

GLEW: THANKS!

Human Ethology Newsletter

1977

#19

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WHAT'S IN A NAME?

The July Human Ethology newsletter contained a request for nominations concerning a name for our organization. The submitted names are listed below. Please vote for one; write the name you prefer on a post card and address it to Cheryl Travis, Dept. of Psychology, Univ. of Tenn., Knoxville, TN 37916. Post cards must be received by Dec. 15.

- Society for Human Ethology
- Human Ethology Society
- Society for Evolutionary Studies of Human Behavior
- Society for the Ethology of Humans

A simple plurality will decide the name we select. However, there has been some question as to whether or not the name should also include a designation of "international" so as to reflect the participation of all members. This will probably be decided by the executive steering committee.

NOMINATION OF OFFICERS

The permanent officers of our organization will consist of a President and an 8-person executive committee, chaired by the President. The President shall be elected by the membership. The executive committee will be elected by the membership with the PROVISIO that the elected members must have at least one person from each of the following disciplines:

- | | |
|-----------------|----------------------|
| Animal Behavior | Anthropology |
| Psychology | Other Social Science |

This will be achieved by placing the top vote-getter in each disciplinary area on the committee, the remaining 4 places will be filled by the next 4 highest vote getters.

The January issue of this newsletter will contain the names of the nominees and a brief statement of their backgrounds and interests.

candidates. To nominate a candidate, send his or her name, present position and address, date-place-field of professional degree, one sentence on prior research areas, and disciplinary designation to Larry Stettner, Dept. of Psychology, Wayne State Univ., Detroit, MI: 48202. Nominations must be received by Dec. 20. If you nominate someone, we assume you have received some indication that the individual would in fact be willing to serve if elected. Since you will be able to vote for more than one candidate, you may also nominate more than one candidate.

ABS MEMBERSHIP

As was reported in H-E-N #18, the human ethologists at the national ABS meeting voted to merge with ABS. There were several reasons for this decision. ABS had agreed to serve as a foster parent for the human ethology meetings only for a limited time; it was clear that we would not be allowed to hold our meetings in conjunction with theirs indefinitely. We were benefiting without charge from the work of their executive officers, and we were allotted our own meeting room and listing of abstracts in the program, again without charge. There was also considerable feeling that the human ethologists would benefit from interaction with other animal researchers if paper sessions were more integrated. You are encouraged to join ABS; it is a simple procedure. The cost is \$25.00 for regular membership and \$15.00 for student membership. Members receive the ABS newsletter and the journal published by the society, Animal Behaviour. Send your checks to Ben Beck, Secretary of ABS, Curator of Research, Brookfield Zoo, Chicago, Ill 60513.

TRAINING PROGRAM

The University of Illinois announces a new NIMH training program in Psychology, Biology, Political Science, Anthropology, Education and Afro-American Studies for research on institutional and other kinds of racism, with special attention being given to the misuses of the concepts of heredity in behavior genetics and sociobiology. Application for pre-and post-doctoral traineeships should be made to Jerry Hirsch, Dept. of Psychology, University of Illinois, Champaign, Illinois 61820.

MIDWESTERN REGIONAL MEETING

The Midwestern Regional Meeting of the Animal Behavior Society will be held 17-19 March 1978, hosted by Purdue University. There are 3 (or possibly 4) symposia planned. Students are encouraged to participate. Anyone, regardless of region, is invited to participate. Those interested please contact Judith Breuggeman, Dept. of Sociology and Anthropology, or Erich Klinghammer, Dept. of Psychology, Purdue University, West Lafayette, Indiana, 47907. Meeting areas will be near housing (which will be available on campus). Films are scheduled for Friday evening. Contact Erich Klinghammer if you have a film you wish to show. On Saturday evening a cocktail hour (with beer and wine) and buffet are planned. A cash bar will also be available.

NATIONAL MEETING ABS

The 1978 national meeting of the Animal Behavior Society will be held in Seattle, Washington in June. Send your abstracts to Dr. Joan Lockard, Dept. of Neurological Surgery, RR744 Health Sciences, Univ. of Wash. RI-20, Seattle, Wash. 98195. The DEADLINE for receipt of abstracts is February 25. In order to present a paper you must be a member of ABS or be sponsored by a member of ABS.

HUMAN ETHOLOGY ABSTRACTS

During the spring and summer of 1976 many of you were kind enough to contribute papers for a collection of Human Ethology Abstracts. Most of the people listed on the human ethology roster established by Don

Omark received pre-publication sets. The collection has been published by Man-Environment Systems, 1977, 7, 3-34. I have a limited number of reprints which I would be happy to share, especially with those of you who live outside the United States or who do not have access to M-ES.

NEW ADDRESSES

Jerry Barkow is on a sabbatical year, his new office address is Anthropology of Development, 3434 McTavish St., McGill University, Montreal, Quebec. H3A 1X9 Canada.

Wade Mackey has taken up residence in the Rural Sociology Department, 226 Scobey Hall, South Dakota State University, Brookings, South Dakota, 57006, USA.

If anyone has an address for Terry Hacwes please forward it to me, (CBT).

NEW BOOKS

McGuire, Michael T. & Fairbanks, Lynn A. (Editors) Ethological Psychiatry: Psychopathology in the Context of Evolutionary Biology. New York: Grune and Stratton, 1977.

This book contains contributions from R.A. Hinde, M.R.A. Chance, G. Weisfeld, A. Esser, and R. Deutsch. Various themes are explored, including the view that ethology can provide psychiatrists and clinical psychologists with a basis for defining normal human social behavior.

PICTURES OF FACIAL AFFECT

Paul Ekman and Wallace Friesen have developed a series of 110 slides depicting a dozen persons who were photographed repeatedly while attempting to express one of six emotions. These slides were selected from a large number because of the consistent agreement among judges about the emotion being expressed. The entire set is available at \$100 from Consulting Psychologists Press, Inc., 577 College Ave., Palo Alto, California 94306.

FORUM

In newsletter #18 I introduced the Forum component of this publication. The goal of the forum is to foster reflective comment and discussion on human ethology. Appropriate papers should discuss social/political implications of a human ethology; propose a research problem not previously addressed by ethologists; or point out inadequacies in existing theories or research strategies. Responses, Critiques, and Rebuttals are welcome.

As editor, I solicited several people for contributions to this issue of the newsletter. The topic I suggested was the relationship between ethology, anthropology, and cross-cultural research. Some of the papers were a bit long for the available space, and I had the difficult task of eliminating large sections of submitted material in some instances. The time schedule of the newsletter prevented a mutual dialogue with the authors and I therefore took considerable editorial license with their work.

On Sociobiology and Anthropology

Derek Freeman

Dept. of Anthropology, Australian National University

I have been a student of anthropology for just on 40 years, and for about half of this time have been making something of a nuisance of myself within anthropology by advocating that my colleagues should pay proper attention to genetics, the neurosciences, ethology, evolutionary biology and the like, in their theorizing about human behaviour.

For example, in a paper published in Current Anthropology in 1974, I proposed that "an authentic science of anthropology" must be based on a paradigm that gives recognition to the interaction of biological and cultural variables. "The modern biological theory of evolution", I argued, "is basic to such an anthropological paradigm, being in no way incompatible with the recognition of the emergence of learned behaviour and symbolic systems as factors of ever increasing significance in human evolution and history".

We are now, regrettably, confronted by a new movement, from within biology, which, far from being interactionist, arrogantly proclaims (as does E.O. Wilson on p. 547 of his "New

Synthesis"), that the humanities and the social sciences, including anthropology, are to be swallowed up by sociobiology: with the unique behavioural diversity of Homo sapiens being explained as the direct product of natural selection and the inclusive fitness of our genes (which Richard Dawkins has recently likened to "successful Chicago gangsters").

Although Darwin sometimes exaggerated the significance of natural selection in human affairs, he nonetheless gave clear recognition to the importance of what is now termed cultural, or exosomatic evolution. Thus (in the Descent of Man in 1871), Darwin gave it as his opinion that humans are "capable of incomparably greater and more rapid improvement than in any other animal".

There is another tradition within biology characterized by an active depreciation of the significance of exosomatic processes in human evolution, with a consequent exaggeration of what is claimed to be the genetic determination of human history and behaviour. I shall refer to this biological sub-culture as geneticism, which the Medawars (in their recent book The Life Science) have lucidly defined as "the enthusiastic misapplication of not fully understood genetic principles in situations to which they do not apply". I would add that geneticism, thus defined, is virtually identical with Social Darwinism, which purports to explain the whole spectrum of human action, including politics and morals, in terms of natural selection.

Geneticism arises because of a failure to recognize the presence of the non-genetic, or exosomatic, factors which decisively contribute to human action and history. Exosomatic is a useful term, for it contrasts clearly with endosomatic, or genetic evolution. It is the direct equivalent of what anthropologists call cultural evolution.

To the cultural anthropologist, sociobiologists like E.O. Wilson and W.D. Hamilton, seem woefully purblind to the significance of exosomatic evolution. This, fortunately, is very far from being the case with other biologists. Indeed, one of the best accounts of exosomatic evolution known to me is to be found in the writings of Sir Peter Medawar. Medawar lists the three main characteristics of exosomatic (or exogenetic) evolution in his contribution to Growing Points in Ethology

which was published last year under the editorship of Bateson and Hinde. It is, as Medawar remarks, "the distinctively human form of evolution", and its main characteristics are:

Firstly, that it is Lamarckian in style; that is, what is invented or learned is passed on at once, and becomes part of the cultural heritage; this transfer (as Tinbergen emphasised in his Croonian Lecture), is cumulative;

Secondly, cultural evolution is mediated through non-genetic channels; that is, it is carried mainly in symbolic systems; and

Thirdly, it is reversible.

In all of these respects, as in others, exosomatic evolution is markedly different from genetic evolution. Thus, inasmuch as recent human evolution and history has overwhelmingly depended on the operation of exosomatic processes, it cannot be adequately comprehended by sociobiological theory, which can deal only with the natural selection of genetically determined characters.

It is obvious that the real test for sociobiological theory is the evidence of human behavioural diversity as this has been recorded by anthropologists. And here, as Professor Sahlins has pointed out, human behaviour simply does not fit the predictions of the sociobiologists. Very many human societies are characterized by unilineal descent groups. Why, asks Sahlins, as do I, should humans invest more heavily in say patrilineal than matrilineal kin, when the average coefficient of shared genes to those two kin sets is equal?

What is clearly needed is a genetically based anthropological paradigm that deals with the pervasive and subtle interactions of the biological and the cultural, as also of the human capacity to imagine new possibilities and to make choices. Such a paradigm, I am confident, is now well in sight. (Paper presented in a symposium on Sociobiology at the meetings of the Australian Society for the Study of Animal Behavior, 1977)

In Search of Species Typical Traits

Wade C. Mackey

Rural Sociology, South Dakota State University

As an Anthropologist who is newly interested in the arena of Human Ethology, I have run into a couple of methodological problems which perhaps some of you could help me solve. As even a very quick skimming of the cross-cultural data would demonstrate, different ecologies, different subsistence techniques, and different social structures are found in association with very different "world-views" on the part of the people involved. The different "world-views" are formed in the various cultures at an early age. The socialization traditions which forge these "world-views" seem to be very effective indeed, and the effects upon the growing children can operate at an unconscious or preconscious level in a very subtle fashion. For an example of this process: In the "American" culture, people, when asked to label the song Bolero either as a "red" song or as a "blue" song, will label Bolero as very much a "red" song. When asked why is it "red", they are not sure why Bolero is red, but they are quite sure that it is red. Similarly, Jamaican Rumba is definitely an "apple" song and not an "oatmeal" song. I have no idea why strong agreement ($p < .001$) is reached in these associations. The participants are likewise unaware of how the associations are developed. The results are undoubtedly a derivative of the American socialization process, but the details of how the process works are quite a mystery to me.

The required separation of the impact of socialization traditions from the impact of the human developmental process (species characteristic behavior) would seem to necessitate the use of the same testing paradigm in different cultures. That is, if socialization traditions are to be eliminated as the primary generator of any given behavior pattern, then it seems that it is absolutely necessary, if not sufficient, (1) to design a testing instrument which is valid for all the cultures in question and not culturally biased in any direction; (2) to run or administer that test in as many cultures as is logistically and politically possible; and (3) to find results that are very similar across very dissimilar cultural contexts.

It seems then, and only then, that the behavior pattern in question could be a strong candidate for inclusion as a product of Homo's evolutionary heritage. Without a cross-cultural perspective, any data on human behavior patterns generated from western Europe and its extensions are equally likely candidates for culture-specific traits as well as for species-characteristic traits.

If some of you would share with me how you deal with these problems of the necessary, but not sufficient, imperative of a cross-cultural perspective and yet still keep an ethological orientation, I would be very appreciative indeed.

Human Ethology and Cross-Cultural Research

Jerome H. Barkow

Anthropology of Development, McGill University

Most human behavior is culturally patterned and organized. Cultures are themselves evolved systems with a complex internal logic. Anthropologists usually describe culturally-patterned traits and try to relate them to the general integration of the culture-environment system as a whole. An ethologist is not content with such analysis but instead places the culturally-patterned trait in its species context: How do other cultures pattern this behavior, what is its ontogenesis in this and other societies, with what traits is it homologous in other species, & what does evolutionary theory have to say about it?

Many "Human ethology" researchers base their generalizations on the behavior of the most atypical human population to have existed in the past 10,000 years: Europeans and their descendants. An animal ethology based solely on the behavior of zoo populations would be recognized as absurd: but it would be more defensible than a human ethology based on residents of our highly industrialized, densely populated, technological societies. Nor should we imagine that research on children is exempt from this criticism, and that the young are somehow culture-free and unaffected by the unusual aspects of our culture. It may be very convenient to begin research with our local populations—surely we are not in all ways atypical! But without replication in other cultures, our findings are uninterpretable. Cross-cultural research for a human ethol-

ogist is necessity, not luxury.

There are some positive, and exciting, reasons for going off to the field. The sociobiologists, Robert Trivers in particular have given us what amounts to a theory of human nature. Who else, since Freud, has tried to fit sibling rivalry, sex differences, and moral behavior into a single theory? Anthropologists are needed, here, but they are not to be trusted with the entire task. We need human ethologists in the field.

One set of problems which demands attention to cultural factors is related to the inclusive fitness axiom of sociobiology: that individuals act to maximize their fitness. However, individuals also act in ways that are culturally patterned. Therefore, it is necessary to study the relationship between adherence to cultural norms and inclusive fitness. For example, in a recent paper, Blurton Jones and Sibley have been asking whether !Kung women, by spacing birth and not foraging as often as might be possible, are actually maximizing their reproductive success. I am quite confident that we will shortly be finding that many cultural norms are indeed consistent with the maximization of inclusive fitness axiom. If this assumption is accurate, then it will be the human ethologists who must explain how such congruence is mediated. What mechanisms, on the individual level, lead to the emergence at the cultural level of normative patterns which maximize fitness?

Human ethologists are needed because anthropological techniques are relatively limited. The anthropologist is skilled in participant observation, the interviewing of informants, the learning of languages. He/she is likely to be quite ignorant of naturalistic observation techniques, research design, and quantitative analysis. There are a few pioneer cultural anthropologists doing biosocial research—Napoleon Chagnon comes immediately to mind—but the techniques they use involve the careful collection of detailed, quantitative, empirical data. Traditional ethnography doesn't work. I tried it, this past May and June.

I spent most of those two months doing a preliminary study of the Migili, a little-known people of perhaps 30,000 in Nigeria's

Middle Belt. My first order of business was finding out who these people were, how they made a living, what their settlement pattern was, their kinship and social organization, their beliefs, the languages they spoke, their technology and what clothes they wore, and so on. Along the way, I tried to focus on topics of apparent biosocial interest. It was obvious that in only six weeks all I could really do (and expected to do) was to gather the background preparatory to a more focused future study. In the best of all possible worlds, I would go back with a team, not once but several times. And the team would include individuals with specialized skills in ethological & ethnographic data collection.

Personal experiences among the Migili of Nigeria confirmed that field work is at least one order of magnitude more difficult than staying at home. About 65% of the time was spent battling car breakdowns, gasoline shortages, chronic dysentery, heat, humidity, and malaria. Based on these experiences, the following suggestings are offered for those who wish to engage in cross-cultural research.

Collaboration, though it has obvious drawback, is by far the most practical way of working in other settings. To conclude not with criticism but with a few real-life suggestions, look for already established projects in other parts of the world, regardless of the discipline with which they are associated (my Migili work was part of an oral history project, for example). Contact your counterparts in local universities as your earliest step in choosing a region in which to work. Discuss your plans with your foreign students, some of whom may be ready to facilitate or even to collaborate with you, should you choose their home region. BE sure to make a portion of your research problem-centered--the problems of your hosts, not your own. You may be able to gather useful medical information, for example, or work on local education difficulties--whatever your hosts feel you might contribute.

Cross-cultural research is always difficult, yet a human ethologist has little choice in the matter. If the field is to be something more than a sort of quantitative ethnography, we must work among diverse groups.

SUBSCRIPTIONS

This is the last issue of the Human Ethology Newsletter to appear in 1977. Beginning January 1978 the newsletter will appear quarterly. The annual subscription fee will be \$3.00 and is effective on a calendar year basis (January to December). Checks should be made out to Human Ethology Newsletter and mailed to Cheryl Travis.

METHODS

An elegant and highly versatile method for establishing an index of stereotypy has been developed by Sue Riechert. It was initially applied to the analysis of the frequency structure of multiple behavior patterns during encounters between pairs of subjects. However, it is not limited to frequency data; it can also be applied to data sets based on amplitude, duration, and even presence-absence. A central tendency of stereotypy is established by comparison of data from each encounter with an expected sequence of actions in a theoretically average encounter. This offers the opportunity of establishing stereotypy provoked by special types of encounters, e.g. competitive or affiliative, or it may be applied to questions of stereotypy in special kinds of dyads, e.g. child-child encounters vs. child-adult, winners vs. losers, or sex composition of the dyad. The index can be calculated by hand, but Sue has developed a computer program for it. Those of you who are interested may write directly to her: Dr. Sue Riechert, Dept. of Zoology, University of Tennessee, Knoxville, TN, 37916. The following is an abstract of the paper where she developed the measure.

Games Spiders Play: Behavioral Variability in Territorial Disputes

The agonistic behavior of spiders involved in territorial disputes is considered with specific reference to variability. A measure of stereotypy is developed which compares the observed frequency distributions of specific action patterns within each sequence to an expected set of values. Multivariate analyses demonstrate that low stereotypy is related to between sequence and within functional category variability. Sequence cost and complexity are the two major sources of variance among sequences. Winning spiders consistently exhibit less stereotypy in displaying than losing individuals.