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## Chapter 11

### **Evolutionary Psychology and the challenge of adaptive explanation.**

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## A. Introduction

I (RG) often start my undergraduate lectures on evolutionary psychology with the following question, “Who thinks humans are animals?” After a few moments of reflection (and a bit of prompting from me that people are certainly not vegetables or minerals), most students agree that humans are indeed animals. I then ask, “How many people think humans are the product of evolution?” Typically around 80% of the class think humans are products of evolution. However, when I then ask, “Given that humans are products of evolution, how many of you think that human behavior can be explained in evolutionary terms?” only a few brave souls raise their hands. This ambivalence to evolution and evolutionary explanations is not restricted to Auckland undergraduates. Many biologists, who routinely explain all sorts of biochemical, morphological, and behavioral aspects of organismal diversity in evolutionary terms, balk at explaining human behavior and cognition in the same way (Ahouse & Berwick, 1998, Gould, 1997; Lewontin, 1998). Coyne (2000) expresses this view with considerable vigor,

The latest deadweight dragging us (evolutionary biology) closer to phrenology is evolutionary psychology, or the science formerly known as sociobiology. If evolutionary biology is a soft science, then evolutionary psychology is its flabby underbelly.

Why are many evolutionary biologists comfortable giving evolutionary explanations to differences in lactate dehydrogenase activity in humans, but decidedly uncomfortable with evolutionary explanations of human rape? To some it appears that these evolutionary biologists suddenly become creationists or cultural determinists when the topic turns to human behavior (Sarich, 1995). However, another possibility exists. The view we defend in this chapter is that there is nothing wrong *in principle* with taking an evolutionary approach to human behavior or cognition. In practice, however, the impoverished view of evolution and psychology adopted by many Evolutionary Psychologists, and the weakness of their empirical science, is frankly rather embarrassing (see Lloyd, 1999; Lloyd and Feldman, 2002 for a similar perspective on Evolutionary Psychology).

Before proceeding any further we should emphasize that the target of our critique here is *not* a broad, comparative evolutionary approach to psychology (evolutionary psychology or “evolutionary

psychology in the round” (Heyes, this volume). Instead, our attack is confined to the specific program of Evolutionary Psychology associated with the “Santa Barbara church of psychology” (Laland & Brown, 2002). Evolutionary Psychology (EP) is a nativist approach to cognition that views the human mind as a collection of modules design by natural selection to solve the problems faced by our Pleistocene ancestors. This program was christened in the *Adapted Mind* book (Barkow et al, 1992), and proselytised to the lay public in Steven Pinker’s (1997) modestly titled book *How the Mind Works*. Its followers have applied EP doctrines to everything from social reasoning to preferences for green lawns and certain genres of erotic fiction (“slash” novels – see Symonds & Salmon, 2001). EP’s narrow focus on unique human adaptations, a monomorphic view of the mind, massive modularity, and a cartoon view of the Pleistocene environment have all been the subject of considerable critique (Fodor, 2000; Griffiths 2001; Hull 2002; Irons, 1998; Karmiloff Smith 2000; Samuels, 1998; Stolz and Griffiths, in press). Here we assess how successful EP has been in meeting the challenge of that most quintessentially Darwinian task – adaptive explanation.

### **A. The Challenge of Adaptive Explanation**

EP is adaptationist and proud of it. The standard explanatory strategy followed by Evolutionary Psychologists has been dubbed “reverse engineering” (Pinker, 1997). In normal engineering humans attempt to design solutions to problems. In “reverse engineering” Evolutionary Psychologists take current features of human cognition and posit that they are adaptive solutions shaped by natural selection to problems posed by life back in our Environment of Evolutionary Adaptedness (Tooby & Cosmides, 1992). This might be a good explanatory strategy if three criteria were commonly satisfied:

1. all traits were adaptations
2. the traits to be given an adaptive explanation could be easily characterized
3. plausible adaptive explanations were difficult to come by.

If these criteria apply then a plausible adaptive explanation of a trait would have a high probability of being correct. Taken to an extreme, if only one plausible adaptive explanation was possible, then any adaptive explanation encountered would have to be correct. Sadly, life is not so easy for

evolutionary biologists. The challenge of adaptive explanations is that all three of these criteria are frequently violated.

### ***Are All Traits Adaptations?***

Evolutionary theory has changed considerably since Gould and Lewontin (1979) attacked the Panglossian program that regarded “natural selection as so powerful and the constraints upon it so few that...its operation becomes the direct cause of nearly all organic form, function, and behavior” (pp. 582-583). Even the most ardent adaptationists do not defend the claim that all features of organisms are the direct products of natural selection. (See Dawkins, 1986 for a defense of what Godfrey Smith [2001] terms “explanatory adaptationism” rather than panadaptationism. Nowadays processes such as genetic drift, pleiotropy, and epistasis are well known and the evolutionary literature replete with discussions about spandrels and exaptations and analyses of developmental constraints and phenotypic integration. The debates within evolutionary biology are thus not about the existence of these phenomena, but rather about their relative importance and our ability to disentangle their effects.

### ***What Is the Trait?***

The correct characterization of traits to be given adaptive/historical explanations is by no means obvious or straightforward. Lewontin (1978) illustrated this with the now “classic example of the human chin.” Although the chin may seem like an obvious trait, present in humans and absent in our ape relatives, the chin is actually not a discrete trait at all. It is the consequence of different degrees of neoteny in the dentary and alveolar growth fields of the lower jaw. Individuating traits in terms of the problems they are solutions to does not solve this difficulty. The same issues apply to correctly characterizing environmental problems. For example, Sterelny and Griffiths (1999) observe that mate choice could be considered as either a single problem (choose a good mate) or an ongoing mosaic of problems (choose a mate with the ideal waist-to-hip ratio, choose a mate who is kind, choose a mate who is rich, choose when to be unfaithful to your mate, choose when to desert your mate). The correct classification of traits to be given adaptive explanation is thus an important and nontrivial issue.

### ***Are Adaptive Explanations Hard to Come By?***

If the first two criteria pose some difficulties, the third is even worse. Plausible adaptive explanations are far from rare. Don Rosen once quipped that only two factors seem to constrain adaptive explanations – the inventiveness of the author and the gullibility of the audience (Rosen, 1982, p. 271). Although this might be a little harsh, it is certainly not uncommon to have numerous plausible adaptive explanations for a trait. For instance, the series of large bony plates found on the back of the dinosaur *Stegosaurus* were variously proposed to be an adaptation to avoid predation, attract mates, or cool the body (Lewontin, 1978). Similarly, speculations on the adaptive significance of evolutionary increases in hominid brain size include ecological problem solving (Clutton-Brock & Harvey, 1980), the aquatic ape hypothesis (Morgan, 1982), serial cognitive processing in throwing objects (Calvin, 1983, 1993), the social brain hypothesis (Byrne & Whiten 1988), and thermoregulation (Falk, 1990). The fact that a plausible adaptive explanation can be conceived of does not therefore mean that it must be correct. In evolutionary biology this is where the real work starts, not ends.

Therefore, given the obvious reality of adaptive design (Lewontin, 1978), how do evolutionary biologists tackle these issues? How do they meet the challenge of adaptive explanation? A quick scan through recent issues of the journals such as *Evolution* or a flick through books such as Rose and Lauder's (1996) *Adaptation*, or Orzack and Sober's *Adaptation and Optimality*, reveals that evolutionary biologists today deploy a combination of engineering style optimality models, developmental studies, and phylogenetic analyses to tackle the challenges of adaptive explanation. Developmental and phylogenetic studies help appropriately identify traits and investigate the range of phenotypic variation available for selection to act on. They can also be used to analyze the extent to which traits are coupled, and thus able to be independently optimized by selection, and infer the relative timing of evolutionary novelties. Optimality models help identify aspects of good design in an explicit quantitative manner (Orzarck & Sober, 2001; although see Gray [1987] and Kennedy & Gray [1993] for some concerns about the empirical success of this approach). Most significantly of all, comparative tests based on explicit phylogenetic methods are

now routinely used to discriminate between competing adaptive as well as non-adaptive explanations.

A few examples will emphasize this point. Consider the following “promiscuous primate” hypothesis. Profet (1993) proposed that menstrual bleeding, far from being an inconvenient quirk of biology, is actually an evolved mechanism to rid the uterus and oviducts of sexually transmitted pathogens. She noted that this hypothesis predicts that species with promiscuous female mating systems should have more pronounced menstrual bleeding. This was a strikingly novel and plausible hypothesis. Unfortunately for true believers in the infallibility of reverse engineering, this prediction is not correct. Strassman (1996) conducted a rigorous comparative test of the “promiscuous primate” hypothesis. She mapped promiscuity and menstrual bleeding on to an estimate of primate phylogeny based on morphological and molecular data. She used a concentrated changes test to see if menstrual bleeding was more likely to evolve in promiscuous species. She found no statistical association between the two variables. Strassman argued that the phylogenetic distribution of menstrual bleeding was consistent with an alternative mixed adaptive and incidental by-product explanation. She suggested that given the high cost of maintaining endometrial tissue, it would be more efficient to regress and regenerate the endometrium in a cyclic fashion. As an incidental consequence of this cycling, pronounced menstrual bleeding would occur in species with too much tissue and blood in the endometrium to be reabsorbed. This, she argued, explained why menstrual bleeding correlated with phylogeny rather than promiscuity. The copious bleeding in chimps and humans was a consequence of the large size of the uterus in relation to the body mass of these closely related species.

It could be argued that although physiology might involve a complex mix of adaptation and spandrels, surely for obvious morphological or behavioral adaptations it must be much easier to characterize the selective pressures that have shaped them. The “hammerhead” or “cephalofoil” of hammerhead sharks provides a salutary warning to those who think that evolutionary explanation ends with a plausible story. This structure is a paradigm organ of apparent adaptive complexity. The lateral expansion of the head involves substantial changes to the visual, olfactory, and electric

field sense organs. It had been suggested that directional selection led to incremental increases in the relative size of the cephalofoil through time. Martin (1993) tested this plausible hypothesis by mapping relative head width on to an mtDNA phylogeny of Hammerhead sharks and their close relatives (fig. 11.1). In contrast to the incremental expansion scenario, he found that the basal *Eusphyra* genus had the largest relative head width. Relative head width remained reasonably constant over the rest of the tree except for the derived bonnet head lineage (*Sphyrna tiburo*), in which it was reduced. Martin (1993) suggested this surprising result might reflect the interplay of two selective forces, - one to increase lift and maneuverability, and the other to enhance prey detection by separating the sense organs. He outlined a way this more complex adaptive scenario could be investigated using comparative methods.

[Insert Figure 11.1]

Let us be very clear here. We are not making an anti-adaptationist argument. We are simply arguing that a plausible story is not enough to meet the challenge of adaptive explanation. Lest it seem that we are arguing that evolution is always perverse (“natural selection works in mysterious ways”), we finish this section with a case drawn from our own work where a plausible scenario was supported by phylogenetic analyses. Van Tets (1965) suggested that wing-waving displays found in Pelecaniforms were derived initially from flight intention movements. These flight intention movements were incrementally elaborated into sky-pointing and then into various forms of wing-waving (fig. 11.2a). We tested this hypothesis by mapping van Tets’ scenario as an ordered character state on to a tree based on osteological and DNA/DNA hybridization data (Kennedy, 1996). The incremental evolution scenario fits the tree extremely well, requiring only two more character state changes than the minimal value. A randomization test showed that this result was much fewer than would be expected due to chance. Recent phylogenetic analyses of Pelecaniform mtDNA sequences (Kennedy, 1999; Kennedy, Spencer & Gray, 2000) further support this initial result, and require only one character state change in addition to those proposed by van Tets (fig. 11.2b).

[Insert Figure 11.2a & Figure 11.2b]

### **3. How does Evolutionary Psychology meet the challenge of adaptive explanation?**

In the introduction to his classic 1966 book on adaptation, G.C. Williams, the patron saint of Evolutionary Psychologists, notes,

The ground rule - or perhaps doctrine would be a better term - is that adaptation is a special and onerous concept that should be used only where it is really necessary.

This seems to be one doctrine that members of the Santa Barbara church are reluctant to follow. The ability of EP to test adaptive explanation in a rigorous manner by deploying comparative tests is severely constrained by Evolutionary Psychology's doctrinaire focus on unique human adaptations. In contrast to the strenuous efforts of current evolutionary biologists to use adaptive explanations with care, much of EP consists of little more than folk wisdom with a *post hoc* adaptive story (e.g., Buss, 1994). This is exactly why many evolutionary biologists find their psychological cousin more than a little embarrassing. In the interests of saving evolutionary biologists from future embarrassment we would like to propose two tests – the Grandparent Test and the Lesser-Spotted Brown Gerbil Test. The grandparent test is a filter for folk wisdom with a plausible post hoc story. It asks, “Does this work give us any insight into human behavior and cognition beyond popular knowledge?” The Lesser-Spotted Brown Gerbil Test asks, “Would this research be publishable in major international journals if the species was a small noncharismatic mammal rather than our own?” Many studies in evolutionary psychology fail these basic tests (e.g., Buss [1994]; Thornhill & Palmer [2000]; see Coyne [2000] and Coyne & Berry [2000] for critiques). Rather than focus on the worst examples of EP in action, we would like to examine two areas where EP appears to have passed at least the Grandparent Test, and perhaps the Lesser-Spotted Brown Gerbil Test as well. Two of the areas where EP has gone beyond mere folk wisdom, and appears to have generated real insight into human psychology, are attractiveness judgments and cheater detection.

#### **4. Adaptive Preferences for Female Body Shape?**

A quick scan through films and fashion magazines reveals that slim is beautiful for women in current Western culture. For example, the body-mass index of fashion models is substantially lower than that of normal women (Tovee et al., 1997). Folk wisdom therefore suggests that weight should be of primary importance in Western male judgments about female body shape. Not so,



says Evolutionary Psychologist Devendra Singh. Although Singh (1993) agrees that weight has some influence on attractiveness judgements, he argues that there is a more reliable cue to a woman's potential reproductive success: her waist-to-hip ratio. He cites evidence showing that low waist-to-hip (WHR) ratios are correlated with youthfulness, good reproductive physiology, and long-term health. He suggests, therefore that, "WHR could act as a wide first-pass filter, which would automatically exclude women who are unhealthy or have low reproductive capability" (Singh 1993, p. 304). Singh goes on to argue that

WHR, the first filter, should be culturally invariant in its significance and its relationship to female attractiveness. The fact that WHR conveys such significant information suggests that men in all societies should favour women with a lower WHR over women with a higher WHR for mate selection or at least find such women sexually attractive. (1993, p. 305).

At first glance this appears to be an excellent example of EP in action. It certainly flies past the grandparent test. Although the idea that an "hourglass" female body shape is attractive is not exactly novel, Singh goes beyond folk wisdom in three important ways. First, he predicts the optimally attractive WHR (0.7). Second, he predicts that this variable is of primary importance (it will act as a "first-pass filter"). Finally, he predicts that it will be a culturally invariant feature of male psychology.

Does this relatively novel, quantitative claim about an evolved psychological basis for male mate selection pass the Lesser-Spotted Brown Gerbil Test? Again, at first glance it does. Singh and colleagues have marshalled an impressive range of empirical studies purporting to show that, regardless of the existing cultural variability of the desirability of plump or thin body types, there is a male preference for female figures with a WHR of 0.7. They appear to demonstrate that this preference is robust across cultures and varying age groups (Singh, 1993; Singh & Luis, 1995; Singh & Young, 1995; Henss, 2000). The primary way they have investigated WHR preferences is by asking their subjects to rank line drawings of women (in bathing suits) on the basis of their attractiveness and a number of other similar variables such as healthiness and youthfulness. The drawings contained 12 different body shapes – four waist-to-hip ratios (0.7, 0.8, 0.9, 1.0) x three

weight classes (“underweight”, “normal”, “overweight”). Across all the weight classes WHRs of 0.7 seems to be the most preferred.

Putting quibbles aside about the ecological validity of this task, even researchers studying the behavior of the lesser-spotted brown gerbil would be impressed with such a robust effect. However, they might also wonder whether this result might be an artifact of the experimental design. Two major methodological concerns have recently been raised about the studies based on Singh’s line drawings. Tovée and associates (1999) pointed out the line drawings varied WHR within each weight category by altering the width of the torso. This not only changes the WHR of the stimuli but also the apparent BMI. The stimuli thus confound WHR differences with changes in BMI and so the apparent preference for a WHR of 0.7 could simply be preference for slim body shape. Tovée and colleagues (1998, 1999) investigated the relative importance of these two variables using color photographs of real women dressed in leotards and leggings with their faces obscured. Strikingly, they found that although WHR and BMI were both statistically significant predictors of attractiveness ratings, their effect sizes were vastly different. Variation in BMI accounted for around 74% of the variance, whereas WHR account for about 2%. Folk wisdom 74, Evolutionary Psychology 2.

Tassinary and Hansen (1998) have noted another problem with Singh’s line drawings – there are no WHRs below 0.7. It is possible therefore that men might prefer even lower WHRs, but are constrained to select the predicted value of 0.7 by the absence of these body shapes. Tassinary and Hansen (1998) presented their subjects with a set of 27 newly created line drawings, each varying systematically and independently in weight, waist size, and hip size; the lowest WHR being 0.5. They found no consistent preferences for any particular WHR, but found that overweight figures were consistently ranked as least attractive. Tassinary and Hansen’s new stimuli have been criticized as less naturalistic and appealing than Singh’s (1993) original line drawings (Henns, 2000). It is also possible that the large number of stimuli they used may have reduced the reliability of their participants’ rankings because of discrimination problems (Mehrens & Lehman, 1978), and so reduced the impact of any WHR preference. To investigate this possibility, we (MH

and RG) – (Heaney, 2000) conducted a study with line drawing based on Singh's (1993) original stimuli, but manipulated the waist to include figures with WHR's as low as 0.5 (fig. 11.3). The overwhelming preference of our 147 University of Auckland undergraduate male subjects was figures in the "normal" weight range with a WHR of 0.5. The next most highly ranked shape was a WHR of 0.6 in the "normal" weight range. Only eight out of the 147 subjects assigned their top rank to figures with a WHR of 0.7 in the "normal" weight range. Just two men preferred any of the figures in the "overweight" class.

[Insert Figure 11.3]

Given that WHRs of 0.6 and 0.5 are not normally attainable - without the help of a corset or some other form of body enhancing gadgetry - evolutionary explanations might struggle to account for this seemingly "nonadaptive" preference. Why then, were stimulus figures in the "normal" weight range with waist-to-hip ratios of 0.5 and 0.6 ranked as the most attractive body shapes? One potential biological explanation is that this task may tap into a generic psychological mechanism of enhanced responding to exaggerated features, or "supernormal" stimuli (Eibl-Eibesfeldt, 1970). Enhanced responsiveness to supernormal stimuli has been documented in a variety of species. Male fireflies, for example, prefer models of fireflies that contain a larger illuminated area and larger amounts of yellow than is contained in the light of females of their own species. Similarly, when given the choice, the ringed plover prefers to roll eggs that are four times larger than its own into its nest, even though the egg is too big for the plover to sit on and incubate adequately. Finally, the parasitic behavior of the European cuckoo, whereby it lays its eggs in the nests of other birds who then proceed to feed and raise the cuckoo hatchling, often to the detriment of their own young, provides another good example of species responsiveness to supernormal stimuli. The capacious open mouth of the young cuckoo elicits stronger feeding reactions in the foster parent than does the hungry, open mouths of its own young (Eibl-Eibesfeldt, 1970). The WHR of healthy reproductively aged women typically ranges from 0.67 to 0.80, whereas in healthy men it ranges from 0.85 to 0.95 (Singh, 1993). The preference for extremely low WHRs found in our study therefore may be caused by this generic psychological response to supernormal stimuli. WHR is a

signal sexual difference. Exaggerating this difference elicits stronger preferences. No specific evolved psychological mechanism for adaptive mate selection needs to be invoked.

Defenders of the EP faith might object that this generic response to supernormal stimuli will produce adaptive outcomes when faced with the normal range of female body shapes. This response misses the point repeatedly made by Gould and Lewontin (1979). Not all useful outcomes are produced by specific adaptations. There is no need for natural selection to mold a specific WHR selection mechanism when a generic mechanism already produces the same result. This argument does not deny that enhanced responding to supernormal stimuli may be an adaptation. It merely emphasizes that it is more likely to be an adaptation for a general domain of evolutionary “problems,” rather than a specific adaptation for mate choice. This domain-general mechanism might exhibit considerable plasticity in the stimuli that trigger it. In some cultures, or for some men, it might be triggered by low WHRs and in others large breasts. The relevant input signaling sexual difference could be shaped by fashion. Culture and biology could work together.

The final nail in the coffin for the WHR theory has been the recent demonstration that WHR preferences are far from culturally invariant. Yu and Shepard (1998) found that Matsigenka men in an isolated area of Peru prefer the “overweight” figures. Within weight classes they preferred figures with the highest rather than the lowest WHR. One man from Yomybato commented that the “overweight” figure with a WHR of 0.9 was healthy, whereas the others must have had diarrhoea or fever or were “almost dead.” Similarly, Westman and Marlowe (1998) found that the Hadza men of Tanzania did not prefer a WHR of 0.7. Rather, Hadza men did not even consider WHR when selecting potential mates: They were more interested in the weight of potential mates and expressed a preference for heavier females.

None of the problems that have been discovered with the WHR theory mean that the initial idea was sloppy science. It was certainly better than much of the folk psychology with *post hoc* adaptive stories that passes for evolutionary psychology (e.g., Buss, 1994). What has been poor is the lack of critical evaluation of the evidence for this theory. Paper after paper and book after book has

touted this research as an excellent example of EP in action (Buss, 1999; Miller, 2000; Miller & Todd, 1998; Pinker, 1997). If EP really is to meet the challenge of adaptive explanation, then its proponents should require more from their classic cases than a plausible story.

### 5. Adaptive Reasoning?

To understand how an organ such as the kidney works, it is beneficial to understand its purpose and what factors influenced its design. EP posits that the mind contains similar cognitive “organs” whose functions can be best understood by knowing their evolutionary “purpose” or selective history. Perhaps the most prodigious example of this logic in action is found in the social exchange algorithm credited to Leda Cosmides (1989).

Cosmides’ theory of a social exchange algorithm grew from the work of cognitive psychologists on a test called the Wason Card Selection Task. When given the original Wason Task, a generalized *if P, then Q* rule (Fig 11.1, 11.4a) people do poorly (Wason, 1966). This is of interest because perfect performance can be attained by the application of two simple rules. Turn over the E (*P*) to check there is an even number (*Q*) on the other side (*Modus Ponens*) and turn over the 3 (*not Q*) card to check there is not a vowel (*P*) on the other side (*Modus Tollens*). People tend to select the E and the 4 (“affirming the consequent”). Either the human mind gets confused by this rather simple propositional calculus or the presumptuous human mind has decided that it’s less important to disprove the law than to check its strength.

When faced with a specific situation, such as breakers of social laws, where it becomes more appropriate to select violations in the rule, rather than adherence to the law, people adopt an ethologically appropriate strategy and seek out these law breakers. This was shown when Griggs and Cox (1982) found a version of the Wason task where people did provide responses that coincided with the logically correct selection. This task was orientated around a drinking-age problem (Fig. 11.1, 11.4b). In this task, rather than making the typical mistake and ensuring that all people who have met the requirement (being over 19) are enjoying the benefit (drinking beer),

people check that no one who did not meet the requirement (people under 19) are enjoying the benefit (beer).

[Insert Figures 11.4a & 11.4b]

This effect was later explained in evolutionary terms by Cosmides (1989). Game Theory modeling had shown that for cooperation between individuals to hold, spread, and persist within a population one must be able to detect people who are cheaters (Axelrod & Hamilton, 1981). Cosmides argued that due to this consistent pressure humans had evolved a cognitive mechanism for cheater detection. She made certain predictions about how such a mechanism should behave. It should only be evoked specifically by social exchange situations, where there is a requirement, benefit, and cheater (as these were identified as the important variables by the game theory modeling). This mechanism also should be able to detect violators even in novel social exchange situations where it fails to detect violations in non social exchange situations.

This model is an example of the potential of evolutionary psychology. It focuses a large body of research under the banner of a unifying theory, makes clear predictions, and appears to provide novel insight into human cognition. It flies past the grandparent test. However, the cheater detection effect is not the uncontested triumph for EP it is sometimes claimed to be. In the next two sections we shall argue that experiments on this effect are seriously confounded, and so open to alternative explanations. We conclude that the designation of cheater detection as an adaptation is premature.

***A mechanism not dependent on Requirement, Benefit and Cheater.***

It is worth noting that, although the original Wason task was a measure of logical reasoning, by introducing the cheater context one has changed the nature of the task. People simply pick the cards based on the context scenarios, regardless of the logical instruction (Cosmides, 1989) and even in the absence of it (Fiddick, Cosmides, & Tooby, 2000). The card selection is determined by the context and not by any application of the *if, then* rules. So the question becomes are there any other context factors that could be causing people to pick out the correct cards? Maybe people, in

some contexts, try to test the strength of a law (if a car is red, then it is fast; if a car is fast then it is red), whereas in others they are prompted to test for exceptions to the rule (if a car is red, then it is fast; if a car is slow, then it should not be red). Both are valid strategies depending on the context in which they are placed.

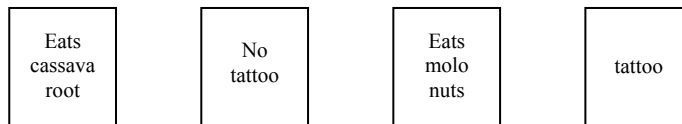
Knowing that the rule is largely irrelevant, and that selection is made on the basis of the context, an alternative hypothesis to a Social Exchange Algorithm explanation is that cheater detection scenarios have some general properties that cause exception-testing cognition (see Table 11.1 for some examples). It is the context of the scenario that determines which aspects of the law are relevant to test (Sperber, Cara, & Girotto, 1995). This explains the cheater-induced correct selection on the Wason task, but not support the idea that there is an evolved device for the detection of cheaters. If these properties were general to many cognitive processes, then this would discredit the idea that an evolved algorithm was responsible for cheater detection. If the removal of these properties from a cheater scenario caused the cheater detection effect to disappear, then empirical support for Cosmides' theory evaporate.

[Insert Table 1]

In accordance with what they termed relevance theory, Sperber, Cara, and Girotto (1995) have presented a three-part "recipe" to ensure correct card selection in the Wason task. This recipe specifies that: the P-and-not-Q case is easier to mentally represent than the P-and-Q case (underage drinkers versus legal-age drinkers); the P-and-not-Q case should be of more importance than P-and-Q case (breakers of the law versus followers of the law) and that the rule should be clear and unidirectional (there is no implication that legal-age drinkers *should* be drinking beer). In an earlier paper, these authors have shown that cheater facilitation can be demonstrated in the absence of the cheater effect if these specifications have been met (Sperber et al., 1995).

Given that, at the very least, the results of cheater detection experiments are able to be interpreted in a manner that does not require cheater detection cognition, why is this theory so frequently touted as the jewel in EP's crown? Perhaps part of the answer is that the benefit of only altering one variable and holding everything else constant seems to have escaped the investigators of this field. Cosmides controlled for familiarity and content by creating complex, wordy scenarios of both

cheater detection and descriptive natures. An example of such is the Cassava root problem used by Cosmides (1989) and again by Gigerenzer and Hug (1992). In the cheater situation, tattoos are given to men when they marry and they may then eat the aphrodisiac cassava. In the noncheater version, men get tattoos when they marry and usually move to the south where the cassava root primarily grows. The same cards (shown below) were presented for both scenarios accompanied with the rule “If a man eats cassava root, then he must have a tattoo on his face.”



An examination of the cassava root problem looking at extraneous influences on selection strategy revealed several confounds. Liberman and Klar (1996) noted several differences between the cheating and normal scenarios. First, in the noncheating scenario, there is no specific violating instance: it could be a man with no tattoo eating a cassava root or a man with a tattoo eating molo nuts that is the exception to the rule. Second, whereas the rule for cheating is strict and bears no exception, the rule for noncheating employs such terms as “usually” and “primarily”. (Both of these confounds serve to reduce the importance of the P- and not-Q alternative). Third, the noncheating rule is more easily interpreted as bidirectional. If a man lives in the north he will eat the cassava root and if he eats cassava root he will live in the north. In the cheating version it is clear that everyone wants to eat the aphrodisiac cassava and no one wants to eat the bad tasting molo nuts.

When these confounds were reversed, although preserving the basic cheater architecture (cheater, requirement, benefit), Liberman and Klar (1996) were able to completely dissociate the cheater detection effect across the range of scenarios used by Gigerenzer and Hug (1992), with noncheating scoring the 70% (typical of cheating conditions), and cheating scoring 30% (typical of non-cheating conditions). When more vigorous efforts are made to keep everything but the essence of cheating between control and experiment, the effect completely disappears across the five studies used by Gigerenzer and Hug (1992), including the cassava root scenario used by Cosmides (1989). Clearly, this indicates that the enhanced performance is not the result of the cheating scenario but of the often associated confounds.



Cheater scenarios have intrinsic to them the properties that seem to evoke exception-testing cognition (summarized in Table 11.1), a cheater is more *salient* than a law follower, and the breaker of a contract is of more importance than someone who has met the requirement for something but not yet cashed in on the benefit (which also has the benefit of making the rule clear and unidirectional). It is for these reasons, and not sloppy experimental control per se, that cheater scenarios have and will produce correct selection. But the finding that the “recipe” alone can produce correct selection and the removal of the “recipe” in cheater scenarios results in incorrect selection, indicate that the observed phenomenon was not the result of a cheater detection module, or even cheater detection itself, but a correlation with other variables, best summarized by the Sperber et al “recipe.”

A further problem lies in the perception of what the altered performance is actually measuring. Sperber and Girotto (this volume) argue that because the people ignore the question in social Wason tasks, such tasks are best conceptualized as two-feature categorization tasks, where subjects pick out people who have taken the benefit and those who have not met the requirement. They compared this to other two feature tasks (a glider would be a “flying vehicle” and “have no engine”) and found that people have very little difficulty in responding accurately.

### ***Evidence for a modular evolutionary adaptation?***

The saga of cheater detection contains another cautionary tale for Evolutionary Psychology. Essential to the design of the mind is the ability to create new cognitive processes in response to the environment *within one's lifespan*. In situations where the same environmental pressures that allegedly shaped a module to suit life on the savannah are still active today (as they are with cheater detection), much more stress needs to be placed on proving that the resulting cognition cannot be the product of the flexible mind. Cosmides and her supporters present two arguments in support of the notion that cheater detection is a specifically evolved characteristic rather than a product of learning. First, they claim that evolved modules function independently of general

processing, are automatic, generally opaque to awareness, and domain specific. Second, they argue that evolved modules are common to humankind and found across cultures (Cosmides & Tooby, 1997). The implication that learning (or broader experiential effects) cannot produce similar outcomes is spurious.

A classic example of the distinction between implicit and explicit cognitive function is to ask bicycle riders what they would do if their bike started tilting to one side. Many bikers respond that they would lean to the other side to right themselves. This is incorrect and would result in the person falling off the bike every few meters. Surprisingly, people seem to be quite capable of riding over long distances with no mishap. When placed on a bike, if it starts to tilt, people turn the handlebars, using their momentum to right their center of gravity, avoiding the fall. This function is independent of general processing, exists without awareness, and is specific to the bike-riding domain. So do we have an evolved bike-riding module? The answer is obviously no. Obvious because there is an apparent learning phase to bike riding where people do fall off every few meters (and, yes, because of the paucity of bicycles in the Pleistocene). In an area where the learning phase is less apparent, and the Pleistocene pressure more plausible, such “modules,” created by the most fundamental of learning processes, can readily but incorrectly be heralded as evolved traits.

Cosmides and Tooby's second method of inferring adaptation is equally problematic. Just because a behavioral trait is found across cultures does not necessarily mean it is a product of adaptive evolution. Comparative psychologists have emphasized for decades that species-specific behaviors can arise through species-specific patterns of experience (Gray, 2001; Gottlieb, 1976; Lerhman, 1970). People the world over eat soup out of a bowl and not off of a plate. Gravity acts the whole world over and people adjust their behavior in light of this. The whole world over there is a benefit to cheating (providing you don't get caught) and a benefit to being able to know when you're being cheated. The fact that cheater detection is crosscultural does not automatically mean it is an evolutionary adaptation.

Over the last 10 years cheater detection theory has arguably been the best example of a Stone Age module in our modern mind. The idea was very seductive. However, as we have repeatedly emphasized much more than a plausible story is needed to meet the challenge of adaptive explanation. Perhaps the most disturbing indictment for EP as a discipline is that in the last 10 years there has been no convincing argument as to why this relatively simple ability has to be the product of evolution and not learning.

## **Conclusion**

In its enthusiasm to repudiate behavioral creationism and social construction EP has embraced a cartoon version of Darwinism. However, we are not suggesting that psychologists should abandon Darwinism and the power of adaptive explanation. On the contrary, we believe that the future for evolutionary psychology lies in taking the challenge of adaptive explanation much more seriously. Dispensing with the current exclusive focus on unique and allegedly universal human adaptations is an essential prerequisite for this improved adaptationism. Hypotheses about unique features (autapomorphies in the jargon) cannot be subject to comparative tests. A broader evolutionary psychology would include comparative tests both across a range of species, and within our own species (Griffiths, 2001). Behavioral and cognitive evolution did not begin, nor abruptly end, in the Pleistocene. It would also be helpful if Evolutionary Psychologists abandoned their *a priori* commitment to other dogmas such as massive modularity and the monomorphic mind. Evolutionary biologists know that the extent of both phenotypic integration and heritable variation are empirical issues, and so should Evolutionary Psychologists. Finally, in the move from Evolutionary Psychology to evolutionary psychology, psychologists could use studies of behavioral and neural development to characterize appropriate traits for adaptive explanation in the same way the evolutionary biologists currently link developmental and evolutionary analyses. In short, in rising to the challenge of adaptive explanation, evolutionary psychologists need to act less like evangelists and more like current evolutionary biologists.

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## Figure captions

**Figure 11.1** mt DNA phylogeny of the Hammerhead sharks (adapted from Martin, 1993).

Average head widths (scaled as a percentage of body length) are shown beside each taxon. Head width does not increase from smallest in the basal lineages of the phylogeny to largest in *S. tiburo*.

**Figure 11.2.** (a) van Tets' hypothesis for the sequence of evolutionary changes in Pelecaniform male advertising displays. (b) The most parsimonious reconstruction of display changes mapped onto a maximum likelihood tree (Kennedy, 1999). The displays are overlain on the branches they are inferred to have changed on. The numbers correspond to the displays labelled in Fig. 11.2a. These changes all represent the gain of a new display state, except at point 8, where the cross through the display represents a loss.

**Figure 11.3.** The line drawings used for Heaney's (2000) study of male WHR preferences. These drawings are adapted from Singh (1993). Unlike Singh's figures, they include waist-to-hip ratios below 0.7. The waist-to-hip ratio range is from 0.5 to 0.9.

**Figure 11.4.** An adaptation of (a) Wason's (1966) original task, and (b) the Griggs and Cox (1982) "drinking problem" version of the task. The letters below the cards in italics indicate the role of each card in the *if P, then Q* rule and would not normally be present.