HUMAN ETHOLOGY NEWSLETTER

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Dept. of Psychology

Summer 1980
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Univ. of Tenn.
Knoxville 37916

CALL FOR NOMINATIONS

The International Society for Human Ethology has a formal structure consisting of an eight person executive board, elected by the membership. The executive board was initially organized so that at least one representative from each of the following areas be on the board:

Animal Behavior
Psychology
Anthropology
Other Social Science

Other appropriate fields would certainly include political science, communication, or other similar field. Nominations might also come from the natural sciences of biology or ecology. The only stipulation is that that nominee have an active research interest in the theory and methods of ethology as applied to the study of human behavior.

This structure was initially established at the human ethology meetings held in conjunction with the Animal Behavior Society at Pennsylvania State University in 1977. An election was held in the winter of 1978 with the provision that the four nominees who received the most votes would serve for three years and the next four highest recipients of votes would serve for two years. In subsequent years the nominees will be elected to two year terms. The procedure insures that only half of the executive board is elected during any given year and there is always some continuity.

Current members of the executive board are I. Eibl-Eibesfeldt, C. Travis, W. Charlesworth, W. McGrew, J. Lockard, D. Omark, G. King, R. Simons. Lockard, Omark, King, and Simons will fulfill their terms this year and four positions on the board will thus become vacant. In the past we have found that the call for nominees and the subsequent election is a good opportunity to discover the breadth and energy reflected among the membership in general of our society. You are encouraged to nominate yourself and others who would be interested in serving on the Board.

A nomination should contain the name and address of the nominee, a clear interest in serving if elected, and the major research area or degree area of the nominee, the date, place, and degree type. The nomination should also include a one sentence statement about prior research interests and/or activities. Send nominations to Dr. Joan Lockard, Dept. of Neurological Surgery, Univ. of Washington, Seattle, Washington 98195 USA, by Oct. 15.

FOR IMMEDIATE RELEASE

EARTHWATCH PRIMATE PROJECTS

Observing the feeding patterns of rhesus monkeys in the Kathmandu Valley or studying the mating habits of the barbary apes of Gibraltar are two opportunities offered by EARTHWATCH to would-be primatologists to increase knowledge of these animals and of man.

What food does a yound rhesus monkey select? What are the choices of a pregnant female, a nursing mother, and adult male? Under the direction of Dr. Bernadette Marriott of John Hopkins University, EARTHWATCH volunteers will focus on these

questions as part of her on-going study of the sacred rhesus monkeys of Nepal. Since these monkeys are biologically similar to humans, and have adapted to a wide range of social and physical environments, the results will add to the understanding of human nutrition.

EARTHWATCH, a non-profit organization in Belmont brings together the scientist and the amateur who want to work and learn. Volunteers can enjoy the scenic Kathmandu Valley rimmed by the snow-capped Hymalayas. The only skills needed are a watchful eye and patience. Two teams will work from Nov. 3 to 24th and Nov. 28 to Dec. 19th, 1980.

Are the wild female barbary apes more promiscuous than the tame females or the males of either group? How do the females select a male partner? The comparative mating behavior of the two groups of barbary apes - one wild, one tame, will be the focus this year of the tenth year study by Dr. Frances D. Burton of the University of Toronto.

The first team from EARTHWATCH begins Jan. 3rd, 1981, a two week stay on the rocky promontory of Gibraltar. Volunteers will photograph the monkeys and record animal behavior in detailed field notes.

For more information on these and other expiditions, write EARTHWATCH, 10 Juniper Road, Belmont, MA 02178.

PROFESSOR/ASSOCIATE PROFESSOR, DEPARTMENT OF PSYCHOLOGY, UNIVERSITY OF MINNESOTA.

The Department of Psychology of the University of Minnesota invites applications for a position in clinical psychology at the Full Professor or Associate Professor level beginning September 16, 1981. Applicants should have an interest in and commitment to biological psychopathology research. Expertise in behavior genetics is highly desirable. Participation in teaching graduate seminars in research methods and assessment, and undergraduate and graduate courses in behavior genetics desirable. Supervision of student doctoral research is expected. Salary competitive. The University of Minnesota is committed to the policy that all persons shall have equal access to its programs, facilities, and employment without regard to race, creed, color, sex, or national origin. Women and minority candidates are encouraged to apply.

Send vitae, three letters of reference, and a statement of long-term research interests to: Dr. James N. Butcher, n438 Elliott Hall, Department of Psychology, University of Minnesota, 75 East River Rd., Minneapolis, Minnesota 55455, by January 15, 1981.

CONFERENCES

A SHAMBAUGH CONFERENCE:
BIOBEHAVIORAL STUDIES OF POLITICS: NEW DIRECTIONS IN EMPIRICAL RESEARCH
THE UNIVERSITY OF IOWA
NOVEMBER 2-5, 1980

Proposals are solicited for papers to be presented at a research conference sponsored by the Department of Political Science of the University of Iowa. The general theme of the conference is to be new directions in political behavior research. More specifically, the conference will focus on political behavior research which is ex-

plicitly grounded in biobehavioral theory and tied to the perspectives on functioning human communities which emerge from that theory.

Papers for the conference need deal with no single problem of political behavior. However, partly with an eye to later publication of the papers in a single volume, the following general guidelines have been adopted:

1. Papers should be predominantly substantive rather than methodological.
2. The behavior to be explained should be explicitly political.
3. The research presented in the papers should be well advanced or complete.
4. The research presented should be both systematic and empirical in method.
5. The questions addressed in the research should be explicitly linked to significant macro-level theoretical concerns-i.e., the routes from micro-level investigation to macro-level theory should be clearly spelled out. (And some attention should be given to the researchable questions lying along those routes.)
6. Alternative research designs for addressing the same question.

The conference will pay an honorarium of $500 for accepted papers, plus the cost of travel and lodging for their authors. The Conference will also pay the travel and lodging expenses of a limited number of discussants.

Proposals and inquiries may be addressed to either of the co-organizers of the conference:

Professor Douglas Madsen
Department of Political Science
University of Iowa
Iowa City, Iowa 52242

Professor John Walhke
Department of Political Science
University of Arizona
Tucson, Arizona 85721

fall forum

EVOLUTIONARY BIOLOGY AND POLITICAL AUTHORITY

Fred Willhoite has suggested the title of the fall forum and will serve as the special editor for the issue. Commentaries may address any of the following points and should be submitted to Fred no later than Oct. 15.

In what ways, if any, should ethological findings and theories affect our understanding of power and authority in human political structures? Does the prevalence of "dominance hierarchies," or "centripetal attention structures," within social groups of most higher primates suggest that there is a significant phylogenetic component in the behavioral processes which produce and perpetuate political hierarchies? Or are hierarchical gradations within political systems adequately explained by historical circumstances, ideologies, cultural norms, economic arrangements, political competition, and similar factors ordinarily considered by political scientists? Or is this an inadequate, perhaps even a misleadingly dichotomous statement of possible answers to the original questions? Are there any ways in which sociobiological theory can help us think more clearly about these questions?

The spring issue of the newsletter also contains a reference list of pertinent articles which partially addresses these issues. Comments should be limited to 1,000 words if possible and should rely on a minimum of formal references that require full citation at the end of the commentary. Mail essays unfolded with protective covers to Fred Willhoite, Coe College, Cedar Rapids, Iowa, USA 52402

International Human Ethology Meeting

During the recent Animal Behavior Society meetings in Ft. Collins, the ISHE members decided to plan for an international human ethology meeting in 1982. A committee was formed to pursue this possibility, with Ron Weigel and Gail Zivin as co-chairpersons. One proposal made was to investigate the possibility of holding an international human ethology meeting in conjunction with the annual meetings of ABS in Duluth, Minnesota in August 1982. Since ABS has been most cooperative in allowing us to meet with them over the past seven years, and since we believe the ABS offerings would be very interesting to our non-North American colleagues, we feel this is an attractive option. This is only one of a number of options for an international meeting, but it seems to be a viable one.

Should we have a North American site for an international meeting, it is imperative that we make every effort to facilitate the attendance of non-North American human ethologists to such a meeting, so it will truly be an "international" meeting. Various possible sources of travel funds are being investigated.

We request that non-North American ISHE members fill out the following questionaire about attending an international meeting at a North American site.

Questionaire for non-North American ISHE Members Concerning Attendance at an International Human Ethology Meeting

Name:
Address:

Please check all appropriate alternatives:

___ I would attend an international meeting in North America without financial assistance.
___ I would only attend a meeting on East/West Coast (circle one) of U.S./ Canada (i.e. city with international airport, such as New York, Montreal, Los Angeles).
___ I would only attend a meeting if $100 to $200 financial assistance were provided.
___ I would be more likely to attend an international meeting held in conjuction with another society's meetings (e.g. annual Animal Behavior Society meetings).

Other comments:

Please return to: Dr. Gail Zivin
Dept. of Psychology
Beaver College
Glenside, PA 19038

BOOKS

Burghardt, Gordon M. The Developement of Behavior: Comparative and Evolutionary Aspects: Garland STPM Press. 443 pages $37.50. Two complementary Approaches, Fossils to Psysiology, Invertebrates, Fishes and Reptiles, Feathered Reptiles Primates, Music, Tools, and Play.

Chevalier-Skolnikoff, Suzanne & Poirier, Frank E. Primate Bio-Social Developement: Biological, Social, and Ecological Determinants: Garland STPM Press. 656 pages $45.00. Biological Determinants of Socialization, Social Influences and Socialization, Ecological Determinants of Socialization, Evolutionary Perspective.

Lehner, Philip N. Handbook of Ethological Methods: Garland STPM Press. 419 pages $24. 50. Reconnaissance Observation, Delineation of Research, Design of Research, Experimental Manipulation, Data Collection Methods, Data Collection Equipment, Introduction to Statistical Analyses, Statistical Tests, Analytical Methods, and Interpretation of Results.

Strayer, Fred F. & Freedman, Daniel G. Dominance Relations: An Ethological View of Human Conflict and Social Interactions: Garland STPM Press. 528 pages $37.50. Dominance in Primates, Studies of Dominance in Children, Dominance and the Individual, Dominance and Social Ecology.

Scott, John Paul. Critical Periods. Academic Press, August 1978. 416 pp. $29.50. Through the presentation of selected key papers illustrating the effects of early experience on subsequent social behavior, the editor develops a theoretical and practical concept that can be applied to any organizational process - embryonic development, the organization of language and intellectual capacities, the organization of personality, or the organization of systems - and relates this theory to the general concept of systems.

Müller-Schwarze, Dietland. "Evolution of Play Behavior. Academic Press, May 1978. 416 pp., $32.00. Play behavior in animals and humans is an active and growing subdiscipline of animal behavior studies. This volume provides investigators of play behavior in animals and humans with ready access to the landmark literature in this rapidly-growing field.

Banks, Edwin M. Vertebrate Social Organization. Academic Press, 1977. 432 pp. $34.00.

Werner, Emmy Elisabeth. Cross-Cultural Child Development: A View From Planet Earth. Monterey: Brooks/Cole Publishing Co., 1979. 355 pp. Integrating know ledge from several fields, Werner presents research findings dealing with interrelationships of physical, social and psychological aspects of child development in "developing" countries and also addresses further policy and research needs.

Freedman, Daniel G. Human Sociobiology: A Holistic Approach. Free Press, 1979. $12.95. Freedman disputes the view, held by many sociobiologists, that the gene -- "the selfish gene" -- must be the primary unit of analysis if one is to understnad evolutionary change, kinship patterns, even relations between the sexes. His approach is holistic: he considers the impact of heredity and social environment in his compelling examination of human behavior.

Rosenblatt, J.S., Hinde, R. A. Beer, C. & Busnel, Marie-Claire (Editors). Study of Behavior. Volume 9. Academic Press, 1979. 279 pp. $21.50.

JOURNALS

ANNOUNCEMENT OF NEW JOURNAL: AMERICAN JOURNAL OF PRIMATOLOGY

The rapid growth of interdisciplinary interest in primates within recent years has dictated the need for an efficient domestic journal devoted to publication of high quality manuscripts spanning the entire spectrum of primatology. The AMERICAN JOURNAL OF PRIMATOLOGY has been established to fill that need.

The majority of articles published in the AMERICAN JOURNAL OF PRIMATOLOGY report research in which the subjects are nonhuman primates; however, exceptional articles on human biology and behavior are occasionally published. Manuscripts in which a strong comparative perspective is displayed are particularly suited for publication in the Journal.

Instructions for Contributors and information regarding subscription may be obtained from the Editor. Books for review may be sent to the Reviews Editor.

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The Journal is published by Alan R. Liss, Inc., 150 Fifth Avenue, New York, NY 10011.

Although the Journal is not allied with the American Society of Primatologists, a substantially reduced subscription rate is available to ASP members. The rate for institutions and nonmembers of ASP is $70.00 per year. ASP members may subscribe for $25.00 per year. For information regarding membership in the American Society of Primatologists write to Dr. W. Richard Dukelow, Treasurer ASP, Endocrine Research Unit, Michigan State University, East Lansing, MI 48824.

FUNDING AND VOLUNTEER SUPPORT FOR FIELD RESEARCH

Interested scholars in need of funds and volunteer support for their 1981 field research should contact The Center for Field Research. This private, non-profit organization and its affiliate, EARTHWATCH, arrange support for 70 research projects each year through the field assistance and financial contributions of interested volunteers.

Proposals are reviewed on the basis of scholarly merit and the project's need for teams of volunteers in the field. There are no limits on geographic location, and proposals in any recognized academic discipline are considered. One example of the kind of project sponsored by The Center is the study of return migration conducted by Dr. George Gmelch of the State EARTHWATCH volunteers and funds, Dr. Gmelch gathered data about this social phenomenon in Ireland in 1978 and Newfoundland in 1979.

The Center invites proposals from post-doctoral scholars of all nationalities, and actively encourages women and minority investigators to apply. Upon favorable review of a preliminary proposal, a full proposal will be invited for the May 15 deadline (for work taking place December - June) or the October 1 deadline (for work taking place June-December).

If you are planning field research in 1981, write for mor information, or send a two-page preliminary proposal outlining your objectives, dates, and funding and volunteer needs to:

Nancy Bell Scott
Research Coordinator
Center for Field Research
Box 127-C, 10 Juniper Road
Belmont, MA 02178

reviews

Life strategies, human evolution, environmental design: Toward a biological theory of health. Valerius Geist, Springer-Verlag, New York, 1978.

Geist wants to identify and describe a lifestyle that maximizes health. Such a lifestyle could be discerned from some composite of the lives of healthy people, but even that requires some theory of health and a definition of health beyond simply lack of disease. A theory of health would have the added benefit of allowing us to make decisions about the structure and progress of society, the utilization of the environment and resources, and the appropriateness of economic programs according to their impact on a healthy lifestyle rather than according to fad, political expediency, bureaucratic ineptitude, or special interest. Geist proposes that the theory be built on evolutionary information with health defined as the "maximization of our diagnostic features." These features are the phenotypic characteristics of humans which identify us as mammalian primates, but distinguish us as a species. Hence, health is more than freedom from disease. Indeed, Geist asserts that freedom from disease is a consequence of maximized diagnostic features.

To identify when a species' features are maximized, one must distinguish maintenance from dispersal phenotypes. Under natural conditions, there is a dynamic balance between the density of the population and the available resources. Since the individual is subject to the rules of reproductive and inclusive fitness, it is often the case that some of the individual's characteristics will be underexpressed or sacrificed to save energy for reproduction. Therefore, this maintenance phenotype is "unhealthy." However, under conditions of superabundant resources, as when the population disperses into an unexploited niche, the individual's characteristics can be fully expressed without interfering with reproduction. This rare dispersal phenotype represents the maximization of the species' diag-

nostic features and, conditions permitting, speciation. Therefore, to identify our diagnostic features we must know the evolutionary environment of our formation as a species.

Using information from physical and cultural anthropology, the evolution and behavioral ecology of large mammals, and paleontological ecology, Geist reconstructs the ecological conditions of our evolution, the adaptations they support, and the types of diagnostic features likely to have been maximized. However, before attending to human evolution, Geist uses several chapters to demonstrate how knowledge of the specific ecological conditions of resources and the animal's physical characterics can be used to comprehend the different strategies of communication, aggression, and dominance displays. Unfortunately, space limits do not permit a discussion of either the insights or confusions he brings to these topics. I am afraid we must still await a more solidly reasoned account of these topics.

Chapter 6 introduces several of Waddington's notions about genotype-phenotype-environment relations which make clearer how the genome can adjust to different environments. Hence, organisms can exist in non-optimal environments (and usually do), but still have the ability to develop a dispersal phenotype under conditions of resource abundance. Chapters 7 and 8 speculate on how the occupation of a nocturnal niche created the diagnostic features of mammals. Homeothermy, which allowed exploitation of the nocturnal niche, is an inefficient strategy with a high cost of living. Therefore, under maintenance conditions mammals barely avoid extinction--a strong cautionary note to those who would affect resource distribution.

Drawing on his own work with large mammals in tundra and periglacial ecosystems, Geist makes a persuasive case for the periglacial ecosystem as providing a superabundance of resources. The glacial periods were times of dispersal phenotypes and speciation while interglacials were times of population collapse and maintenance phenotypes. Each climatic change associated with the glacial cycles brought changes in the ecosystems of hominid geographic dispersal. Consequently, we carry adaptations evolved in wet savannah, steppe, and periglacial ecosystems. Each altered our social systems and ability to extract environmental knowledge. Culture became our primary adaptive strategy.

Our intellect and social relations enabled us to control our population even under harsh conditions. Cultural mechanisms supplanted biological mechanisms of population regulation. They also provided for the invention of agriculture which, in turn, resulted in the artificial environments of our existence. Agriculture created a female generated resource surplus and reduced some of the functional significance of males. However, with less time spent in resource acquisition, males could engage in activities which would become economic strategies in which their value and dominance were determined by material possessions. These economic strategies counted socially, therefore, they dictated what ought and ought not be done. Eventually, the character of the social group, from political institutions to family form, was dictated by economic values. According to Geist, a society based on economic values is removed from the conditions of our evolutionary environment and hence we are unhealthy.

Geist is right that the achievement of health would require alteration of the foundations of our socioeconomic system. He is wrong in concluding that professionals have not recognized the unhealthiness of western societies because of a philosophical position which denies the importance of biology in favor of cultural, moral, and philosophical factors in human affairs. Our apathy and inaction stems as much, or even more, from our ignorance and depreciation of these factors as it does of the biological ones.

Few non-biological positions would disagree with a model of a healthy life-style that includes "...a great amount of diverse, skillful, physical activity, of intense learning of knowledge and skills, of complex interactions with non-peers, of long lasting intense social bonds, of developing mastery over a broad range of difficult tasks, and a high level of discipline over one's intellect and emotions, and also a life filled with humor, good fellowship, and a thorough exercise of bodily pleasures and a diet both abundant and of high quality." Of course, Geist makes some more precise recommendations ranging from specific types of exercise (no jogging) to the composition and number of members in an extended family.

While I might agree with his general goals and conclusions, his argument has not convinced me of the importance of his specific recommendations. Much of the argument's force resides in the assumption of specific biological imperatives in human existence. Of course, given this assumption, biologists might be seduced into believing that they have special insight into just what those imperatives are and how they should be met. However, until we can distinguish science from pseudoscience (cf. A. Chase, The legacy of Malthus biology will be no better than any other source for identifying and justifying the imperatives for human existence. Moreover, if Geist seriously believes that an evolutionary argument will induce those changes in the socio-economic system for which thoughtful people have been striving for generations, then he has fallen prey to the Naturalist's (sic) fallacy. Nevertheless, here is one theory of health. Are there any others?

George F. Michel
Department of Psychiatric Research
Children's Hospital Medical Center
Boston, Massachusetts

* FORUM *

The spring forum called for a critique of the field of human ethology, asking for comments on problems in theory and methods, and suggestions for the future course of development within human ethology. Two commentaries were held over to the summer issue of the newsletter, because of the number of responses and length of comments submitted for the spring issue. The two comments presented here continue the dialogue. Peter Corning admonishes ethologists to forego simplistic biological models of human behavior which ignores, "mind," "consciousness," and "purpose." Steven Peterson argues for an emphsis on mechanisms and testable hypotheses, rather than speculating about the broader evolutionary questions. Their comments continued on the following pages, suggest that some of the basic issues of the field have yet to be resolved.

HUMAN ETHOLOGY: "HOW" VERSUS "WHY"

Steven A. Peterson
Alfred University

At a theoretical level, probably the most useful takeoff point for future work in human ethology is Tinbergen's four questions, which can be collapsed into "How?" (What are the mechanisms underlying behavior? What is the ontogeny of behavior?) And "Why?" (What is the survival value of the behavior? What is the phylogeny of behavior?) To this point, it seems to me that, especially with the development of sociobiology, there is undue emphasis on the "Why?" questions which, while obviously important, are not easily testable. Researchers often seem to be content with what amounts to esoteric parlor games, as they speculate on the likely evolutionary history and survival value of particular behaviors. Obviously, these are important questions, but I think that too much effort is put into these and much of that effort is nothing more than intellectual exercise with no serious attempt to test the allegations. More rewarding to pursue are likely to be the "How?" questions, which deal with mechanisms underlying behavior and the ontogeny of behavior.

A focus on the "How?" questions also forces the human ethologist to confront the reality that culture is an essential part of human ontogeny. This means that the study of culture is an essential part of a human ethology. It sometimes seems that analysts make the most strenuous efforts to avoid discussing culture qua biological variable. The implications of accepting culture as a critical biological variable? Cultural anthropologists, environmentalist-biased sociologists, etc. cannot have their findings shrugged off because they refuse to admit biological influences on behavior. Those who assert that biology is irrelevant, it seems to me, are demonstrably wrong. But their research findings provide valuable data for the human ethologist, and their research must be studiously and carefully evaluated. Many of the ethological popularizers (and some who should know better) end up rejecting the social sciences and their findings out of hand and then substitute their own intuitive "insights" into what human behavior is really like.

Before we can explain human behavior, we have to describe it. Here we would do well to follow the example of the early ethologists. They began by cataloging behavior and creating ethograms. A high priority item for human ethology should be to develop a human ethogram. Much material is already available , but more is clearly needed. The work of Eibl-Eibesfeldt (with his cross-cultural photographic effort) is an important example of one possible path that can be taken to improve our knowledge of the human behavioral repertoire across many cultures, although there are imposing problems here. More work needs to be done, then, in the mundane realm of simple description.

After having organized the wealth of material from different cultures, we must undertake the enterprise of categorization so that we can better understand the nature of both continuities and discontinuities across cultures. We must also avoid the facile conclusion that just because we have continuity across cultures we have, a priori, evidence of a biological base for that behavior. Roger Masters, a biologically oriented political scientists, and Marvin Harris, an environmentally oriented anthropologist, have both demonstrated that, theoretically, it is not difficult to explain constants across different societies in cultural terms. In other words, human ethologists must seriously consider alternative hypotheses and reject these before the biological explanation can be accepted (of course, I do not believe in separating culture and biology, but I am doing so here to make a point, since many analysts distinguish cultural from biological explanations).

The consideration of human ontogeny as a variable suggests strongly that the work of cognitive developmental theorists like Jean Piaget be used better to understand (a) the sequence of ontogeny and (b) the underlying factors. I

would argue, in fact, that Piaget is a better human ethologist than some people who call themselves human ethologists. He begins by painstaking description of ordinary behavior and then tries to tease out the causal factors--both maturational (the mechanisms of behavior, in Tinbergen's terms), social (the educational process), and individual (equilibration, or self-regulation).

Quickly to summarize, my suggestions for future exploration would be:

(1) An increased emphasis on the "How?" questions and a de-emphasis on the intellectually sexy, but difficult to test, "Why?" questions;
(2) Increased emphasis on description--not very rewarding intrinsically when compared with grand theorizing but, ultimately, more profitable if we are serious about a human ethology;
(3) An awareness that culture is a basic biological variable which cannot be "bracketed" when examining human behavior;
(4) An increased utilization of social scientific theorists whose views are compatible with human ethology (e.g., Jean Piaget).

WHAT (COULD BE) (IS NOT) WRONG WITH HUMAN ETHOLOGY (CHOOSE ONE)

Peter A. Corning
Stanford University

It has been said that an ethologist may be defined as someone who has a fundamental misunderstanding of human conduct. Similarly, in his recent, rather caustic critique in the American Psychologist, anthropologist Sherwood Washburn defined human ethology as "the science that pretends humans cannot speak" (1978:414).

Exaggerations? Perhaps, but such utterances may also serve as beneficent warnings to human ethologists that they should try to avoid repeating the mistakes of the past. Some middle-aged supporters of ethological approaches to human behavior still bear battle-scars from the 1960s, when certain popularizers fomented bitter controversy by applying some of the formulations of classical ethology to man a bit too facilely. Unfortunately, the founding father of "classical" sociobiology, Edward O. Wilson, failed to "remember the past" and was "condemned to relive it" (as Santayana warned). In his "New Synthesis" (1975), Wilson managed to exclude completely the "mind" as an independent source of behavioral causation. It was not an oversight on his part. We were assured that social behaviors can be accounted for satisfactorily under the headings of "phylogenetic inertia" and "ecological pressure", and that human cultural evolution is the product of "autocatalysis". One could perhaps characterize this as the entomological model of man and society. No wonder many social scientists were off-put.

It is commonplace these days to hear lipservice being paid to the proposition that both "nature" and "nurture" determine the behavior of complex organisms. One also commonly hears ritualistic assertions that, of course, the differences between humans and other species are as important as the similarities. Presumably, such intellectual genuflections are designed to disarm potential antagonists and counter charges of "nothing but" reductionism. And yet, such protestations notwithstanding, one discovers that the not-so-hidden agenda in Eibl-Eibesfeldt's ethological New Testament is to try to fit human behavior into Lorenz's classical framework (which, ironically, Lorenz himself now concedes is too "simplistic", even for many non-human animals). Specifically, Eibl-Eibesfeldt asserts that human ethologists are finding evidence of "fixed action patterns", "innate releasing mechanisms", "releasers" and so forth in human behaviors. We are even informed that verbal behaviors in humans may be "functional equivalents" which "substitute" for phylogenetically-based nonverbal behaviors (1979: 2, 54-56 and passim).

I submit that Eibl-Eibesfeldt's model is defective. To illustrate the problem, consider one of Eibl-Eibesfeldt's own examples -- the human smile. Psychologist Daniel Freedman's classic studies of smiling in blind infants (plus other studies done subsequently) have provided powerful support for the hypothesis that there is an innate component in smiling behaviors (see Freedman 1974). Furthermore, Eibl-Eibesfeldt and others have established a credible case for the view that smiling may be a cross-cultural universal. But does this mean that smiling is a "fixed action pattern"? It is not a trivial point that humans are unique among higher animals in being able to manipulate smiles. They can calculatedly avoid smiling or "turn on" smiles as the occasion demands. And, significantly, there is evidence that the neurological substrate for such behaviors is organized differently in man. It is clear that human smiles involve an interaction between innate, developmental and situational components, and that cybernetic processes are involved. Yet the classical Lorenzian model doesn't even have a place for consciousness and purposive self-control. Whether or not there is as sharp a dichotomy between non-human "behavior" and human "action" as ethologist Vernon Reynolds (1976) asserts, it is abundantly clear that deliberately contrived purposes are of great, and sometimes even paramount, importance in humans.

It is noteworthy that, in contrast with Lorenz's epigones, Niko Tinbergen's intellectual offspring have broken completely with the classical, "instinctualist" tradition and currently have no agreed-upon model of behavior, though they are united in opposing reductionist models derived from genetics, neurobiology, or hydraulics (Bateson and Hinde 1976). In fact, it is significant that many ethologists these days are borrowing concepts, research tools and theory from the human sciences.

Eclecticism and pluralism may also be an appropriate strategy for human ethology at this point, but I believe it is possible to go a step further, and that it is highly desirable to do so -- precisely because it may help human ethologists avoid some of the oversimplifications (and the tunnel vision) that have been all too evident in the past.

In a forthcoming volume on the origins and evolution of politics (Corning, in press), I suggest that it may be useful to apply an <u>interactional perspective</u> (which Eibl-Eibesfeldt incants but does not take to heart), in a more self-conscious, more consistent, and more rigorous manner to the analysis of human behaviors. Briefly, what an "Interactional Paradigm" (the term I employ) would entail is explicit efforts to discover the influence of, and the interactions among, specific causal factors at various levels of biological organization. The "model" that, I argue, best fits such a paradigm and research strategy (as a first approximation), is a hierarchically-organized, nested set of cybernetic systems that encompass multiple "levels" of causation, from the molecular to the most inclusive political systems. Not only is social causation a result of the interaction among factors on the same level of organization, but there are critically important two-way interactions between levels. As Donald T. Campbell has pointed out, both "upward" and "downward" causation are known to occur.

One of the more compelling illustrations, to my mind, might be the problem of health and disease, the multifarious causes of which range from single dominant genes to "customs", to acts of Congress and world weather patterns. Even a single medical syndrome -- say lung cancer -- may involve multiple, synergistic (or dysergistic) causes, most of which are still poorly understood. Are the causes of political violence, to take a behavioral example that is more central to my interests, likely to be any less complex? Even if it could be shown that specific gene, or gene complex, is associated with violent behaviors, this cannot even begin to account for the often purposeful nature, much less the observed variations, in political violence.

Others may prefer models other than the one that I have barely sketched here. But, in any case, an explicit strategy aimed at attempting to discover and integrate the sources of causation at various levels of biological organization -- including those that involve cognition, language and socio-cultural phenomena -- is an inescapable prerequisite, I believe, to the emergence of a mature science of human behavior -- whatever label that science may ultimately be given. Though it may not now be possible for human ethologists to resolve many of the sub-

stantive research issues, it is possible for them to choose the paradigm within which their research efforts will be framed. That is, needless to say, the choice to which I was referring in the title of this brief commentary.

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NEWS

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CURRENT METHODS IN NONVERBAL COMMUNICATION RESEARCH

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In September of 1979 the Scientific Affairs Committees of NATO sponsored an advanced study Institute on methods of research in nonverbal communication. This 10-day Institute was held at Birkbeck College in London, England, under the leadership of Paul Ekman (University of California School of Medicine, San Francisco) and Klaus Scherer (University of Giessen, West Germany). In all 149 people from 24 countries participated. Most were Europeans, although others came from as far away as Australia and Chile. There were nearly equal proportions of graduate students, junior faculty, and senior faculty participants. The entire group spent each day attending formal lectures or small seminars conducted by the 17 Institute faculty members. The Institute was envisioned as an opportunity for specialists in nonverbal communication research, coming from a number of fields, to present new methods developed over the past decade.

Mario von Cranach lectured on decision-making in nonverbal communication research. He pointed out that when one chooses and defines the behavioral units to be observed, one must make conscious decisions concerning the level at which one is working. Typically one works either at a level which is called molar, functional, and social; or one works at a level which is characterized as being molecular, structural, and physical. Of course these distinctions are not absolute; a researcher may, for example, recognize function when taking a structural approach, in that small "pieces" of behavior may be seen as triggering one another. Von Cranach argued that it is extremely difficult to make purely "physical" statements about behavior, without giving behavior a "social" interpretation. We are, after all, products

of enculturation. The best strategy is probably to admit one's social biases and attempt to learn the social meanings of the group under study.

Problems encountered in giving social interpretations to behaviors seen in other cultural groups were discussed by Pio Ricci-Bitti. He cautioned that, while six emotions seem to be expressed the same way universally (happiness, surprise, fear, anger, sadness and disgust), other emotions such as contempt seem to vary in their expression from culture to culture. When one examines gestures across cultural groups, one finds even more ambiguity.

Robert Rosenthal stressed the value of judgement studies. He admitted the importance of studying the expressions of specific emotional states; but he argued for additional work on the interpretation of the individual viewing such expressions. In other words, researchers should utilize naïve observers, showing them pictures of facial and bodily movements and asking, "What emotions do you see?"

The faculty members discussed problems encountered in their research. One serious problem concerns replication of results. Many journals are reluctant to publish replications, which are often viewed as "old hat." Moreover, re-analyzing the original researcher's films or tapes means encountering the problem of consent. In most cases the original researcher obtained consent for the data to be revealed only to his own team; carte blanche has not been granted for their use by other researchers. Another ethical problem, familiar to many of us, is dealing with human subjects' right committees' expectation that the subjects' prior consent will be secured. Research indicates that knowing one is being observed can affect overt behavior and autonomic responses.

Paul Ekman described his Facial Action Coding System (FACS). This is a technique for coding facial movements in humans. The system successfully predicts self-reported emotional states and has other applications. A kit for learning the technique can be purchased through Dr. Ekman. Harriet Oster has modified the technique for use with neonates. Other modifications would be necessary to apply it to elderly people and to nonhuman primates.

Jan van Hooff described techniques for analyzing sequential data. One simple method for describing how a series of behaviors is ordered in time is to tally the frequencies of each pairwise sequence, e.g., behavior A follows behavior B 15 times. The resulting data can be depicted with a pathway diagram, in which arrows of varying thickness connect circles representing the behaviors; the thickness reflects the frequency of the sequence. See G. P. Baerends, et al., Behavior, 8: 249-334, 1955.

Van Hooff has employed a principal components analysis to observations of chimpanzee social behavior (in M. von Cranach and I. Vine, Eds., Social Communication and Movement, Academic Press, 1973). The behaviors were analyzed into five components, characterized as play, aggression, affinity, excitement, and submission. Each of these, in turn, was analyzed to further specify how the individual play behaviors, for example, were temporally related to each other. McQuitty (Educ. Psychol. Measur., 1966, 26: 825-831) performed a cluster analysis on van Hooff's data. He depicted the results by means of "branches" connecting the behaviors together, a pair of behaviors that were closely related temporally being on the same "limb."

Van Hooff states that sequential analyses that are based on information theory, such as the Markov process type model that yields probabilities of behavior A leading to behavior B and to behavior C, are falling out of favor. See Hutt and Hutt, Direct Observation and Measurement of Behavior (Charles C. Thomas, 1970) for a discussion of this and other sequential analysis techniques.

Time series analysis is a very promising technique. It involves the use of recordings of the real time at which each behavior occurred, not just the sequencing of behaviors. The idea is to identify behaviors that trigger others, as evidenced by one behavior following the other at a characteristic interval. For example, if behavior A characteristically precedes B by 5 seconds, than A probably triggers B. This technique has the advantage of revealing these

causal relationships even if other, irrelevant behaviors entervene between A and B. The technique involves plotting "log survivor curves," which reveal whether the intervals between the two behaviors are of a characteristic duration or are random. It can also be used to discover rhythmicity in, for example, mother-infant interactions. See D. R. Cox and P. A. W. Lewis, The Statistical Analysis of Series of Events (Methuen, 1966) and P. W. Colgan, Quantitative Ethology (Wiley, 1978).

These and the other major lectures presented at the NATO Advanced Study Institute on nonverbal communication methods will be reprinted in a book edited by Paul Ekman and Klaus Scherer, to be published late in 1980.

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