



Evolutionary psychology: the emperor's new paradigm

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For some evolutionary psychology is merely a field of inquiry, but for others it is a robust paradigm involving specific theories about the nature and evolution of the human mind. Proponents of this paradigm claim to have made several important discoveries regarding the evolved architecture of the mind. Highly publicized discoveries include a cheater-detection module, a psychological sex difference in jealousy, and motivational mechanisms underlying parental love and its lapses, which purportedly result in child maltreatment. In this article, I argue that the empirical evidence for these 'discoveries' is inconclusive, at best. I suggest that, as the reigning paradigm in evolutionary psychology has produced questionable results, the evolutionary study of human psychology is still in need of a guiding paradigm.

Introduction

What is evolutionary psychology? It depends on whom you ask. Some say it's simply the study of human behavior and psychology from an evolutionary perspective [1]. So conceived, evolutionary psychology is a *field of inquiry*, like mechanics, which is defined not by any specific theories about human behavior and psychology, but by the kinds of question it asks about them. But several prominent researchers – led by Tooby and Cosmides [2], Pinker [3], and Buss [4] – have argued that an evolutionary perspective on human behavior entails specific doctrines regarding the nature and evolution of the human mind. For these researchers, evolutionary psychology is a *paradigm*, akin to Newtonian mechanics, consisting of interwoven theoretical claims, methodological commitments, and empirical results. I will call this paradigm 'Evolutionary Psychology' (capitalized) to distinguish it from the field of inquiry ('evolutionary psychology').

The goal of Evolutionary Psychology is not simply to discover the evolutionary causes of psychological traits, but actually to discover our psychological adaptations [2,4,5]. Evolutionary Psychologists argue that our psychological adaptations are 'modules', or special-purpose 'minicomputers' [6], each of which evolved during the Pleistocene to solve a problem of survival or reproduction faced by our hunter-gatherer ancestors [5,7]. Evolved modules are purportedly discoverable by 'reverse engineering' the mind from the vantage of our Pleistocene past,

figuring out the adaptive problems our ancestors faced and then hypothesizing the modules that evolved to solve them [2,5]. Evolutionary Psychologists claim several discoveries using this approach, including a cheater-detection module [8,9], a sex difference in jealousy [10–12], and motivational mechanisms that cause parents to abuse stepchildren more than genetic children [13–15].

In this article, I will examine the principal pieces of evidence cited in favor of these three 'discoveries' and indicate why I think the evidence fails to support Evolutionary Psychology's claims. My arguments, however, are intended to be suggestive, not definitive. Interested readers, especially those bent on rebuttal, should consult the detailed arguments I provide elsewhere [16].

Detecting cheating in the evidence for modularity

It is widely agreed that the evolution of reciprocal altruism – the mutual exchange of fitness benefits – creates selection for *cheaters*, non-reciprocators who accept the benefit of another's altruistic act without providing a benefit in return. The evolution of cheaters, in turn, selects for the ability to detect cheaters. Accordingly, Cosmides [8,9] hypothesizes that the human mind is innately equipped with a 'cheater-detection module'.

Evidence for Cosmides' hypothesis derives from studies with the Wason selection task, in which subjects are given a conditional 'rule', *if P, then Q*, together with four two-sided cards displaying information of the form *P, not-P, Q*, and *not-Q* (see Box 1). Subjects are instructed to turn over the cards necessary to determine whether the rule holds. The correct solution is to turn over the cards displaying *P* and *not-Q* to see whether their other sides contain *not-Q* and *P* respectively, because those, and only those, cards can violate the rule.

Evidence for a cheater-detection module

The principal evidence for a cheater-detection module consists of two results, each purportedly inconsistent with the hypothesis that the mind reasons with general logical principles.

First, there appears to be a 'content effect' in the selection task (Box 1A). The frequency with which subjects select the correct cards varies widely, apparently as a function of the frequency with which *not-Q* is selected. This performance differential appears to indicate that cognition is sensitive to the *content*, not the logical form, of conditionals. Cosmides [8] claims that analysis of a wide range of results reveals that subjects perform best when

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Box 1. Detecting cheating in the Wason selection task

A. The content effect

As the following examples show, the frequency with which subjects choose the correct cards in Wason selection tasks appears to vary as a function of the content of the problems.

Abstract problem [17]

Select the card(s) that you definitely need to turn over to determine whether the following rule is true (see Fig. 1):

If a card has an 'R' on one side, then it has a '2' on the other side.
(P) (Q)

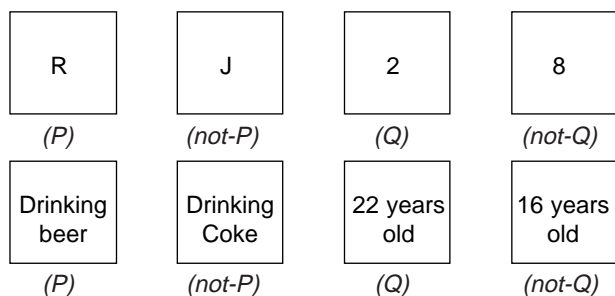


Fig. 1. The cards used in the abstract problem (top), and 'social contract' problem (bottom). The logical classification of problem elements shown in parentheses did not appear in the tasks given experimental subjects. See main text for discussion.

'Social contract' problem [18]

Select the card(s) that you definitely need to turn over to determine whether someone is violating the following rule:

If a person is drinking beer, then that person must be over 19 yr of age.
(P) (Q)

The frequencies of card selections in these two versions of the task are shown in Table I.

Table I. Frequencies of various card selections (in percentages)

Problem	Card(s) selected			
	P alone	P and Q	P and not-Q	Other
Abstract [17]	33	46	4	17
'Social contract' [18]	20	0	73	7

B. The 'switched conditional' selection task

Cosmides [8] gave two groups identical instructions, but gave one group a conditional that was 'switched' around from the standard social-contract conditional given to the other group.

Instructions: You are a member of an island culture in which men get facial tattoos upon getting married. The island has a native plant called 'cassava root', an aphrodisiac that makes men who eat it irresistible to women. Since sex between unmarried people is taboo on the island, the island's elders have enacted the following rule:

Standard rule

If a man eats cassava root, then he must have a tattoo on his face.
(P) (Q)

'Switched' rule

If a man has a tattoo on his face, then he eats cassava root.
(P) (Q)

Which cards must you turn over to determine whether someone is breaking the rule? (see Fig. 1Ia). Cosmides [8] found little difference in card selections between the two groups (Fig. 1Ib).

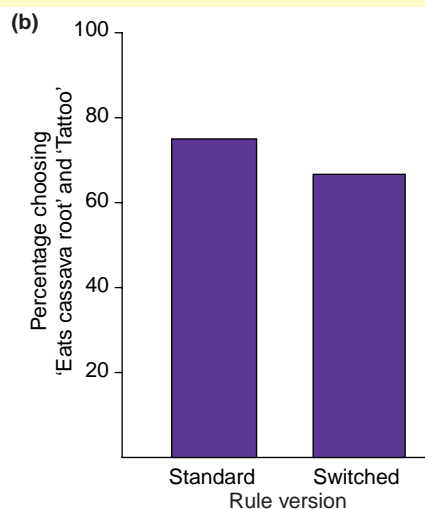
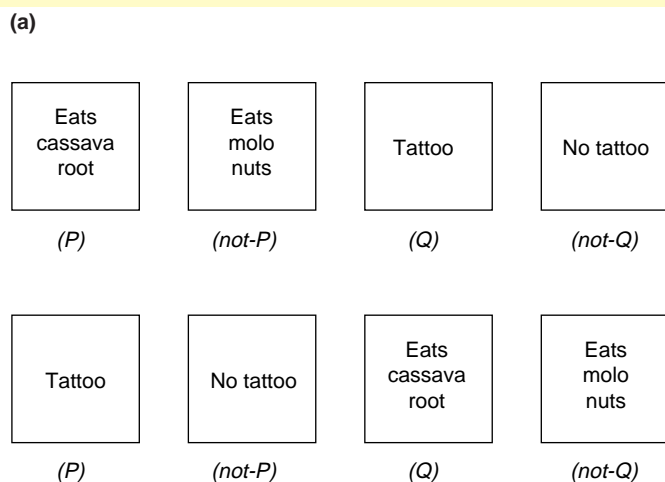


Fig. 1I. (a) The cards used in the selection task using the standard rule (top), and the 'switched rule' (bottom). (b) Card selection was found to be similar in the two versions of the task (modified from [8]).

reasoning about violations of conditionals that express 'social contracts' (situations in which an obligation is incurred in receiving a benefit). And this purportedly indicates cognitive specialization for detecting cheating (accepting a benefit without fulfilling the requisite obligation).

Second, when the cards representing the correct solution differ from those representing cheating, subjects

appear to ignore logic and choose the cards that represent cheating. For example, Cosmides [8] gave two groups identical instructions, but gave one group a conditional that was 'switched' around from the social-contract conditional given to the other group (Box 1B). She found that most subjects chose P and not-Q in the standard social-contract version of the problem, but that nearly as many chose the same cards in the 'switched' version,

Box 2. Indicative versus deontic 'conditionals'

Arguments for the cheater-detection module presuppose that all *if* sentences have the same logical form (see [20]). But *if* sentences actually come in a variety of logical forms [21]. The two logical types that have figured prominently in selection tasks are 'indicative' and 'deontic' *if* sentences. Indicatives, such as (1) below, make *factual assertions* that one state of affairs, expressed by *Q*, is conditional upon another state of affairs, expressed by *P*. By contrast, deontics, such as (2), *impose obligations* that one bring (or have brought) about the state of affairs expressed by *Q* under the condition expressed by *P*.

(1) If **it's snowing in Chicago**, then **my flight has been cancelled**.

(*P*)

(*Q*)

(2) If **you drive a motor vehicle**, then **you must have a valid driver's license**.

(*P*)

(*Q*)

As the logical form of a sentence determines its entailment relations, differences in the entailment relations of indicative and deontic *if* sentences demonstrate a difference in their logical forms. Consider: The negation of (1) entails '*it's snowing in Chicago but my flight hasn't been cancelled*' (*P* and *not-Q*), which entails '*it's snowing in Chicago*' (*P*); but the negation of (2) entails only '*you needn't have a valid driver's license to drive a motor vehicle*', which does *not* entail '*you drive a motor vehicle*' (*P*).

Indicatives are genuine conditionals, the logical form of which involves two propositions and a dyadic propositional operator (the *if*); but deontics are not logically conditional [22]. Deontic *if* sentences *categorically* impose obligations in their 'consequents' (which contain a monadic deontic propositional operator, such as '*must*', and the proposition, *Q*, on which it operates), while expressing in their 'antecedents' (*P*) the conditions under which the obligation is in force. Thus, whereas the logic of (1) is *if P, then Q*, the logic of (2) is *must Q (under the condition that P)*.

despite their being the logically incorrect *not-P* and *Q* cards. Thus, Cosmides concludes, subjects do not apply logical principles in solving social-contract selection tasks, but simply focus on whether someone has accepted a benefit without fulfilling an obligation.

The results prove nothing

Neither result, however, is evidence of a cheater-detection module, for both results are compatible with a non-modular mind that reasons with general logical principles.

First, the idea that there is a content effect in selection tasks falsely presupposes that all conditionals have the same logical form [19]. Indeed, the so-called content effect is typically induced by pairing 'indicative conditionals' with 'deontic conditionals' in selection tasks. But these have different logical forms and warrant different patterns of inference (see Box 2). Moreover, because deontic conditionals impose an obligation to make (or have made) *Q* true, the prohibition against *not-Q* is more perspicuous in selection tasks involving deontic conditionals. Accordingly, attention is immediately drawn to the *not-Q* card, and the frequency with which it's selected increases [22]. Thus, differential performance on Wason selection tasks actually demonstrates a 'logic effect': subjects apply general logical rules in solving selection tasks, but apply different rules to indicative and deontic conditionals (as they would apply different rules to conditionals and conjunctions). Furthermore, differential performance on many selection tasks is actually an artifact of using indicative conditionals that express *arbitrary*, not simply abstract, relationships between *P* and *Q*. When subjects are given indicatives expressing *practical* relationships between *P* and *Q* (as in (1) in Box 2), the frequency of correct responses equals that for deontics, with 69–90% of subjects choosing *P* and *not-Q* [23–25].

Second, the idea that subjects ignore the logic of conditionals to focus on cheating falsely presupposes that changing the wording of conditionals in selection tasks changes how subjects mentally represent their logical forms [26]. In Cosmides' 'switched' problem, subjects were presented with a background story that

clearly required *Q* of those of whom *P* is true, but were then asked to evaluate compliance to *if Q, then P*, which not only didn't make sense in the context of the background story, but also didn't contain an obligating 'must' (see Box 1B; cf. [8], p. 217). Under such circumstances, language-comprehension mechanisms process the conditional together with the background information and output a mental representation of the logical form of the conditional that makes sense given the background information (as we do, for example, when we represent the logic of the expression 'all is not lost' as 'not all is lost'). Subjects' inferences then deploy the mental representation of logical form, not the experimenter's wording. Thus, Cosmides' 'switched conditional' experiments fail to show that subjects selected the 'logically incorrect' cards; subjects simply selected the logically correct cards relative to a sensible interpretation of the problem [27,28]. (Similar arguments apply to results obtained by Gigerenzer and Hug [29] and Fiddick *et al.* [20].)

Sex differences in jealousy

Buss argues that jealousy evolved as an emotional alarm that signals a partner's potential infidelities and causes behavior designed to minimize losses of reproductive investment. But infidelities pose different problems for the two sexes, Buss claims [10]. For men, a female's sexual infidelity entails the potential fitness costs of parental investment in another male's offspring. For women, it is a male's emotional involvement with another woman that potentially entails fitness costs in the form of lost parental resources. Thus, Buss hypothesizes, there is an evolved sex difference in the 'design features' of the jealous mind [12]: the male mind is designed to respond primarily to cues of sexual infidelity, and the female mind is designed to respond primarily to cues of extrapair emotional involvement ('emotional infidelity').

The principal evidence for this hypothesis is a sex difference in responses to 'infidelity dilemmas' (Box 3). Questionnaire studies with infidelity dilemmas always find that more men than women report the thought of a

Box 3. Infidelity dilemmas

To test for a sex difference in jealousy, Buss *et al.* [10] administered the following questionnaire to American subjects. This questionnaire, or a minor variant of it, was subsequently used in studies in five additional societies. (The identifiers of emotional and sexual infidelity, shown in brackets, were not given in the original questionnaire.)

Instructions: Please think of a serious committed romantic relationship that you have had in the past, that you currently have, or that you would like to have. Imagine that you discover that the person with whom you've been seriously involved became interested in someone else. What would distress or upset you more? (*please circle only one*):

Dilemma 1

(A) Imagining your partner forming a deep emotional attachment to that person [emotional infidelity].

(B) Imagining your partner enjoying passionate sexual intercourse with that other person [sexual infidelity].

Dilemma 2

(A) Imagining your partner trying different sexual positions with that other person [sexual infidelity].

(B) Imagining your partner falling in love with that other person [emotional infidelity].

The percentages of respondents choosing sexual infidelity as more distressing than emotional infidelity in surveys in the USA, and subsequently in other countries, are shown in Tables I and II.

Table I. Percentage choosing (B) sexual infidelity as more upsetting in Dilemma 1 (by survey sample)

	Survey sample											Avg.
	USA [10]	USA [12]	USA [30]	USA [31]	USA [32]	USA [33]	China [32]	Netherlands [30]	Germany [30]	Korea [12]	Japan [12]	
Male	60	76	61	55	53	73	21	51	28	59	38	51
Female	17	32	18	32	23	4	5	31	16	18	13	22

Table II. Percentage choosing (A) sexual infidelity as more upsetting in Dilemma 2 (by survey sample)

	Survey sample									Avg.
	USA [10]	USA [12]	USA [30]	USA [34]	Netherlands [30]	Germany [30]	Korea [12]	Japan [12]		
Male	44	43	44	47	23	30	53	32	38	
Female	12	11	12	12	12	8	22	15	13	

partner's sexual infidelity to be more distressing than the thought of a partner's emotional infidelity.

The questionnaire data are questionable

However, this sex difference, in itself, does not confirm Buss's hypothesis. Buss claims that men focus on cues to sexual infidelity because of potential cuckoldry, whereas women focus on cues to emotional infidelity because of potential withdrawal of parental resources. That there should be a sex difference is a by-product of these primary entailments of Buss's hypothesis. To confirm the hypothesis, it is necessary to confirm these primary entailments – to confirm, for example, that males care more about sexual infidelity *than they do about emotional infidelity*, not simply that they care more about sexual infidelity *than females do*. But the data don't show this. Indeed, on average, only half (51%) of male subjects chose sexual infidelity as more distressing than emotional infidelity in response to one dilemma (Box 3, Table I), and 62% chose emotional infidelity over sexual infidelity in response to the other (Box 3, Table II).

In addition, homosexual men are even less likely than heterosexual women to choose sexual infidelity as more upsetting than emotional infidelity [35–37], which fails to conform to the predicted sex difference in the evolved 'design features' of the mind. Moreover, Harris [38] found that, although heterosexual males who were asked to imagine their partners' having sex with another male showed greater physiological arousal than those who were asked to imagine their partners' forming an emotional attachment to another man, males who were asked to imagine having sex with their own partners showed just as great a physiological arousal as those imagining being cuckolded. This indicates that males

exhibit greater reactivity to imagining events with sexual content *in general* than to imagining events with emotional content. Thus, Buss's questionnaire results are confounded by males' greater reactivity to sexual than to emotional scenarios (cf. [10], p. 255).

An alternative explanation

Rather than indicating a sex difference in the evolved 'design features' of the mind, the data on the whole indicate a difference in sex-typical, learned, situation-specific beliefs about the likelihood that a sexual infidelity portends abandonment (see [16], Chap. 6). For it is well-established that female sexual infidelity is strongly correlated with dissatisfaction in the primary relationship, whereas male sexual infidelity is not [39–44]. Female sexual infidelity therefore signals a greater threat to a relationship than does male sexual infidelity, and more men than women accordingly find sexual infidelity more distressing. This explanation is further supported by the low numbers of German and Dutch men choosing sexual infidelity as the more distressing (Box 3, Tables I and II); female sexual infidelity is not as strong a signal of potential abandonment in these cultures, because they 'have more relaxed attitudes about sexuality, including extramarital sex, than does the American culture' ([30], p. 362). Finally, homosexual males, as a group, are less likely than heterosexual males or females to believe that sexual infidelity poses a threat to the primary relationship [37], which explains why they find sexual infidelity less distressing.

Discriminative parental solicitude

'Parental investment is a precious resource', Daly and Wilson argue, so 'selection must favor those parental

psyches that do not squander it on nonrelatives' [14]. As a result, motivational mechanisms of parental love have evolved to be triggered by (genetic) offspring. Once triggered, parental love serves as 'inhibition against the use of dangerous tactics in conflict with the child' [14]. Because evolved mechanisms of parental love are not triggered in substitute (non-genetic) parents, 'angry lapses of parental solicitude' in conflict situations more frequently elicit 'dangerous tactics' from substitute parents than from genetic parents [14]. Accordingly, Daly and Wilson predict that children cared for by step-parents will be at greater risk of maltreatment than children cared for by genetic parents [13].

The principal evidence cited in support of this hypothesis is Daly and Wilson's one-year study of child maltreatment in Hamilton-Wentworth, Ontario [13], in which they found that children living with a step-parent were far more likely to be maltreated than children living with both genetic parents (Table 1). Indeed, children under the age of 5 who lived with a step-parent were 40 times more likely to be maltreated than same-aged children living with both genetic parents.

Problems with Daly and Wilson's sample

Daly and Wilson's sample consisted of cases of 'maltreatment', which include not only physical abuse, but sexual abuse and acts of 'unintentional omission' considered as neglect by a child welfare professional. First, however, sexual abuse and physical abuse are distinct phenomena with distinct etiologies. Indeed, intrafamilial child sexual abuse is rarely accompanied by physical abuse [45], so it doesn't consist in 'the use of dangerous tactics in conflict with the child'. Second, the class of 'unintentional omissions' often includes allowing truancy and failing to secure a child with a seat belt in a car, which also do not involve 'the use of dangerous tactics in conflict' situations. Because Daly and Wilson claim that stepchildren are at greater risk because the 'inhibition against the use of dangerous tactics in conflict' is not triggered in step-parents, cases of *physical abuse*, rather than maltreatment generally, form the appropriate sample against which to test their hypothesis (see [16], Chap. 7).

Elliott Smith and I [16] analyzed cases of physical abuse of children by an adult in loco parentis, as reported in the Third National Incidence Study of Child Abuse and Neglect (NIS-3). We found a relative risk to children living with a step-parent that is significantly lower than the elevated risk of maltreatment found by Daly and Wilson (Table 2). For example, children under the age of 5 who lived with a step-parent were around 8 times more likely to be physically abused than same-aged children living with both genetic parents.

Table 1. Maltreatment risk for children living with a step-parent and a genetic parent, relative to children living with both genetic parents, by child's age ($N=99$)^a

0–4 yrs	5–10 yrs	11–17 yrs
40.1	19.4	9.8

^aStudy in Hamilton-Wentworth, Ontario, 1982–1983.

Table 2. Physical abuse risk for children living with a step-parent and a genetic parent, relative to children living with both genetic parents, by child's age ($N=726$)^a

0–4 yrs	5–10 yrs	11–17 yrs
8.2	3.2	3.4

^aNIS-3 study, USA, 1993.

Bias in official case reports

These results still appear to confirm Daly and Wilson's hypothesis. However, our study, like Daly and Wilson's, relied on official case reports of child abuse. And child welfare professionals often take the presence of a step-parent in the household to be partly diagnostic of whether a bruise or broken bone resulted from an accident or abuse [46,47]. Thus, the results of both studies are possibly confounded by a reporting bias against step-parents in official records.

Daly and Wilson reject this possibility on the grounds that, if there were a reporting bias, 'we would expect the bias, and hence the overrepresentation, to diminish as we focused upon increasingly severe and unequivocal maltreatment up to the extreme of fatal batterings' [14]. 'At the limit', they argue, 'we can be reasonably confident that child murders are usually detected and recorded' [48]. As Canadian police records showed that stepchildren under the age of 5 yrs were far more likely to be victims of fatal maltreatment than same-aged children living with both genetic parents [14,15], Daly and Wilson conclude that comparable findings regarding non-fatal maltreatment are not confounded by a reporting bias against step-parents.

But there are two counter-arguments to this conclusion, which call into question the representativeness of official case reports of child abuse and maltreatment. First, independent studies in four US states each found that only 40–50% of all child maltreatment fatalities, including inflicted injury fatalities, were coded as maltreatment fatalities on death certificates or in police records [49–52]. If only half of all maltreatment fatalities appear in official records, a study that relies on official records alone misses half the cases. Second, if there is a reporting bias against step-parents, its effects are due primarily to a reporting bias against 'live-in boyfriends' of the genetic mothers of abuse victims, as they account for the overwhelming majority of officially recorded maltreatment by step-parents [15,16,53]. However, analysis of Colorado records found that maltreatment fatalities at the hands of 'other unrelated' individuals, which included 'live-in boyfriends', were 8.71 times more likely to be recorded as maltreatment fatalities on death certificates than maltreatment fatalities at the hands of genetic parents [50]. If, as Daly and Wilson argue, the effects of any reporting bias should be less in cases of fatal maltreatment than in cases of non-fatal abuse, this degree of reporting bias in cases of fatal maltreatment implies a higher degree of reporting bias in cases of non-fatal abuse, which is then more than sufficient to account for the overrepresentation of stepchildren in the NIS-3 data (Table 2). Thus, the available evidence indicates that American physical abuse data are sufficiently confounded by reporting bias that they cannot confirm Daly and

Wilson's hypothesis. This suggests that their Canadian maltreatment data are similarly confounded and do not support their hypothesis.

Conclusion

I have suggested that the principal pieces of evidence cited in support of three of Evolutionary Psychology's 'discoveries' in fact fail to establish the claims of Evolutionary Psychology. I believe that *all* of the evidence cited in support of these 'discoveries' suffers similar evidentiary problems (see [16] for details). Moreover, I contend that Evolutionary Psychology's other 'discoveries' enjoy no better empirical support. For example, I argue elsewhere that there is no good evidence to support Buss's claims about evolved mate preferences ([16], Chap. 5) and a female psychological adaptation for strategic infidelity ([16], Chap. 6). Evolutionary Psychology's failure to produce solid empirical discoveries, I suggest, stems from problems with its theoretical framework – in particular, its reliance on 'reverse engineering' the mind from the vantage of our Pleistocene past, its assumption that the adaptational architecture of the mind is massively modular, and its doctrine of a universal human nature. Thus, although the Evolutionary Psychology paradigm is a bold and innovative explanatory framework, I believe it has failed to provide an accurate understanding of human psychology from an evolutionary perspective. The field of evolutionary psychology has yet to find a guiding paradigm and remains, in my opinion, an open frontier.

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