



FORUM

Reply to Smith et al.

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We are pleased that Smith et al. (2000) have drawn the attention of *Animal Behaviour's* readership to a broader range of evolution-minded research on human behaviour than we were able to do in our brief review article (Daly & Wilson 1999, hereafter D&W). We are disappointed, however, that their way of doing so is to contrast the alleged virtues of 'human behavioural ecology' (HBE) with the alleged sins of 'evolutionary psychology' (EP), caricaturing both.

In D&W, we defined our scope (page 509) as encompassing works in the human sciences that are both 'psychological', by virtue of their 'focus on how people acquire and evaluate information and how they use that information in behavioural decision-making', and 'evolutionary', by virtue of adopting an 'adaptationist, selectionist conceptual framework' (page 509). We called that domain 'human evolutionary psychology' (HEP), and we cited Darwin's (1872) *The Expression of the Emotions in Man and Animals* as its first instance. Thus, when Smith et al. say that HBE 'began in the 1970s' and has 'a considerably longer history than EP', they are simply defining EP differently, and when they take offence at our having included some HBE research under the rubric of HEP, the issue is again definitional rather than substantive. Indeed, most of Smith et al.'s complaints about the 'misleading' nature of our review hinge on their insistence that the phrase 'evolutionary psychology' (EP), even when used by us, really means what they say it means, regardless of how we defined it.

Well, perhaps our choice of the EP label was impolitic, although we took care to justify its appropriateness and to explain that we were not defining it exactly as others have. (As psychologists, we sometimes forget how offended other scientists can be when their work is described as 'psychological'!) And we grant that we should have made the word 'some' explicit rather than implicit when we wrote 'For present purposes, HEP encompasses [some] work by nonpsychologists . . .', so

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that Smith et al. would not have imagined an implicit 'all' in its place. However, we do not think that our differences are purely semantic. One more substantive question on which we apparently disagree is whether Smith et al.'s characterization of HBE versus HEP corresponds to a real dichotomy of thinking and methodology within the community of researchers pursuing an evolutionary analysis of human behaviour. We think not.

Smith et al. would presumably agree that their dichotomy does not distinguish two sets of people, because they cite different works by ourselves as exemplary of both approaches. So does the distinction reside in the research questions and methods? Smith et al. maintain that it does, but we question whether real research programmes match their abstract characterization of the alleged alternatives. Consider their example of 'fertility reduction via contraceptive technology in modern society' (page F23). Smith et al. assert that D&W 'simply assume it is maladaptive' (our actual statement was that contraceptive technology 'might (sic) have destroyed any association between reproductive success differentials and the proper functioning of psychological mechanisms'), and they contrast this with the superior HBE approach as follows: 'human behavioural ecologists take a number of analytic steps. They start by investigating whether the behaviour is currently adaptive . . .' The odd thing about this description of a prototypical HBE approach is that none of the people whom Smith et al. cite as having completed their series of 'analytic steps' has taken this first one. It is not even clear how they could, nor why they should.

How could one find out whether contemporary contraceptive use is 'currently adaptive'? If experimental subjects were given perforated condoms, or placebos in place of pills, maybe they would leave fewer descendant gene copies 100 years hence than a comparison group with more effective contraceptives, and maybe such a result would actually inspire a novel hypothesis about human 'phenotypic design' (which Smith et al. identify, on page F23, as the objective of their starting with such a test). But why bother running such a study even if you

could? The implications of any conceivable result would at best be murky, and there are lots of other ways to generate testable hypotheses about phenotypic design more quickly. Although Smith et al.'s accusation that we advocate a 'blanket rejection of fitness measures' (page F23) is untrue (as they well know, because they cite a paper of ours in which we found it useful to report reproductive data), we gladly plead guilty to harbouring the conviction that measures of attained fitness constitute one small part of the evolutionist's tool-kit and are more often than not the wrong tools for testing particular hypotheses derived from evolutionary (adaptationist, selectionist) models.

Smith et al. are not alone in their claim that a basic, defining step in behavioural ecological research is assessing 'whether the behaviour is currently adaptive', but this idea is a myth. Even with the recent explosion of DNA fingerprinting, only a minority of the papers appearing in journals such as *Behavioral Ecology* or *Behavioral Ecology and Sociobiology* report measures of attained fitness, and almost none address the question of whether animals are actually successful in choosing from the available alternatives the behaviour that maximizes their fitness. What these journals do in fact contain is highly psychological: studies of foraging decisions, mate choice criteria, predation risk assessment, rules governing the allocation of parental effort, and so forth. The human literature is not different in this regard. Smith's admirable 1991 monograph, *Inujjuamiut Foraging Strategies*, for example, contains nothing about the observed behaviour's reproductive consequences, and it is none the worse for the omission.

The two groups of researchers who call their fields 'HBE' and 'EP' have much more in common than Smith et al. imply. Both borrow heavily from the theoretical and empirical literatures of evolutionary biology and animal behaviour (which was one of the main points of D&W). Both address broad questions and narrow ones, using both formal and informal theorizing, sometimes well, sometimes poorly. Both groups occasionally do cross-cultural research, but mostly do not, concentrating instead on the sources of behavioural variation within one study population. Both treat such variation as the evolutionarily structured contingent responsiveness of a universal human nature, and usually analyse their data as if heritable variation were random noise, even though they know it may not be. Both groups treat human nature as a complex integrated set of adaptations.

Both groups of researchers rely heavily on utterances as data and are therefore vulnerable to various sorts of error (another focus of D&W's discussion); both use a variety of validity checks and unobtrusive measures in an effort to keep their reliance on self-report from becoming excessive. Both try to use multiple converging methods to address questions of interest, but have also been known to publish quick-and-dirty studies using a single dubious measure. Both are aware that proximate causation and selective history ('ultimate causation') are not isomorphic and take pains to distinguish them, yet sometimes fail. Both treat species-typical goals as proxies of expected fitness, and understand that the 'expected' in this phrase

refers not to cognitive expectation, but to some sort of statistical expectability in ancestral generations.

Both groups are also well aware that evolved adaptation is necessarily adaptation to ancestral environments, which may or may not differ significantly from current ones. Smith et al. obviously dislike the term 'EEA' (environment of evolutionary adaptedness), but we see little genuine difference in how the two groups treat this basic issue in their research and theorizing. Indeed, Hill is a co-author of one of the works (Eaton et al. 1994) that D&W cited as exemplary of how the EEA concept is and should be used. If there is a genuine disagreement here, we think that it again concerns the question of whether fitness measures are essential. Smith et al. accuse us of 'assuming' and 'asserting' that behaviour in novel environments is ubiquitously maladaptive, but we made no such assumption or assertion. The point that we and others have repeatedly tried to make is that determining whether contemporary behaviour is adaptive would often (not always) be useless. Understanding human vision as a suite of evolved adaptations, for example, will not be advanced by research designed to determine whether the sighted still outreproduce the blind in the modern world. It will be advanced by theory and research on the structure and function of human and other visual systems.

D&W also stressed that the EEA concept plays the same role in the design and interpretation of research in non-human behavioural ecology as it does in human research. Although it is seldom explicit (but see Coss et al. 1993; Byers 1997), consideration of the study animal's EEA is an implicit part of the interpretation of results, especially in experimental research where the issue of the 'ecological validity' of experimental manipulations is largely an issue of whether they mimic variations that the animal has evolved to track. Perhaps it is because psychologists are predominantly experimentalists whereas HBE research is more often observational and correlational that we differ from Smith et al. in the importance that we attach to the EEA concept, but in any case we cannot see that they have refuted our discussion. Kacelnik & Krebs (1997) used fly fishing to make the point that the feeding behaviour of trout is adapted to the past, not the present, and Smith et al. reply that it 'would be more useful' to assess whether modern trout confronted with anglers are still gaining more from striking at floating insects than they are losing. But 'useful' how? What could one infer about 'phenotypic design' from showing either that the trout are still choosing optimally or that they are not? In our opinion, not much. A more promising approach would be to ask whether trout residing in different watersheds with different histories of angling have evolved different response thresholds or decision rules. And if one were to conduct such a research programme, it might equally well be deemed a prototypical instance of 'behavioural ecology' or of 'evolutionary psychology'. In practice, they are pretty much the same thing.

Evolution-minded research on human behaviour, like other animal behaviour research, is vulnerable to a variety of threats to its validity. There is plenty of room for specific criticism of specific pieces of research and

theorizing. However, the claim that there are rival schools of thought or factions called EP and HBE, one of which is subject to egregious conceptual errors that the other avoids, does not correspond to the reality that we encounter in our reading or at the annual meetings of the interdisciplinary Human Behavior and Evolution Society. We are all in this together.

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