
Book Review

THE ADAPTED MIND

Edited by Jerome Barkow, Leda Cosmides, and John Tooby,
Oxford: Oxford University Press, 1992

This is an important book. Several of its authors have made original contributions to evolutionary theory (e.g., Daly 1978; Orians 1979; Cosmides and Tooby 1981; Tooby 1982); and many have studied behavior in nonhuman species, including nonhuman primates (Feistner, McGrew, Symons), kangaroo rats (Daly, Wilson), blackbirds (Orians), and dolphins (Mann). That makes them uniquely qualified to bring evolutionary theory to the study of psychology (see too Crawford, Krebs, and Smith 1987). These papers cover topics from social exchange cognition, to mate choice and retention, to pregnancy sickness, to language, to visual perception; the perspectives are often new, the work is often scholarly, and the conclusions are often persuasive.

But not persuasive enough. Contributors to this book use "selectional-thinking" (Symons, p. 141) and "evolutionary logic" (Wilson and Daly, p. 289) as "heuristics"—"providing aid or direction in the solution of a problem, but otherwise unjustified or incapable of justification" (O.E.D.). But neither Darwin's theory of natural selection, nor its recent modifications (e.g., in Fisher 1930; Williams 1957; Hamilton 1964), is "unjustified or incapable of justification." They are deductive theories, and give rise to falsifiable hypotheses (e.g., Darwin 1859: 135, 146, 148).

The Adapted Mind advocates two kinds of research (e.g., Tooby and Cosmides, and throughout). First is the identification of adaptation by "design." Second is the identification of adaptation by Pleistocene reconstruction. There are problems with both.

The problem with using "design" as a criterion for identifying adaptation was recently put concisely by George Williams. "Unfortunately those who wish to ascertain whether some attribute of an organism does or does not conform to design specifications are left largely to their own intuition, with little help from established methodology" (1992: 41; see too 1966: 260). "Design" assessments in *The Adapted Mind* are often after the fact; traits are judged too complex to be determined by chance, and natural selection is argued to be the probable determining force. That judgement will often be right (contrast Dawkins 1986 with

Received June 14, 1993.

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Ethology and Sociobiology 14: 397-402 (1993)
© Elsevier Science Publishing Co., Inc., 1993
655 Avenue of the Americas, New York, NY 10010

0162-3095/93/\$7.00

Gould and Lewontin 1979). But it can amount to putting God back into the study of evolution; God is the evolutionist in this case.

The problem with Pleistocene “modeling,” that is, with reconstruction of selective pressures in past environments, is that our knowledge of those environments is scant (e.g., Foley 1992). And what evidence exists on contemporary hunters and gatherers, even in the relatively few, relatively marginal habitats that remain, suggests a great range of variability (e.g., Service 1975). Detailed, long-term studies of living foragers show inconsistency with respect to mating (and probably breeding) system (e.g., contrast Hart and Piling 1960 with Hill and Hurtado 1994), fertility (e.g., Bentley et al. 1994), parental care (e.g., Hewlett 1992), and foraging (e.g., contrast Lee 1979 with Smith 1991), among other things. The “Pleistocene models” in this volume are often exceedingly facile; virtually none of the data from paleoanthropology or ethnography are used. As a result, to paraphrase Symons, the central hypothesis in Darwinian psychology appears to be that “human behavior will have a *surprising* number of Pleistocene precedents” (p. 154).

Two standards of evidence are better. One models optimality within species. The other uses the comparative method. The comparative method makes testable, falsifiable predictions about species’ fits with their environments—about their “designs” (e.g., Ridley 1983; Harvey and Pagel 1991). Several contributors to *The Adapted Mind* might strengthen their case for adaptation by comparison. Silverman and Eals, in their chapter on spatial ability, cite work by Gaulin and others linking sex differences in maze running to sex differences in range size in monogamous and polygynous species. Silverman and Eals’ own hypothesis, that sex differences in spatial ability are determined by sex differences in foraging strategy, might similarly be corroborated by comparisons across species. And Wilson and Daly, in their paper on sexual jealousy, note that “male sexual proprietariness is likely to evolve in any animal species with internal fertilization and paternal care” (p. 292). They cite Davies’ work on dunnocks and Møller’s on swallows for “illustration”; more systematic comparisons would shore up their assertion.

Optimality models make testable, falsifiable predictions about individuals’ fits with their environments—again, about their “designs” (e.g., Krebs and Davies 1991; Smith and Winterhalder 1992).¹ Many papers in *The Adapted Mind* could strengthen their case for adaptation by building and testing more elaborate models. Cosmides and Tooby, in their paper on cheater detection, end with a series of questions on how a

¹ This has been done under various labels, including behavioral ecology, evolutionary ecology, sociobiology, and ethology. I use “optimality modeling” here in referring to studies of a single species; but comparative studies model optimality as well (e.g., Harvey and Pagel 1991). Optimality models may be qualitative or quantitative, single or multivariate, depending on the problem and the data at hand (e.g., Parker and Maynard Smith 1990).

number of contextual variables might affect social exchange cognition. They ask: “Under what conditions should one cooperate with a person on a short-term basis, as opposed to a long-term basis? Should one’s willingness to tolerate cheating differ in short-term versus long-term relationships? . . . What role do groups and coalitions play in shaping patterns of assistance? What is the role of aggression, retaliation, and status?” (pp. 210–211). There is a body of evolutionary theory relating to these questions (e.g., Trivers 1971; Axelrod and Hamilton 1981; Nowak and Sigmund 1992); it could be used to predict how social cognition will alter with context. In another paper, Pinker and Bloom take issue with Gould and others in arguing that language is an adaptation, evidenced by its design. They write: “In one sense our goal is incredibly boring. All we argue is that language is no different from other complex abilities . . . and that the only way to explain the origin of such abilities is through the theory of natural selection” (p. 452). Again, it might be less boring, and more convincing, to show that language usage varies adaptively. A large literature exists on the evolution of communication (e.g., Dawkins and Krebs 1978; Krebs and Dawkins 1984; Harper 1991); it predicts how signals might change among different users in different contexts.

The best evidence that any trait is a product of selection is the demonstration that it contributes to, or contributed to, its bearers’ fitness. That might be done relatively directly, for example, by measuring lifetime reproductive success (e.g., Clutton-Brock 1988a). It might also be done by measuring some proxy, like the effects of wing-clipping on flight, or of clamp-cutting on mating (Williams 1992; Thornhill 1990), given a clear understanding of how the proxy relates to, or related to, fitness (e.g., Clutton-Brock 1988b). As Reeve and Sherman (1993) point out, the range of phenotypes compared can include simulated (e.g., Axelrod and Hamilton 1981) and experimental (e.g., Møller 1988) variants, as well as those which naturally occur. Profet’s is the only paper in *The Adapted Mind* to make its case for adaptation by citing direct evidence of fitness effects. She notes that “women who vomit or experience severe nausea during early pregnancy have lower risks of spontaneous abortion than women who experience only mild pregnancy sickness” (p. 327). Further, controlled studies showing that nauseated, pregnant women ingest fewer teratogens, particularly during fetal organogenesis, and bear live children with fewer birth defects, would strengthen her argument that pregnancy sickness is an adaptation to prevent the ingestion of toxins.

An a priori assumption that most traits are, in fact, vestiges—that they no longer raise their bearers’ fitness in modern environments—mandates a reconstruction of their effects on fitness (e.g., reproductive success) or fitness proxies (e.g., mating success) in the past, in order to show that they were once subject to selection. Editors of *The Adapted Mind* eschew measuring current utility (e.g., Tooby and Comides, p. 55). In their words, they avoid asking the question, “‘How is

Susan increasing her fitness by salting her eggs?’’ Instead they ask, ‘‘What is the nature of the evolved human salt preference mechanisms—if any—that are generating the observed behavior and how did the structure of these mechanisms mesh with the physiological requirements for salt and the opportunities to procure salt in the Pleistocene?’’ In my words, they’re asking, ‘‘How *did* Susan increase her fitness by salting her eggs?’’

Obviously, it is much more precise to measure current utility. As Williams, again, points out, the inadequacy of such an approach ‘‘arises when findings are negative, as must often happen in studies of human adaptations’’—though he concedes, elsewhere, that ‘‘it is surprising that human socioeconomic behavior in various twentieth century societies is as biologically adaptive as is commonly observed’’ (1992: 39, 98). Where behaviors fail to promote fitness, it may be because past selection failed to design adaptations (e.g., Dupré 1987), or because traits selected for in the past fail to develop or to function as designed in environments that are dramatically changed (e.g., Turke 1990a,b). Where current utility is demonstrated, Pleistocene modeling, among other things, can strengthen the inference that related behaviors also promoted fitness—were selected for—in the past (see Betzig 1989). Where current utility is not demonstrated, Pleistocene modeling, and other kinds of ‘‘phylogenetic reconstruction’’ (Tinbergen 1963), can suggest whether or not underlying traits were ever subject to selection.

The alternative to using the comparative method and optimality theory with precision is to use them with imprecision.

In spite of all of which, this book has the feel of scientific revolution. Much of the rhetoric is old (compare Tooby and Cosmides’ chapter on ‘‘The Foundations of Culture’’ with Alexander’s *Darwinism and Human Affairs*); but the audience is new. The next generation will build better models, use better methods, and draw more careful conclusions: It will move toward normal science (Kuhn 1962). That transition began in animal behavior decades ago. It began in anthropology a decade ago. In psychology, it’s happening now.

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