Anti-Science and the Pre-Darwinian Image of Mankind

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Yeguada, however, is an intriguing site. Looking at pollen and phytolith taxa, and particulate carbon from core samples for the period 14,300 to 8700 B.P., Piperno, Bush, and Colinvaux (1990) found at circa 11,000 B.P. a sharp rise in secondary growth taxa and a concomitant rise in levels of particulate carbon, with over 90% of the weed phytoliths exhibiting carbon coatings. They attributed the secondary vegetation and carbon influx to repeated human disturbance. However, *Cecropia* and *Heliconia* occur along lake and river edges in seasonally flooded forests undisturbed by humans. The carbon coatings do suggest burning, but it is difficult to guess why preagricultural humans would be taking the time to cut, dry, and burn vegetation. Some form of incipient vegeculture is a possibility, but until direct evidence of human occupation and land use is obtained, this site and others in Panama must remain equivocal and begging for further investigation.

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Anti-Science and the Pre-Darwinian Image of Mankind

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In accusing Napoleon Chagnon, Marvin Harris, and us of "biological determinism" and of sharing a "ratomorphic image of mankind," Clayton Robarchek (*AA* 91:903–920, 1989) displays profound misunderstanding of evolutionary biology, of psychology (which he advocates as biology's anti-discipline), and ultimately of the scientific enterprise.

According to Robarchek, ratomorphizing is rampant, and the principal villains are behavioristic psychologists, ethologists, sociobiologists, and ecologists. With the possible exception of Skinnerian behaviorists, his choice of targets could hardly be less apt. Evolutionists (ethologists, ecologists, and sociobiologists) are precisely the behavioral scientists who do *not* ignore the differences among animal species.

Behavioral ecology and sociobiology's pursuit of principles that can describe and account for the diversity of social phenomena in the living world is the antithesis of "superficial cross-specific analogizing" (p. 905). The collective agenda of evolutionists is to elucidate how such a seemingly simple creative process as evolution by selection can have given rise to life's diversity. Convinced that the unique attributes of any species, including humankind, cannot be understood without a comparative perspective, evolution-minded behavioral scientists pay explicit attention to the ways in which ecological and social variations have framed the diverse adaptive problems that species-typical psyches have evolved to solve (see, e.g., Daly and Wilson 1983). The assumption behind such theorizing and research is that all complexly organized attributes of living creatures (including the brains/minds that, in Robarchek's words, "actively construct reality") have been constructed by a history of natural selection. This is, by the way, hardly a radical assumption, since the only available alternative explanations for the adaptive design of psyches are anthropomorphic creation myths.

Ironically, the only "superficial cross-specific analogizing" to be found in Robarchek's article is committed by its author. One instance is his (unreferenced) citation of someone's failure to find a significant relationship between "dominance" and reproductive success in squirrel monkeys, as if this constituted a general disconfirmation of such relationships. Even if assertions of null hypotheses on the basis of hearsay could be taken as evidence for anything, whoever imagined that the same variables would account similarly for fitness variance in all animal species, regardless of social organization? The particular hypothesis against which Robarchek invokes this non-evidence concerned the evolved social psychology of "face and relative status" in human beings (Wilson and Daly 1985:61), not "dominance" in squirrel monkeys. More generally, Robarchek erects a lumpen-category of "lower animals," making all nonhuman animals alike in their otherness. This anthropocentric taxonomy has no scientific status. It is a vestige of religious ideology, persisting only as a prop for the continued exploitation of a creation over which we were given divine dominion.

We need, Robarchek maintains, "an approach that puts human beings back in the models, not as empty ciphers . . . but as active participants in their own destinies, as goal-directed decision makers picking their ways through fields of options and constraints" (p. 908). This isn't a bad description of the way behavioral ecologists and sociobiologists treat both humans and nonhumans. But Robarchek fails to grasp the complementarity of evolutionary and proximate causal accounts, and insists upon opposing them. He castigates us (Wilson and Daly 1985) on the one hand for evolutionary arguments that he deems incapable of articulation with "psychological-level motivational processes" (p. 908), and, on the other hand, for the "egregious reification" (p. 906) of suggesting that an evolved sex-, life-stage-, and circumstance-contingent "taste for risk" might affect activities as diverse as gambling and violence. The psychological constructs we invoked—"taste for risk," "competitiveness," the valuation of "face" and "honor"—may indeed prove to be too global, and may need to be supplanted by more domain-specific ones, but it is illogical to complain about the very enterprise of proposing such psychological constructs in one breath and to then demand psychological accounts in the next.

In advancing "human intentions and consciousness" as an (implicitly exhaustive) list of "the proximate causes of human action" (p. 904), Robarchek espouses a naive folk psychology discredited by decades of psychological science. In modeling decision processes, cognitive scientists and behavioral ecologists are largely (and appropriately) agnostic about consciousness: despite intense interest in consciousness, its nature and functional relevance remain unclear, as does the degree to which it deserves a privileged place in the explanation of human action. What is clear is that people do not need to be conscious of what they are doing in order to ascribe meanings and make decisions, nor do they necessarily have accurate retrospective insight into the causes of their own or anyone else's choices of action (e.g., Nisbett and Wilson 1977).

A central point of our 1985 paper was that lethal conflicts among poor young men arise from decision processes more "rational" than implied by the prevalent characterization of such conflicts as "trivial altercations." Robarchek alludes to our utilitarian analysis as if it were his discovery, then suggests that it renders an evolutionary functional account "redundant." In fact, we discussed utilitarian considerations at length because we were arguing (1) that sexual selection has shaped human decision-making mechanisms, and (2) that attention to theories of sexual selection can help scientists address such questions as "just *why* men should value intangible social resources like 'face' enough to risk deadly conflict over them" (Wilson and Daly 1985:60). In asserting that "Occam's razor" makes utilitarian and evolutionary accounts "alternatives," Robarchek fails to understand that the structure of the psyche and the reasons why that structure evolved are distinct, complementary issues.

The complementarity of proximate causal, developmental, and evolutionary explanations of behavioral phenomena has been evident to behavioral biologists at least since Tinbergen (1963). Robarchek propagates a myth when he asserts that "sociobiology's early proponents" made "extreme claims of genetic determinism," and that these "have been substantially moderated" by the acknowledgment of gene-environment interaction in development (p. 905). No one ever doubted such interaction. The questions of psychological interest are how behavioral control mechanisms are organized to process adaptively significant information and respond thereto, how they come to be so organized in ontogeny, and how and why they came to be so organized in phylogeny.

Perhaps Robarchek's incomprehension of the distinction between evolved mechanisms and the selection pressures that shaped them explains his fallacious supposition that adaptationist (sociobiological) explanations of contingent action require that the action be demonstrably fitness-promoting: since we do not demonstrate that homicide in Detroit is adaptive, our selectionist arguments are irrelevant and wrong. This is analogous to maintaining that humankind's sweet tooth must presently be fitness-promoting if evolutionary arguments are to be relevant to its explanation. Fitness is not a psychological goal. Psychological mechanisms have evolved as means to the end of fitness in the social and material environments of evolutionary adaptation. For Robarchek, Wilson and Daly's commonsensical proposition that competitive success has tended to contribute to fitness in evolutionary history is an "unjustified a priori" assumption (pp. 905–906), but his complaint is fantastical in the absence of a coherent theoretical account of why one might suppose these things to have been *un*coupled.

A subtext of Robarchek's critique is that the human mind has not been shaped by natural selection to do anything in particular, a point he has argued more explicitly elsewhere. According to Robarchek and Dentan:

> all human beings are, under the right circumstances, capable of violence, as they are of nonviolence or, for that matter, of picking up marbles with their toes. All three are "universals," but only in the sense that they are universal human behavioral *potentials* (whereas, for example, muscle-powered flight is not), along with capacities for an almost infinite number of other activities. [1987:361, emphasis in original]

At first glance, this argument is unexceptionable, but what is its intended implication? The apparent point is that the evolved psychological and morphological attributes of human beings are no more likely to exhibit functional design for violent conflict than for arbitrary actions like marble-picking. Nonsense. There are myriad reasons, both theoretical and empirical, to suppose that differential success in intraspecific violence was significant in hominid evolution (e.g., Alexander 1979; Chagnon 1988; Daly and Wilson 1988a; Trinkaus and Zimmerman 1982), and no reason to suppose that marble-picking capability is anything other than an epiphenomenon of other adaptations.

In the end, Robarchek's critique is an antiscience tract. The first clue is his use of language, with words like "determinism," "reductionism," "materialism," "mechanically," and "automata" deployed disparagingly. Robarchek calls for a more psychological approach, but psychological hypotheses are nothing more nor less than attempts to account for diverse action in terms of common mechanisms and processes. All psychologizing is therefore an exercise in "reductionism," "reification," and, yes, the dread "determinism," too, as is any attempt to see order in anything.

The second clue to Robarchek's anti-science agenda is his manifest contempt for data, for hypothesis generation and testing, and for the formulation of genuine alternatives. By considering how selection is likely to have affected social psychologies, we have discovered many novel homicide risk patterns in relation to age, sex, relationship, marital status, and economic circumstance (Wilson and Daly 1985; Daly and Wilson 1988a, 1988b, 1990). Robarchek doesn't gainsay the phenomena we demonstrate; doesn't criticize the selectionist arguments by which we predicted them; doesn't offer alternative explanations for them; doesn't offer any reason to doubt our ideas about how selection might have shaped the sex-specific life-span development of circumstance-contingent risk-proneness. He simply asserts that such theorizing "adds nothing to our understanding of either urban or jungle homicide" (p. 907).

Robarchek's alternative to hypothesis testing and data analysis is a single case description of a dispute settled without violence (although in the shadow of a threat thereof). How he imagines that this anecdote routs the ratomorphizers remains obscure. Did someone doubt that people often settle disputes nonviolently? The question his story raises, but does not begin to address, is why some disputes are resolved without recourse to violence and others are not. Progress toward an answer will only come from analyses of multiple cases, analyses that somehow transcend idiosyncrasy to identify the cases' abstract similarities and distinctions: reductionist analyses.

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"Agnostic about Consciousness"—Science, Anti-Science, and Ratomorphic Psychology: A Reply to Daly and Wilson

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While Daly and Wilson's view of science as nothing but hypothesis testing is untenable, their recognition of the importance of data and of hypothesis testing as central to the practice of science is a welcome improvement over their response to Knauft's (1987) testing and disconfirmation of a series of sociobiological hypotheses. Then they argued that, empirical evidence notwithstanding, "efforts to disconfirm sociobiology are . . . futile" (Daly and Wilson 1987:483).

To the charge of "superficial cross-specific analogizing," Daly and Wilson now respond with cross-specific homologizing. They object to what they call my "lumpen-category"

> of 'lower animals,' making all nonhuman animals alike in their otherness. This anthropocentric taxonomy has no scientific status. It is a vestige of religious ideology, persisting only as a prop for the continued exploitation of a creation over which we were given divine domination.

(Earlier, however, they maintained that "Evolutionists . . . are precisely the behavioral scientists who do *not* ignore the differences among animal species.") Contradictions and sanctimonious posturing aside, however, there is an issue here. From a perspective that sees human beings solely as biological machines, there may be no differences between people and rats (the ratomorphic image of mankind), but from a perspective that sees the essence of humanity as cultural, there is a chasm separating us from the rest of the animal kingdom. To those holding the former image of humanity, it may seem reasonable to be "agnostic about consciousness"; to those holding the latter view, it is oxymoronic.

Daly and Wilson's argument is rooted in a "commonsensical proposition that competitive success [implicitly, in violent conflict] has tended to contribute to fitness in evolutionary history." Any questioning of this assumption they see as "fantastical in the absence of a coherent theoretical account of why one might suppose these things to have been *un*coupled."

Revealed here is the assumption, central to many of these sociobiological just-so stories, that the modern human behavioral repertoire continues to reflect biological adaptations to specific (if entirely speculative) circumstances that were (allegedly) adaptively significant during some earlier (but unspecified) formative period of "evolutionary history."

The entire course of hominid evolution, however, has been in the direction of increasing and accelerating reliance on learned as opposed to (and at the expense of) genetically programmed behavior. The trend accelerated dramatically during the *Homo erectus* phase, permitting (and evidenced by) a rapid expansion of culture that permitted an explosive radiation into habitats previously closed to precultural hominids. The reason for this adaptive success is straightforward: learned, socially transmitted (cultural) behavior is capable of vastly greater flexibility than genetically programmed "behavioral control mechanisms." The two are, in fact, antithetical. The flexibility of culture, its primary adaptive advantage, is limited by the rigidity of innate responses. As culture became increasingly important as a means of coping with novel adaptive problems, that *in itself* constituted a powerful selective force, since cultural flexibility could be maximized only at the expense of genetically programmed behavior.

The impact of culture on biological evolution can be profound when the adaptive significance is great, as can be seen in the rapid coevolution of language and its associated biological structures. The selective forces exerted against genetically programmed behavior would have been at least as great, since the expansion of cultural behavior could have come only at its expense. *That* is how they got *un*coupled!