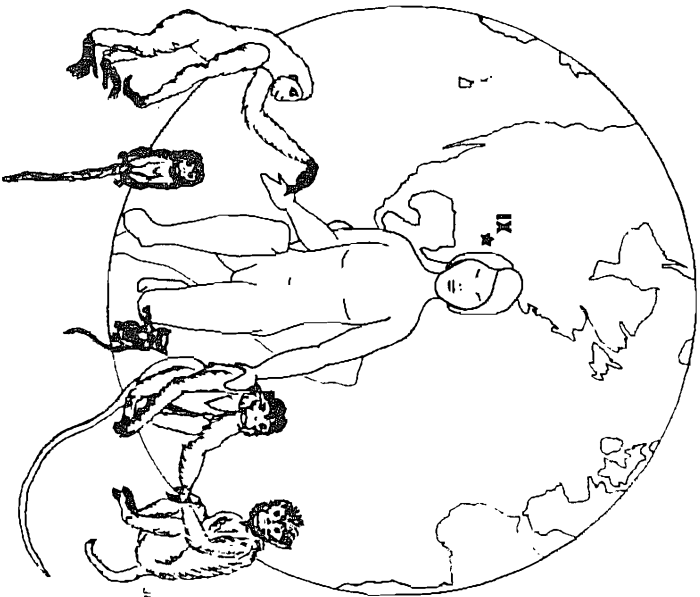


A MEETING OF FRIENDS



HUMAN ETHOLOGY NEWSLETTER

JOHN B. LOCKHARD, EDITOR
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LOGO

Our masthead for this issue reminds us of our upcoming international primatological meeting in Atlanta. We applaud Jocelyn Penner for her artwork, which captures the essence of the meeting. Ms. Penner also drew the logo on bonding for the March issue.

ATLANTA

The IXth Congress of the International Primatological Society (IPS) will meet in Atlanta, Georgia from August 8-13, 1982 with the American Society of Primatologists (ASP) and the International Society of Human Ethology (ISHE). This will provide ISHE with an opportunity for an international human ethology conference. If you have not already registered, write to:

Ms. Cathy Yarbrough
IPS Congress Office
Verkes Primate Research Center
Emory University
Atlanta GA 30322

or call her at (404) 329-7709 for information.

Announcing Important Modifications of the Program:

1. The keynote speaker has now been announced for the opening session on Monday, August 9 at 8:30 AM. Helmut Hofer, Professor Emeritus of Tulane University will speak to us about "The Origins of Modern European Primatology and the IPS."
 2. ISHE's business meeting has been changed from Tuesday, August 10 to Thursday, August 12 at 5:45-6:45 PM.
 3. ISHE members will have a cash bar social hour on Tuesday, August 10 from 7:00-8:00 PM. This was not noted on the program.
 4. The symposium "Evolutionary Biology of Tarsius" has been changed from Friday, August 13 to Thursday, August 12 at 8:00-10:00 PM.
 5. The film "Korup; An African Rainforest" will be shown twice: on Tuesday, August 10 at 1:30 PM and again on Thursday, August 12 at 9:15 PM.
 6. Students may have their registration fee waived if they volunteer for 8 hours of work during the conference, doing such things as running projectors. Contact Cathy Yarbrough at the above address if you are interested.
- Included in the schedule are the following symposia (and their respective chairpersons) of possible interest to ISHE members:
- Mon. Aug. 9: "Nonhuman Primate Models for Human Growth and Development" Elizabeth S. Watts, Dept. of Anthropology,

Tulane University, New Orleans.

Mon. Aug. 9: "Self Deception: An Adaptive Strategy?" Joan S. Lockard, Sch. Med. and Dept. of Psychology, University of Washington, Seattle.

Tues Aug. 10: "The Study of Adaptive Aggressive, Dominance, and Conflict Resolution Strategies in Humans and Nonhuman Primates" Ronald M. Weigel, Neuropsychiatric Institute, UCLA.

Wed. Aug. 11: "Cognitive and Perceptual Prerequisites to Language: Studies of Primates' Natural and Synthetic Communication Systems" Michael Petersen, Dept. Psychology, Indiana University; Charles Brown, Dept. Psychology, University of Missouri-Columbia; Duane Rumbaugh, Dept. Psychology, Georgia State University and Yerkes Primate Research Center.

Thur Aug. 12: "Primate Ethopharmacology: Models for Neuropsychiatric Disorders" Klaus A. Miczek, Dept. Psychology, Tufts University.

Thur Aug. 12: "Development of Expressive Behavior: Processes of Interaction between Biological Bases and Social Learning" Gail Zivlin, Dept. of Psychiatry and Human Behavior, Thomas Jefferson Medical College, Philadelphia.

Thurs Aug. 12: "Psychosocial Factors in Stress" Gary Moberg, California Primate Research Center, University of California-Davis.

There will also be a number of enjoyable social events:

Sun. Aug. 8: Opening Reception
Mon. Aug. 9: Old-fashioned Barbecue
Wed. Aug. 11: Banquet

Please note that Congress registrants may choose to attend (1) the barbecue, (2) banquet, or (3) both barbecue and banquet. Advance reservations are required. Apparently there was some confusion on this.

Field trips are scheduled to the Yerkes Primate Research Center and Yerkes Field Station on August 8 and 13 (one trip to each center on each of these days). You will need to preregister for these trips.

LONG TERM GOALS

Michael McGuire, Chairman of the ISHE Long Term Goals Committee presents the following for your consideration:

Last year I. Eibl-Eibesfeldt and W. Charlesworth were in charge of this Committee and a number of issues were identified which deserve further discussion. These are listed below for general information and as topics on which members might wish to comment prior to the August meeting in Georgia. In addition, if there are other topics anyone would like to have discussed it would be helpful if you could forward such information to me so I can put an agenda together and collate ideas. Please address correspondence to:

Michael T. McGuire
Neuropsychiatric Institute - UCLA
760 Westwood Plaza
Los Angeles CA 90024

Preliminary Agenda Items -- Long Term Goals

1. Should the ISHE have more structure?

Comment: Presumably more structure means the development of bylaws, possible incorporation, etc.

2. Should there be a human ethology journal?

Comment: This topic has been the subject of discussion for several years. To try and focus the issue I will provide data from the first two years of Ethology and Sociobiology (e.g., number of manuscripts; number accepted; types accepted; number of subscribers) which might be relevant to a journal decision.

3. Should there be more formal communication with other disciplines?

Comment: In the past, arguments have been advanced which advocate both an increase and no change in formal communication. Possible advantages to increased fields and vice versa; (b) possibilities of obtaining grants may increase because our work is known by others; and (c) the quality of our work may improve because it can be related to other disciplines.

4. Are there special ways of obtaining research monies that we should know about?

Comment: The answer to this questions changes daily (at least at UCLA). Thus, I will try to collate all the suggestions sent to me, check out possibilities, and report findings at the August meeting.

5. Are there novel ways of appealing to and/or training students?

Comment: It is probably most appropriate for those who wish to say something on this subject to plan a brief presentation at the August meeting. Please let me know your names.

Other possible agenda items include:

- (a) Should we have regional meetings focussing on particular topics?
- (b) Are there alternative -- novel -- financing plans for research work?
- (c) How can we behave to increase the opportunity of people being hired into academic positions?

BLOOMINGTON

The Semiotic Society of America has expressed interest in holding a joint meeting with us in 1984 in Bloomington, Indiana. This Society publishes the American Journal of Semiotics (Schenkman Publishing Co., 3 Mount Auburn Place, Cambridge MA 02138), edited by Irene Portis Winner and Thomas G. Winner. Semiotics is understood as the study of all sign processes from their psychobiological foundations to their expression in animal and human communication. It pays special attention to all human sign systems, verbal and nonverbal, and the messages and texts they generate. Let's plan to discuss the possibility of a joint meeting in 1984 at the Atlanta meeting. If you would like to send your comments in advance of the meeting, write to: Gail Zivlin, Dept. of Psychiatry and Behavior, Thomas Jefferson Medical College, 323 Curtis Clinic, 1015 Walnut St., Philadelphia PA 19107 or to Ronald Weigelt, Human Ethology Laboratory, Neuropsychiatric Institute, UCLA, Los Angeles CA 90024.

MUNICH

The International Society for the Study of Behavioral Development (ISSBD) will hold its annual meeting July 31-August 4, 1983 in Munich, West Germany. ISSBD is a small, very well respected, not-laboratory bound research society in child development and has a strong European component. In this regard, the possibility exists for the International Society for Human Ethology to hold its 1983 annual meeting immediately after the ISSBD meeting, conjointly with the German Developmental Psychologists conference in Regensburg (150 km from Munich). If we do meet in Germany, we will NOT hold a joint conference with ABS in 1983 (ABS will meet in June, 1983 at Bucknell University, Lewisburg, Pennsylvania).

Dr. I. Eibl-Eibesfeldt has suggested another ISHE meeting in Seewiesen, West Germany in the not too distant future (1985 or 1986) and we could hold a joint meeting at that time with a different organization.

Please respond within 10 days after receipt of this newsletter as to whether or not you will be likely to attend an ISHE meeting August 5-7, 1983 in Regensburg, West Germany. THIS IS A PRELIMINARY POLL ONLY and is to be used for planning purposes. Please send in your response, whether positive or negative, along with any comments to: Gail Zivlin, Dept. Psychiatry and Human Behavior, Jefferson Medical College, 323 Curtis Clinic, 1015 Walnut Street, Philadelphia PA 19107.

_____ I think I will be likely to attend the conference.

_____ I don't think I will be likely to attend the conference.

Name and address:

Comments:

ERRATA

In paragraph 2 of Clara Jones' Mini Communication (HEN 3(5):23, 1982), the second sentence should have read "the differential...and/or high quality mates." Apologies for any inconvenience this has caused.

SPRING FORUM → SUMMER FORUM

More time was needed to address the question on bonding posed in our March newsletter, so it will be carried over as our Summer Forum. Please send your responses by August 1 to Nicholas Blurton Jones, Graduate School of Education, UCLA, 485 Hilgard Avenue, Los Angeles CA 90024.

BOOK REVIEWS

THE SOCCER TRIBE. By Desmond Morris. London: Jonathan Cape. 320 pp. (1981)

Reviewed by William C. McGrew, Dept. of Psychology
University of Stirling, Scotland

Desmond Morris' contributions to human ethology now span 15 years, if one dates their beginning from the publication of The Naked Ape or from his editorship of Primate Ethology, both in 1967. The first was provocative, controversial and immensely popular; the second was one of the formative anthologies in primatology, and contained Blurton Jones' pioneering paper on children in nursery schools.

Now comes Morris' latest (fourteenth) book, providing the opportunity for stock-taking. The Soccer Tribe combines two of the approaches that the author has used before: it is based on a single, comprehensive analogy, as in The Human ZOO. In this case, the game of football is likened to the culture of some prototypical, primitive tribe. The other approach is manifest in the format so successfully exploited in Manwatching. (This, by the way, has just been reissued in a smaller-sized version--a pocket-sized guide to the subject.) Here again is the "coffee table" format: large, glossy, simple text, full of colour photographs. Doubtless the book is already a commercial success. Its appearance coincided with the final stages of qualification for the 1982 World Cup. In case we needed reminding, the publishers

ran a contest which involved answering ten easy questions from the book's text. First prize: June holidays to the World Cup in Spain.

But what about the content of the book? First of all the reviewer is bound to reveal that he (like the author) is an avid follower of the sport. My conversion came in one of the semi-finals of the 1966 World Cup, when I joined 100,000 fanatics at Wembley in watching England beat Portugal, 2-1. This disclosure is necessary because unless you are already a football fan, you need not bother to read this book, unless you are keen to become one or are especially curious. That someone should write a book unlikely to appeal to the large North American market may therefore seem misguided. However, given that an estimated quarter of the world's population (i.e., about 1,000,000,000 persons) tuned in to the 1978 World Cup final in Buenos Aires, the book should sell all right elsewhere.

It is divided into seven sections which cover the game from all angles: tribal roots (e.g., the soccer match as status display), tribal ritual (e.g., taboos and tactics), tribal heroes (e.g., the players' behaviour), tribal traditions (e.g., the trophies), tribal elders (e.g., coaches), referees, tribal followers (e.g., fans and mascots), and tribal tongue (e.g., slogans and songs). All are handsomely illustrated with excellent action photos and colourful charts. As with National Geographic and others of that ilk, there are lengthy, chatty captions to the illustrations, for those who cannot be bothered with the text.

Morris' tribal analogy provides no startling insights. Like all "ethologising" (to use Hilary Callan's term), its initial impression of surface validity depends on its clever selectivity in choosing examples. Soccer is seen as the fourth stage of a process whereby survival hunting led to sport hunting led to arena blood sport led to arena ball sport. (I suspect that one could just as easily derive golf from gathering, but that's another story.) It may well be that the hues chosen for team uniforms are those which signify warning colouration amongst other species, but this analogy (like most of the others) breaks down if pushed too far. Better to read and enjoy than to analyse.

Morris also makes use of a third approach, as exemplified in another of his recent (and unjustly neglected) books, Gestures. This is to categorise acts on structural grounds and then (sometimes) to count their occurrence in convenient, real-life settings. Thus, football players show 12 patterns of behaviour in defeat, ranging from the Outrage Display to the Weeping Face. There are seven styles of refereeing, the modal score of a match

is 1-0, and the supporters of Oxford United managed to produce 251 different chants in only 15 home matches. The extent to which the reader will persist in the pursuit of such trivia provides a direct measure of addiction to the sport.

When the author does tackle issues, the results are worth paying attention to. He makes a convincing case that violence associated with football is really minimal, given the mass attendances, even in these days of television. Further, he rightly throws cold water on the idea that soccer's salvation lies in some sanitized, sensationalised entertainment for the family. The essence of the game is its simplicity, and its success is based on the most elemental traits, e.g., courage, stamina. Attempts to tart up the game through modernising it are thus misguided.

In summary, Morris has written a pleasant book on football. It will be enjoyed by those who like the game. For those who do not, or who are ignorant of it, the book is an unusual way of looking at the world's most popular ball-sport. Either way, Morris makes no serious contribution to human ethnology, which to be fair to him, was probably never his intention anyway.

CULTURAL TRANSMISSION AND EVOLUTION: A QUANTITATIVE APPROACH.
By L.L. Cavalli-Sforza and M.W. Feldman.
Princeton: Princeton University Press. 388 pp. (1981)

Reviewed by Eric Fischer, Dept. of Psychology
University of Washington, Seattle

That cultures evolve is something that few thoughtful people would deny. But what are the main forces that shape pattern and direction in cultural evolution? In the last few years, evolutionary biologists, spurred in part by the successes and excesses of sociobiology, have begun to consider this question seriously, after a very long period of virtual neglect. One result of this interest has been the appearance of four books on the subject (Pulliam and Dunford, 1980; Bonner, 1980; Lumsden and Wilson, 1981; and the volume being reviewed here).

Cavalli-Sforza and Feldman have written the most ambitious work to date, an attempt to provide a general theoretical framework for viewing cultural transmission and evolution. It summarizes and expands upon several papers they have published together over the last decade. Not only do they develop mathematical models for several different kinds of cultural transmission, but they also attempt to relate their results to cultural data, and they discuss both

models and data in light of larger issues.

The result is a rather formidable but well balanced and precise treatment of a very important issue. This does not mean, however, that the book marks a major advance in our understanding of the mechanisms by which cultures evolve. Only time will tell, but I rather suspect that it does not, though it is certainly a worthy effort.

The main focus of the book is on modelling. In judging its usefulness, there are at least three questions to be considered: (1) How realistic are the models? More specifically, is there reason to doubt the validity of biological importance of key assumptions? (2) To what extent do they explain or organize existing data? (3) Do they yield any significant new information, either in terms of counterintuitive predictions or identification of important research problems?

The models that Cavalli-Sforza and Feldman develop are essentially extensions of current population genetics theory. Like standard population genetics models, they incorporate selection, mutation, migration, and drift, and they deal with discrete and continuous (quantitative) characters. But there is an important difference. Unlike genetic models, transmission is not tied to reproduction and is classified into three different modes or directions, drawn from the theory of epidemics. Transmission from parents to children is vertical, that between individuals within a cohort is horizontal, and everything else is oblique. Cavalli-Sforza and Feldman's main goal is to determine how mode of transmission interacts with other factors to determine the fate of cultural traits.

While various refinements (such as uniparental vs. biparental transmission) make them potentially applicable to many different situations, the models themselves incorporate no known or postulated mechanism of cultural transmission. Rather, they provide a framework within which one can categorize the spread of many cultural traits. It may prove interesting to see what kinds of traits fall into which categories, but from such work we will learn little about the actual way cultural transmission and evolution work. However, the discovery of non-trivial patterns in the modes of transmission for particular categories, such as sexual behavior, may lead to important hypotheses about the nature of cultural evolution.

For the most part, Cavalli-Sforza and Feldman consider only single locus models, so that no process analogous to recombination is included. This omission is only important inasmuch as the "linkage" between cultural traits varies in strength, but it surely does. Considering the already

complex nature of the models, it is certainly understandable that Cavalli-Sforza and Feldman did not include recombination, but it is surprising that they do not even discuss it. In general, they are admirably thorough in pointing out potential difficulties and omissions in their work. For example, they recognize that while they deal with mutation on cultural traits as if they were random events, in reality they are often highly directed -- particularly where innovations are devised to solve particular problems.

Cultural traits and objects range from the trivially simple, such as surnames, to the enormously complex, such as institutions. Cavalli-Sforza and Feldman deal only with simple characters. It is, of course, most logical to start with fairly simple assumptions, but the cultural phenomena of greatest importance are probably the most complex ones. This does not create problems for the models if these complex phenomena can be viewed as accumulations of independent, simple traits, but of course, there are certain to be strong interactions among components, and it is not clear how such interactions will affect the applicability of Cavalli-Sforza and Feldman's results to complex cultural traits.

Further difficulties exist in the characterization of cultural traits. The main problems are in dealing with the sometimes bewildering variability in a trait, such as dress, and the ease with which categories can be created that are totally artificial from the point of view of selection. For example, to what extent should length or width of a tool be considered a legitimate cultural trait? Such a measurement may be correlated with those characteristics that are actually transmitted but may well not be directly subject to selection. If so, it is unlikely that studying length or width of tools can be very useful in illuminating how cultural selection operates. In addition, it is possible for an individual to possess and express more than one state of a character at a time, but their models assume that alternative states are mutually exclusive.

These difficulties are not unique to the study of cultural evolution. Similar problems are also of concern to ethologists (Barlow, 1977) and evolutionary biologists (Gould and Lewontin, 1979), but the somewhat amorphous and intangible aspects of many cultural traits make the problems more severe.

In general, then, there is good reason to doubt the validity of some key assumptions of the models, but it is not possible to say how serious the problem is. How well do the models deal with data on cultural patterns? The answer here is also not clear. There are, in fact, few relevant data. Cavalli-Sforza and Feldman discuss many interesting

examples, ranging from survey data collected by the authors to measurements of paleolithic tools. But these are purely illustrative, and often they do not conform well to expectations.

What kinds of predictions do the models make? The results on the effect of mode on cultural evolution are not counterintuitive. For example, it is not surprising that a mode of transmission where one individual teaches many leads to faster rates of change than parental transmission, nor that drift will not affect the mean value of a trait.

The most important issue addressed by the models has to do with the relationship between cultural and natural selection. Three major questions about this relationship are: (1) What happens when cultural and natural selection conflict? (2) What are the effects of one form of selection on the speed and strength of the other? (3) To what extent is cultural evolution constrained by natural selection?

Cavalli-Sforza and Feldman address only the first issue, leaving coevolutionary interactions for future work. Their most interesting result is that cultural selection can easily lead to the fixation of traits that are opposed by natural selection. While not really surprising, this result may prove helpful in countering the widespread temptation to assume that cultural selection will closely track natural selection.

If this question of conflict between cultural and natural selection seems vaguely familiar, it is probably because it is yet another form of the nature-nurture issue, a sort of Proteus for the social sciences, with some attributes of a phoenix as well. Cavalli-Sforza and Feldman claim that, while conflict is possible, cultural and natural selection should generally act in harmony, because the mechanisms permitting cultural evolution to occur originated through natural selection. But they also claim that cultural selection can act much faster than natural selection. If so, could not the two easily become decoupled, as some social scientists would claim has happened? After all, sexual selection, which is mediated in part, at least, by psychological mechanisms involving mate choice, has led to characters that are clearly and strongly maladaptive.

This does not mean that natural selection is not important in cultural evolution, but merely that the relationship between the two kinds of selection is far from obvious. For example, the analogy with sexual selection suggests that traits under cultural selection and not themselves directly subject to natural selection may well influence the direction of genetical evolution. Thus, human

hairlessness could, in theory, have resulted in part from cultural selection for the wearing of clothing.

Now consider the case where cultural and natural selection both favor the same trait. If cultural selection is much faster, the trait will reach some optimum culturally before much genetic change has occurred, and it is difficult to see how further genetic change could then be favored. Thus, cultural selection may actually slow the rate of genetical evolution.

If the models of Cavalli-Sforza and Feldman are currently of limited value, it is not because they are incorrect, but largely because there is as yet not a way to use them in understanding how cultural evolution works. It is rather like having a theory of population genetics without an accompanying theory of natural selection. When the cultural equivalent of Darwin's theory is eventually developed (if the analogy is in fact apt), these models may well prove their worth. However, the diverse nature of cultural traits suggests that the mechanism of selection may not be as elegantly simple as Darwin's (though one might have said much the same thing before The Origin of Species). The best approach, as suggested by Bonner (1980), may well be to start with animal systems and use the findings there to help focus human studies.

Much of this review has been spent pointing out difficulties with Cavalli-Sforza and Feldman's approach. But the reader should not be misled into thinking that the book is not worthwhile. On the contrary, it is overall the best of the recent works and is well worth careful reading by any ethologist interested in cultural evolution. Its strength lies not so much in the models or data as in the broader discussion, which is thorough, well balanced and stimulating. It is probably the general discussion, rather than the models, which will prove to be the most influential in inspiring future directions for research in this area.

Barlow, G.W. Modal action patterns. In: How Animals Communicate, T.A. Sebeok (Ed). Bloomington, Indiana: Indiana University Press, 1977.

Bonner, J.T. The Evolution of Culture in Animals. Princeton: Princeton University Press, 1980.

Gould, S.J. and Lewontin, R.C. The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist program. Proceedings of the Royal Society of London, Series B, 1979, 205, 581-598.

Lumsden, C.J. and Wilson, E.O. Genes, Minds, and Culture. Cambridge, Mass: Harvard University Press, 1981.

Pulliam, H.R. and Dunford, C. Programmed to Learn: An Essay on the Evolution of Culture. New York: Columbia University Press, 1980.

THE FATHER-INFANT RELATIONSHIP: OBSERVATIONAL STUDIES IN THE FAMILY SETTING. Edited by F.A. Pedersen. New York: Praeger, 185 pp. (1980)

Reviewed by William T. Bailey, Dept. of Psychology
Tulane University, New Orleans, Louisiana

Any ethologist with a serious interest in the development of human relationships will want to read this book. It is sui generis--to my knowledge no other book deals specifically with infant-father research. A second edition of Lamb's (1981) The Role of the Father in Child Development has recently been published; however it is more of an encyclopedia compiling a myriad of topics relevant to the father and child. The Pedersen text, on the other hand, deals specifically with research including not only results but also orientation and methodology. The five chapters other than the editor's concise introduction and overview are presented in the form of expanded conference reports. The chapters by Clarke-Stewart, Lamb, and Parke and Sawin were originally presented at a symposium at the 1977 meeting of the Society for Research in Child Development; those by Belsky and Pedersen, Anderson, and Cain retain that format but include more recent findings.

Pedersen acknowledges that the research reported may well be limited in terms of generalization; sample sizes were relatively small (range 14 to 40), families were overwhelmingly white and middle class, and the studies were conducted in a narrow time range (1972 to 1978). While certainly we will want to see infants and fathers studied in a diversity of settings and cultures, if there are universalities to this relationship which subsume the father's socialization, we might be less concerned about generality than are the authors.

The work presented here would eminently become that of ethologists; what is lacking is a theoretical framework acknowledging a biological basis of the behaviors observed. Lamb, it is true, has conducted his investigations within the purview of Bowlby's attachment theory. Such an approach, however, seems to me to be inadequate. While infant-mother relations in humans are consistent with those in the nonhuman primates, there is a quantum difference between fathering in the human and nonhuman primates. It appears that these differences are distinctly related to our evolution which included adaptation as a social hunter

(C.f., Mackey, 1979). Ethologists with an interest in humans, however, can certainly profit from the "non-ethological" research of others. Kortlandt, in the December, 1981 issue of HEN pointed out that ethology is properly seen more as an orientation than a methodology. Recently I proposed (1982) that the infant-father relationship results from a predisposition of the father and infant to form an intense, enduring bond termed affinity. My presentation of Affinity Theory included material presented in this book -- and rightly so.

A problem of orientation in the Pedersen text is the belief that infant-father relations should properly be studied in the family setting. It is of course true that these individuals are part of a higher system, the family, and need to be studied as such. However, our present knowledge of these relationships, per se, is so limited that more discrete study acknowledging, but not necessarily including the mother might be more profitable at this time.

The findings by these researchers are sufficiently numerous and diverse as to preclude detailing them here. In summary, fathers and mothers are reported to behave in both similar and dissimilar ways to their infants. Reciprocally, infants behave distinctly to their parents of either sex. While there are contemporary social trends towards "equality" in parenting, the findings here suggest that equality in a philosophical sense of quality does occur, but that it does not do so in terms of identical behaviors by fathers and mothers toward their infants. Nor is their any overwhelming evidence that such would be desirable.

Most of the authors presented here have continued to be interested in this topic and form the nucleus of a small but growing number of researchers interested in the infant and its "other" parent. This is a small book in terms of size (a better term might be concise); yet it is well edited and stands alone in bringing together in one volume much of the detail relevant to infant-father study -- methods and orientations that continue to be used. Students of early human social development will probably want to own this book; they certainly will want it in their institution's library.

Bailey, M.T. Affinity: An ethological theory of the infant-father relationship. Paper presented at the Third International Conference on Infant Studies, March 18-21, 1982, Austin, Texas.

Mackey, W.C. Parameters of the adult-male-child bond. Ethology and Sociobiology, 1979, 1, 59-76.

ENVIRONMENTAL FACTORS IN MAMMAL REPRODUCTION. Edited by Desmond Gilmore and Brian Cook. Baltimore: University Park Press., 330 pp. (1981)

Reviewed by Brian A. Glader, Dept. of Psychiatry and Behavioral Science and the Long Island Research Institute
State University of New York, Stony Brook, New York

Editing a book of science chapters is tricky business. Often editors and scholars strive for the goal of updating a complex and expanding field of inquiry. It is a monumental and difficult task. Since so few of us are broadly authoritative, contributing experts are sought, with their ideas and writings woven together by theme and editorial skill. Somewhere a balance is sought between readability and relevance, between state-of-the-art knowledge and cohesiveness of ideas. And when the area of interest is that vast interdisciplinary realm of environmental influences upon reproduction, one hopes for the best result, but will agreeably settle for a close second.

This work, ambitiously titled as it is, may lead the reader into expecting a definitive review of current thinking on the subject. It does not. Where the editors fall short is not their choice of topic items (limited though they were) or their selection of authors (all first rate scholars in their fields). The problem with this book is that it promises more than it delivers. Environment is a big area; so is reproduction. Scientists who wish to keep up with emerging overlaps of these areas would have hoped for clear, broad and deep explanatory reviews of the many subthemes of the overall topic of "How does environment interact with reproduction in mammals?" Instead, we have here a collection of loosely related reviews and research reports.

* Beginning with a classic zoological account of mammalian natural history and paleontology, we are then given a concise discussion of brain, hormone and environmental interactions. Reports of photoperiodic control of breeding cycles in sheep and cattle is then followed by several chapters regarding pheromones and sexual behavior in rodents. Later, two reports on nutrition in (again) bulls and ewes precede and rather specific accounts of tree squirrels, desert rodents and mink. The book concludes with a special section devoted to "environmental factors in human reproduction": four curious in chapters covering demographic breeding patterns in Great Britain, environmental determinants of human sexuality and gender identity, and a brief review of contraceptive technology.

With so eclectic an array of species, topics and formats, this volume would have profited well from some

attempt at thematic cohesion. Instead, we have an assemblage of papers that may or may not relate to one another depending on the reader's viewpoint. To be sure (and fair) the book does contain something for everyone. Between many interesting research reports are useful reviews on: stress and fertility, factors affecting gestation, and nutritional influences on reproduction. In addition to providing the reader with general information across species, these reviews contain occasional speculations to keep the reader awake. And, of course, if your interests combine rodents, cattle, sheep, photoperiods, pheromones and feeding, this is indeed your book. The rest of us will have to pick and choose. And as is so often the case, marsupials, cetaceans and large carnivores get short shrift in favor of small rodents or domestic animals.

THE ETHOLOGY OF HUMAN SEXUALITY

THE EVOLUTION OF HUMAN SEXUALITY. By Donald Symons. New York: Oxford University Press. 320 pp. (1979)

THE SEX CONTRACT: THE EVOLUTION OF HUMAN BEHAVIOR. By Helen E. Fisher. New York: W. Morrow. 253 pp. (1982)

Reviewed by David Alan Munro
Laguna Beach, California

It appears to have taken us a full century to have become aware that two-thirds of Darwin's famous Descent of Man (1871) was called Sexual Selection. Darwin did not then have the information we now have on the speedy rise of Man, e.g., in brain size, but here was the answer to the question raised when Louis Leakey and others discovered our ancestral skulls and began to time our astonishing development.

Now, few doubt the centrality of the task for ethology that this leading provides. We are the sexiest animal as well as the brainiest animal. It would be surprising indeed if these two Guinness Records were unrelated. Let us say that the basic research questions may be formulated: How have our preprogrammed sex behaviors selected for the rise of Homo sapiens?

Our touchstone in this inquiry is what is now called "inclusive fitness," a sex-based measure which is simply an operational spell-out of what Darwin had in mind. State it as the relative success of an individual (or group) in producing fertile offspring. Grandparents have thus proved their fitness; their fitness measure is the number of fertile children they have produced, their relative fitness

is this number divided by some national or world norm. Families prove their fitness by a relatively rapid increase in numbers. Tribes demonstrate superior fitness when they conquer another tribe, murder the males and impregnate the captive females. Rapists are relatively more fit than wilder men; prostitutes more fit than primmer girls; disobedient Catholics more fit than the obedient; playboy males more fit than the circumspect, etc. The measure is unequivocal. Fitness is "countable."

In their different ways the authors here under scrutiny are also concerned with parental investment. This might be stated as the degree to which parents have contributed to the health and socialization of their children in such manner as to increase their survival fitness. That is to say, the well-socialized, non-poor and healthy child -- having itself a higher fitness measure -- retrospectively enhances the fitness measure of its parents.

Unfortunately, sexual selection is a slow process, however relatively fast it has been in our case. Thus, as Jerison points out (1955) it took the brainy dolphin some five million years to attain a brain-body size ratio comparable to our own, while we attained ours in only one million years. But the speed and relative recency of our attainment does us little good research-wise; the process was complete ten thousand years ago when our long hunter-gatherer period came to an end. We have now only the bones and artifacts of our remote ancestors, or probable ancestors, to pore over. We must infer what the old sex-patterns were like, either on the basis of present primitive societies, preferably hunter-gatherer, or on the basis of our closest living primate relatives. This is slim picking, research-wise. Symons insists that speculation upon how estrus was lost and the human family formed is "a game." What is needed for such anthropological speculation is gamesmanship.

Research in anthropology also suffers from its rather disreputable past. Anthropology remained too long committed to a Sunday-supplement search for racy human oddities; so long, in fact, that the search for human commonalities was overlooked. Thus Eibl-Eibesfeldt, ingenious master of the candid movie camera, finds that his shots of threats, greetings, flirtations, etc. encounter few comparable films in the vast anthropological files. And Donald Symons regrets continuously that ordinary and customary sex behaviors have not been recorded.

One might say that, whereas a successful ethology has forced a realistic psychology upon an established psychology, and a sociobiology upon an established

sociology, the coining of the comparable "anthrobiology" yet awaits the recognition of the need for it.

The two books here under review clarify that need. They are anthropological in that they consider the impact of genetic behavioral predispositions upon a variety of societies, extending far back in time and widely across cultural boundaries. Their weaknesses reflect this unwieldy breadth and this scientific newness.

Fisher's book is the weaker of these two. But its critical weakness is not so much the raw newness of anthrobiology as the author's eagerness to accommodate women's movement ideology in her science. This mixes politics, an art of the possible, with science, a search for the probable: a mixture more acceptable in the free-wheeling days of the naked ape and pop-ethology. Under the exacting eye of the Eighties it will not scan. But Helen Fisher deserves an "A" for trying.

Fisher points out that all the famous pop-ethologists are (or were) males: Ardrey (not mentioned), Lorenz, Morris, Tiger and Fox. She singles out for attack the Tiger-Fox thesis (in The Imperial Animal, 1971) that traditionally "males dominated females," a theory which she denounces, saying it "reeks of biological sexism, not to say male pomposity" (p. 139). Unfortunately, Fisher does not mention Tiger's earlier and far more scholarly book on this subject (Men in Groups, 1969).

But just what are the two ethological points Fisher finds at such odds? Tiger maintains, in both books, that out of the close male-male associations developed in hunting and warfare -- and long since duly implanted in the human behavioral genome -- grew the sense of fitness of both a military and then a civil government. Fisher maintains that women, typically engaged in the cooperative enterprise of raising and protecting children, produced the basic forms of government as an extension of this cooperation.

The record, of course, favors Tiger (and Tiger-Fox), if only because males always do the fighting, and the need for a soldier class leads to an all-male age-grading which produces an all-male ruling council -- hedged about with whatever predispositions we have toward the acceptance of male authority figures as opposed to female. To counter this Fisher takes us far back into the history of our kind, pointing out that the basic social bond in primate societies is between mother and child. She sees this as evidence of a basic female authority, also buried in our genes.

But this is hardly reopening the battle of the sexes. It is not an all-or-none dichotomy. What we find in the

tribes of the world is an accepted accommodation. The highly successful Iroquois confederation, for instance, vested ownership of property, including the weapons of war, in their women. This directly involved women in war-peace decisions, if only with a veto. Among the Apache, women -- especially the older ones -- were often the instigators of warfare, and were permitted to run a spear through a captive enemy soldier, trussed up for the ceremony. The systems of intersex political arrangements, as well as the intersex divisions of labor, are multitudinous. What we should say of them is not that this or that system is sexist, but that they did exist, that we need and expect a division of labor by sex in our affairs. In politics, such systems are weighted male, because of our military past; in peaceful pursuits, especially where nurturance is at issue, they are weighted female. But one can hardly say that Fisher has "refuted" Tiger.

This calls in question Fisher's grand theme: the sex contract. Presumably this title suggests Rousseau's Social Contract (1762) and is intended to be equally fundamental. In essence, the intersex contract runs: I'll give you sex; you protect and support my children.

Here Fisher offers her own scenario in the estrus-to-human-family anthropological competition. She relies on Sherrey's nymphomania (in The Nature and Evolution of Female Sexuality, 1966) to make the "political" point that women, unlike men, may experience multiple orgasms without satiation. Women, says Fisher, are "sex athletes" -- the title of her first chapter. They bargain with these generous favors for power over men, and ultimately for authority in the community.

But, says Symons, in this current book cited by Fisher (1979), "The sexually insatiable woman is to be found primarily, if not exclusively, in the ideology of feminism, the hopes of boys, and the fears of men... A woman's claim is... to conceive no children that cannot be raised; to choose the best available father for her children; to induce males to aid her and her children; to maximize the return on sexual favors she bestows; and to minimize the risk of violence or withdrawal of support by her husband and kinsmen..." (p. 92).

Furthermore, the logic of Fisher's sexually insatiable woman might lead us to believe that she, and not her mate, is indeed the sexual athlete, perpetually on the alert, etc. This, Symons conclusively shows, is not the case. He cites The Report of the U.S. Commission on Obscenity and Pornography (1970) to substantiate the thesis that the pornography interest, the voyeurism, in the Nation is almost wholly male. He shows that men, but not women, will be

immediately interested sexually in a person of the opposite sex, on sight. He finds that men but not women are interested in sexual variety, per se. In sum, he finds that caution marks woman's sexuality -- pointing out that even prostitutes bargain cautiously -- while recklessness marks man's sexuality.

These attitudes, however, are by no means either arbitrary or recent. Symons shows that inclusive fitness inheres in them all.

It adds to a man's inclusive fitness to desire and occasionally impregnate the outside female. It decreases a woman's inclusive fitness to jeopardize her family life with an outside fling. And human jealousy, Symons points out, is of a different order in the one case than in the other.

Interestingly enough, Symons lays this added animosity to the male fear of being cuckolded. But as Malinowski has shown (1929) in his famous study of the Trobrianders, primitive males who do not know what causes pregnancy are no less concerned with keeping their wives to themselves and no less outraged by any irregularity. This leads us to believe that the jealous protection of wives from outside males may antedate the discovery of the function of sex in pregnancy. If so, then yet another window opens on our past. For here we find humans behaving as if they understood the function of sex, and in fact behaving as if they understood their place in the evolutionary scheme, including keeping their line inviolate.

This observation puts the Symons approach on the spot. Sexually we do, he would say, what is of interest to our inclusive fitness that we do. Never mind whether we do it deliberately or not. Trial and error takes care of that. Trial and error over many millennia.

Add to this the element of service. Among humans, sex is a service by females for males. This is the universal human but not primate rule. It precipitates a pattern of female enticement in their intrasex competition for the most desirable males. Symons even opines that this is so recent, though universal, that evolution has not had time as yet to mold females physically for such advertisement. There is no "convincing evidence," he says, "that the function of any human female anatomical characteristic -- such as breasts -- is to stimulate males visually" (p. 198). Thus he would relegate to cultural recency the universal sex-adornment and sex-enticement activities of the female. If an evolutionary change has occurred to make modern sex possible it is "in male brains, not female bodies."

To be sure, this is to overlook Morris (1967) on the human breast, and the considerable work of Eibl-Eibesfeldt on self-mimicry (1970), if not Charles Darwin himself on the origin of female beauty (1871). But no matter. Symons, in the presentation of sex as female services is further laying the basis for female strategy -- itself complementary to male strategy.

We inherit our several strategies, Symons tells us, from behaviors most successful in advancing our inclusive fitness throughout the long past of the species. Our women, however sweet and lovely, are all descendants of girls who granted no favors without a quid-pro. Our men, however noble and generous, are all descendants of post-adolescents who took what they could when they could.

In terms of primitive emotions we are thus preprogrammed to act out these same behaviors. But the organism, says Symons, "at least the human organism -- is neither a passive mediator between stimulus and response nor a mindless vehicle of culture, but an active assessor and planner..." (p. 167). We strategize our way into the acceptables of marital sex.

However, Symons concedes, despite the heavy hand of society upon marriage, there is "much more scope for individuals to arrange their own copulations... [which become] adaptations to maximize reproductive success in an environment in which marriage is ubiquitous" (pp. 167-8).

This remark raises questions answered by neither book. It seems to this reviewer that the ever-present (male) desire for novelty in sex is a far more basic and portentous influence in our world than is recognized. Currently I read, for example, of a Japanese "white slave" ring which imports blonde American girls for the local brothels. This is interesting because Japanese society is otherwise so tightly racist. Simultaneously, I read (in Cordelier's *La derobade*, 1976) that West Indian girls and other nonwhites are more popular than the local girls with the customers in Paris bordellos. Travel enticement posters are equally convincing in their display of smiling nonwhite girls. Yet, within marriage, the rules are typically centripetal. It is made difficult for Jews not to marry Jews, Catholics not to marry Catholics, blacks not to marry blacks, the wealthy not to marry the wealthy, etc. I see in this restrictive tendency an influence that could have divided us into separate species, over time. Therefore I see in our overwhelming inclination to these irregular copulation yet another kind of fitness-survival. It is part of our strength that we are one species. We are kept that way by a (male) desire for novelty that grows more intense as it crosses state lines.

I should add here a probable (or inevitable) contribution of biological science with a revolutionary potential. It seems to me certain that paternity questions will soon cease to be questions but become laboratory certainties. This, with its acceptance, will usher in the brave new world. Call it "an aware genetics," not "a scientific genetics."

However, despite all the grim medicalisms -- coitus, intromission, copulation, ejaculation -- the flower of romance still blooms. We still cling human-to-human to shut out the alien sea around us. We still reinvent love time out of mind to confound our better, our rational selves.

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MINI COMMUNICATIONS

In Your Right Mind: Communication Beyond Words

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One of the major areas of interest in the field of human neuropsychology today is the nature of hemispheric specialization for cognitive, sensory and motor behaviors. The older notion of hemispheric dominance, which developed in relation to the saliency of speech and language disruption following left hemispheric lesions, has clearly given way to the notion of specialization of function. The following discussion reviews a body of data related to specialization of the right hemisphere which implicate it in important aspects of human communication, many of which might profitably be viewed in an ethological perspective. The two major areas of focus are a right hemispheric role (a) in the recognition and appreciation of communicative information conveyed via facial expression, and (b) in the processing of the suprasegmental and pragmatic aspects of spoken language.

The right hemisphere has traditionally been most strongly associated with specialized capabilities relative to visuospatial analysis and manipulation. Patients with right parietal lobe damage may demonstrate marked difficulties in the appreciation and reproduction of complex visuospatial material, judging distance, finding their way in space, etc., while patients with right temporal lobe damage are often impaired in aspects of memory for nonverbal visuospatial information. The deficit may extend to both primary recognition of and memory for faces and to the appreciation of the emotional tone conveyed by facial expressions. Prosopagnosia, a deficit in the recognition of familiar faces (sometimes even one's own face in a mirror) is consistently associated with right parietal lesions (Derenzi and Spinnler, 1966; Derenzi, 1968; Benton and Van Allen, 1968; Warrington and James, 1967; Yin, 1970). Inability to learn and remember new faces has been reported following temporal lobe lesions (Milner, 1972). In stimulating mapping studies of the right cortex in patients undergoing neurosurgical procedures, we have localized the perceptual matching of faces to the parietal cortex and short term memory for faces to primarily the posterior superior temporal lobe gyrus (Fried et al., in press). Finally, faster responses to human faces as well as fewer errors in recognition have been found with left than with

right visual field tachistoscopic presentation suggesting a right hemisphere superiority in processing for face material in normal control subjects (Hilliard, 1973; Klein et al., 1976). Although patients may have difficulty on all kinds of visual-spatial analysis, face recognition has been dissociated anatomically from recognition of other complex figures (Yin, 1978). That facial stimuli have a special status for human observers, rather than comprising merely a subclass of patterned visual stimuli in general is also suggested by the developmental data. Ontogenetically, one of the earliest complex visual patterns responded to preferentially by infants is the human face, suggesting an early developing or innate property of the brain to perceive and discriminate human faces (Fantz, 1958; Fagan, 1976; Young-Browne et al., 1977). In an evolutionary sense, face recognition of conspecifics would likely be very adaptive in the development and maintenance of small cooperative social groups.

Much of the information regarding the current emotional set of an individual is conveyed via facial expression. Indeed these nonlinguistic patterns are some of the most widely studied and documented cross-cultural communicative universals. Left visual field (right hemisphere) superiority has been found for the recognition of emotional faces in normal control subjects (Buchtel, 1976). Patients with right hemisphere damage have difficulty relative to those with left hemisphere damage in discrimination and labeling emotions expressed by the face (Dekowsky et al., 1980). Finally, our cortical stimulation mapping studies have indicated that labeling of facial expression can be altered in some patients with stimulation (disruption) of discrete sites on the right middle temporal gyrus (Fried et al., in press).

There is also some evidence to indicate that under some circumstances the production of facial expression may be more dependent on the right hemisphere. The left side of the face, controlled primarily by the right hemisphere, has been reported to adopt those features of facial configuration which convey emotion more completely than the right side of the face when emotions are posed (Campbell, 1978; Sachdev et al., 1978) although spontaneous emotional expressions appear more symmetric (Ekman et al., 1981). Thus there is some evidence that both the detection and production of facial expression would also be very adaptive for individuals in social cooperative groups. Comparative studies of human behavior have pointed to the importance of accurate analysis of the facial expressions of other members of the species to successful intraspecies interactions (Lockard, 1980; Keating, 1981). Misreading an angry facial expression for one of happiness is fraught with hazard. There is reason to believe that the ability to recognize

emotional facial expression is a function for which there would be substantial evolutionary selective pressure. Because of the multitude of facial movements involved, association of complexes of them to type and level of emotional tone would require a sensitive visuospatial pattern analyzer.

The major focus for most studies of human brain asymmetry has been the linguistic system. The left hemisphere of the brain has been clearly implicated in the production and perception of the units or segments of speech (phonemes) and in the sequencing of those segments into meaningful language specific words and phrases via well ordered phonological, semantic and syntactic rules. It has been hypothesized (Kimura, 1976; Mateer and Kimura, 1977) that these aspects of speech and language production may fundamentally be built upon specialized systems in the left hemisphere for complex motor control of both the oral and brachial musculature. More detailed examinations in recent years, however, have supported a right hemisphere role in language processing. Although the right hemisphere is minimally if at all capable of voluntary or propositional language production, it does have some ability to understand simple nouns, adjectives and verbs, as well as some simple grammatical relations (Gazzaniga, 1970; Gazzaniga and Hilliard, 1971; Zaidel, 1973). Nonspeech auditory stimuli such as environmental sounds (Curry, 1967; Carman and Nachson, 1973) and certain types of music (Gordon, 1975) were the only types of stimuli thought to be processed primarily in the right hemisphere. More importantly, the right hemisphere appears involved in both auditory and visually based aspects of normal communication which are quite different from those aspects controlled by the left hemisphere, namely in prosody and pragmatics.

Prosody refers to the intonation, stress or rhythm patterns which are inherent in the human speech signal. These patterns, which acoustically can be identified as systematic alterations in fundamental frequency, intensity and temporal spacing of events, far from being merely a decorative ornamentation of speech patterns, function in important ways to convey both aspects of linguistic meaning and the emotional tone of the message. An example of meaning clarification is one in which the stress pattern solves what would otherwise be linguistically unclear. Acoustic stress on the red in "Get the red dress from the closet" conveys that the searcher is to retrieve a red vs. a green dress rather than retrieving a red dress vs. a red coat. Our speech is filled with such important clues to the meaning or intent of a message. Patients with right temporo-parietal cortex lesions have demonstrated deficits in both the perception and production of such intonational features of speech (Weintraub et al., 1981; Blumstein and

Cooper, 1974).

The affective components of language, i.e., the cues for emotional tone, are also carried production. The same intonational patterns of speech production. The same sequence of words can have much different communicative intent and effect when produced variably as a statement or as a question, in a calm tone, in anger or in devastating sarcasm according to its prosodic intonation. Indeed, it has been estimated that over 90 percent of a message's effect is conveyed by such suprasegmental characteristics, and less than 10 percent by words (Mehrabian, 1972). Patients with right hemisphere disease have been reported to have difficulty with both discrimination of and production of affectively intoned speech (Tucker et al., 1977; Ross and Mesulam, 1979).

In addition to being differentially affected by lateralized hemispheric lesions, the linguistic and prosodic aspects of communication appear to emerge somewhat independently in development. Normal neonates 24 hours old respond to heard speech rhythms by synchronizing their movements to those rhythms (Condon and Sanders, 1974). Children appear to learn and produce the intonation patterns of the language in the environment during the first year of life well before any significant development in the production of the segmental sound or phonemic system of the language (Dore, 1973; Peters, 1974). Special speech routines that involve rhythmic structure (e.g., patty-cake) are found in all cultures in interactions with infants (Moerk, 1972). It has been noted cross-culturally that mothers alter their speech prosodically when speaking to young children as opposed to older children and other adults. High pitch, exaggerated intonational patterns and alterations in distribution of primary word stress are but a few of the features of "motherese" which human speakers systematically adopt when speaking to young children (Garnica, 1977; Sachs et al., 1976). Similar alterations have been noted in speech directed to foreigners, the retarded, the elderly or any person perceived as a linguistically unsophisticated or disabled listener, presumably in response to the speaker's (correct or incorrect) perceptions of his listener's needs. Indeed there is some evidence that both young children and older adults experience difficulty when attempting to decode speech characterized by unfamiliar or atypical prosodic patterns. Wardhaugh (1976) has argued that women typically vary their intonation patterns more extensively than men to signal endearment, excitement, pleasure, etc. Most current thought relates such features more to culture than to biology. However, given support from many recent studies that women appear to have somewhat more bilateral representation of language than do men, i.e., more right

hemisphere involvement (McGlone, 1980), it would not be surprising if these apparently more right hemisphere dependent prosodic features of language were more striking in females.

Asymmetries also appear to exist in reference to perception of acoustic features of auditory input including the speech signal. The left hemisphere appears particularly capable of handling acoustic information characterized by stimuli which change rapidly over a short time interval (i.e., stop consonants, short duration tone sequences). The perception of more steady state information which characterizes complex tones and vowels has usually been found to be less lateralized or indeed more related to the right hemisphere (Kimura, 1963; Cutting, 1973). This may relate to why the intonational patterns of speech, which are carried out over a longer temporal span appear more right hemisphere dependent.

Speaker recognition is another function which depends critically on the steady state tonal or harmonic structure of the speech signal (Matsumoto et al., 1973). Even with a very limited speech sample (a "Hello" degraded by telephone transmission), the information carried via the fundamental frequency and the harmonic distribution of steady state vowel information is enough to reveal to the listener not only the sex, size and approximate age of the speaker but even the individual identity of its owner (Garvin and Ladefoged, 1963; review by Bricker and Prozansky, 1976). Since the right hemisphere appears to excel in the analysis of tonal or harmonic structure, it is not surprising that deficits in voice recognition have been found to be more prevalent in patients with right hemisphere damage (Van Lancker and Conter, 1982). Even severely aphasic patients with left hemisphere damage often demonstrate normal voice recognition. Voice recognition appears to be a very early emerging capacity. Infants just three days old have been shown to respond significantly more often to their mothers than to other adult female voices (Decasper and Fifer, 1978). Nursery school children are almost as accurate as teachers in recognizing classmates' voices (Bartholomew, 1973). Knowledge of speaker identity as well as speaker sex, size and age might again be highly adaptive in cooperative primate groups composed of multiple unrelated individuals in a wide range of development stages.

It is also interesting that beyond puberty, individuals demonstrate much more difficulty in acquiring new oral language systems without "foreign accents," features of which include vowel coloring and intonational patterns more often than such more easily learned features as grammatic or syntactic use. Marking of individuals who interact with a group post-puberty by vocal features may play a role in clan

or group member recognition. Assal et al. (1976) reported that patients with right hemisphere lesions were significantly more impaired than left hemisphere cases in discriminating among foreign accents.

Pragmatics is defined as a system of rules that explicate the use of language appropriately depending on the situational context. For example, if someone asks "Can you pass the salt?" a response of "yes" without passing the salt is linguistically appropriate (i.e., a yes/no question was asked) but pragmatically inappropriate since that question was not an information question but a request. Linguistic relationships which have figurative rather than literal meaning (e.g., "He was wearing a loud tie") are also thought to be governed under this pragmatic system. Patients with right hemisphere damage have been found to respond to such sentences literally and concretely suggesting that although their language is linguistically correct, it is pragmatically incorrect (Blumstein, 1980). The study of pragmatics is a young one but has proven very fruitful. Included in its domain are investigations of rules for discourse, effects of the social rank of speakers and the situational variables on communicative intent and orderly turn taking in conversation. Although too numerous to cite, many of these areas suggest a highly organized rule base which emerges early in the development of communicative interaction.

In summary, there is evidence to suggest that the right hemisphere plays a major role in the communicative process. The right hemisphere is implicated in the production of perception of prosodic features, those suprasegmental features of speech production which carry information not only about intended meaning but about the speaker's emotional tone, social relationship to the listener and individual identity. The right hemisphere is also implicated in the perception of facial configuration and in the production and perception of facial expression, nonverbal visual cues which supply information relative to both the individual identity and the emotional tone or affect of the speaker. The emergence of and attention to both prosodic features and elements of facial configuration early in ontogeny and cross-culturally supports a strong biologic as opposed to purely cultural basis for these important paralinguistic communicative features. Finally, there is a suggestion that the pragmatics of language, the integrated use of all aspects of communicative behavior relative to communicative intent, setting, speakers, etc., is in major part dependent on the right hemisphere.

A feature which comes to mind in all of these behaviors, whether visually or acoustically based, is that they appear to engage a holistic or Gestalt mode of

perception. Introspectively, one feels that voices and faces are recognized as wholes, without recourse to analysis of special details. Considerable laterality research supports that the right hemisphere utilizes such a holistic mode of processing (Cohen, 1973; Beaton, 1979; Carmon and Nachshon, 1971) and that it is the processing mode which underlies its special abilities. Studies of face perception by Yin (1970) underscore this interpretation. Recognition of inverted faces fails to evoke the expected right hemisphere advantage. Yin (1970) hypothesized that, prevented from grasping a meaningful configuration, recognition depended on analysis of detail minimizing engagement of the right hemisphere. A body of research is emerging which suggests a dichotomy between left hemisphere/featural processing versus right hemisphere/holistic processing.

The saliency of primary spoken languages and their disruption in aphasia have traditionally occupied the primary attention of researchers interested in human communication. However, the prosodic or suprasegmental features of language, the communicatively loaded visual information carried via production and perception of facial expression, and the pragmatic integration for accomplishment of communicative function, are critical to effective intraspecies communication. It is possible that they may reflect a much earlier evolutionary development and thus shed light on more fundamental biologic specializations. At this point, specialization appears most strongly dependent on right hemisphere activity. Human ethology represents the approach to human behavior analysis that hopefully will most integrate the biological and evolutionary bases of these behaviors with their current neuroanatomical and physiological bases.

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RESPONSE TO CALVIN'S THROWING THEORY

In the December, 1981 issue of HEN, William Calvin presented a MINI COMMUNICATION entitled "The Throwing Theory for Language Origins." We present here an interesting discourse between John Ross and William Calvin consisting of responses by Ross, Calvin, and Ross again, in that order.

The Thrown Out Theory of Language Origins

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As far as can be presently determined "undoubtedly humans are the outgrowth of a whole series of selective pressures, which forced our predecessors to learn to eat novel foods, live in colder habitats, develop kinship strategies" (Calvin, 1981, p. 21) as well as a host of other behaviors. However, to postulate that these behaviors, strongly augmented by the ability of the female to throw a stone at a predator or prey with one hand, was the ratchet necessary for human evolution and language development, is very questionable. There are three reasons for this. First, if the female is going to be so good at throwing

stones that the velocity of the throw will kill or maim, then an overhand throw would be predicted. The evidence does not appear to support the selection of high velocity, well aimed, overhand throwing by today's women. Second, if the advent of the "throwing genes" will permit females to protect themselves and their unborn so that they are not chased and thus do not spontaneously abort, we should find data that indicate that nonhuman primates rather frequently spontaneously abort after being chased. Although the data may exist, it appears to be hard to discover. Third, what kind of neural machinery would be necessary for this type of throwing which involves one second, more or less, of throwing time?

Calvin (1981) postulates that the cerebral cortex is in charge of this type of throw, and that there would be the selection for neocortical tissue which would sequence it. The neurobehavioral data, however, would appear to indicate otherwise. The work of Kornhuber (1974) and DeLong (1974) demonstrate that slow, "ramp" movements are under the control of the basal ganglia, and thus, the cortex. These are the slow movements that require some type of sensory feedback to control the next step in the movement. For example, the grooming of one primate by another. The moving of the hair, the seeing of a piece of debris, the picking at it with a precise finger movement, the tactile and visual input that the object is in the fingers, and the resulting movement of the fingers, would be under cortical and basal ganglia control.

Kornhuber (1974) further indicates that the movements necessary for the throwing of an object, such as a rock, may be initiated by the cortex but is under the control of the cerebellum, medullary and pontine nuclei. A ballistic movement, such as swinging a bat, throwing a ball or a stone, is a movement that is carried out without feedback. The individual "sets" the proper "switches" beforehand, the object is moved according to a predetermined trajectory, and the coordinated pattern of muscle movements is carried out without feedback until the subject realizes whether the movement was done correctly or not. It is important to realize that this whole pattern of movements has feedback only at the termination of the sequence, whereas in a ramp movement each part of the movement is under the control of feedback.

Thus, there does not appear to be much anatomical "data" to support the idea of female stone throwing being part of the ratchet which helped set the stage for human evolution and language development, particularly considering that women are less laterally differentiated than men (Lewy and Reid, 1978). As well, the sequencer postulated by Calvin (1981) has to be moved from the neocortex to the

cerebellum. However, the observational study of Lockard et al. (1979) and the literature review therein clearly indicate that women do prefer to carry their young in their left arm. There must be an explanation for this behavior other than throwing.

If it is still postulated that left hemisphere dominance of language and sequential functioning is going to be the explanatory mechanism for left arm holding of the young child, then it should be quite possible to demonstrate that right handed, noninverted and inverted writers should show left hand holding of young children. It has been postulated (Lewy and Reid, 1978) that the former of these groups uses the left cerebral hemisphere to coordinate their right hand motor activity whereas the latter uses the right hemisphere. In noninverted right handed people the majority of the pyramidal tract crosses in the medulla to control the right hand, and in the inverted writer the pyramidal path does not decussate as completely and thus the right neocortex controls the right hand. Thus, these two groups should carry in the left arm. Similarly, the noninverted and inverted left hander should prefer to carry the child in the right arm and leave the left arm free for better motor control. An observational study could resolve this.

However, the assumption that the writing hand will give a good measure of the language hemisphere as well as of motor function is questionable. The motor function of writing appears to be a pyramidal function. But is the act of arm movement or a throw under pyramidal control? Brooks and Stoney (1971) and Hefner and Masterson (1975) indicate that the function of the pyramidal system is associated with finger dexterity and not with large movements; finger manipulation of a stone but not its throw. Arm movements would be extrapyramidal functions, and to the extent that they demand cortical control they should demand about the same in any advanced primate -- or at least less differentiation between species than is shown in pyramidal neuroanatomy. Thus we cannot predict a small step to language sequencing.

As previously indicated, Lockard et al. (1979) have pointed out that women prefer to carry children in their left arm, and that one of the reasons for this is so that the child can hear the maternal heartbeat. It appears that this explanation is as good an explanatory device as we presently have, and that the notion that women show this behavior as a result of a selection for stone throwing which then evolved into language functioning does not hold up.

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Throwing Theory Thrown Out for Target Practice

Response by William H. Calvin
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The title of Dr. Ross' commentary is delightfully inventive, but I am in too much agreement with what he says to believe that his title adequately encapsulates his text. The theory was, however, thrown out to provide everyone with some target practice on a "fast path" proposal for encephalization.

His first point, that modern female hominids are not noted for their throwing abilities, may well be valid but surely someone else will write about practice effects, about the exclusion of girls from the Little League, etc. My interest is in early hominid practices, hoping to get at the question of origins and bigger brains, not present-day hypertrophy (I avoided talking much about baseball pitchers for the same reason). There is, of course, even less data on early than on modern female throwing abilities. We will just have to hope that the theory will inspire someone to design a good experiment or look at fossil shoulder joints,

comparing the sexes.

The second point concerns selection pressures, a commentary on my suggestion that the fetus/infant survived in accordance with its mother's acquired abilities as well as its own talents. I too am unaware of any data on the point; yet the mother/infant coupling for selection provides, in theory, an attractive path for selecting many kinds of abilities, not just throwing. I agree that there must be an explanation for the "left-armed" infant carrying other than throwing. Indeed, I suspect that right-handedness is likely a consequence of left-armed infant carry, which is itself perhaps an adaptation to pacify newborns which complain when being weaned from the sound of the heartbeat to which they became accustomed in utero. Hunting mothers would need to keep infants quiet; because of the heart's left-sided aspect, this would be best done with the left-armed carry, so mothers with left brain motor sequencing and right-handed throwing might be better hunters than mothers whose best throwing arm was also their best infant-pacifying arm (Calvin, 1983, in press). Maternal throwing provides some unique and especially strong possibilities for Darwinian selection mimicking neo-Lamarckian inheritance.

But, whatever the fate of this maternal augmentation to selection, throwing by non-maternal hominids is still a behavior exposed to strong selection pressures -- and one which has the potential for a hominid population explosion, as hominids dependent upon subtropical foraging could now live in the colder climates, secondarily utilizing the widespread habitats of their prey, birds and small mammals, in proper omnivore predator fashion. I hope that the maternal possibility will not overshadow the more mundane selection aspects.

Neural machinery is, of course, at the core of my argument and Ross properly focuses upon it. But I am not sure that cortical vs. cerebellar, male vs. female, or pyramidal vs. extrapyramidal dichotomies are going to further the argument. It may be, of course, that the pyramidal system's specialization in the distal musculature is relevant to the key timing step of the throwing sequence: the release of the rock by the fingers during the narrow "launch window" near the top of the throwing arc (Calvin, 1982; 1982, in press). Since other primates do not typically engage in precision throwing behaviors, our monkey lesion experiments tend to focus upon deficits in fine manipulation tasks such as picking worms out of holes. Handedness is strongest, however, for ballistic movements such as throwing and hammering, not for fine movements such as threading needles (Annett, 1970) whose antecedents may well be the primate grooming and seed-foraging behaviors.

Hammering by chimpanzees to crack open nut shells (Boesch and Boesch, 1981) is an action which is probably more closely related to throwing than are the typical pyramidal tract behaviors in the literature -- and the most sophisticated techniques are largely practiced by the females.

The possibility of testing some of these ideas on individuals with left brain language but right brain sequencing is an attractive one, though such results might reflect more on how to make the best out of a less-than-optimal arrangement than upon the easiest evolutionary path.

I agree that ballistic movements must rely heavily upon pre-programmed sequences rather than feedback. I too would have picked cerebellum as my prime candidate for precision timing (see, for example, Koosis et al., 1981), had I not been so familiar with the manual and oral-facial sequencing specializations of the left hemisphere -- and with the interactions between concurrent speech and manual sequencing (Lomas and Kimura, 1976). At this stage, we cannot distinguish between cortical and cortical-subcortical loops; the stroke evidence is too crude, and the stimulation data (Ojemann and Mateer, 1979) merely shows surface sites where the ongoing behaviors can be disrupted, which is not necessarily where those behaviors "reside." Analogous clinical opportunities have not existed to test human basal ganglia in a similar way. While we cannot eliminate any possibilities at this stage, selection favoring a left hemisphere sequencer could uniquely provide neural machinery for a variety of other functions known (Bradshaw and Nettleton, 1981) to reside primarily in the left hemisphere.

I see preprogrammed sequences for ballistic movements as providing neural machinery very similar to what might be handy for the recursive aspects of analyzing our sequential language with its emphasis on word order. In the former case, one needs a buffer in which to load the proper motor sequences during "get set," which can be rapidly and precisely emptied at "go" to orchestrate a sophisticated rapid movement. That same buffer would be just the thing for holding the received components of a long sentence (for example, this one) while phonemes were first analyzed, then phrases, then the whole sentence analyzed with something simpler holding the place of the phrases. Because phoneme discrimination seems intimately related to making a "motor model" of what you hear (Ojemann, 1982), the idea of a motor buffer being used for recursive neurolinguistic analysis is not unreasonable.

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And The Target Was Preyed Upon?

Response by John A. Ross
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As Dr. Calvin (1982) has said so well, there appears to be little that we can disagree on and little that we can really say we understand when it comes to the underlying

neural machinery that controls either "throwing" or "language." If that is correct, and if I am to offer any type of response to his response, then I have two alternatives. One would be to rush to the library, look up more references which may or may not agree with Calvin (and me) or push back my chair and try to think. I prefer the latter at this point.

Calvin's third and fourth paragraphs (1982) are at the root of the issue. What are the selection pressures which led to (1) throwing, and (2) language. Are throwing and language the result of one set of pressures, that is, both outcomes of one "step" in human evolution, or are they the result of two "steps"? In either case, what would be the fastest and optimal path for encephalization?

Before changing the level of the discussion I would like to add three points. First, Kinsbourne (1978) supports the notion that ramp movements and language production may be interrelated by writing "...we have been able to show a similar relationship between overt and covert vocal activity and motor performance on the right side of the body. Briefly, if the limbs on the two sides of the body are concurrently active in an unrelated manner, concurrent vocal activity is more effective and less characterized by error if it conforms to the overall program of the right-side limbs. If the right-sided limbs are employed in action unrelated to that of the vocal apparatus, there is much mutual interference; if the left-sided limbs are active in the exact same way, the interference is slight or absent" (p. 558). Second, Nottebohm (1971) sectioned the left hypoglossal nerve of the chaffinch after its adult song pattern had been developed and destroyed the song, whereas sectioning of the right left it unaltered. Thus, asymmetry of vocal behavior does not appear to be atypical. Third, Kinsbourne also believes that asymmetry is the base condition from which the symmetrical evolves -- pressure to grab from left or right or to defend from left or right, caused the evolution of relative muscular and skeletal symmetry (1978, p. 560). However, the idea that the weight of the liver and lung shift the center of gravity of the body to the right (Buchanan, 1862) and thus the balance is shifted to the left, leaving the right arm free for throwing (Harris, 1980) may be a bit much.

Despite these diversions of more or less merit, we have been discussing various neuroanatomical models of a variety of species in an attempt to answer the above question(s). It might be more instructive if we were to step backward and take a different view of the situation and then see if the available evidence fits a model rather than build a model from the available evidence.

Over the years, a number of models of human evolution have been developed, but until the most recent, such as Tanner's (1981), there has been an apparent male sex bias in them. If we could find a balanced evolutionary model, balanced in terms of male and female neuroanatomical and behavioral evolutionary development and interactions, maybe we could model an encephalization "fast path."

If it is true, as Tanner suggests, that female hominids were originally predators as well as gathering animals and that the males had to keep up, that eventually the brain grew so large that the females had trouble hunting (Wilson, 1975), and if we go along with Lovejoy that the male began to take protein back to his female (1981), can we find a "fast path"? We have to realize that although Tanner suggests that humans have minimal dimorphism, there are few of us who have trouble distinguishing the sexes and that there are morphological and physiological differences today that appear to have been with us for a long time. These differences include the typically taller male, the male with better oxygen carrying capacity and the female with the greater long term energy output capability. Something had to account for these changes, and I bet that rapid encephalization went along with them.

It may be true that the average female chimpanzee has better dexterity than the male (Boesch and Boesch, 1981) when hammering on nuts, that the adult appears to be right handed (Chorazyha, 1976) and that the average female prehuman may have had better ability to strike at a leopard with a stick than her male. But striking with a stick for protection and predation may not involve the same pieces of neural tissue as does throwing. It certainly would be less efficient as a predatory weapon. Which sex originally threw might also be unimportant as those who first threw might have been selected for and the "throwing genes" given to all progeny. But at the same time, if these "genes" were involved in the autocatalytic reaction (Wilson, 1975) then the larger brain which eventually evolved would have turned hunting over to the males and the more important (in terms of total calories) gathering mainly to the females. If the Iking San women are going to carry their children on their hip for four years (Lee, 1979), these women would have a very difficult time being effective predators.

It does not seem at all difficult to hypothesize that the animal which threw the first stone reaped a benefit by increasing its own and its kin's inclusive fitness. That was at least part of the ratchet that turned "them" into "us." I also find it difficult to believe that the "end point" of this throwing was a female trait (which is not to say it did not occur) rather than a male "ended it" after her could easily have started it but a male "ended it" after her

hips "evolved" for the birth of the large brained child.

I would not want to try to build or refute any model for language production or understanding. Calvin's (1982) final paragraph has some very interesting thoughts. However, what was the force that led to its evolution? We can postulate that "throwing genes" were beneficial; were "talking"? Obviously yes. We know that chimpanzees have the cognitive skills for rudimentary language production and foresight problem solving (Tanner, 1981). I do not find it difficult to propose that as the "throwing genes" aided in the evolution of the human brain, so did intraspecific communication capabilities. Women appear to be more sensitive to nonverbal communications than men. Thus, I would not be surprised to find out that they are more sensitive to verbal and that neural evolution of language capability and use was a female trait. If we now recall that humans evolved so that dad would return to mom and not to any female (as you might expect in a dimorphic species) with his protein or gathered food, and to put in more parental effort (Trivers, 1972) than in many primates, then we have the proper situation for those kin groups with a subtle genetic advantage in both "throwing" and "talking" to out-reproduce those who do not. Thus, I believe that they were independent but interrelated steps.

Buchanan's (1962) theory appears a little silly, but his idea of asymmetry plus endless articles on right hand/left hemisphere predominance leaves me in the position of believing that left hemisphere communication (Nottebohm, 1971) and right handedness (Chorazyha, 1976) are simply "animal." The rapid encephalization took off from these points because of bipedalism, tool manufacture, etc., and those with the best tools had the higher inclusive fitness and thus rapid encephalization occurred.

I am afraid that I have taken Calvin's original theory of throwing and language origins (1981), as well as his response (1982), and broadened the scope of the discussion beyond all hopes of resolution. But as Calvin (1982) indicates, our technology is too poor to allow us to resolve the questions he originally raised by working at the neuroanatomical level with either human or nonhuman primates. Thus, I hope that by bringing another side of the issue into dim focus that we may be able to bring differing aspects and pieces of information to bear on the same issue of rapid encephalization and to eventually solve it.

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BULLETIN BOARD

The Canadian Journal of Zoology has been publishing primary research papers in the field of zoology since 1951; in particular, animal ecology, biochemistry, physiology, parasitology, systematics, evolution, behavior, morphology and ultrastructure. It is an international journal (42 percent of the papers published in 1980 were contributions from outside Canada). Editor is G.O. Mackie, Dept. Biology, University of Victoria, P.O. Box 1700, Victoria, British Columbia, Canada, V8W 2Y2.

The Journal of Comparative Ethology/Zeitschrift für Tierpsychologie publishes experimental, descriptive, theoretical and review articles on basic research. Human ethology articles are appropriate although not a main focus of the journal. Gordon Burghardt, a member of ISHE, edits the journal. His address is: Dept. of Psychology, University of Tennessee, Knoxville TN 37996.

Newly elected officers of the American Society of Primatologists are: Andrew Hendricks, President; Donald G. Lindberg, President-Elect; Anthony M. Coelho, Jr., Executive Secretary; and W. Richard Dukelow, Treasurer.

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Annual Reviews, Inc. has published two new volumes of interest. For both of these, write to Annual Reviews, Inc., 4139 El Camino Way, Palo Alto CA 94306.

The Annual Review of Psychology (1982) includes such chapters as: (1) Social psychology of intergroup relations, by Henri Tajfel; (2) Social motivation, by Janusz Reykowski; (3) Touch in primates, by Ian Darian-Smith; and Human behavior genetics, by Norman D. Henderson. Editors are Mark R. Rosenzweig and Lyman W. Porter.

The Annual Review of Anthropology (1981) includes such chapters as: (1) A critical review of models in sociobiology, by B.J. Williams; and (2) Human adaptation to arctic zones, by Emilio F. Moran. Editor for this volume is Bernard J. Siegel.

UPCOMING MEETINGS

Animal Behavior Society annual meeting. August 15-20, 1982 at University of Minnesota, Duluth. Highlights of the meeting include: (1) Keynote speaker Gerard Baerends (Rijksuniversiteit Groningen) -- The "innate" releasing mechanism" revisited and revised, (Aug 18); (2) Invited paper session on Deception, organized by Robert Mitchell (Aug 16); (3) Workshops on (a) Deception, (b) Least darkness fall: Science in religion; (c) Legislative matters relevant to animal behavior--endangered species, animal welfare, and budget concerns; (d) The use of film and videotape in documenting animal behavior. For information on the program, contact Terry Christenson, Dept. of Psychology, Tulane University, New Orleans LA 70118. Questions concerning the conference should be directed to Mitzi Doane, Dept. of Psychology, University of Minnesota, Duluth MN 55812.

Northeast Regional ABS Meeting. October 29-31, 1982 at Northeastern University in Boston. The keynote speaker will be Patrick Bateson. Scheduled symposium "The Ecology of Communication," organized by Fred Wasserman and Jim

Traniello. Suggestions for other symposia are invited. For information, write to: Martin Block, N.E. ABS Meeting, Dept. of Psychology, Northeastern University, Boston MA 02115. Advance registration is due by September 1.

VII Annual Meeting of the Societe Quebecoise pour l'Etude Biologique du Comportement (SQEBC). November 4-7, 1982 near Sherbrooke, Quebec, Canada. A symposium on "Optimizing Behavior Under Ecological Constraints: Sex and Food" is scheduled. Reports of studies on higher vertebrates are requested, but those on other animal groups will be considered as well. Contact: Andre Cyr, Dept. Biologique, Univ. de Sherbrooke, Sherbrooke, Quebec, Canada, J1K 2R1.

Western Society of Naturalists. December 27-30, 1982 at California State University at Los Angeles. For details, write to David H. Montgomery, Dept. of Biological Sciences, California Polytechnic State Univ., San Luis Obispo CA 93407.

American Society of Zoologists. December 27-30, 1982 in Louisville, Kentucky. ASZ is meeting jointly with five other societies, including the Animal Behavior Society. Steven Austad is organizing a symposia for the ABS and Division of Animal Behavior, ASZ on "Alternative Reproductive Tactics." Deadline for submission of presentations is August 27. Papers on the Animal Behavior symposium topic are especially encouraged. For information, write to: Mary Wiley, American Society of Zoologists, Box 2739, California Lutheran College, Thousand Oaks CA 91360.

AAAS Annual Meetings have been changed to the Memorial Day weekend (Thurs.-Tues.) beginning in 1983. The schedule for future meetings is as follows: 1983 May 26-31, Detroit; 1984 May 24-29, New York; 1985 May 23-28, Los Angeles.

Future ABS Annual Meetings are set for: 1983 June 19-24, Lewisburg, Pennsylvania; 1984 August 13-17, Cheney, Washington.

Future ASZ-ABS Meetings. ASZ and ABS will hold joint meetings in: 1984 December 27-30, in Denver; 1985, December 27-30, in Baltimore.

Meeting Reminders

XIth International Congress of Anthropological and Ethnological Sciences. August 14-25, 1983 in Quebec City, Quebec and Vancouver, British Columbia, Canada. Contact: Executive Secretary, XIth ICAS, Dept. of Anthropology and Sociology, University of British Columbia, 303 NW Marine Dr., Vancouver, B.C., Canada V6T 2B2.

18th International Ethnological Conference. August 29-September 6, 1983 in Brisbane, Australia. For details, write to: Glen McBride, Animal Behaviour Unit, University of Queensland, St. Lucia, Queensland, Australia 4067. Final date for receipt of papers and registration is November 15, 1982.

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.

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