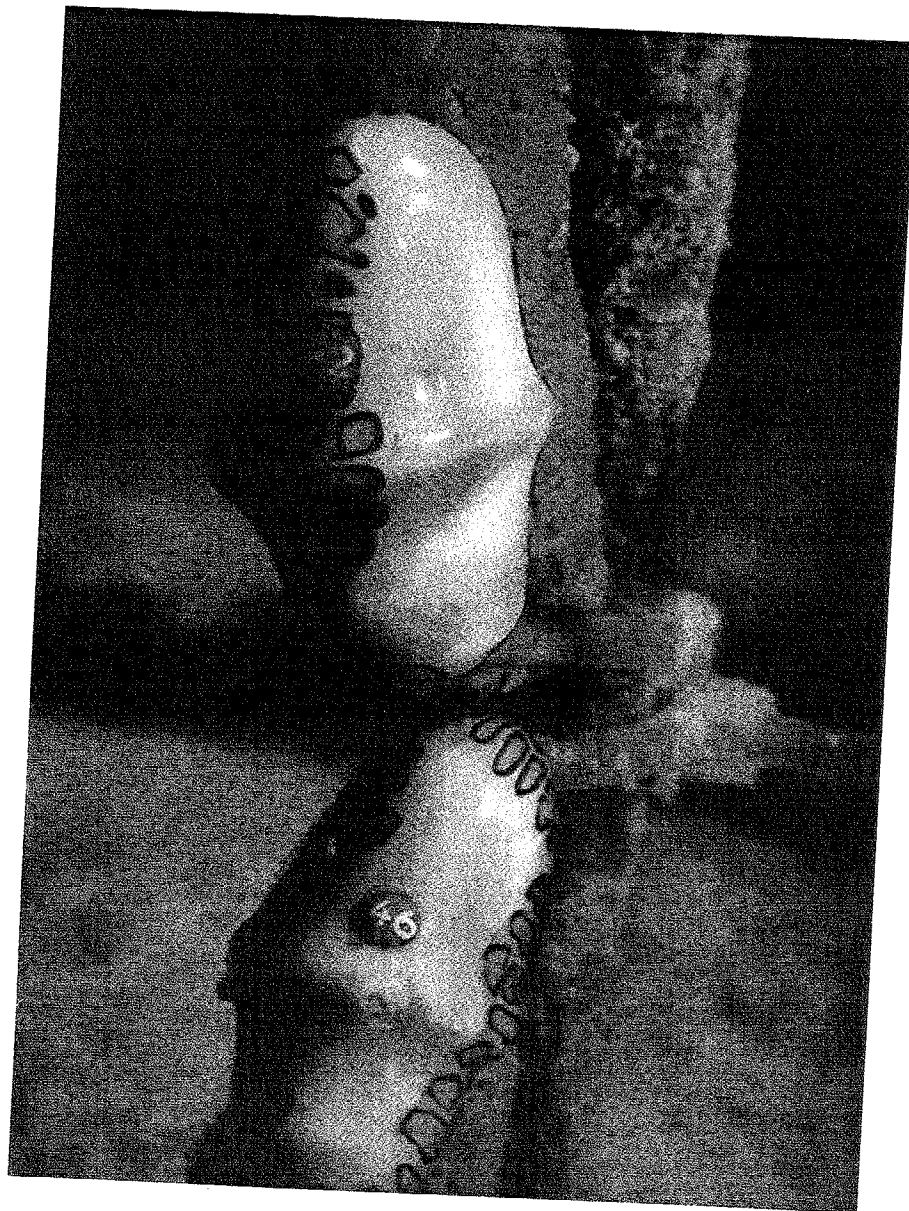


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**BULLETIN OF THE
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SOCIETY OF
AMERICA**



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Cover Photo: Two snails (*Cyphoma gibbosum*) occupy the same coral colony in an experiment designed to measure the influence of mate-searching on host, and thus feeding, preferences. Snails were identified using colored and numbered tags designed for bees. Both males, such as red 32, and females, such as green 46, frequently formed pairs with members of the opposite sex. Consequently, the foraging patterns of snails in the presence of members of the opposite sex were different from the foraging patterns of solitary snails or snails in single-sexed groups. The photograph was taken in the course of research for the article "Mate- and oviposition-influenced host preferences in the coral-feeding snail *Cyphoma gibbosum*," *Ecology* **74**(7):1959-1969, 1993, by Joshua P. Nowlis.

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ON THE DISTINCTION BETWEEN AD HOMINID AND AD HOMINEM AND ITS RELEVANCE TO ECOLOGICAL RESEARCH. A REPLY TO SCHEINER

As practising scientists, we need to have a clear understanding of the forces that lead ideas to be accepted or rejected, particularly if these forces are attitudes without a rational basis. I recently suggested (Keddy 1922a) that some of the negative responses to Rob Peter's recent book (Peters 1991) simply reflect different kinds of human attitudes, and that these have not changed all that much in over 2000 years. In the last *ESA Bulletin* Samuel Scheiner (1993a) charges that my article ("missive") was "ad hominem rhetoric."

Dr. Scheiner appears to have misunderstood the fundamental distinction between ad hominem and ad hominid. The former is a criticism of a particular person; the latter is a commentary upon a species. The misunderstanding perhaps arose because I was discussing ground that is unfamiliar to some natural scientists, perhaps because I carelessly allowed some colourful historical examples into the *ESA Bulletin*, perhaps because I simply failed to express myself clearly. Let me try again.

Scheiner opens by insisting "I am neither a money-grubbing temple defiler nor a hippie-bashing Chicago cop." He thus errs right at the start. All of us, including John Lawton, Paul Keddy, and Samuel Scheiner, have these kinds of behaviour in us. Different circumstances, such as books we read, can evoke these parts of ourselves without any rational process being involved.

This is hardly an original observation. More than 200 years ago David Hume wrote "...The mind is a kind of theatre where several perceptions successively make their appearance, pass, re-pass, glide away, and mingle in an infinite variety of postures and situations." More recently, E. O. Wilson (1978) reminds us "Like most other mammals, human beings display ... a spectrum of responses that appear or disappear according to particular circumstances." Apart from the examples Scheiner objected to, the list of possible emotive states for hominids also includes hero, victim, mother, father, shaman, and lover. Jung called these archetypes. In the Buddhist tradition these inherent behaviours are sometimes pictured as minor deities. All these sources agree in one view: that we are an

ever-shifting panoply of psychological states. If the arising of truncheon-wielding policemen within us seems improbable, we need only watch our state of mind the next time someone cuts us off on the expressway, or cuts a research grant.

Hume was by no means the first in his observation. The absence of a single, solid, rational self has been demonstrated for millennia in both western (Blakney 1941) and eastern (Guenther 1986) philosophical and psychological traditions. Yet we are continually misled by the beliefs, (1) that we are a single, simple, solid self, and (2) that this self is primarily a rational, thinking, autonomous entity.

What has this got to do with the practice of science? Quite simply, we cannot accurately interpret any event, such as a paper, talk, or book review, if we build this analysis upon an incorrect belief. Yet it seems we persist in believing that rational forces predominate in scientific decision-making. Kuhn (1970) and Hull (1988), who have studied the behaviour of scientists, would disagree. This erroneous assumption of simple rationality also complicates the search for solutions to environmental problems (Keddy 1992b). Similarly, in many ecological debates we may assume that there is a rational basis for the differences in opinion when there is not. It is one thing to aspire to rationality. But if we erroneously assume or insist it is present when it is not, we waste both time and money.

Consider the possible origins of our behaviour (Wilson 1978), Leaky and Lewin 1992). If we saw two hominids fighting over a carcass on the African plains, would we inquire about the rational basis for their difference of opinion? If we observed one high-status animal biting a low-status animal, would we ask for the data to justify the behaviour? What we can piece together of human beings tells us that ape instinct has long been a complex mixture of emotive states such as aggression, cooperation, suspicion, status-seeking, obeisance, dominance, and compassion.

Closer to home, how many "questions" asked at ESA meetings are not questions at all, but rather the equivalent of growling, "I'm over

here; don't forget that I am part of our group too." How many bad reviews are really a way of saying "Your work hurts my feelings by making me feel less important than I believe myself to be," or "Your behaviour is a threat to my dominant status—keep your place"? How many review volumes are serious scientific attempts to cover a field, and how many are simply an opportunity for a tribe to get together and groom one another?

If we are honest about ourselves, we know such things to be true to varying degrees. They will be true as long as we are hominids. Recognition of this becomes particularly important when we try to analyze current disagreements in ecology, be they in published papers, book reviews, or anonymous referee's reports. Imagine taking each disagreement, criticism or review, and trying to calculate the ratio of "rational" to "other" content of the disagreement. Would it be 25, 50, or 75%? If the rational component is greater than 50%, we might dignify the disagreement with the appellation of debate. Many so-called debates that consume grant money, conference time, and journal space, may be simply territorial disagreements that no amount of research money or scientific data will resolve. We might as well admit it, quit trying to collect the data to resolve the issue, and work on something else.

One example is reaction to Grime's (1977) suggestion of three primary plant strategies. Many of the verbal criticisms I have heard directed at Grime express the belief that we need more than three strategies (e.g., Grubb 1985). Are there three, or more? In many ways, this is largely a question of how much generalisation one is comfortable with. Some of us are content with broader generalisation than others. This seems to be a fundamental characteristic of human personality, not a matter of scientific debate. If anyone doubts this, let them watch a conversation between my father-in-law and me. He often irritates me because it seems as if he cannot give me a simple answer to a simple question, but answers any query in what feels to me like endless detail. I like it simple—sometimes. In return, he probably finds me annoyingly vague and evasive. He probably also thinks there are more than three plant strategies! There is no point in trying to collect data to resolve our differences because they reside in the nonrational parts of our humanity. We could, of course, try to rephrase the criticisms

of Grime by asking how much detail in plant strategies is necessary to achieve specified levels of accuracy in our predictions. The fact that no one has done this only reinforces my point.

The hominidness of ecologists is something that demands more conscious attention. When bridges are built badly, they fall down, usually sooner than later. Badly designed airplanes fall out of the sky. Even tyrants cannot rewrite the laws of genetics. Ecologists have established a useful conceptual framework for exploring living systems (Scheiner et al. 1993b). But it seems that many ecological procedures, views, and debates within this framework persist precisely because there is often nothing external to cause their disintegration, except perhaps the tenure, exhaustion, or death of the participants. We may be practising in a discipline where papers and ideas are sorted not so much by external tests against nature as by internal pressures of political alliances and ambition. In this respect ecology comes dangerously close to being a humanity, subject to all the political intrigue and trendiness decried by Camille Paglia (1992). In her recent essay, "Junk bonds and corporate raiders: academe in the hour of the wolf," she writes: "The self-made Inferno of the academic junk-bond era is the conferences, where the din of ambition is as deafening as on the floor of the stock exchange. The huge post-Sixties proliferation of conferences . . . produced a diversion of professional energy away from study and toward performance, networking, advertisement, cruising, hustling, glad-handing, back-scratching, chitchat, groupthink" (p. 221).

In the absence of strong self-correcting mechanisms (such as the collapse of bridges), we need to be explicitly aware of the degree to which the course of ecological research is influenced by habitual and irrational hominid behaviour. There is no easy and obvious solution. But we cannot deal with a problem at all unless we are consciously aware of its occurrence—and in this case, bringing these issues to conscious awareness may well be the antidote.

Conscious awareness of the problems caused by hominid behaviour will illuminate and expose behaviour that may otherwise substitute for real scientific activity. There are several advantages that accrue to individuals who are aware of such behaviour as it happens. At a minimum, one can take better evasive action. In fact, public awareness of gamesmanship and status-seeking reduces its value to those prac-

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tising it. Individuals can perhaps also escape from trying to solve popular problems that are inherently territorial rather than ecological. It may also be reassuring to recognize those criticisms that are painful but have relatively low rational content. This could help avoid being pushed into dead ends of enquiry. Conscious awareness of hominid behaviour patterns would also have significant advantages at the collective frame of reference, that is, to our discipline as a whole. It would help us exorcise activity that masquerades as rational enquiry but actually distorts ecology and debases genuine academic activity. There are grounds for optimism. Any behaviour that benefits both individuals and their tribe can be expected to spread. For this to occur, it is sufficient that we are aware of ape instinct when it arises and know it for what it is.

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