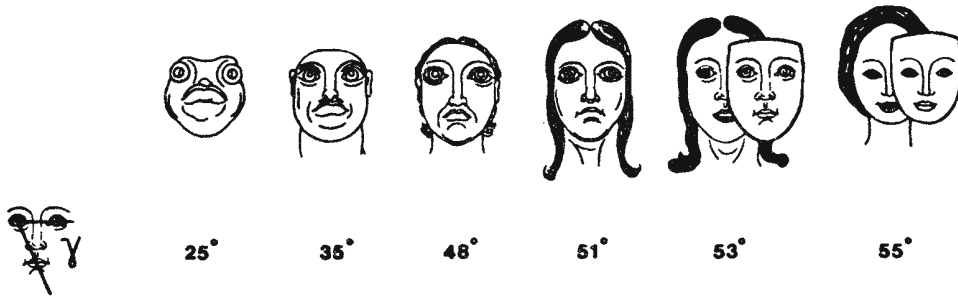


PATTERNS BEHIND THE MASKS



# HUMAN ETHOLOGY NEWSLETTER

JOAN S. LOCKARD, EDITOR  
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## LOGO IS GO!

Credit for the intriguing logo above goes to Wolfgang M. Schleidt, Dept. of Zoology, University of Maryland, whose inspiration came in part from the 18th Century views of Johann Caspar Lavater (Essays on Physiognomy: Designed to Promote the Knowledge and Love of Mankind, London: W. Tegg, 1848). The rightmost face was drawn by Monika Schleidt, providing an esthetic and modern rendition of the idea(s) portrayed. The curvature of the faces precluded good computerization, thus a xerox reduction was used. Further discussion of this logo will be forthcoming in the next newsletter; please send your interpretations to the editor.

The call for logo ideas for future newsletters is continuing. Symbolic representations of concepts relevant to human ethologists are encouraged. Your contributions will help to prevent a monopoly by the editor. As for the logo of the last issue, the numbers were ages and the sports car with the dollar-sign hood ornament represented resources. The intended interpretation was a balance of female choice by the opportunity of serial polygyny. If you saw it differently, let us know.

## WELCOME NICK!

Nicholas Blurton Jones took a permanent position at the University of California at Los Angeles on July 1, 1981. He plans to continue his work in human ethology and evolutionary theory. His principal

appointment is in the Graduate School of Education with joint appointments in the Departments of Anthropology and Psychiatry and Biobehavioral Sciences. Dr. Jones' new address is: Graduate School of Education, Moore Hall, UCLA, Los Angeles CA 90024. He is currently looking for a Staff Research Associate; please check the newsletter's BULLETIN BOARD for the position description. It is jolly good having you in the States, Nick!

#### NOMINATIONS FOR ISHE BOARD

The executive board of ISHE is composed of eight elected members who serve staggered two year terms. Each year four new members are elected. In order to insure a variety of viewpoints, theoretical perspectives, and methodological strategies the board is composed of people from several disciplines including animal behavior, anthropology, political science, primatology, psychology and sociology. This, of course, is not an exhaustive list of related disciplines.

Members of the executive board elected in 1981 are Robert Adams (psychology), Gordon Burghardt (animal behavior), Wade Mackey (anthropology), and Gail Zivin (psychology). Members of the board whose terms will expire at the end of 1981 are I. Eibl-Eibesfeldt and William McGrew (animal behavior), and William Charlesworth and Cheryl Travis (psychology).

Please recruit and nominate members for election to the executive board. Self-nominations are entirely appropriate. The ballot will appear in the winter issue of the newsletter. Nominations should include: Name, affiliation, degree area, and research interests limited to 100 words. Send the nomination and necessary information to Cheryl Travis, Department of Psychology, University of Tennessee, Knoxville TN 37916. Deadline for receipt of nominations is November 15.

#### HUMAN ETHOLOGY ABSTRACTS IV

Human Ethology Abstracts IV will be appearing in the March 1981 issue of Man-Environment Systems, which should be in print in October 1981. Copies will be available for \$3.00 from ASMER, P.O. Box 57, Orangeburg NY 10962. Details about a package deal to make all four editions of HEA available will be announced in the next Newsletter. We appreciate all of the hard work that Larry Stettner and Karen Olson put into compilation of these abstracts!

BECOMING EVANGELISTIC

Membership in the International Society for Human Ethology is, in the long run, somewhat lower than desirable to sustain an organization. We currently have 226 members; it would be far better if we were 350-400 strong. Our numbers broken down by country and state are as follows:

U.S. 181. Alabama 0, Alaska 0, Arizona 3, Arkansas 0, California 27, Colorado 0, Connecticut 2, Delaware 0, Florida 4, Georgia 2, Hawaii 1, Idaho 0, Illinois 14, Indiana 5, Iowa 3, Kansas 2, Kentucky 3, Louisiana 3, Maine 1, Maryland 0, Massachusetts 6, Michigan 11, Minnesota 4, Mississippi 0, Missouri 1, Montana 2, Nebraska 4, Nevada 0, New Hampshire 2, New Jersey 6, New Mexico 5, New York 25, North Carolina 2, North Dakota 0, Ohio 2, Oklahoma 0, Oregon 0, Pennsylvania 9, Rhode Island 1, South Carolina 1, South Dakota 0, Tennessee 6, Texas 2, Utah 0, Vermont 1, Virginia 3, Washington 2, West Virginia 1, Wisconsin 5, Wyoming 0, D.C. 2.

Canada 21. Alberta 1, British Columbia 2, Nova Scotia 3, Ontario 10, Quebec 5.

Foreign 24. Australia 2, Colombia 1, England 4, France 1, Holland 1, Israel 2, Italy 3, Japan 2, The Netherlands 3, New Zealand 1, Scotland 1, Spain 2, Switzerland 1.

The chairperson of our membership committee, Gordon Burghardt, recently wrote the following:

"It is imperative that the society increase its membership and subscribers, particularly in North America. Please xerox the form in this issue and distribute to colleagues and students. Point out the intellectual diversity and value of the newsletter and its usefulness in teaching, the entree to meetings, and so forth. We need to have a greater representation of those working in all areas of the field to justify our existence."

At the end of the newsletter is a membership form. If each of us were to ask one colleague to join ISHE, we could double our membership by the next issue.

PAST ISSUES

In the last issue of the newsletter, it was stated that we would need a commitment of 100 orders before reprinting volume 1 (Issues 1-17, 1973-1977) and volume 2 (Issues 18-31, 1977-1980) of the Human Ethology Newsletter in a softbound form. The response to this proposal was less than overwhelming, but there is an alternative for those people who would like back issues. We have found that the volumes can be made available on an individual basis for \$15 per set

(or \$6 volume 1, \$10 volume 2). The composition of each volume would be xeroxed copies of the pertinent past issues, bound with brass clips in a colorful file folder. If interested, send your prepaid orders to the editor.

ISHE ROUNDTABLE AT ABS, 1981

Committee Report on Long-Term Goals (William Charlesworth and I. Eibl-Eibesfeldt). During the long range planning part of our meeting at the Animal Behavior Society meeting, the question came up about structuring ISHE a bit more. This was interpreted as drawing up a constitution and by-laws, electing a president, secretary, etc., in a fashion similar to other similar organizations. After a brief discussion, the general feeling was that there is no pressing need at the moment for any move in this direction and that the question should be shelved until our meeting next year. Currently everything is running well, but a good portion of the Society's official work is being done by a very few people and the time may come when we may want to strengthen their positions at their local institutions by giving the Society more formal status. Something to think about between now and next year.

We also hope that the point made at ABS about meeting with groups other than animal people will be noted in the Newsletter. We certainly do not want to weaken contacts with ABS or primatology, but as pointed out, it is very important to develop contact with groups that work with humans, especially anthropologists (despite the feelings many of them may have about the lack of relevance of ethology and sociobiology for understanding humans). In addition to the interdisciplinary contact which, per se, is always valuable, there is always the funding issue. Since most of our funding comes from foundations etc. which support research with humans, we must keep the channels open with them.

Call for New Members (Gordon Burghardt) See BECOMING EVANGELISTIC, page 3.

Call for Nominations to ISHE Executive Board (Cheryl Travis) See NOMINATIONS, page 2.

Committee Report on International Meeting (Gail Zivin and Ron Weigel) We are all set for 1982. See UPCOMING MEETINGS, page 45.

Popular Magazine on Human Ethology. See following section.

Workshops and Symposia:

A Teaching/Textbook Workshop for next summer is in the planning stage. William Charlesworth is coordinator and wishes to enlist participation from others. He envisions a historical and conceptual

approach. Write directly to him for more information, at: Institute for Child Development, University of Minnesota, 51 East River Rd, Minneapolis MN 55455.

A Cross-Discipline Workshop was suggested by Tom Hay (West End Creche Child and Family Center, 3704 14th St., Detroit MI 48208. Carol and Glenn Weisfeld (1334 E. Joliet Pl., Detroit MI 48207) also expressed an interest. Those similarly motivated should start communicating. See also FALL FORUM question, page 21.

Two symposia are being planned by our members so far for the international meeting in 1982: Fred Strayer on the topic of a Biological Approach to the Family; and Ron Weigel on the subject of Adaptive-Aggressive, Dominance, and Conflict-Resolution Strategies in Human and Nonhuman Primates. If interested in more details, write to them directly. Fred Strayer, Laboratoire D'Ethologie Humaine, CIRADE, C.P. 8888, Montreal, Quebec, Canada H3C 3P8. Ron Weigel, Neuropsychiatric Institute, Human Ethology Laboratory, University of California, Los Angeles CA 90024.

<u>ON GOING POP?</u>
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Growing Pains

Some of the not too subtle cues that a science is developing are those evident in the utterings of the commercial world. A recent advertising brochure by the Ford Motor Company (Ford's Insider, 1981) was devoted almost exclusively (apart from its advertising) to the subject of nonverbal communication. For example, titles of two subsections were "College Doublespeak: How to Read Between the Lines," and "Those Lips, Those Eyes: What Your Face Communicates." There was even a short blurb on "Elevator Etiquette" (p. 19):

"The close, windowless quarters of an elevator create instant groups that are governed by tacitly accepted rules of behavior, says Phoenix psychologist Layne Longfellow, who has compiled a list of common elevator customs. If you could read the handwriting on the wall, it might look something like this:

- \*Face forward.
- \*Fold hands in front.
- \*Don't make eye contact.
- \*Watch the numbers.
- \*Don't talk to anyone you don't know.
- \*Stop talking with anyone you do know when anyone you don't know enters the elevator.
- \*Avoid brushing bodies."

(Reprinted with publisher's permission from Insider, 1981, 13-30 Corporation, Knoxville TN 37902)

Another such growing pain is in the coverage of topics, most often in the form of sensationalisms by certain lay magazines. For instance, the sociobiology of rape was addressed in Playboy's April, 1981 issue (28(4): 112-114,172,224-229) in an article by Richard Rhodes in which he quotes from an interview with E.O. Wilson on the subject as follows:

"I described to Wilson the information included in this report. He considered it and responded: "I think you're onto something very logical. It's a very worthwhile proposition that, ugly as it sounds, rape does, indeed, give genetic advantage. If the rapist can escape unpunished--and apparently most do--then he has put himself reproductively a little ahead of the game. Rape may very well have evolved as a behavior pattern, a way of extending sexual behavior into the realm of violence and stalk. We really have to examine directly the dark side of human nature. We're talking about inherited predispositions in the form of learning rules, predispositions that make it very likely under a wide range of environments that you will develop one pattern of behavior, often quite complex, structured and predictable--rather than another. The hypothesis that rape is one of those patterns may be superior, fitting more of the facts than the hypothesis that rapists are simply psychopaths, mad dogs, wrong in the head. It's logical and it's possible."

Similarly, Esquire's March, 1981 issue (95(3): 25-35) had two sections by Digby Diehl on the male crisis of middle age, "Looking at Forty" and "Looking Back at Twenty-nine." Although couched in lay jargon, the discourse unmistakably speaks to the topic of mating strategies. The captivating lead in big type to the first segment was,

"You think you have it made: a great life, a good job, abundant confidence, energy to burn. Then, sometime during your thirties, you face the inevitable trauma of impending middle age. Suddenly, you're pushing forty...or is forty pushing you?"

The sequel that followed Part One began with, "From the vantage point of forty, you examine the past and find that a discernibly different person was occupying your shoes and using your good name."

Lest you think that your new editor has the time to read broadly--and also out of self defense--the above contributions were submitted from out-of-discipline colleagues who brought the materials to my attention. My disclaimer speaks to the danger I feel in popularizing human ethology when it is still very much an adolescent science. The following thoughtful paragraph from the only letter on this subject from our membership is a cogent statement with respect to this view.

"Dear Joan Lockard,

...You asked for responses to David Munro's suggestion to have a popular Ethology Today like Psychology Today. I do not support the idea because I do not think we have very much to offer the world yet; we are too ignorant about the process of development. I see this as a key issue because ethologists tend to be more concerned with ultimate causation, evolution and adaptation, whereas social scientists and decision makers are more interested in proximate causes. It can be argued that their interests should be broadened to encompass both long and short term mechanisms of causation, but as we are presently so ignorant about the links between the two, I think we should lie low until we have something substantial to offer.

I would not like to see ethology dragged into the mud at this stage by trying to sell half-baked ideas or promise easy solutions to current world problems. Most problems require complex solutions."

Yours sincerely,

Pauline Nye (signed)  
Senior Lecturer in Psychology  
University of Otago  
Dunedin, New Zealand

#### Call for the Question

At the Animal Behavior Society annual meeting in Knoxville, Tennessee this past June, a Human Ethology Roundtable was scheduled. Approximately 50-75 people attended, many of whom are members of the International Society for Human Ethology. The group voted unanimously that there should not be at this time a popular magazine on human ethology, nor should ISHE lend its name to promoting such a publication.

#### Minority Position

"Dear Joan Lockard:

...It is not without significance that I was in New York at the very time you were meeting in Knoxville, saying to editors that there is a necessity now for ethology to go public again. It is also not without significance that had I been there the vote might well have gone the other way.

Academes need periodically to be reminded that they are only custodians of scientific knowledge, not owners. They have no right to delay the distribution of information for their own self-serving or timorous purposes. For example, if critical-period analysis is

to reform early education, in a manner far beyond Piaget's imagining, then each year we delay the reform sends another whole class of children away without it. Something similar may be said for the uncorrected errors in the Freudian laying-on-of-hands. And if you reply that I'm indulging in mere speculations as to these importances I grant the possibility, but we can't find out while ISHE arbitrarily classifies its data top-secret.

I thus consider the determination, as you report it, "that there should not be at this time a popular magazine on human ethology" indefensible and irresponsible. And I am appalled that 50-75 certified academes can be found to cop-out so unanimously. (It is legit for ISHE to decide not to promote one.)

To be sure, the "at this time" probably served to salve consciences, but if they'd taken that clause seriously they'd have debated the "when."

Nor do I grant much credit to the contention that present data are raw, unfinished. Our data will become finished upon the anvil of reality. The beginning of ethological wisdom in education, for example, will come when ethologists begin making mistakes in education that are measurable, timeable.

However, I am quite aware that the resolution in Knoxville does [not] commit ISHE members as individuals, and I look forward to a different verdict coming from them. No doubt, whatever your personal views, you will grant them space..."

With best wishes,

David A. Munro (signed)

<u>SUMMER FORUM: Animal Rights</u>
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Human Ethology and Animal Ethics

Gordon M. Burghardt  
Department of Psychology  
University of Tennessee, Knoxville

The response to the posed questions (in the June Newsletter) on the relationship of human ethology to issues of animal rights and animal welfare has not been overwhelming. Although the short deadline was certainly a factor, it also may be that members of the International Society for Human Ethology do not really see any important issues facing them in this area. The two contributions (below) that were submitted did not come from human ethologists and, accordingly, do not specifically address the forum questions. In view of this outcome, I have considered the questions one by one and



articulated here some of my thoughts in formulating them. I encourage responses to both this commentary and the provocative essays by Kleese and Weldon.

Question 1: What, if any, contribution could or should human ethology make to the debate on treatment and use of non-human animals in research?

The role of non-humans in research is but one of many considerations that color ethical decisions concerning our treatment of them (Table I, below). It is quite clear that there is a controversy of considerable dimension on the treatment and use of non-human animals in research (e.g., Burghardt and Herzog, 1980; Kleese, this issue). The recent issue of Laboratory Animal (1981, 10(4)), which passed my desk today, contained a series of angry letters responding to an article in an earlier issue by Dr. Andrew Rowan of the Humane Society who tried to convince laboratory animal researchers and managers about the need to consider alternatives to the use of animals. This, of course, is akin to advocating temperance to liquor industry executives or organic farming to chemical companies.

If we as human ethologists are concerned with studying humans by utilizing methodological and theoretical approaches shown to be invaluable in the study of other animals, then we should necessarily be concerned with issues of animal rights. These controversies are not unlike those with respect to the use of human subjects in research. In the recent past, the American Psychological Association, the Federal Government, and others have spent thousands of hours on ethical issues in psychological and medical research. Initially, guidelines demanded that behavioral research involving human subjects had to be approved by many different committees in the same light as more intrusive studies that utilized potentially dangerous procedures. After a number of years, common sense prevailed; observing people in public places is not now considered in the same category with injecting people with cancerous drugs, locking them up in isolation rooms, or giving them electric shock. What is amazing is that this debacle occurred in the first place. For instance, the procedure that my graduate students had to go through in order to observe kids doing common things in public was appalling. Why was the mentality so indiscriminant? Human ethologists were burned by this overzealous ethical concern and were prevented or discouraged from doing things that novelists and poets have done for years. It would seem that as soon as one becomes systematic about one's observations, their intent becomes suspect. Perhaps that is why human ethologists have shyed away from ethical issues. They just want to be left alone. However, such an approach may be short-sighted.

Human ethologists have been in the forefront of luring researchers interested in human behavior from the laboratory. Although the main impetus emanated from concerns with methodology and data validity (e.g., Hutt and Hutt, 1970), ethical issues were

just below the surface. These issues addressed both the treatment of individual subjects and the applicability of laboratory results and artificial test situations to serious problems faced by people in the real world. Thus, since human ethologists have tried to study behavior in naturalistic settings and have had experience with ethical issues in their own research, they might be ideal contributors to the search for both less traumatic and more valid ways of studying animals.

TABLE I

CONSIDERATIONS ENTERING INTO ETHICAL EVALUATION  
OF RELATIONS WITH OTHER SPECIES

(Modified from Burghardt and Herzog 1980)

A. Human Benefit

1. food
2. clothing
3. transportation
4. recreation
5. basic research
6. pests and competitors
7. danger and disease
8. domestication
9. food, drug, and pollutant testing

B. Anthropomorphic

1. pain and suffering
2. goriness
3. phylogenetic similarity
4. humanoid appearance
5. mental similarity
6. cuteness
7. size
8. longevity
9. disgusting habits

C. Ecological

1. rarity
2. diversity
3. ecological balance

D. Psychological

1. habituation
2. aesthetics
3. spiritual and religious
4. call of the wild
5. individual variability
6. behavioral plasticity

Question 2: Is it important for human ethologists to support behavioral research on all animals and in all contexts, or should they mainly be concerned with primates, field studies, "painless experiments," or qualify their support in other ways?

This second question was meant to focus on some specific aspects of the first question. I suspect that we would not want to be blind supporters of those who, with minor qualifications, advocate unrestricted research on any animal with whatever means the

investigator judges to be appropriate. Beyond this assumption, there are two alternatives. We may take individual stands on these issues based on our own values, philosophy and feelings, or we may try to respond professionally as "human ethologists." The latter seems to be viable only if we give reasoned counsel that goes beyond out parochial interests. Why should human ethologists be more concerned about primates than caterpillars, or, given Weldon's essay, sea snakes? It is easy to oppose exploitation or maltreatment of primates on the premise that primates are more closely related, and scientifically highly relevant, to the clientele of human ethologists. Again, since human ethologists typically do field studies rather than laboratory experiments, perhaps our main concern should be with defending field work and to be less sympathetic to laboratory animals. I think there are problems with both the taxon- and setting-based criteria and that they should not be used; the attitude, approach, and perspective of the researcher is far more important. I think the most effective course open to us is to remember that human ethology studies rarely are invasive in the sense of causing psychological or physical pain. Therefore, we should be particularly effective in arguing for a science of behavioral study that can minimize moralistic controversy by use of sophisticated observational and analytical methods as well as naturalistically poised questions.

Question 3: How do the issues of "animal" and research ethics relate to the conservation and the study of endangered non-human and human populations?

This question was meant to bridge the study of our species with those of other species by looking at those demes of our conspecifics that are declining, endangered, or threatened as cultural and genetic units. Should we take the view espoused by Desmond Morris (1967) that small isolated cultures are basically backwater relics of human evolution of no great interest in the study of "typical" people. Is the implication that they, in effect, should be allowed to become extinct, like the dodo, because they are unable to cope in the modern world; why hold back the tide of change? Let the resource exploiters, third world autocracies, and missionaries enforce "progress." Is not this argument similar to those that we often hear in response to concerns about California condors, whooping cranes, and snail darters, not to say some primates?

I was also hoping that we would have some interchange about what ethics are really all about. There is a literature on this which is growing and Kleese (to follow) cites some of it. The sociobiological movement has brought an awareness of biological imperatives to philosophers. I would like to think that it has also helped scientists become aware of philosophical issues that are too easily repressed when it comes to their own research, but which could be guidelines as to how to treat others of our own species. Some reasons espoused for preventing cruelty to animals are not unlike those given to impressionable children. A society more humane to animals will be more humane to people, is a common view.

But we need studies that will actually look at the attitudes and behavior of children towards animals and their actual behavior (in terms of cruelty) as to how they treat human conspecifics. The behavior of kids with animals includes pulling wings off flies, burning snakes in bags, purposely stepping on insects, fishing, hunting, raising farm stock, collecting insects, keeping pets, and science-fair, live-animal projects. Is there any relationship between participating in these activities and moral and ethical values?

Finally, I think human ethologists are in a good position to think through the delicate issues involved in the comparison (often found in the animal-liberation and animal rights literature) of how animals are used, with that of human slavery. While some of the rhetoric heard is outrageous, there is an issue here. We have to simultaneously recognize the biological and social factors that pit demands for humane treatment and respect with our ubiquitous behavior of raising animals in confinement (farms), killing them for food, "controlling" them against their will (e.g. coyotes, mosquitoes), exhibiting them in cages, making our homes their prisons, and cutting them up for the advancement of knowledge. Comparing this dilemma with that facing thoughtful slaveholders in the U.S. may sharpen our perspective, albeit uncomfortable. Although today we feel slavery is unquestionably wrong on moral grounds alone, anyone who thinks that at that time the slave situation was a completely black-white issue is historically inept. The book by Miller (1977) on Thomas Jefferson's conflict over slavery is, I think, a telling example of the intellectual and practical struggles of a person who had an eminently humane, democratic, and rational approach to life. Pressures and fears similar to those that led to Jefferson's years of rationalization may affect us too. The incompatibility of animal rights with the monetary and scientific gains obtained from using animals as resources is not easily resolved. The treatment of animals may be our century's "Wolf by the Ears" (Miller 1977; Burghardt 1980).

Burghardt, G.M. and Herzog, H.A. Jr. Beyond conspecifics: Is  
brer rabbit our brother. Bioscience, 1980, 30, 763-768.

Miller, J.C. The Wolf by the Ears: Thomas Jefferson and Slavery.  
New York: Free Press, 1977.

Morris, D. The Naked Ape. New York: McGraw-Hill, 1967.

Hutt, S.J. and Hutt, C. Direct Observation and Measurement of  
Behavior. Springfield, IL: Charles C. Thomas, 1970.

### Animal Rights on the Professional Front

Paul J. Weldon  
Department of Zoology  
University of Tennessee, Knoxville

Mere mention today of animal rights is liable to conjure up mental images of shock punishments, brain implantations and other manipulations generally regarded as laboratory bound. Be certain that these are not misrepresentative of the practices targeted by humane activists, for much of the burgeoning front of the animal rights movement has indeed been brought to bear against the activities of laboratory scientists. Of course, not all of the criticisms leveled at these experimentalists have been wholly unimpassioned and data based, and this, quite expectedly, has given rise to staunch advocacy on both sides of the fence. These are not, after all, issues that call for resolution by referral to crisp differences on a histogram or to persuasive statistical treatments. But clear-cut abuse is extant in scattered corners of animal experimentation, and defenders of the rights of animals have rendered us mindful of the need for steps of improvement.

Concerns over animal rights, however, project far beyond that of the psychologically stressed subject in the laboratory. They touch firmly upon an environmental issue that has been noticeably with us for some time, though the aspect I dwell on here is not one ordinarily thought of as being in the vanguard of the so-called animal rights crusade. I am thinking of issues surrounding the appropriation, or misappropriation of animals from their natural settings. The most serious offenders here, of course, are the plundering mercenaries of the pet and animal skin trade. Few would suspect, however, that those who profess to entertain special fascination for animal life, the field oriented biologists, might also be culpable.

The Endangered Species Act and similarly striped measures do exist, of course, to curb overcollecting and overkill. Are these steps not sufficiently strident to keep potential transgressors from among professional ranks in check? Biologists are, after all, subject to the same punishment as that meted out to anyone else.

There can be no doubt that the impact of such legislation and the enforcement of it has indeed been profound. It should come as no surprise to readers of this Newsletter that this has seldom been as effective as it has in the efforts protective of primate species. Currently enforced measures of protection have placed the emphasis where it belongs, and that, of course, is on the behalf of species flirting with drastic population downswings and on the brink of extermination.

But a species is usually ordained worthy of protection long after destructive inroads have splintered populations into isolated parcels. Obviously, they should be reached before being reduced to

such vanishingly small numbers. This, unfortunately, is not always realistic, especially where the species involved is among the more arcane and little known to those interested in initiating official protective measures. As with some of the issues revolving around the treatment of laboratory animals, there are some grey areas encountered in the rapprochement of the demands of research and animal welfare. But in some stark cases, a larger share of sympathy must fall in favor of the exercise of professional restraint.

Consider the study of Tu (1974) published in Journal of Herpetology. This investigator, during three field trips to the Gulf of Thailand over a span of five years, collected in all 14,282 sea snakes of various species. "As soon as the identification of species was complete, the snakes were decapitated, and the venom glands excised." In the results section, however, he confesses that "the method described for obtaining venoms is not very good since the crude material contains much cellular debris" and that "the toxicity is lower than that obtained by the milking method." Pity that Tu did not hit upon the use of this more conventional method of venom extraction at some earlier stage of his collecting campaign since this might have rendered unnecessary the slaughtering of multitudes of snakes.

For those parties interested in examining Tu's specimens, not a clue is given as to the whereabouts of the remains of the snakes he caught, though the author does cite several supporting grants and the names of a few colleagues.

From my encounters with the literature, Tu's study has the opprobrious distinction of being among the most recklessly executed. His are not typical of methods of field sampling in herpetology, and neither are they representative of approaches adopted in other field-oriented disciplines. But the fact that this paper found its way into a refereed journal suggests some severe short-circuiting of concerns for animal welfare along conventional channels of review and editorship. The need for heightening sensitivities over animal welfare on professional fronts can scarcely be overemphasized. I suggest here a few lines of action that might contribute to such an effect.

The first is an appeal to authors who themselves appropriate animals from the field to cite collecting permits and other authorization obtained. Editors and reviewers should prevail upon journal contributors to act in accordance with such a policy, particularly where substantial numbers of animals are taken. This takes little more space than that occupied by an acknowledgement to an anonymous reviewer, and it at least places in the forefront a symbol of respect for animal underexploitation. At most, a popularization of this practice might remind less experienced workers of the proprieties of field research.

Museum, university, and laboratory collections should be designated in published reports as repositories for

once-experimental animals. It is just possible that research demands unrelated to those that inspired the initial collecting could be satisfied by researchers availing themselves of specimens already obtained. Stomach content analyses and the procurement of specimens for undergraduate course demonstrations are just a couple of "salvage" projects that come to mind. When the gates of communication are opened, both the duplication of research effort and accumulation of stress placed on local populations will be significantly cut.

My last point is a plea for vigilance. Few onlookers are in the position of appreciating the precariousness of wild populations as well as those individuals who have studied them. It is up to members of the scientific community to "keep their ears to the tracks" and rise to the defense of potentially oppressed populations. We should not lose sight of the fact that the safeguarding of animal resources is as high a professional priority as we possess. To paraphrase William Beebe, another heaven and another earth must come to pass before they can be replaced.

Tu, A.T. Sea snake investigation in the Gulf of Thailand. J. Herpetology 1974, 8, 201-210.

Toward an Ethics of Animal Experimentation:  
A Look at Methods and Assumptions

Deborah Kleese  
Hudson Valley Health Systems Agency  
Tuxedo NY

It is sometimes implied by researchers that one must accept the legitimacy of all experimentation or forfeit the right to carry out any experimentation. From a purely ethical standpoint this has some credence. For instance, use of animal experimentation is often defended on utilitarian grounds; in this framework, it is suggested that it is occasionally difficult to adequately assess the immediate usefulness of a given research project. Projects may have future gains or payoffs that are unforeseen by the original researchers. The safer tactic seems to be to approve of all research, with the assumption that the truly useful results will justify the less heuristic ones.

It can be argued, however, that a consequentialist approach alone is insufficient. The methods and the assumptions that buttress the methodology must be evaluated as well. Since my training is as a psychologist, I will mainly use psychological research as examples.

The major problems with animal experimentation may rest more heavily on methodological/epistemological grounds than on ethical grounds. Some experiments be they with non-humans or with humans,

are by virtue of their quest for good control, confounded by a host of variables that question the external validity of the results. These concerns have been aptly critiqued in the social psychological literature (Kelman 1972; Miller 1972; Orne 1962, 1968; Rosenthal 1963, 1966) and, to some extent, by neurophysiologists (Bullock 1970; Weiss, 1968, 1970; Walter 1968). The general issue of the "perturber-observer" strikes at a fundamental concern with the nature of much psychological research, and brings us to the issue of epistemology.

With the exception of Koch (1959, 1961, 1971, 1981) among others, few psychologists today have undertaken a thorough study and critique of the assumptions surrounding their theory and research. Interestingly, the strongest voices examining the scientific method and its implications for our worldview have come from scholars working in the "periphery" of the typical bounds of the discipline. These include Mumford (1970), Marcuse (1964) and Habermas (1970, 1971). What is oddly provocative here is that the focus of study of the aforementioned critics also includes human behavior, much as psychology does. However, these writers, whose focal point has been the study of social systems, have been less enamored with the tenets of scientism as a sufficient methodology for the study of behavior.

Enmeshed within the tenets are certain assumptions about the control of variables. These premises had been fairly effective within the physical sciences and have been affirmed as the outcome of scientific principles in the parallel development of technology. The same assumptions that have tracked the development of science and technology have implicit consequences for the development of a method mirroring that of the physical sciences within the social/behavioral sciences.

Mumford (1970) discusses the evolution of science as technology and the ramifications of this amalgamation in contemporary Western culture, suggesting that a major premise of the interdependence of science and technology is an assumption of infinite growth and control:

"The chief premise common to both technology and science is the notion that there are no desirable limits to the increase of knowledge of natural goods, of environmental control; that quantitative productivity is an end in itself, and that every means should be used to further expansion." (p. 127)

Now, the mechanistic/empirical traditions that embodied and ultimately laid the framework for a model of prediction and control of variables had its logical consequence in the prediction and control of non-human and human behavior. This is the legacy that psychology has inherited.



The first half of the twentieth century and well into the beginning of the second half of this century marked the reign of the paradigm of subject-as-object. The "objectification" of subject, as a logical outcome of the mechanistic/empiricistic traditions embracing the "objectification" of nature, enabled psychologists to extract the seminal elements of the laboratory method for the study of behavior. This model was so attractive that the behavioristic tradition, linked to the empirical roots of the seventeenth and eighteenth centuries, would suggest that the development of a technology of behavior was possible through these methods. Animals, in this framework, became regarded as another aspect of nature to be "objectified" and controlled.

It is not surprising that the limits of these methods were primarily critiqued by social psychologists, whose domain included the communicative structures so necessary in Marcuse's and Habermas' analyses. Some rumblings were heard among comparative psychologists also. Certainly Beach (1950) and Breland and Breland (1951, 1961) alluded to methodological problems. The primary thrust of their arguments, though, was toward the limits of use of a specific subject population and of a specific laboratory design, respectively. Nevertheless, a secondary premise to both of these is, again, the problem of external validity.

The adoption of a method encompasses both certain assumptions about the universe as well as certain values about that universe. Kuhn (1962) has discussed this issue and has defined a scientific paradigm as embracing both the practice of a science and the values of a science. Both Habermas and Koch have tackled the ideological implications of science, and have noted that the assumptions of a science stretch into the socio-political realm as well.

It is time for psychologists to question the assumptions on which their methodologies rest; to ignore these is to fall prey to what Koch (1981) so aptly phrased "epistemopathic" practices. If we accept that experimentation cannot be evaluated separately from its methods and valuations, then the following must be considered when evaluating the legitimacy of animal experimentation:

1. Animal research made legitimate solely for the sake of research knowledge. C.R. Gallistel (1981) has argued, in defense of animal research, that:

"...I place a very high moral value on the advancement of human understanding. Those for whom science has no moral value will find my argument without force, assuming that they are also unmoved by the prospect that such understanding will alleviate human suffering." (p. 360)

Implicit in this statement seems to be the assumption that advancement of human understanding is the highest imperative. The "morality" of science is a curious phrase. If we accept that the

rise of science reflects a historical process rooted in developments evidenced in sixteenth-century Western culture on, then the present form is an amalgam of several worldviews and practices. These are as much the result of sociopolitical processes as they are reflective of some sort of morality. Perhaps Mumford (1970) is correct in noting:

"...The belief that science developed solely out of a pursuit of knowledge for its own sake is at best only a half-truth, and at worst, mere self-flattery or self deception on the part of scientists. As with the holiness of saints, which has bestowed unwarranted authority on the grosser worldly claims of the Christian Church, the total effect of scientific ideology has been to provide both the means and the justification for achieving external control over all manifestations of natural existence, including men's own life." (p. 106)

Animal studies purely for the sake of curiosity, or as a function for a quest for knowledge ignore the larger issue of validity. As psychologists, we have often times blindly followed the assumptions of our methods and generated data of little relevance outside of a laboratory context. This stems from a tradition which, as pointed out by Mumford (1970), illustrated the following: "...its pragmatic efficiency counterbalanced by its conceptual superficiality." (p. 68)

The crisis in comparative psychology has as much to do with its inability to examine the very underpinnings of its approach as it does with the perceived lack of interest in a comparative approach to psychology. First, this branch of psychology has to confront its rather schizoid assumptions-- i.e., on the one hand, the assumption that there is credence, due to certain common principles of structure/function, to applying a cross-species approach, and on the other hand, the practicing of a notion, a la Descartes, of animals as objects/machines.

One could argue that many psychologists apply a mechanistic notion of behavior to humans as well as to non-humans and thus avoid a logical fallacy. I am not implying a retreat into either zoomorphism or anthropomorphism. Rather, if one acknowledges that the episodic nature of the laboratory method seriously questions the external, and often times even internal validity of a research design, then one can make the same case for much of animal experimentation, where the social and ecological matrices of a species are ignored or rendered unimportant by the experimenter.

The attractiveness of the ethological approach has, to a large degree, been its method. Although one can take issue with some of the theoretical assumptions that some ethologists embrace, the

methods have constituted a milestone in the field of animal behavior. It is time that psychology paid more attention to the various naturalistic methods.

2. Are there alternative means of generating data that do not cause undue pain or harm to our subjects. Just as the effects of expectancy on the part of the experimenter have generated a crisis in the field of social psychology and a search for more reality-based methods, so is the onus of responsibility on those working with animals to generate alternative methods. The use of naturalistic and semi-naturalistic methods needs to be explored further; if the "control" of the laboratory is introducing as many confounding variables as it attempts to allay, then the validity of the method must be taken to task.

3. Is the experiment necessary? The answer to this question should include an examination of the following:

Ethical grounds. It is not enough to accept the premise that, since one never knows whether an experiment will yield meaningful data, it is justifiable to carry out all experimentation. One has to make certain decisions about the possible usefulness of results. Gallistel was correct in noting that when the consequence is the alleviation of human suffering, an experiment is justified. As humans, our ultimate allegiance should be to our own species. However, rarely are experiments as clear-cut as Gallistel would have us believe. The notion of applying cost-benefit analysis implies that the variables are always easily determined and quantifiable. As Modlin (1973) puts it: "Costs and benefits must be estimated not only in quantitative, dollar terms but in terms of qualitative and aesthetic judgements." (p. 158)

Methodological grounds. Animal experimenters need to be more creative and more critical of their designs. Simple-minded or single-minded research which ignores the contextual significance adds little to our understanding of the species under study or of behavior in general. Such experiments may even qualify as unethical by virtue of the unsoundness of the methodology.

Examination of underlying assumptions of our work. It is incumbent on all scientists to discover the philosophical roots of their discipline and to seek to understand how certain methodological "givens" are derived from certain basic presuppositions about nature.

Decisions regarding appropriate treatment of animals in experimentation may ultimately rest on the insights we derive from an examination of our own intellectual history.

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Postscript: Some readers may not be aware of two writings related to the issue of animal experimentation: 1) Guidelines for the Use of Animals in School Science Behavior Projects, developed by the APA Committee on Animal Research and experimentation, in American Psychologist, 1981, 36, 686. Send requests for reprints to the Scientific Affairs Office, American Psychological Association, 1200 Seventeenth St., N.W., Washington DC 20036; and 2) Statement by the American Society of Zoologists on HR 556, the Research Modernization Act, currently in Congress, regarding funding of studies of alternative methods of animal research that do not use live animals. For a copy, contact the editor.

**FALL FORUM: Please respond by October 15**

The fall forum question posed by Thomas Wiegele and Roger Masters is:

How can human ethology illuminate the study of politics?

Please send your comments on this topic to Tom at the Center for Biopolitical Research, Northern Illinois University, Dekalb IL 60115, or to Roger at the Department of Government, Dartmouth College, Hanover NH 03755.

FORUM UPDATE

Although no responses from our membership to the Spring Forum questions on future utility of the term Sociobiology were received, the book review by S.L. Washburn that was substituted has indeed raised eyebrows in nonconcurrence. For your interest, two reactions are presented below. I wish to thank the authors for taking the time to reply. It would seem that, contrary to the implicit intent of the forum question which raised the issue as to the status of Sociobiology, the controversy that the term engenders is still serviceable in clarifying ideas and issues.

Comments on S.L. Washburn's review of Kenneth Bock's HUMAN NATURE  
AND HISTORY: A RESPONSE TO SOCIOBIOLOGY

William Charlesworth  
Institute of Child Development  
University of Minnesota

Professor Washburn's review (Newsletter, June 1981) of Bock's book drives another wedge between biology and the social sciences. This is unfortunate since human social systems and biological matters are obviously very much related. Population density, resource availability, physical health, nutrition, reproductive strategies, etc. are all implicated in the development of human social and cultural systems just as these systems, in turn, are implicated in the shaping of biological matters. Only scientists overly committed to defending their own disciplines keep them apart.

That "the universals of biology or genetic theory cannot account for recent history of differences between cultures" is an unnecessarily limiting statement at our present state of knowledge. We do not know yet if such universals can or cannot account in any significant way for man's cultural achievements because they still have not been extensively and systematically applied. And they will not be unless interdisciplinary boundaries are completely broken down. In my estimation, the people who can best test human sociobiological theory at present are social scientists willing to learn from sociobiologists and willing to commit themselves (tentatively at least) to the theory, forcing specific hypotheses from it, and spending time testing them empirically. Trying to wipe out the theory with rhetoric seems less intellectually challenging than trying to wipe it out with facts.

Speaking of rhetoric, there should be an editorial rule that sentences associated with sociobiology, with efforts to "justify slavery," imperialism, racism, genocide, and to oppose equal rights or ERA" should always appear next to sentences associating environmentalist/learning theory, with efforts to justify propaganda, psychological terror, false advertisement, public indoctrination of hatred of foreigners, class enemies, minority

groups, and so on and so on. Juxtaposing sociobiology and learning theory in this manner ought to show how unproductive it is to claim through innuendo or otherwise that science will lead to pseudoscience, will lead to man's inhumanity to man: ergo no science. Actually, one could argue that since man is such a cultural/learning animal we should have greater fear of learning theory since learning has far more power over man's behavior than genes. More specifically, if humans were not such learning animals, they would not learn all that Galton trash: ergo stop learning research so that bad guys will not use the data to teach the trash more effectively.

I agree with Professor Washburn on the futility of finding genes for culture and transportation systems. But I do not think it hurts to have wild working hypotheses (a la Paul Feyerabend) to open new areas of research. There is a big difference between a theory that aims at explaining facts and a theory that seeks to generate them. I put sociobiological theory in the latter class and would be surprised if most sociobiologists would not do the same. Ten years ago the notion that ethology would be relevant for studying human behavior was ridiculed in many circles. Today it is accepted, despite the fact that it still has an uphill fight to make a substantial novel contribution to our understanding of human behavior. I predict that the same thing will happen to sociobiology.

Comments on S.L. Washburn's review of Kenneth Bock's book

Roger D. Masters  
Department of Government  
Dartmouth College

There is something funny about Professor S.L. Washburn's review of Bock's Human Nature and History (reprinted in the June Newsletter from the New York Review of Books). Washburn says that Bock has countered recent sociobiological writing by "showing the way human actions make history by demonstrating again and again that this history cannot be explained by genetic or other biological factors...Changes in technology certainly affect the way people live, but we would learn nothing from biology about the causes of these changes." Apparently, Washburn thinks that all evolutionary models of human social behavior are based on the assumption that genes cause everything we do. How else are we to interpret such phrases as: "The fundamental problem with...sociobiological theory is that no genes are known for altruism, aggression, or other categories of behavior. As a genetic theory without genes, sociobiology has great difficulty in presenting any substantial evidence for its numerous explanations."

It is hard to believe that Professor Washburn has never encountered the distinction between "proximate" and "ultimate"

causation (Kummer, 1971; Barash, 1977). Genes are causes at the level of individual phenotypic expression (albeit always in interaction with environment and experience throughout the development process). Natural selection operates at a different level, since it concerns the ultimate causation of differences between and within populations over time. While some popularizations of sociobiology have misled superficial readers into thinking that the theory requires that the proximate causation of all behavior be under narrow energetic control, virtually no serious theoretical presentation of inclusive fitness theory makes such an assumption. As Richard Alexander put it, this approach presumes nothing "more complex or deterministic than learning through ordinary positive and negative reinforcement schedules" (1977:13) since it focuses on questions of ultimate causation.

In fact, inclusive fitness theory is a cost-benefit theory very much like rational actor or decision-making theories currently commonplace in the social sciences. Hence numerous scholars have shown the similarity between sociobiological theory and game theory (Maynard Smith, 1978), economic theory (Hirshleifer, 1978), and rational choice theory (Margolis, 1981; Aelrod and Hamilton, 1981). Such models can describe phenomena ranging from conscious human choices to instinctive behavior in lower animals -- not by reducing human thought to genetic determinism, but rather by pointing to environmental variables that influence the costs and benefits of alternative responses (however they might be acquired). And despite the canard that sociobiology is reactionary, there is more than enough evidence that the theory can have varied ideological implications -- some of which are quite radical (Tiger, 1980; White, 1981; Masters, 1982).

It would not be useful or proper to speculate, ad hominem, on the reasons for Washburn's caricature of inclusive fitness theory. Perhaps it is time to focus on empirical tests of this approach rather than programmatic statements pro and con. To this end, it would be interesting to know what Professor Washburn thinks of a concrete application of inclusive fitness models to explain cultural differences between societies. I would like to know, therefore, whether Professor Washburn has encountered any explanation of the mating system of traditional Northern India, known as "hypergamy" or "hypergyny," that is superior to the work of Mildred Dickemann (1979a; 1979b)? And since Dickemann's analysis treats ecological catastrophe and rigid social stratification as key variables, is Professor Washburn concerned that it will challenge the autonomy of the social sciences?

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BOOK REVIEWS

I am happy to report that our book review committee is active and productive. However, the formation of a committee does not at all preclude individual members from submitting their own timely reviews. Other contributions are encouraged and will be considered for inclusion in the Newsletter.

THE ECOLOGY OF PRESCHOOL BEHAVIOUR. By Peter K. Smith and Kevin J. Connolly. Cambridge: Cambridge University Press. 383 pp. (1981)

Reviewed by W.C. McGrew, Department of Psychology  
University of Stirling, Scotland

Ethological studies of children began 18 years ago in pre-schools, as observed by Blurton Jones. Since then, most of the founder child-watchers have diverged, diversified, or disappeared. (This author, e.g., 10 years ago took what was meant to be a temporary detour into studies of chimpanzees and has yet to emerge).

Smith and Connolly have continued to work the main vein, however, and this book is the product of their extended labours.

The book is a research monograph which describes a series of studies undertaken over 3 years in a play-group in Sheffield. Because they were able to shape the formation of the group, from choice of subjects and staff to the contents of the playroom, they were able to conduct elegant manipulations from the start. For example, each study was done on 2 independent groups, allowing simultaneous replications of results. At the same time, they were careful to remain within the norms of pre-school education in the U.K., thus maximising the ability to generalize their findings to the real world. This is not the elite university nursery school composed of the offspring of academic staff.

The key word in the title is ecology, which indicates the authors' concern with the most basic social and physical variables in the environment. Therefore, they examined the effects of such things as number of children, amount of space, quality and quantity of resources, staff-child ratios, directed versus unstructured regimens. The effects were measured in a variety of ways from sociograms to staff ratings, but the mainstay was recording 89 molecular units of behavior carefully defined in an appendix. (In fact, as is often the case, only a minority (27) of these categories are used in the text.)

It is impossible to summarise adequately the results here, but in the final chapter Smith and Connolly are able to provide practical advice on how to decrease or increase aggression, fantasy play, attention span, same-sex versus cross-sex subgroups, etc. Many of the results are little more than confirmation of intuitive suspicions, but there are surprises too. For instance, the best way to produce creativity in the use of objects in free play is to remove smaller toys, thus reducing freedom of choice. If this sounds heretical, the reader is referred to Chapter 7. Also, on a simpler point, the inaccuracy and unreliability of even trained staff members' impressions is pointed out (albeit tactfully), when these are compared with empirical findings. This emphasizes all the more the value of direct observation.

The book's strengths are many: The studies are admirably designed. (They are not, by the way, experiments, as no hypotheses are posed or tested.) They are explicitly described, so that any replication should be readily done. Appendices of almost 40 pages provide complete details on subjects and raw data on the 89 behaviour units, should the reader wish to do any re-analysis. Attention is drawn to the many studies of children done in the 1920's and 30's in the U.S., using observational methods. These anticipated many of the findings of the child ethology boom, and Smith and Connolly give them proper credit while citing their shortcomings. This makes the bibliography of about 250 references even more valuable.

The book's weaknesses are fewer: By and large, the presentation, though clear and cogent, is on the dry side. There are virtually no anecdotes to help bring to life the events so systematically recorded. Sometimes this would have provided relief from the mass of results. This is compounded by the paucity of illustrations; there is not a single photograph in the book, and with the exception of a few sociograms and floor plans, all results are presented in tables--there are no graphs. Methodologically, the authors cite advances in observational techniques borrowed from studies on non-human primates, but many key decisions seem to have been made on dead reckoning rather than by calculation, e.g. frequency and duration of sampling, though systematic means are available (e.g. Kramer et al., 1977).

Conceptually the book is impressive, but there are issues for debate. Smith and Connolly rightly point out that previous studies of the effects of varying conditions of density on children's behaviour have ignored or confounded even the primary variables of space and group size. They remedy some of these but go on to the dubious extreme of considering space as a resource, per se. Apart from an organism's personal space, this makes little sense. Space is not in itself a resource; it is neutral, as is time. Ecologically, space is only meaningful in relation to the distribution of resources, whether these be calories or Wendy Houses.

Another point for debate is the minimal consideration given to the pre-school environment in a broader perspective. It should not be treated as a given institution, the form of which is taken for granted, an entity only in need of tinkering adjustment. Apart from being ethnocentric (and what miniscule proportion of the world's children take part in any form of pre-school education?), it may be evolutionarily unsound. Konner (1976) has pointed out the un-naturalness of confining children in narrowly banded age groups, by comparison with the wider (and richer?) social networks of children in gathering-hunting societies. Weisner and Gallimore (1977) have shown that care-taking by other children, both kin and non-kin, is an important aspect of child-rearing in many non-Western societies. By isolating children in pre-schools from others, we may be depriving them. Smith and Connolly only touch on this point in passing--it deserves more.

In summary, the book is highly recommended to anyone doing observational studies of human behaviour. It should be required reading for anyone studying children in play-groups, nursery schools, kindergartens, etc. Unfortunately, the book's cost (£25.00) may deter students from buying it, but one hopes that it will be made available and urged upon them.

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PROGRAMMED TO LEARN: AN ESSAY ON THE EVOLUTION OF CULTURE. By H.  
Ronald Pulliam and Christopher Dunford. New York: Columbia  
University Press. 138 pp. (1980)

Reviewed by Edward J. Korber, Department of Psychology  
Queens College, Flushing, New York

With variations within contemporary social science disciplines  
being far greater than variations between disciplines, it is not  
surprising to find a proliferation of texts attempting to deal with  
the latter. Programmed to Learn: An Essay on the Evolution of  
Culture is one such effort which, as the authors have hoped,  
attempts to bridge the gap between diverse "biological and  
social-scientific viewpoints." Not surprisingly, this work does  
suffer from a number of inadequacies. However, Pulliam and  
Dunford's effort to integrate empirically-based concepts from a  
broad spectrum of disciplines not their own is a laudable one  
deserving further attention.

The text, divided into eight brief chapters, notes and index,  
takes the reader from the basic supposition that genetic selection  
"favors" the evolution of "learning mechanisms" to an account of  
cultural evolution which, as the authors put it, owes its origin and  
its rules to genetic evolution but has a momentum all its own. The  
task is an arduous one, but the theme suggesting that cultural  
"traits" spread within and between populations because they enhance  
relative fitness is consistently developed throughout the book. One  
will also recognize a reductionistic trend in that propositions  
about aggregates follow from the propositions dealing with  
individuals.

In chapters two and three, the authors argue that the impact of  
the process of natural selection lies in the setting of limits for  
universal neurophysiological mechanisms underlying motivation and  
learning phenomena. The hypothesized process is one such that  
biologically adaptive choice is fostered while allowing for  
diversity in the development and maintenance of behavioral patterns,  
a position similar to that held by Durham (1979) and Ruyle (1973).  
This argument is further elaborated in chapter four, where the  
authors speculate on the adaptive value of observational learning,  
"the basic mechanism by which information is culturally transmitted  
from generation to generation," and in chapter five, where social  
exchange and cognitive balance theory are introduced to account for  
adaptive innovation within groups.

Chapters six, seven and eight provide brief treatment of Cavalli-Sforza and Feldman's mathematical modeling in addition to a less formal explication of the manner in which the behavior of individuals and the consequences of their actions feed back to perpetuate the development, maintenance and elimination of behavioral patterns, that is, cultural traits.

There is no doubt that those more familiar with the works of social learning theorists and interbehaviorists (i.e., Bandura 1977; Miller and Dollard 1941; Kantor 1959; Burgess and Bushnell 1969) will note the absence of reference to their efforts. In light of the authors' heavy reliance on "learning mechanisms" to help fill the gap between sequences of codons and the cultural transmission of ideas, one might expect greater reference to and exploration of this relevant "social science" literature. However, when all is considered, readers of this slim volume will probably find Pulliam and Dunford's effort thought provoking and useful both in the class and in their own efforts to integrate relevant empirical research into a meaningful conceptual framework.

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#### MINI COMMUNICATIONS

Short empirical or theoretical papers on new questions or unresolved issues of interest to our members are invited for consideration as Mini Communications. Let colleagues facilitate your novel research directions or suggest alternative approaches to your nagging scientific problems by submitting a contribution for comments, interaction and/or visibility.

We have Ian Vine to thank for this issue's Mini Communication, a theoretical discourse on the short and long of morality in increasing inclusive fitness. It touches not only on the role of selfishness but self-deception in the guise of righteousness.

### The Social Evolution of Morality

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Bradford, England

Early attempts to discover an evolutionary pattern in the development of human societies failed, partly because of misapplications of natural selection theory, and partly because of ethnocentric prejudices. Recently interest in social evolution has recovered, especially amongst anthropologists and others stimulated by the emergence of sociobiology as a discipline. In this paper I explore the implications of recent sociobiological analyses for our understanding of the functions of moral codes and how they change. In order to reach the more interesting implications it will be necessary to assume several disputed premises without justifying them here, although I believe that this could, in fact, be done. My conclusion is that morality is very much a mixed blessing.

Sociobiological research has clarified our understanding of how biological evolution operates. As I have summarized elsewhere (Vine 1980a), it shows that normally selection works on the phenotypical behaviors of individuals, favoring traits which are adaptive in the sense that they maximize an individual's "inclusive fitness" (IF) relative to reproductive competitors. In other words, traits will be selected if they tend to maximize the presence in the gene pool of future generations of the owner's particular gene alleles -- through encouraging the survival and reproductive success of the owner, the owner's offspring, or others with whom many genes are shared through common genealogy (e.g., siblings). Thus, traits predisposing towards behavioral altruism (acts which benefit others adaptively more than they do the agent) will only be selected if they help closely related others ("kin selection"), or those who in the longer term will return favors ("reciprocity selection") since only in these cases do they maximize an agent's IF. More indiscriminate altruism is unlikely to have a genetic basis in any species, since biological selection would oppose this.

The analysis is applicable to humans because heritable dispositions need not be fixed instincts. We may say that a trait is "natural" and universal for our species if it reliably develops in all individuals across the normal range of human environments and lifestyles, and is stable across generations. Such traits are likely to be ones which our genotype specifically facilitates; but they can rely heavily on environmental experience, so long as that is virtually guaranteed by exposure to the physical world and to

"minimal socialization" of the kind every human must receive to survive. Natural human pro-social traits will thus be limited to those which tend to maximize IF, including mutually beneficial cooperation, inhibiting selfishness for longer term benefits, and some acts which benefit close kith and kin. We can acquire further pro-social traits through culturally facilitated learning, but such moral education may have to counter natural "selfish" biases against such traits which are biologically maladaptive to the individual. Intelligent reflection by a human agent may sustain and develop either what is optimal in IF terms in the long run, or what is not, but what has appeal for other reasons (especially in circumstances where survival and reproduction pressures are not limiting).

Cultural selection appears to parallel biological selection in most important respects (Langton 1979). Apart from the speed with which cultural selection can spread innovations, the major difference is probably that these are less random than genetic mutations. Several sociobiological analyses of culture have suggested the intriguing theory that with some exceptions, traits will only attain cultural heritability if they also tend to maximize the IF of most individuals (e.g., Alexander 1980; Durham 1979). According to Durham, the exceptions will be for traits with only marginal adaptive significance, in that they make trivial demands on time and energy budgets and have little connection with our central concerns of biological survival, courtship and reproduction, child-rearing and so on; or traits subject to "group selection" in the rare circumstances where discrete social groups operate as evolutionary units; or traits subject to manipulative or coercive pressure from powerful subgroups. In the latter case, power elites may induce even a majority of group members to act against their IF interests -- a point I shall return to later. Durham thus advocates a "coevolutionary synthesis" whereby both culture and biology typically encourage traits which tend to promote the IF of most members of any human group of society -- and we need not be concerned about the relative contributions of genes and environment. If this is true, we may consider social evolution as a general process whereby human traits arise and are spread within social systems, and are sustained or replaced by various means, largely according to how adaptive they are for the individual member in the light of a cluster of factors, including the group's ecology and mode of subsistence, its social structure, its relations with other groups, the inertia and momentum of its cultural processes, and the biologically shaped dispositions of its members.

It is important to stress that such a view deals with physical behaviors and their consequences, and says nothing directly about proximal causes, or the conscious beliefs, values and motives associated with particular traits. Other forms of personal and social selection may operate here, including ones which Durham takes to involve "arbitrary symbolic value." It remains to be shown how often culturally heritable norms, belief systems, rituals and so on will have a content entirely unshaped by adaptive considerations. But at the level of adaptively relevant behavior the theory suggests

that individuals will not typically act against their long term IF interests in any viable society. Nevertheless, it has implications which differ between societies insofar as they can be placed at different stages on a dimension of evolution from small, tight-knit, inbred, egalitarian subsistence cultures towards large, fragmented societies with highly differentiated social structures and surplus resources. At the more "primitive" extreme, natural pro-social traits will largely suffice to sustain the necessary interdependence of members and the order and harmony needed to maximize individual's IF. At the more "advanced" extreme a more formal morality will need to be specifically inculcated and sustained by institutional sanctions, even if only to guarantee long term IF self-interest, because of the more complex requirements for order and harmony. In addition, structural differentiation will increase the opportunities for power elites to form, and overtly or covertly to induce many individuals to act against their self-interest. This may be done to cover any contexts where the overall adaptive optimum for the group does diverge from that for a significant number of individuals -- or it may be abused by the power elite for purely sectional interests (to unfairly enhance their own IF).

The other major sociobiological perspective on culture is one in which biological and cultural selection are seen as potentially opposed, because cultural selection is held to maximize what is adaptive for the group as a whole, even when this is to the systematic disadvantage of some sections of it. Campbell (1975) sees cultural selection as favoring traits needed for the general welfare of the group, and thus having to counter the "selfish" bias in what is natural for the individual. He is led to adopt this view because, although there are ample psychological demonstrations of the power of self-interest in our behavior, we do sometimes behave in biologically maladaptive self-sacrificial ways (e.g., heroism in warfare). He argues that culturally sustained "moral tradition" is the source of such traits; but his most interesting hypothesis is that moral preaching will embody precepts which actually exaggerate the altruism and curbs on selfishness required for the group to survive and thrive, because our natural selfish biases will always preclude us from acting up fully to a moral precept. By exaggerating what is required cultural tradition can engender behavior which is actually roughly at the "bio-social optimum" for the group. Furthermore, Campbell assigns an important adaptive function to religions and other transcendental belief systems, which postulate an authoritative utopian moral ideal, more stable and trustworthy than any values promulgated by secular leaders under constant temptation to abuse their power for selfish gain.

I believe that despite their apparent divergence, a theoretical synthesis of the Durham and Campbell perspectives on cultural selection is possible. For the most part, especially in "primitive" societies, the data suggest that what is adaptive for the group does coincide with IF adaptiveness for individuals (e.g., Alexander 1980). But in more "advanced" societies especially, the natural pro-social tendencies are less likely to maximize IF because of the greater



complexity of relationships and associated uncertainty of long term reciprocity. Culturally sustained moral norms may for the most part simply counter our bias towards short term benefit, and encourage traits which are only beneficial to us in the longer term. Evolutionary processes are only probabilistic, so in some cases this will actually act to the disadvantage of individuals, due to unforeseeable accidents, or when norms which are actually maladaptive have attained temporary viability in the culture. Also, evolutionary wisdom is always about what was adaptive in past environments, so in time of rapid change its edicts may be outdated.

Furthermore, Durham surely underestimates the extent to which power elites can manipulate cultural processes to their sectional advantage. Where an elite is benevolent we have Campbell's case of maximizing what benefits the group, but commonly it will be corrupt. Only in open and democratic societies can such abuse readily be checked, limiting the self-serving biases of the moral legislators themselves. Here the value of the inertia and momentum in moral tradition is a further protection.

Elsewhere I have argued for a trend in moral evolution towards extending the definition of the "moral in-group" to encompass larger segments of humanity within the web of mutual obligations (Vine 1980b). The interdependence of societies within the modern world makes this adaptively necessary, and I believe that empirical trends can be detected towards this ethical philosophers' ideal. Kohlberg's stages of moral development are readily interpreted in such a way, and his cross-cultural research findings are thus compatible with the social evolutionary hypothesis. What the present analysis hints at is the real possibility that attaining truly universalistic moral commitments requires forms of social experience only attainable through occupying social roles of a kind only found in "advanced" societies, and presently only accessible to a privileged middle class. This relates to the hypothesis concerning power elites, for it will rarely be in their selfish interest to promote a truly universalistic morality. But they do stand to gain by inducing people to extend their moral in-group to the limits of the society that they control (roughly, Kohlberg's conventional Stage 4 morality). This encourages the self-sacrificial patriotism needed for expansionist warfare. Also, by stressing the differential obligations associated with various role positions they can more readily exploit the pro-social tendencies of the lower orders. Furthermore, their control of information coupled with the existence of discrepancies between moral practice and moral ideals, can legitimate their use of coercion to "enforce moral standards," both internally and in crusading wars. By highlighting human "imperfections," and attributing them to perversity, they can claim that out-groups have forfeited the right to humane treatment. If Trivers (1971) and other sociobiologists are right in claiming that IF considerations also lead to selection for deceptive and self-deceptive traits, power elites can readily pursue their own self-interest in the name of morality, sometimes without even realizing it.

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- Carew, J., Layzer, J. and Goodson, B. Naturalistic videotaping: A technological advance awaiting recognition by academics.
- Cox, D. An ethological analysis of sex-differences in naturally occurring preschool aggression.

- Darvill, D. and Cheyne, J.A. Sequential analysis of responses to aggression.
- Dunn, J. Infants and siblings.
- Fogel, A. Face-to-face interaction with 2-month-olds.
- Gaensbauer, T.J. and Schultz, L.A. Emotion and attachment: Interrelationships in a modified "strange situation" paradigm.
- Gustafson, G.E. and Green, J.A. Infants' initiations to observers of mother-infant interactions.
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Beck, B. Endangered species: Opportunity and challenge.

Borgia, G.\* Why do Bowerbirds build bowers?: A preliminary report on mate choice in the Satin Bowerbird.

Burghardt, G.M.\*, Bock, B.C. and Batts, B.G. Bicephalic serpents: Cooperation and conflict in the ultimate split brain.

Burley, N.\* Directionality of color band preference in the zebra finch.

Charlesworth, W.R.\* Field studies of problems and problem solving behavior in humans.

Chase, I.D.\* Dynamics of hierarchy formation: The sequential development of dominance relations in chickens.

DeBoer, M.M. Sex differences in infant behavior during mother-infant play.

DeCasper, A.J. Do human infants eavesdrop in the womb?

Essock-Vitale, S.M.\* and McGuire, M.T.\* Assessing predictions from the theories of kin selection and reciprocity using human social-support networks.

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Gauthier, R.\* A metric approach to dominance structure in a colony of baboons.

Gauthier, R.\*, Bolduc, J. and Strayer, F.F. The paradoxical crowding effect among captive monkeys.

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- Moore, M.M. Female choice and mischoice: The science of solicitation.
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Conservation News. The International Primatological Society's April, 1981 IPS News describes two conservation education projects. The first is in Cameroon, aimed at educating school children and villagers living in the immediate vicinity of three proposed forest

national parks and whose lifestyle is, therefore, likely to be affected by them. Director of this project is Philip Agland. The second project is in Ghana. IPS Secretary for Africa, Dr. Emmanuel Asibey, has been able to establish a conservation education facility in the Bia National Park. Bia is one of the few remaining pristine forests in that region of Africa. It contains three species of Cercopithecus, three Colobus, one Cercocebus and chimpanzees.

Wanted: Human Facial Expressions. For a research project on the computer analysis of human facial movement, Wolfgang M. Schleidt is looking for existing 16mm movie material of faces, preferably taken from the front, which show spontaneous or elicited facial expressions (no actors acting!). Do not send any material; just drop a line that states what kind of material is potentially available. Also, he is curious whether anyone else is working on such a project (or has given up, or is planning to start), and would like to hear about it. He has about one year's worth of experience, interactive computer programs for data analysis (including 3D rectification to standardized front view), limited equipment, but NO funds. Contact: Wolfgang M. Schleidt, Department of Zoology, University of Maryland, College Park MD 20742.

APA Update. Committees of the American Psychological Association which might be of interest: Committee on Animal Research and Experimentation, Perrie M. Adams (Chair), Donald R. Meyer, Jeri Sechzer, Richard Solomon, Gordon J. Gallup, Evalyn S. Segal; Committee on Research Support, Frances D. Horowitz (Chair), Victor G. Laties, J. Bruce Overmier, James G. Greeno, Richard E. Nisbett, William F. Prokasy. Divisions of the APA which might be of interest: Physiological and Comparative Psychology, President Richard L. Solomon, President-elect William A. Mason, Secretary-Treasurer William B. Schrader; Developmental Psychology, President Lewis P. Lipsitt, President-elect Jeanne H. Block, Secretary-Treasurer Carolyn U. Shantz; Psychology of Women, President Rhoda K. Unger, President-elect Michele Andrisin Wittig, Secretary-Treasurer Jeanne Maracek, Newsletter editors Cheryl Travis and Pamela T. Reid.

Walk On the Wild Side... The Seattle Zoological Society and Nature Expeditions International present an 18-day safari to study and photograph the natural history of the different environments of Tanzania, with special interest in wildlife observation and behavior. Traveling primarily on foot or in four-wheel drive vehicles, explore Arusha, Lake Manyara and Serengeti, in addition to investigating Lake Natron for birdlife, Mt. L'Engai for volcanic geology, and Olduvai Gorge for remains of early man. Highlights include camping and observing wildlife in Ngorongoro Crater and walking in rarely visited Empakaai Crater. A "dream safari" led by a wildlife authority; challenging and adventurous, including some of the least visited areas in East Africa's spectacular Rift Valley. Departure: February 1982. Trip Length: 18 days. Land Cost: \$2090. Airfare: \$1570 RT from Seattle. Maximum number: 16 participants. Tour Guide: Hank Klein, Assistant Curator of

Education, Woodland Park Zoological Gardens, 5500 Phinney Ave. No.,  
Seattle WA 98103; (206) 625-4550.

UPCOMING MEETINGS

American Association for the Advancement of Science annual meeting. January 3-8, 1982 in Washington DC. A symposium devoted to "Ethological Approaches to the Study of Politics" will be held January 6 at the Capital Hilton Hotel. Albert Somit will be presiding at the symposium, and papers will be given by: Carol Barnes Barry on "Ethological Methods in the Study of Basic Types of Political Behavior"; Michael T. McGuire on "Biological Correlates of Social Status Changes in Vervet Monkeys"; Roger D. Masters on "Nice Guys Don't Finish Last: Aggressive and Appeasement Gestures in Media Images of Politicians"; Glendon Schubert on "Ethological Politics"; and Lionel Tiger on "Toward a Veterinarian Theory for a Crowded Planet". Discussant is John Wahlke. For more information, write to Roger Masters, Dept of Government, Dartmouth College, Hanover NH 03755.

Virus Laboratory Workshop. February 24-26, 1982 in San Antonio. This is a workshop on the laboratory activities of the NIH and WHO Collaborating Center for Reference and Research in Simian Viruses. It will be held just prior to the Conference on "The Use of Nonhuman Primates in Exotic Viral and Immunologic Diseases" to be held February 28-March 3 in San Antonio. For information, write to Dr. S.S. Kalter, Southwest Foundation for Research and Education, P.O. Box 28147, San Antonio TX 78284.

American Association of Physical Anthropologists annual meeting. April 1-3, 1982 in Eugene, Oregon. In past meetings, numerous papers have been presented in the areas of human ethology and sociobiology. One need not be a member of AAPA to present a paper. Deadlines for submission of symposium proposals is October 1, 1981, and for poster presentation plans is December 31, 1981. Preregistration fees are \$15 for student members of AAPA, \$25 for regular members, and \$30 for nonmembers. For additional information, write to Ron Weigel, Human Ethology Laboratory, Neuropsychiatric Institute, UCLA, Los Angeles CA 90024.

International Symposium on the Conservation of the Liontailed Macaque. May 19-22, 1982 in Baltimore MD. Further information available in June, 1981 Newsletter.

2nd International Symposim on Marine Biogeography and Evolution in the Pacific. July 5-7, 1982 in Sydney, Perth, Melbourne, Australia. More information in June, 1981 Newsletter.

20th International Congress of Applied Psychology. July 25-31, 1982 in Edinburgh, Scotland.

International Society for Human Ethology (ISHE). ISHE will meet with the International Primatological Society and the American Society of Primatologists as part of the IXth Congress of the International Primatological Society in Atlanta, Georgia during August 8-13, 1982. This meeting will be hosted by the Yerkes Regional Primate Research Center of Emory University. The meeting site is the Colony Square Hotel in Atlanta. Housing will be available at the Colony Square, and less expensive accommodations will be available in the dormitories of the Georgia Institute of Technology.

The meetings will focus on numerous symposia and workshops, which will integrate the interests of the three participating societies. The deadline for submitting symposia proposals and abstracts for the International meeting is January 1, 1982 (postmark). General information about the meetings, as well as the necessary forms will be sent to ISHE members soon.

This is the first human ethology conference since the 1975 meeting in Sheffield, England. We urge that you mark these dates on your calendar and make plans to attend. We particularly hope that non-North American human ethologists will make every effort to attend. Atlanta is an excellent location, having an international airport with non-stop flights to such European cities as London, Amsterdam, Brussels, and Frankfurt. Atlanta is an exciting city with many cultural attractions, being a modern cosmopolitan city, yet retaining much of the culture of the Old South.

For additional information, please contact: Ron Weigel, Human Ethology Laboratory, Neuropsychiatric Institute, UCLA, Los Angeles CA 90024; or Gail Zivin, Dept of Psychiatry and Human Behavior, Thomas Jefferson Medical College, 1015 Walnut Street, Philadelphia PA 19107.

Animal Behavior Society. The next ABS meeting will be held in August 1982 at the University of Minnesota, Duluth. Sponsor will be Mitzi Doane. The 1983 meeting will be held in June at Bucknell University, Lewisburg, Pennsylvania. Douglas Candland will be the sponsor. In August 1984, Steven Christopher will sponsor the ABS meeting at Eastern Washington University, Cheney, Washington.

#### Meeting Reminders:

International Ethological Conference. September 1-9, 1981 in Oxford, England.

6th International Congress of Human Genetics. September 13-18, 1981 in Jerusalem, Israel.

Law and Behavioral Research Symposium. September 25-27, 1981 in Monterey Dunes, California. Activities include roundtable discussions on: 1) Motivation, Moral Development and Justice, 2) Law and Justice as Biological Phenomena, and 3) Group Control and Social Organization. At each of the roundtables, 5-6 papers will be

discussed. For information, write to Roger Masters, Dept of Government, Dartmouth College, Hanover NH 03755.

International Conference on the Human-Companion Animal Bond. October 5-7, 1981 in Philadelphia. More details in June, 1981 Newsletter.

Meeting of the American Association of Zoologists. December 27-30, 1981 in Dallas. At this meeting, the Division of Animal Behavior and the Animal Behavior Society are sponsoring a symposium on the "Optimization of Behavior." More details in June, 1981 Newsletter.

## SURVEY

Several recent articles in the Laboratory Primate Newsletter (Caine et al., 1979, 18 (1), 25-26; Hughes & Lang, 1980, 19 (3), 11-12;; Kessler et al., 1980, 19 (2), 9-10; Rhine et al., 1981, 20 (2), 5-7) have reported predatory behavior in captive primates. We suspect that many investigators witness these, but do not report them due to their seemingly anecdotal nature. Therefore, we have initiated a survey of predatory behavior in captive primates in order to ascertain how common it is. If anyone has witnessed captive predatory behavior, please help us by completing the following questionnaire and mailing it to the address below, even if the information is incomplete. Any help you can give will be greatly appreciated.---  
Susan Clarke and G. Mitchell, Dept of Psychology, University of California, Davis CA 95616

Your name \_\_\_\_\_ Primate Species \_\_\_\_\_

Captive or wild-born? \_\_\_\_\_ Gender \_\_\_\_\_ Age \_\_\_\_\_

Approx. date of predatory incident \_\_\_\_\_. If known, approximate  
 duration of incident (capture to end of consumption) \_\_\_\_\_

Name of captive facility \_\_\_\_\_

Caging environment (describe size, habitat, etc) \_\_\_\_\_

Indoor \_\_\_\_\_ Outdoor \_\_\_\_\_ Time of day \_\_\_\_\_

Number of animals in cage \_\_\_\_\_ Prey Species \_\_\_\_\_

How many animals participated in capture? \_\_\_\_\_ How many animals  
 participated in consumption? \_\_\_\_\_ If more than 1 animal consumed prey,  
 describe how consumed (was there food-sharing?, snatching?, etc) \_\_\_\_\_

What part of prey was eaten first? \_\_\_\_\_

Was the prey killed before eating? \_\_\_\_\_

What remains were not eaten? \_\_\_\_\_

Describe capture \_\_\_\_\_

Please describe normal diet in as much detail as possible \_\_\_\_\_

Any additional comments/impressions (Please write these on back of survey form)



INTERNATIONAL SOCIETY FOR HUMAN ETHOLOGY

Membership and Newsletter

The ISHE was formed with the goal of promoting ethological perspectives in the study of humans. It encourages empirical research that addresses the questions of individual development, environmental, ecological and social processes which elicit and support certain behavior patterns, the function and significance of behavior, and comparative and evolutionary problems. The Society maintains an elected executive board and a number of committees, publishes a quarterly newsletter, collates an annual selection of human ethology abstracts, and meets annually in conjunction with the Animal Behavior Society, the International Primatological Society or another major society.

Membership to ISHE and subscription to the newsletter is US \$5.00 each calendar year. Checks must be drawn on U.S. or Canadian banks; otherwise send U.S. currency.

Name \_\_\_\_\_

Address \_\_\_\_\_

\_\_\_\_\_  
\_\_\_\_\_

Phone \_\_\_\_\_

I primarily identify myself as a ... (e.g. zoologist, psychologist, anthropologist)

\_\_\_\_\_

My major research/teaching interests are

\_\_\_\_\_  
\_\_\_\_\_  
\_\_\_\_\_

Please enclose US \$5.00 for 1981 dues and mail with this form to:

International Society for Human Ethology  
Joan S. Lockard, Ph.D.  
Department of Neurological Surgery (RI-20)  
University of Washington  
Seattle, WA 98195 U.S.A.