
Human Behavior and Evolution Society

Fall, 1996
Volume V, No. II

Editor: Elizabeth M. Hill
Associate Editor: Kevin MacDonald

The View From The President's Window

Richard D. Alexander

Dilemmas Of Darwinists: A Few Tips of the Iceberg

Almost a half century ago the Penn State primatologist, Charles R. Carpenter, wrote as follows:

The prevailing 'climates of opinions,' including scientific value systems and attitudes of the majority of our research colleagues, are of such a character as to impose an unusual burden of proof on us [students of animal societies] for the professional status of our efforts and achievements. Those of us who are interested in comparative behavior, naturalistic behavior, and interactions of organisms are required to accumulate, and even dramatically to present, an overwhelming weight of evidence on very significant theoretical problems for these areas of research effort to be accepted and duly accredited. Our work is put into a disadvantageous position by the historic flood of poor natural history writings in our fields of interest. At the same time, the prestige of our work is not greatly enhanced by excellent qualitative writings during this quantitative-laboratory age.

For Darwinian students of human behavior little has changed. Some of the ways we get into trouble would be humorous if they were not so disheartening. Here are some I have noticed. I hope my comments about them have some usefulness.

1. Students of human behavior can get into trouble for being scientific. Social talk tends to be about what ought to be. This is so even when people are talking about what is; they continually divide the reality of sociality into things that ought to be and things that ought not to be. Of course they admire the former and denounce the latter. Sometimes they talk about reality for no reason other than to cast it in terms of morality.

People probably do these things because morality requires social constraint, because the immorality of others may affect them negatively, and especially because they want

to assure others they have only moral thoughts and will recognize and punish immoral acts. In our kind of sociality reputation is everything. But acceptable behavior varies with geography, culture, time, situation, and intent. So conducting one's self morally in ways that do not incur acceptably avoidable costs becomes a dauntingly difficult balancing act, as everyone who has filed long forms for income tax knows. Some of our reactions are conscious and some unconscious, the latter a result of the ontogeny of conscience, which makes some moral acts intuitive and helps us look especially good to potential associates. Some students of behavior, of course, believe that the human brain has been designed as a tool for use in just such social matters. (I subscribe to the advocacy in the bumper sticker "Commit random acts of kindness and senseless acts of beauty," but I keep wanting to add, "Consider doing it anonymously.")

Darwinian students of human behavior try to talk (and write) only about what is, when they analyze human sociality, presumably because they wish to be regarded as scientists. They avoid talking about what ought to be because they do not wish to be regarded as moralistic. Even if they are trying to analyze moral opinions or behavior, judging is not their concern. Anyway, they are unlikely to regard themselves as experts on moral issues. But by talking about what is and avoiding judgments, they give the impression of accepting or advocating that what they report is either what has to be or what ought to be.

Thus, by avoiding moral decisions about what has been and what is -- by trying to be scientific -- students of human behavior actually give the impression of being immoral rather than merely amoral as they might expect. To many, amorality, in discussions of human behavior, implies immorality anyway; to understand this attitude, one need only think about nonscientific discussions in which such stances are taken. Moreover, amoral discussion is one thing, amoral discussants another.

By refusing to pass judgments, students of human behavior can also be regarded as genetic determinists. Those so judged are easily tagged with additional labels such as racist, sexist, and social Darwinist. They connect to the images of socially and morally undesirable people. They can be judged immoral even if they really are merely trying to be scientific.

2. Students of human behavior can get into trouble for expressing moral opinions. Sometimes Darwinian students of human behavior cannot resist the lures presented by incessant questioning about what ought to be, and, while being interviewed as scientists, deliver their personal judgments about what is moral, or about the extent to which what is can be altered into whatever someone supposes ought to be. Occasionally they attribute their opinions about the nature of right and wrong to the results of their scientific work on the evolution of human behavior. I don't think I have ever seen justification for such an attribution.

Far into an argument by mail I recently asked a prominent philosopher whom most regard as "on our side" how (then) he thinks right and wrong can be determined. He replied "with science." If moral systems are social contracts, however, developed because of underlying conflicts of (reproductive) interest that don't go away and therefore must be mediated by endless compromises, that won't work. It's one thing to use science to find out whether an act already judged wrong has been committed, as in courts of law; it's another to use it to decide whether a given act is right or wrong. Consider abortion, a persisting conflict of interest, mainly between mother and embryo, but also involving father, other relatives, taxpayers, and moralists with still other interests. Whether we like it or not, right and wrong are still decided by majorities or influential minorities. Regardless of how their minds are made up, the resulting social world is just as real as the physical world, as anyone shunned, jailed, or about to be executed well knows.

3. Students of human behavior can get into trouble for doing comparative analyses. Studies of adaptation depend on comparative analysis; it is the evolutionist's (both the homologist's and the analogist's) main tool. But anyone who describes differences between human individuals or groups -- no matter how carefully and accurately -- is likely to be regarded as racist, immoral, and a genetic determinist. This is true even for someone who describes only the behavior or other attributes of a single society, because these attributes can be compared to our own society, and some kind of pejorative conclusion dreamed up. Human audiences seem easily tuned to judge human differences as better versus worse (what a chance I take!), and, ironically, when they do this they often blame the expositor. Selection is always a potential blight if there is confusion about reproductive signifi-

cance and current human values. And there always is. I have wondered if this is why biological anthropologists for so long attributed so much of evolutionary change, hence heritable trait differences, to genetic drift or mutationism; it's difficult to place values on differences resulting from chance.

4. Students of human behavior can get into trouble for arguing that natural selection is the principal guiding force of evolution. Cladists and other biologists who have joined the expanding interest in developing branching sequences or phylogenies of all organisms, using molecular as well as all other kinds of information, favor drift and mutation for a reason different from population geneticists. First, they believe they do not need to take natural selection and adaptation into account to generate accurate and useful sequences of speciation events -- as they say, to study the "historical" aspects of evolution. Second, because selection can take off in any of a large number of different directions, and can change rates of evolution in ways that to phylogeneticists seem wickedly capricious and obfuscating, many students of branching sequences would be happier if selection didn't even exist. They want it to be trivial. They treat it as noise, and as a potential monkey wrench in their phylogenetic works. It threatens their molecular clocks and conceals the homologies they thrive on by creating those horrible convergences and parallelisms that silly adaptationists use so effectively. For cladists parallelisms (homoplasies) are tricks evolution plays on them, potentially embarrassing mistakes that one must understand well enough to keep them out of the phylogenetic picture. Not surprisingly, cladists sometimes vote with those who label students of adaptive human behavior as immoral, unscientific, or pursuing useless enterprises. But they've got a huge underlying problem. In their zeal to be parsimonious (meaning to invoke the smallest number of unlikely changes), they eventually have to understand selection well enough to know how unlikely different "traits" (changes, characters) really are; it won't do just to weight them all evenly and minimize the number of changes (as director of a systematics museum, I'm sensitive about this).

As with community ecology, cladistic work tends to be restricted to views at or above the species level, where adaptation is not an easy concept; moreover, the initial kicks in speciation processes are not likely to be results of natural selection but rather chance extrinsic isolation and sampling errors; the divergence will be affected by mutational differences between the populations. At the other end of the spectrum are molecular biologists, whose studies would be easiest and most potent if development were as particulate as heredity; the world (including people who control purse strings) would also like that to be true because it's infinitely easier to understand (so easy, indeed, that it's tempting just to accept being wrong!).

Causes of complex phenotypic attributes could then be reduced to actions of individual genes, and because genes would be seen as having singular effects it wouldn't hurt to, say, delete or change them to remove an undesired effect. Of course, molecular biologists working on humans tend to concentrate on medically important genes, so they don't get into as much trouble as students of human behavior do, searching for heritable differences in social behavior.

These things result in incidental alliances among cladists, social scientists, community ecologists, molecular biologists, theologians, philosophers, and others -- more or less against Darwinian students of human behavior. If we can be patient this dilemma will resolve itself, because there just isn't any way to deny forever the importance of natural selection.

5. Students of human behavior can get into trouble for using the word "biological." To most biologists, "biological" means "concerning the study of life." To most others -- and even some biologists -- it means "genetic, anatomical, or physiological" -- seeming to refer to traits felt somehow to be closer to the genetic background or having more heritability in their variations. Such people can be recognized because they make these contrasts: "Biological or cultural" and "biological or social." Think what "sociobiology" seems to mean under this second meaning: "Social genetics." No wonder. There's nothing wrong with the topic; indeed, it is close in meaning to the phrase "evolutionary genetics," which HBES people associate with such young biologists such as Steven Frank, David Haig, Lawrence Hurst, David Queller, and Kern Reeve (and such "old" ones as Bill Hamilton, Bob Trivers, and George Williams). But neither social genetics nor evolutionary genetics seems an apt label for the overall study of human social behavior.

In my first essay in this newsletter I wrote about use of the term "biological parent" when "genetic parent" is meant; I'll not repeat that. But I'll allow myself an "ought" on the topic of "biology." Our discipline ought to retain fiercely its basic tie to the discipline of biology because, once any part of us loses it, the descendants of that part will tend again to float off into social or medical sciences that generation by generation will increasingly ignore or misunderstand evolution. Without a continual input and revision of information from modern biology as the science of life, to form and re-form our approach, we are constantly in danger of losing the Darwinian flavor that gives us direction.

It's too bad that, so far, departments of biology don't like to spend money on people who study the human species (unless you have tenure when you start, as I did). In 39 years at Michigan I have acquired 30 doctoral students and graduated 25; included is a single student of human

behavior. Biology departments prefer to leave humans to the social and medical sciences; those sciences, however, have so far had almost nothing to do with evolution and adaptation.

6. Students of human behavior can get into trouble for not understanding development. Of course no one understands development (ontogeny). But wrong hypotheses about frogs and bluebirds don't cause moral tirades. Ethologists since Lorenz and Tinbergen, and currently an occasional evolutionary psychologist, have tended to regard mechanisms with cryptic ontogenies as "innate," "inborn," or "instinctive," meaning that differences are heritable, and contrasting that with trait differences that are socially determined. But the ontogenetic continuum is not adequately described by such dichotomies; thus, there are trait variations owing to genetic variations, others owing to learned traits that can in turn be passed on by learning (culture), others owing to environmental variations whose effects cannot be transmitted by learning, and apparently all kinds of hierarchies and combinations of these.

Optimally, our hypotheses need to be so constructed as to accept any ontogenetic hypotheses that might turn out to be correct. Some may believe we can't do this, but I feel safe using the ultimately onerous guide Dobzhansky (1961) provided, which I suspect may be the most satisfactory model available. I quoted it in my first newsletter essay, and I quote it again here:

Heredity is particulate, but development is unitary. *Everything in the organism is the result of the interactions of all genes, subject to the environment to which they are exposed. What genes determine are not characters, but rather the ways in which the developing organism responds to the environment it encounters.*

Because we don't understand development, as scientists we probably must be conservative about moral issues and what to do about them. Anyone who doubts this, and who perhaps doesn't understand how easy it is to use (misuse) science for policy, should read Deichmann (1996). Early ethologists saw adaptive behavior as good for the species, and some of them were able to translate this to mean not the species as a whole but the *Volk*, and then to use this version of science to help decide who should be eliminated (or "selected") to help the *Volk*. They also saw their principal notion, the instinct, more or less as follows (this definition is from a 1949 Cambridge Round Table, reported by Thorpe 1951):

An inherited and adapted system of co-ordination within the nervous system as a whole, which when activated finds expression in behaviour culminating in a fixed action pattern. It is organised on a hierar-

chical basis, both on the afferent and efferent sides. When charged, it shows evidence of action-specific-potential and a readiness for release by an environmental releaser.

To me, this definition carries within it what Kennedy (1954) called the "obscurity at the core" as he compared the tenets of ethology to Freudian psychology. Using it one could develop together the concepts of group-level selection [as preserving the *Volk*: "To us Volk and race are everything, the individual is virtually nothing." (Konrad Lorenz in Deichmann 1996, p. 189)] and instinct (as inherited) to label people as not corresponding to the "desirable type of their race" (p. 192). People could be described as having the "wrong" morals" (being "ethically inferior," p. 188) or having wrong social responses ("elements who have fallen out of their relationship to the whole," p. 193), thus the wrong instincts, thus the wrong genes ("racially foreign elements"). Hence, the promotion of breeding within the Volk and *Ausmerzung*, usually translated to mean "elimination" (of undesirables) in an analogy between eugenics and the breeding and culling of domesticated animals and plants ("... in the large field of instinctive behavior humans and animals can be directly compared... We confidently... predict that these studies will be fruitful for both theoretical as well as practical concerns of race policy." p. 186; Lorenz in Deichmann 1996). Lorenz referred to the Old Testament commandment to "Love your neighbor as yourself" by saying that, "Since race and Volk are everything to us, the individual almost nothing, this commandment is quite self-evident demand for us." (Deichmann 1996, p. 191). I include these quotes to emphasize the form taken

by an effort to make a particular social use of an imperfect science (as all science is), in this particular case by the ruling power structure of a society.

For most people, perhaps, the buck never "stops here," at least on big issues. When it does, you find out quickly if your house is in order.

7. Darwinian students of human behavior can even get in trouble for telling the truth. Well, trying to tell the truth as they know it, which is as close as anyone can come. But stubborn efforts at "truth-telling" (even if they are not flamboyant) can also cause trouble in everyday social life. And that's food for thought in regard to all of this. Excelsior.

References

- Carpenter, C. R. 1950. General plans and methodology for field studies of the naturalistic behavior of animals. In: J. P. Scott (ed). Methodology and techniques for the study of animal societies. *Ann. N.Y. Acad. Sci.* 51:1006-8.
- Deichmann, U. 1996. *Biologists under Hitler*. Harvard University Press.
- Dobzhansky, T. 1961. [discussion] In: J. S. Kennedy (ed). *Insect Polymorphism*. Symposia of the Royal Entomological Society of London, Vol. 1. London: Royal Entomological Society, p. 111.
- Kennedy, J. S. 1954. Is modern ethology objective? *British Journal of Animal Behaviour* 2:12-19.
- Thorpe, W. H. 1951. The definition of some terms used in animal behaviour studies. *The Bulletin of Animal Behaviour* 9:34-40.