking, though, pair bonding driven by the need of providing effective child care has its tensions because of the conflict of evolutionary interests of the two parents. This is best reflected in the fact that *social* monogamy does not go always together with *sexual* monogamy. On the contrary, in most species characterized by social monogamy, sexual monogamy remains less than perfect. As a rule, there are occasional extra-pair matings, and therefore despite biparental care, the male's paternity is far from guaranteed. This is the

norm, and it is only in truly exceptional cases that social monogamy is followed by full sexual monogamy. One of these exceedingly rare, aberrant examples is the California mouse. In this species, "bonded males shun other females even if the females are in estrus, and bonded females ignore other males," and furthermore "genetic tests of paternity confirm the species' till-death-do-us-part fidelity" (Morill 1998, 1983). But humans—in California and elsewhere—seem to be much closer to the prevailing norm in the animal world than to the California mouse. The fact is that, as Jared Diamond says, "both the institution of marriage *and* the occurrence of extramarital sex have been reported from all human societies" (Diamond 1992, 53). These two things together definitely create a problem.

Given their considerable parental investment, men need to be reasonably sure that their care is directed to the appropriate beneficiary (i.e. their own offspring). Looking at Figure 1, the optimal curve shows that going up (increasing the contribution of paternal care) requires moving to the right (increasing paternity certainty). Another way of putting it is to say that under the conditions of mandatory biparental care, there is a strong selective pressure on men to develop some kind of psychological mechanism that would raise the probability of their helping behavior being targeted to the right object. Although I of course readily concede the point, belabored in much of the anti-adaptationist literature, that optimal design may not always be achievable, still it would be really surprising if in this case, with such a pressing need, natural selection did not come up with *some* answer. We know very well that with paternity, as with other evolutionary problems, the Darwinian process is extremely resourceful and inventive in finding a solution. To illustrate how extraordinary and unexpected are the ways of the blind watchmaker, look at the following marvelous adaptation for assessing paternity in house mice:

When a male mouse encounters a neonate in his territory, he normally kills it, but he abruptly switches to infant care during the period when the neonate might be his offspring. Here, the "decision rule" is: switch from kill to care 18-22 light-dark cycles after you have mated and ejaculated. This rule is followed regardless of the female with which the male has mated, whether the mated (or any) female is with the male during this period, or whether or not the neonates are his offspring. When experimenters change the natural light cycle to artificial day lengths of 22 or 27 hours, the males still "count" the cycles, not the actual time. (Kummer 1995, 30)



With the incomparably more powerful and versatile brain in humans it stands to reason to expect even more sophisticated adaptations as a response to the problem of paternity uncertainty. And it is exactly in this context that the emotion of sexual jealousy comes to mind as a possibly relevant psychological phenomenon. According to one good definition of sexual jealousy, it is “the state of being concerned that one’s sexual exclusivity is or might be violated” (Wilson and Daly 1992, 302). So defined, jealousy smoothly fits a role in a Darwinian scenario. To spell it out, (1) if men happen to be specially concerned about their exclusive sexual access to their wives, (2) if on the basis of that emotion they strive hard to protect and defend that exclusivity, and (3) if they are at least partly successful in that endeavor, the impact on paternity is quite transparent. Full exclusivity entails zero paternity uncertainty. But even less than fully realized exclusivity can still serve the same evolutionary purpose in a relatively effective manner.

This state of affairs is actually just one instance of a very general evolutionary predicament. As Margo Wilson and Martin Daly (1992) say, “Male sexual proprietariness is likely to evolve in any animal species with internal fertilization and paternal care” (p. 292). Internal fertilization makes the establishment of paternity difficult, yet without some level of paternity certainty, paternal care as a trait cannot be selected for. If the trait is vital, though, the logic of the situation calls for a way to boost the male’s confidence of paternity. And as the male here cannot be in the immediate contact with the fertilized egg after copulation and guard it, an obvious second-best solution is to guard the female instead. This may look obvious, but does it really happen? Is this piece of armchair biology confirmed by empirical evidence?

Apparently, it is. First, paternal care is indeed much more common in animals with external fertilization, where paternity is for evident reasons pretty secure. But second, and more important, in the species most similar to us in the relevant respects, males do exhibit exactly the predicted behavior: proprietariness and the sexual control of females. The relevant respects here are internal fertilization and biparental care. Rather surprisingly, the animals that massively share these two traits with us are neither primates nor even mammals, but *birds*.

Birds are unique among vertebrates in that biparental care is the norm, occurring in over 90% of the 9,000+ living species. Biparental care, usually involving shared incubation of eggs and feeding of young, is found in almost all monogamous birds with altricial young. (Clutton-Brock 1991, 132)

Our conjecture, derived from very general evolutionary considerations, that paternal investment associated with paternity uncertainty will lead to the sexual control of females by the males is fully borne out in the avian case.

The research on swallows and dunnocks (along with work on many other birds [*a long list of references follows*]) provides strong evidence that paternally investing male animals have evolved sexual psychologies designed by selection to reduce both the likelihood of cuckoldry and its costs once incurred. We may expect no less of the evolved psyches of paternally investing *Homo sapiens*. (Wilson and Daly 1992, 297)

With birds, one has almost to be a creationist to deny that the behavior in question is an evolutionary adaptation. But how does it connect with a Darwinian account of men's jealousy? Is the very idea of linking these two phenomena just the result of crude anthropomorphism? Not necessarily.

The basic idea of this whole section can be summarized in the following "equation":

$$PC + PU = PB$$

Paternal care (PC) and paternity uncertainty (PU) are usually associated with proprietary behavior (PB) of males. The left-hand side of the formula applies to birds and humans in the same sense, without our having to resort to metaphor or double meaning. The right-hand side looks trickier but is not really so (or not much). For, despite the plain fact that proprietary behaviors of male swallows and men are so tremendously different in terms of behavioral complexity (so much so that many of us rebel at the attribution of "sexual psychology" to birds), it still remains possible that at a certain level of generality the behavior of both is describable by using the same and entirely unequivocal concept. For instance, the male members of both species may behave in such a way that, as a result of that behavior, sexual access of other males to their mates is restricted and the probability of their own paternity is thereby significantly increased.

In birds, as we saw, the key behavioral disposition is richly documented as well as widely regarded as the product of natural selection. To establish the same claim about humans would clearly require a lot more explanatory work.<sup>4</sup> Yet the discussion so far enables us to make a small step in that direction. Briefly, given that (1) humans are characterized by PC and PU, and (2) that PC and PU tend to bring about PB (the relation inferred from the middle-level evolutionary theory of

parental investment and also confirmed by numerous observations of PC-plus-PU animals), we should look for, and be prepared to find, PB in our own species. This theoretically motivated expectation does not imply dogmatism. On the contrary, it is perfectly compatible with our being aware of the immensity of the task and with our knowing that, for all that matters, what we are looking for may in the end simply not be there.

### PREDICTIVE SUCCESS

The evolutionary logic, rehearsed above, whispers in our ear: "Men who were indifferent to the potential sexual contacts between their wives and other men are not our ancestors" (Buss 1999, 149). But what do empirical data say? Is there really a psychological trait in men that makes them particularly sensitive to potential sexual contacts between their wives and other men?

An obvious candidate for the role is sexual jealousy. And indeed, the strong presence of this trait is authenticated through four different lines of empirical evidence:

- (a) ethnographic record,
- (b) psychological research,
- (c) statistical data about family violence and homicide, and
- (d) divorce cases and causes of conjugal dissolution in a number of cultures.

I will here just briefly comment on (a) and (b). For (c) and (d), I refer the interested reader to Daly and Wilson (1988), Wilson and Daly (1996), Buss (2000), Ghiglieri (2000), and the literature cited there.

(a) Ethnographic record is important because any psychological state that is an evolutionary adaptation tends to be manifested in all (or almost all) cultures. The universality or near-universality of a trait is not in itself sufficient to establish its biological roots, but if a trait is highly variable across cultures, this does undermine a corresponding adaptationist hypothesis (or at the very least extensive additional argumentation would be needed to rehabilitate it).

The theory we are dealing with faces no difficulties in this respect. As a matter of fact, violent sexual jealousy as a distinctive characteristic of males is recorded practically in all known human societies. Historically, the norm has been to have laws forbidding the wife's adultery, not husband's (Ghiglieri 2000, 150-51). Even that famous

counter-example with the Eskimos, their nonjealous men and the alleged custom of wife sharing turned out to be a factoid: in reality, among the Eskimos, as in most other contemporary and traditional societies, male sexual jealousy is the most frequent cause of violence and homicide.

In a classic study of human sexual behavior (Ford and Beach 1951) it was claimed that in a sample of 139 societies, violent male sexual jealousy was absent in only 7 (or 5%). As if such data were not already impressive enough, subsequent research has even disproved that the exceptions were genuine in every single of these 7 purportedly anomalous cases (Daly and Wilson 1988, 205)! Hence the following conclusion does not appear to be exaggerated:

But is there even one exotic land in which the men eschew violence, take no proprietary view of their wives' sexuality, and accept consenting extramarital sex as good, clean fun? The short answer is "No," although many have sought such a society, and a few have imagined that they found it. (Daly and Wilson 1988, 203)

(b) A strong test of evolutionary psychology of jealousy is obtained by deriving the following prediction: if the specific form of that emotion in men is indeed selected as a solution to the tension between the need for parental care and the fact of paternity uncertainty, then it appears to follow in a pretty straightforward way that men should be specially sensitive to the cues of *sexual* infidelity, and surely more sensitive to that than women. (From the Darwinian point of view, women also had to solve a host of evolutionary problems, but establishing maternity was not one of them.)

Indeed, the prediction about sex differences in human jealousy seems to be well confirmed. When asked whether they would be more upset by their partner's emotional involvement with another person (E) or with their partner's sexual relationship with another person (S), it turns out that men are typically more upset by S, and women by E. The difference is highly significant. A total of 83% of women found E more upsetting whereas only 40% of men did so. In contrast, 60% of men were more upset by S, and only 17% of women reacted in that way (Buss et al. 1996a).

When earliest psychological studies appeared pointing to these sex differences in jealousy, they suffered from three weaknesses. First, the data came from a highly selected and idiosyncratic population—American undergraduates. Extrapolation from this narrow group to humanity as a whole looked a bit dubious. Second, there was a gen-

eral worry that information based on subjects' reports about their own mental states (answers to questionnaires) is notoriously less than fully reliable and should not be taken at face value. Third, even if conclusively proved to be authentic, the sex difference in jealousy might still have an alternative, nonevolutionary explanation.

All these three difficulties have been recognized and addressed. First, the research on human jealousy has been expanded to countries with different cultures and sexual moralities to test the robustness of the sex difference discovered in the American environment. In corroboration, very similar results were obtained in studies conducted in Korea, Japan, the Netherlands, and Sweden (Buunk et al. 1996; Buss et al. 1999). Second, the first-person reports of subjects were shown to be in full agreement with measures of their physiological arousal; this independent check made the reports more trustworthy. The handling of the third problem is described more extensively in the next section.

#### THE FAILURE OF RIVAL EXPLANATIONS

DeSteno and Salovey (1996) offered a very interesting alternative explanation of the sex difference in jealousy. Their explanation is known as the "double-shot hypothesis". Basically, the suggestion is that men and women do not react differently to emotional and sexual infidelity as such, but that their different responses may simply result from different beliefs they have about how these two kinds of infidelity are connected. If men believe (rightly or wrongly, it does not matter) that women are unlikely to have sex unless they feel emotionally involved with their partner, then it is easily understandable that men are more upset by their partner's S-relationship (sexual relationship) with another man than with her mere E-relationship (emotional relationship). For, under the circumstances, S nearly implies E whereas E is not necessarily (or probabilistically) connected with S. In other words, S being an indication of E, it actually represents a double loss, or a "double shot" (S + E), and it is therefore manifestly worse than a single loss (E). *Mutatis mutandis*, if women believe that men's emotional involvement somehow naturally lead to sexual relationship, it is again plain why they would be more upset by their partner's E-infidelity than S-infidelity. Now the situation is reversed: E is usually accompanied by S, constituting a "double shot", while S is a single, lesser evil. Briefly, then, according to this account, the reason why men are more concerned about their partner's S-infidelity is that they

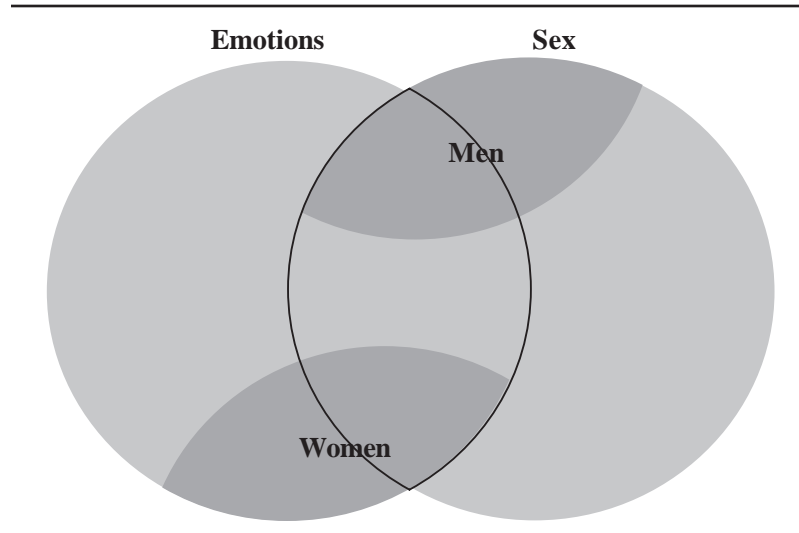


Figure 2: Double-Shot Hypothesis

believe that in women S does not come alone (it is usually combined with E), in contrast to E which is largely independent from S. On the other hand, women are more jealous about men's E because they think that in men, E tends to go together with S, whereas S is neutral with respect to E or not-E.

The asymmetry is clearly visible in Figure 2, where areas representing men and women occupy essentially different parts of the two main circles. For men, E-area is completely inside the S-circle, while for women S-area is entirely included in the E-circle. Therefore, if this picture represents the reality, any woman would justifiably be more worried about her partner's emotional infidelity because it would imply sexual infidelity as well. But men would have more reason to be concerned about sexual infidelity.

It seems there is evidence that E-S connections work the way as described, asymmetrically for the two sexes. But then, in order to examine whether men and women respond in a different way to S-infidelity and E-infidelity as such (and not because of what they are connected with) there are basically two test designs that immediately come to mind. First, one could ask both men and women whether they would find one of the two following infidelity scenarios more

upsetting: (1) E and not-S or (2) S and not-E. If women consistently said that (1) is more upsetting, and if men chose (2), this would show that these two situations have a different “intrinsic upsettingness” for the two sexes, and the “double shot” hypothesis would lose credibility. Second, the question might be raised about the “E + S” situations, or more specifically about which *aspect* of that double infidelity (emotional and sexual) the subjects found more upsetting. Again, if the consistent sex-difference asserted its presence even here, the double-shot hypothesis would suffer another blow, and the evolutionary hypothesis would be indirectly confirmed. Indeed, as soon as the double-shot hypothesis was proposed, evolutionary psychologists hastened to check it in the two ways just described (see Buss 2000, 57-60), and it actually did not pass the test.<sup>5</sup> This kind of eagerness to pit one’s own pet hypothesis against the rival theory in a highly risky empirical confrontation is certainly the last thing we would expect to see in Dr. Pangloss’s followers.

## CONCLUSION

In recapitulation, my analysis of the Darwinian account of men’s sexual jealousy (that was picked out as representative of contemporary evolutionary theorizing in psychology) did not disclose anything remotely resembling a just-so story. Rather, what has emerged is a hypothesis that is congruent with some basic principles of evolutionary theory, and that has survived several tests that have exposed it to the risk of empirical disconfirmation. Therefore, when Paul Griffiths criticizes evolutionary psychology of emotions on the grounds that its adaptive scenarios are typically “so easy to come by that they provide little or no support for the existence of the features they predict” (Griffiths 1997, 110), and that an equally good alternative story is usually available (*ibid.*), his diagnosis is contradicted by the detailed exploration undertaken in this article.

All in all, then, the result of our single-case oriented examination of evolutionary psychology is that in the end, it receives a clean bill of health. Although, admittedly, it has occasionally suffered from some youth-related diseases there are no signs of any alarming, malignant or life-threatening condition. But despite there being no special reasons for worry, just for the peace of mind of everyone involved, it is advisable to schedule regularly further medical (and methodological) check-ups.

## NOTES

1. This "flight from generality" explains why the usual names of Tooby and Cosmides do not loom large in this article. For reasons to be explained later, my search for an appropriate test-case hypothesis has led me to a theoretical context where the work of other authors is in the foreground.

2. The idea here is, of course, that the best explanation is probably true (as argued, for example, in Lipton 1990, 2000).

3. Of course, harems are common among many mammal species but they are not necessarily an exception to this rule. For, in nonhuman animals, even when males appropriate access to many females, paternal care can be absent because the males may not take care of the young at all.

4. To strengthen the evolutionary hypothesis about male human jealousy as an "instantiation" of proprietary behavior (PB), one would have to show with more detailed modeling that in the ancestral environments fitness-increasing consequences of this trait (concerning paternity) did offset its possibly negative effects (e.g., the fact that fits of jealousy can antagonize the partner, or make violent conflict, injury, or even death more likely).

5. Another weakness of the double-shot hypothesis is that it leaves totally unexplained why men and women have these different beliefs in the first place. For other methodological objections raised against this hypothesis, see Buss et al. (1996b).

## REFERENCES

- Buss, D. M. 1999. *Evolutionary psychology: The new science of the mind*. Boston: Allyn and Bacon.
- . 2000. *The dangerous passion: Why jealousy is as necessary as love and sex*. New York: Free Press.
- Buss, D. M. et al. 1996a. Sex differences in jealousy: Evolution, physiology and psychology. *Psychological Science* 3:251-55.
- Buss, D. M. et al. 1996b. Sex differences in jealousy: Not gone, not forgotten, and not explained by alternative hypotheses. *Psychological Science* 7:373-75.
- Buss, D. M. et al. 1999. Jealousy and the nature of beliefs about infidelity: Test of competing hypotheses about sex differences in the United States, Korea, and Japan. *Personal Relationships* 6:125-50.
- Buunk, B. P. et al. 1996. Sex differences in jealousy in evolutionary and cultural perspective: Tests from the Netherlands, Germany and the United States. *Psychological Science* 7:359-63.
- Clutton-Brock, T. H. 1991. *The evolution of parental care*. Princeton: Princeton University Press.
- Daly, M., and M. Wilson. 1988. *Homicide*. Hawthorne, NY: Aldine de Gruyter.
- DeSteno, D. A., and P. Salovey. 1996. Evolutionary origins of sex differences in jealousy: Questioning the "Fitness" of the Model. *Psychological Science* 7:367-72.
- Diamond, J. 1992. *The rise and fall of the third chimpanzee*. London: Vintage.
- Ford, C. S., and F. A. Beach. 1951. *Patterns of sexual behavior*. New York: Harper & Row.
- Geary, D. C. 1998. *Male, female: The evolution of human sexual difference*. Washington, DC: American Psychological Association.



- Geary, D. C. 2000. Evolution and proximate expression of human paternal investment. *Psychological Bulletin* 126:55-77.
- Ghiglieri, M. P. 2000. *The dark side of man*. Reading, MA: Perseus Books.
- Griffiths, P. E. 1997. *What emotions really are: The problem of psychological categories*. Chicago: The University of Chicago Press.
- Kummer, H. 1995. Causal knowledge in animals. In *Causal cognition: A multidisciplinary debate*, edited by Dan Sperber, David Premack, and Ann James Premack, 26-39. Oxford: Clarendon Press.
- Lipton, P. 1990. Contrastive explanation. In *Explanation and its limits*, edited by D. Knowles, 247-66. Cambridge: Cambridge University Press.
- Lipton, P. 2000. Inference to the best explanation. In *A companion to the philosophy of science*, edited by W. H. Newton-Smith, 184-93. Oxford: Blackwell.
- Mealey, L. 2000. *Sex Differences: Development and Evolutionary Strategies*. San Diego: Academic Press.
- Morell, V. 1998. A new look at monogamy. *Science* 281:1982-83.
- Solomon, R. C. 1998. The politics of emotions. *Midwest Studies in Philosophy* 22:1-20.
- Symons, D. 1992. On the use and misuse of darwinism in the study of human behavior. In *The adapted mind*, edited by J. H. Barkow, L. Cosmides, and J. Tooby, 137-62. New York: Oxford University Press.
- Wilson, M., and M. Daly. 1992. The man who mistook his wife for a chattel. In *The adapted mind*, edited by J. H. Barkow, L. Cosmides, and J. Tooby, 289-322. New York & Oxford: Oxford University Press.
- Wilson, M., and M. Daly. 1996. Male sexual proprietariness and violence against wives. *Current Directions in Psychological Science* 5:2-7.
- Wright, J. 1998. Paternity and paternal care. In *Sperm Competition and Sexual Selection*, edited by T. R. Birkhead and A. P. Møller, 117-45. San Diego: Academic Press.

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