

Human Ethology Newsletter

Editor: Glenn Weisfeld

Department of Psychology, Wayne State University, Detroit, MI 48202 USA
1-313-577-2835 (office) 577-8596 (lab), 577-2801 (messages), 577-7636 (Fax), 393-2403 (home)

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SOCIETY NEWS

Toronto Congress

By Linda Mealey, Program Chair

The twelfth ISHE Congress took place in Toronto, Ontario, Canada 3-7 August 1994. Congress organizer was Irwin Silverman, York University.

3 August opened with a session on Contemporary Environments and Behavior, chaired by Chuck Crawford, Vice-President/President-Elect. Some interesting papers were presented, especially from the European contingent. The evening brought a summary of the history of human ethology by outgoing ISHE President Irenäus Eibl-Eibesfeldt. Eibl reviewed his own massive research program, especially his films of various cultures.

4 August opened with a featured address by Michael McGuire, editor of *Ethology and Sociobiology*, on the topic of behavior as a way of selecting appropriate environments as an attempt at self-medication (reaching a preferred state). He reviewed his research on the role of serotonin in dominance behavior. Whole-blood serotonin rises in vervet monkeys to which other monkeys submit, and falls in males that observe another male copulating. Men rated high on leadership have high serotonin. Prozac, which inhibits serotonin reuptake and hence facilitates its action, causes vervets to rise in rank and reduces their threatening behavior. The rest of the day was devoted to papers on psychiatry

from an evolutionary perspective, especially depression as an involuntary subordinate strategy.

5 August began with a featured address by Karl Grammer on body language between opposite-sex strangers. Karl has taken up computer methods in earnest, and explained some of his methods of studying prolonged interactions recorded on video. His Vienna lab has developed a computer algorithm that can detect when a woman is ovulating, based on her body movements. Karl mentioned that Vienna has seven institutions devoted to ethology. The talks that day covered sexual signals and gender interactions. My favorite presentation was one by Richard Goranson and Marc Mandel which tested Desmond Morris' hypothesis that breasts mimic buttocks. Evidence pointed against the model. The authors proposed instead that mastophilia was selected for because women chose men who were attracted to breasts in the lactating state, i.e., during pregnancy and infancy. You can imagine the liveliness of the lunch discussion which followed this presentation. The day ended with a workshop in teaching human ethology.

6 August started with a session on human development. Two of the talks were on attachment, a topic of renewed interest in psychology. In a featured address, Daniel Freedman spoke on this topic also. Next, Bill Charlesworth, new ISHE President, presented an analysis of changes in the papers published by *Ethology and Sociobiology* over the past 15 years. There has been a sharp decline in observational research, which he lamented. A session on biopolitics followed, and then a methodology workshop, chaired by Nancy Segal.

7 August was devoted to a series of papers on brain and behavior. The congress concluded with the officers' business meeting; see report below.

Business Meeting

By Karl Grammer, Secretary

A plenary business meeting was held 3 August so that all the Congress participants could offer their views on the Society. Differing opinions were expressed regarding the advisability of informing the mass media of our research findings. Some saw nothing but danger--of political controversy, distortions, and inaccuracies. Others felt that publicity is necessary if the evolutionary approach is to catch on, that we should inform the media of research results of general interest. For example, a recent cover story in *Time*, by Robert Wright, was devoted to research in sociobiology and was based on his direct communication with researchers. One suggestion was that we should each take responsibility for writing to the author of any textbook, article, etc. that misrepresents evolutionary theory or research, and point out the errors of omission, commission, or distortion.

Several other points were raised. Members were urged to try to recruit student members. The importance of participation in elections was also noted. Differing opinions were expressed concerning whether or not an attempt to change the name of *Ethology and Sociobiology* is still being contemplated by the Human Behavior and Evolution Society. According to the HBES newsletter, no change is being considered at present. However, this suggests that the issue has not been abandoned entirely. Those who wish to express their views on the matter are advised to remain vigilant. The decision rests with the publisher, so input from readers of and contributors to the journal presumably is in order.

The officers' business meeting was held on 7 August. All six of the current officers attended, plus President Emeritus Eibl-Eibesfeldt and former Secretary Gail Zivin.

The officers were alarmed by the

decline in studies of direct observation of human behavior, as evidenced by Bill Charlesworth's analysis of recent scientific publications in our field. This development makes at least some countermeasures necessary in order to promote a more ethological approach in the human sciences.

The first change is that we decided to rename the newsletter the *Human Ethology Bulletin*, starting in 1995. The publication has gone beyond the stage of a societal newsletter. The new name may make it more visible, as by making it easier to get libraries to subscribe. For the time being, the *Bulletin* will focus on abstracts and reviews of current journal articles and books, and discussion of current research and theoretical issues and ideas. For now, it will not accept original, full-length research reports, because no system for peer review yet exists. However, short communications are still welcome. Glenn Weisfeld was appointed to another two-year term as editor, and was authorized to spend more money on the newsletter if necessary.

The second change concerns the Society's policy toward student members. The next congresses will offer special rates for students, and a young investigator's award. Gail Zivin will chair the committee for the award, and specify the guidelines for submissions.

Nancy Segal was asked to investigate possibilities of publishing notices about the Society and its newsletter in journals and other suitable places.

Bill Charlesworth was asked to revise the ISHE statement of purpose that appears on the application form for membership. He will also assume responsibility for representing the Society to other organizations.

At the plenary session, the members endorsed a small increase in dues. The officers decided on an increase to \$25 for one year, \$60 for three, effective 1 January 1995. This is the first increase since 1990, when the newsletter ran 12 pages. Student rates will remain unchanged: \$10 and \$25, respectively. Karl Grammer offered to help in notifying delinquent members that their membership needs to be renewed.

The next ISHE Congress will take place in Vienna, Austria early in August 1996 and will be organized by the Ludwig Boltzmann Institute for Urban Ethology. Besides the normal program, there will be special sessions on the direct observation of behavior in gender advertisement, mate selection, nonverbal communication, ontogeny, and human-environment interactions. Special addresses will cover evolution and cognition, behavioral genetics, and hormonal control of human behavior. In addition, several workshops are planned on epistemological issues in ethology and sociobiology, digital image processing, computer aided methods for recording behavior, and advanced methods of behavior analysis. The venue for the 1998 meeting has not yet been decided.

Membership Directory

Nancy Segal, Membership Chair, has agreed to prepare the 1995 Membership Directory. This is the last call for submission of information. If you have not yet sent in your completed form, you can find the form on the penultimate page of each of the last three newsletters. Please submit the form **directly to Nancy**; her address is listed in the Officers' box in each newsletter. Be sure your membership dues are up to date. If you do not have one of these past issues handy, consult the current Directory for the information requested.

Back Issues of Newsletter

Copies of past issues of the newsletter are available for distribution to students and other prospective members. Contact the editor.

Election of Officers

The accompanying ballot, or a copy, should be completed and submitted by **15 December 1994** to our Treasurer, Barbara Fuller. It is important to the health and morale of the Society that people vote in our elections.

BALLOT

Secretary

Karl Grammer,
Institute for Human Biology, Vienna

Other

Membership Chair

Nancy Segal,
California State University, Fullerton

Other

Please mail completed ballot or copy by 15 December 1994 to: Barbara Fuller, ISHE Treasurer, University of Colorado, 4200 E. Ninth Ave., Denver, CO 80220 USA.

From the Editor

Allow me to mention a few matters concerning the newsletter.

(1) If you attend the convention of some other related society, please send me a report on the proceedings. To avoid possible duplication, you might confer with other ISHE members in attendance and decide who will prepare the report, and then inform me of who will do so. Such a report might focus on the tenor and general features of the meeting, or it might highlight notable presentations.

(2) If you submit a book review, please list any references in full at the end, and include the publisher's mailing address and the price of the book.

(3) For all submissions, please try to use Microsoft Word; I have a new computer. IBM disks can be converted but they cause a delay. Be sure to include a paper copy too.

(4) As a Forum topic, let's try "Suggestions for Nurturing Students." The Toronto workshop on teaching seemed to be enthusiastically received, so perhaps we should discuss the topic in the newsletter. How can students be recruited among undergraduates? Should specific ethology courses be developed; if so, how? Alternatively, can students be introduced to ethology through other behavioral sciences courses? How can graduate students learn about observational methods and be helped to formulate hypotheses based on evolutionary theory? What can we do to ensure that universities will perceive a need for human ethologists on their faculties? What other problems need to be addressed; if so, how? It would be good to hear from members in different countries, as educational systems vary.

Lastly, let me take this opportunity to thank the many members who have contributed to the newsletter over the last four years, allowing it to expand. I especially thank Linda Mealey, the North American Book Review Editor, for the great amount of work she has done in writing, soliciting, arranging, and editing reviews. She has also acted as an important liaison to HBES, and of course served as Program Chair for the Toronto meeting. In addition, I hope that everyone realizes that the Current Literature section is prepared each month by Bob Adams and his staff at Eastern Kentucky University. This unique feature draws frequent praise for which Bob deserves the credit, and not I.

Membership Renewals for 1995

It is time to renew your membership for 1995 if you have not already done so. Membership is by calendar year, so dues are to be paid by the first of the year. **If the date on your mailing label is earlier than the current year, it is time to renew your membership.** For economic reasons, renewal notices are not sent. Those who do not renew their memberships will be removed from the membership list. Please report any errors, change of address, etc. to the editor. Current dues and directions for payment are given on the last page.

Editorial Staff

Associate Editor

Laurence I. Stettner
Department of Psychology
Wayne State University
Detroit, MI 48202 USA
tel. 1-313-577-2866

Current Literature Editor

Robert M. Adams
Department of Psychology
Eastern Kentucky University
Richmond, KY 40475-0937 USA

American Book Review Editor

Linda Mealey
Department of Psychology
St. John's University
Collegeville, MN 56321 USA
tel. 1-612-363-3135
fax 1-612-363-3202
E-mail LMEALEY@CSBSJU.EDU

British Book Review Editor

A. Stuart Laws
Department of Psychology
The University
Newcastle upon Tyne
NE1 7RU, England
tel. 44-91-222-6000
fax 44-91-261-1182
Telex 53654 (UNINNEW G)

French Book Review Editor

Peter LaFrenière
Université de Montréal
C. P. 6128, succursale A,
Montréal, Québec H3C 3J7, Canada

German Book Review Editor

Karl Grammer
Ludwig Boltzmann Institute for
Urban Ethology/Human Biology
Althanstrasse 14
A-1090 Vienna, Austria

Spanish and Portuguese Book Review Editor

Eduardo Gudynas
c/o ASMER Regional Office
Casilla Correo 13125
Montevideo, Uruguay

ARTICLE

The Nudity Taboo-- Uncommon Sense in Human Communication?

By Wolfgang M. Schleidt, Robert-Hamerlingg,
1/16 A-1150 Wien, AUSTRIA

This is a highly condensed version of my attempt to trace the evolution of *human preverbal* communication. It deals especially with male expression of well-being, status, sexual desire, aggressive intent and shame through the state of the penis, as a spectacular signalling device, and relates all this to the various nudity taboos of "civilized mankind". A first overview in German has recently appeared (Schleidt 1994). Currently I am working--mainly in libraries and museums--on an extended version of this topic for a book in English.

The central hypothesis is that the Western (Judaic-Christian-Islamic) nudity taboo--namely, prohibitions regarding exposure of genitalia, breasts, buttocks, face and/or scalp--originally had little to do with reproduction, aggression, seduction, impending fornication or even the undeserved enjoyment of sexuality. Instead, the nudity taboo has its origin as a communal symbol of submission to a dominant authority, especially as a symbol of the power of this authority to control individual behavior in public, and--within the domain of religion--also in private. This hypothesis is consistent with the connection between accepting or challenging a higher authority and the awareness of corporal nudity, as portrayed in Genesis 3,1-24. Viewed from a different angle, my hypotheses concern the connection between an involuntary sign of diminished status (face blushed and penis shrunk with shame, face turned pale and penis shrunk in terror, etc.) and the voluntary act of covering this signals (by hands, fig leaves, etc.) as confession of personal guilt.

One set of significant collateral

evidence concerns the hypothetical origins of specific human features of facial and penile displays. This includes hypotheses concerning not only the obvious sex dimorphism within this preverbal communication system and its ontogeny (often punctuated by rites of puberty), but also the considerable range of variability in both customs and biological features in different cultures. I must emphasize that I am not advocating a biologicistic "animal heritage" of *human* phylogeny; rather, I point out the need to formulate hypotheses as to the origin of specific *human* features of morphology, physiology and behavior and their use in communication within our very own *human prehistory*. A cluster of related features occurs at the beginning of the human species including upright gait; manual dexterity; increased brain size and, by inference, increased cognitive ability; permanent estrus with striking changes in female features, notably buttocks and breasts; a dramatic change in the pattern of body hair, including specializations in different regions of the body (notably head, face, axillary and pubic region, with marked age and sex-specific differences); new patterns of skin pigmentation (e.g., nipples, linea nigra); a pendulous penis, conspicuous in size and motility; and new features in facial expression, including both movement and color change, i.e., paling and blushing. I want to emphasize that all this not only predates the development of verbal language by several hundred thousand years but also must have included considerable improvement in nonverbal communication skills prior to and during the Paleolithic. Since humans in this pre-agricultural phase were living in small bands of highly interdependent individuals, I assume that the special communication signals of this phase (penile motility, facial expression movements, facial color change) were "honest" indicators (in the sense of Zahavi) of motivational states.

It is reasonable to assume that the Neolithic revolution, with the invention and development of herding, growing field crops and personal ownership of resources (and presumably a revolutionary change in the complexity of verbal communication skills), had a considerable influence on the nonverbal communication system. These changes are likely to have affected the obvious sex

dimorphism in the expression of shame: blushing primarily in females (blushing and paling are largely hidden by a beard) and penile reduction exclusively in males. Such differences, as well as sex differences in the division of agricultural labor, in the extent to which individuals had contact with strangers or engaged in organized warfare, are likely to have introduced a sex bias in early verbal communication. I can't see any good reason why the Neolithic revolution per se, or the subsequent extension of awareness of place (our garden, our pastures) and time (awaiting the growth of crops and livestock) should have required a higher degree of genital modesty or a greater need for deception. Acknowledged status, however, did find its expression in many populations within the ancient coding system of genital pride up to colonial times and even today through emphasizing or exaggerating penis size by all kinds of additions or manipulations including prepuce covers, penis sheaths, circumcision, or tying the penis up to the waist (thus preventing its retraction).

The rise of urban civilizations, composed of large groups no longer tied by personal acquaintance or kinship but associated through subordination under a superior power, once more revolutionized human nonverbal communication. Among urbanites, penile expression movements became obsolete, having been rendered invisible by the splendor of clothing and corpulence of the elders, and deemed an unseemly display in young, obstinate, exhibitionistic males. Facial expressions, however, have changed in a variety of ways under the influence of high population densities in different cultures. The Western civilized male face gains, by shaving the beard, additional movement patterns which in turn can become restrained in a poker face. In the Far East, very likely related to the scarcity of male facial hair, different traditions in facial expression movements within the public domain developed, with emphasis on not revealing emotions.

What, according to our Western views, now constitutes female modesty, namely covering at least the pubic escutcheon and breasts, has an origin completely different from that discussed for the male. Female concerns about these areas are most likely

related primarily to advertising of nutritional and reproductive states. Menstruation, with its conspicuous display of blood, is best understood, again, as "honest communication", on which various cultures have improved by a generous helping of ochre and/or hematite (used even by postmenopausal women). The various forms of miniskirts and mini-loincloths common in many precolonial cultures, which cover female genitalia from frontal view and which are traditionally interpreted by pious travelers and missionaries as signs of rudimentary modesty, can be interpreted equally well as "pubic hair toupees". Striae, caused by overstretching of belly and thigh skin during pregnancy, may be viewed as a "sign of proven performance". This may also apply to the linea nigra, which develops in many women during pregnancy and fades away afterward, but can remain visible as a "permanent record of past breeding success", e.g., in some African populations. It remains unclear what women could be "ashamed" of, other than "disobedience" to a domineering male "authority". Currently, I think the best guess is that female modesty was forced on females by males as part of new roles in some agricultural economies, again starting at the earliest within the Neolithic revolution.

The strong link hypothesized between nudity taboo, penile motility and (male) dominance may explain the magnitude of the Western bias in even discussing this topic in the scientific literature. Nevertheless, I wonder why no one before me has come up with these hypotheses I find so much a matter of common sense. Therefore, I would greatly appreciate receiving any commentaries (which I will be delighted to acknowledge as personal communication, if the planned book finds a publisher) and especially any reprints of relevant papers. This would--through discussing the ideas in my book and listing the titles among my references--add to their immortality in the Citation Index, provided, of course, that I can agree or disagree sufficiently about the relevance to my topic.

Schleidt, W. M. (1994). *Imponierende Uniformen: Kleidung als Signal*. In: *Kulturethologie-Gedenkschrift für Otto Koenig* (M. Liedtke, Hg.). Wien: Peter Lang Verlag.

BOOK REVIEWS

The Biology of the Naked Mole Rat

Edited by P.W. Sherman, J.U.M. Jarvis, & R.D. Alexander. Princeton University Press, 1445 Lower Ferry Rd., Ewing, NJ 08618 USA, 1991. 500pp, pb.

Reviewed by Linda Mealey, Psychology Department, College of St. Benedict, Collegetown, MN 56321 USA.

Naked mole rats are as ugly as their name implies. Commonly described as walking (or more accurately, scurrying) eggrolls, naked mole rats are hairless pink rodents with wrinkly skin; beady, vestigial eyes; and long, protruding upper and lower incisors. Why would people devote a 500-page monograph, let alone their life, to studying these bizarre creatures?

Actually, mole rats are fascinating for a slew of reasons, and study of them is important for a wide range of disciplines, most especially ethology and sociobiology. A hot item now at many zoos and research institutes, naked mole rats are best known as the only example of eusociality in mammals. Naked mole rats, like many bees, ants, wasps and termites, live in colonies consisting of a single governing 'queen', a small number of male consorts, and a large number of sterile female and fertile, but non-breeding, male workers, each 'assigned' to a particular role or task within the colony. Like most colonial ants, mole rat colonies live in a subterranean network of tunnels and chambers. The workers dig the tunnels, sweep debris from passageways, collect food (mostly roots and tubers), monitor intrusions, and help tend to the queen and her young which inhabit one or two breeding chambers.

Like all other mammals, worker mole rats are diploid and, in fact, each male and female is potentially fertile. Male workers produce sperm and are not physiologically different from breeding males; apparently, breeders are selected by the queen. The queen inhibits the reproductive physiology of other females with a combination of chemical and behavioral suppressors still not completely understood.

When a queen dies, several worker females engage in what is often an extended competition involving physical and psychological dominance struggles until, eventually, a single new queen emerges. When a new queen ascends, she undergoes a physical transformation, actually lengthening her vertebrae to extend her body to be able to accommodate a pregnancy of 5-15 pups (more in captivity).

How queens suppress reproductive activity and elicit cooperation from the non-reproductive animals is clearly one of the most interesting questions arising from studies of the naked mole rat. One possibly important factor is that mole rat colonies are highly inbred, thus making reproductive altruism not as costly for the individual as in outbred species. Still, very few of these 'helpers-at-the-nest' ever get to reproduce themselves; why do they stay to help take care of their siblings, nieces, and nephews when they could tunnel off and begin their own family (artificially isolated male-female pairs can and do breed)?

Other interesting questions abound: What determines which animals take on what roles in the colony? What determines who becomes queen? Why are naked mole rats, unlike the other mole rat genera, eusocial in the first place? How different and how isolated are colonies from one another? How does inter-colonial aggression differ from intra-colonial aggression? How and when do whole colonies spread or reproduce? Might study of mole rats, like that of other social mammals, shed some light on complex human interactions?

In addition to their social and behavioral anomalies, naked mole rats exhibit anatomical and physiological anomalies that are of interest. Hairlessness, for example, is an unusual trait in mammals. Why is this species naked? Is their nakedness related to altriciality, to neoteny or, as has been hypothesized for us, the 'naked ape', to self-domestication? Naked mole rats also have fairly rudimentary thermoregulatory abilities, a characteristic they share with the primitive prototheria, or egg-laying mammals. Do they also share with that primitive group a lack of REM sleep? And, with their underground life style and vestigial eyes, naked mole rats have come to rely not only on auditory and chemical communication, but also on tactile communication. What is the perceptual

'Umwelt' of this species? How do all of these anomalies interrelate?

The Biology of the Naked Mole Rat addresses many of these questions. It is clear why this intriguing little animal has excited so many researchers and why this book was included as one of The Princeton University Press series Monographs in Behavior and Ecology. [The series also includes (in order of publication): John Terborgh's 'Five New World Primates', Robin Dunbar's 'Reproductive Decisions', Tom Seeley's 'Honeybee Ecology', David Stephens' and John Krebs' 'Foraging Theory', Jerram Brown's 'Helping and Communal Breeding in Birds', Marc Mangelin and Colin Clark's 'Dynamic Modeling in Behavioral Ecology', and Tim Clutton-Brock's 'The Evolution of Parental Care'.]

This monograph includes much of interest to ethologists as well as to ecologists and sociobiologists, including a chapter each devoted solely to description of the vocal (Chapter 9) and non-vocal (Chapter 8) ethograms of this species. All chapters include excellent, informative charts, drawings, and b&w photographs; a plate of six color glossies is inserted in the front. An appendix even discusses methods for capturing, transporting, and maintaining naked mole rats in captivity--an increasingly common endeavor.

The editors have put together a very nice collection, with chapters that can be read in order or individually. Coverage is both broad and deep, and there is none of the repetition and disjointedness that often appears when chapters have different authors. There is much to be learned here, not just about mole rats, but about how to put together a comprehensive investigation of a species, and a comprehensive summary of a body of research.

Note: For those who are interested in this species and read Dutch, ISHE member Frans Roes has also recently published a book on naked mole rats: *De Naakte Molrat. Over mensen, dieren en evolutie*. Amsterdam: Prometheus, 1993, f19.90.

Be sure to cast your ballot.

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Institute of Child Development
51 East River Road
University of Minnesota
Minneapolis, MN 55455 USA

Vice-President/ President-Elect

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Department of Psychology
Simon Fraser University
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Department of Psychology
Wayne State University
Detroit, MI 48202 USA

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Althanstrasse 14
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Treasurer

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University of Colorado
4200 E. Ninth Ave.
Denver, CO 80220 USA

Membership Chair

Nancy L. Segal
Department of Psychology
California State University
Fullerton, CA 92634 USA

**Renew your membership before 1
January and avoid the rise in dues.**

Telling Lies: Clues to Deceit in the Marketplace, Politics and Marriage

By Paul Ekman. W.W. Norton & Company, Inc., 500 Fifth Avenue, New York, N.Y. 10110, USA. 2nd ed., 1992, \$11.95, 366 pages.

Reviewed by Carsten Herrmann-Pillath, Department of Economics, Gerhard Mercator University of Duisburg, Lotharstrasse 65, 47057 Duisburg, Germany.

This is a review by a complete layman who was attracted to reading the book because as an economist (who is much concerned with the cultural background of economic behavior) I am interested in the question of deceit in the marketplace. Furthermore, since economics has included topics such as the family and political behavior in its broadest area of application, deceit in marriage and politics seemed of importance, too. The book, however, does not adopt the economist's approach to such issues but the psychologist's, and the empirical results it reports rely heavily on experiments and empirical testing. Yet the book is directed not at a specialized audience but at the general reader. Even more explicitly, sometimes it adopts the character of being a practical everyman's guide to detecting deceit. It is because of this approach that I dare to review the book.

To start with a general point, the mere fact of writing and publishing a book on the topic of detecting lies seems to be of particular salience for those interested in human behavior: Why did the author undertake such an effort? As a German, I felt really astonished about the serious concern about lies prevalent in US culture which is reflected by the book itself as well as by its contents. For instance, learning that about half of all McDonalds outlets in the US check their new personnel by using the polygraph was a real surprise (p. 193). It became immediately obvious to me that the general approach towards lies which is reflected by the book might indeed be culturally unique for the US, given the fact that I am very sure that using the polygraph, for instance, in the German public service would produce a public outcry about the government's intrusiveness into private matters. So the book itself may be

regarded as an object for further research into the normative framework governing attitudes towards lies. This is a topic of great importance to the economist since in recent research on institutions and business culture the problem of "trust" has come to the fore. In light of Ekman's book, one understands why in those efforts the US has been classified as a "low trust" culture (see Casson, 1991).

The problem of cultural differences does not play a role in Ekman's argument, aside from the fact that the incentives for lying or the chances for successfully managing deceit might be greater if the people involved belonged to different cultures (p. 67). Indeed, the book is mainly an empirical analysis of lying and a set of recipes for detecting lies. It contains ten chapters. The first six provide the backbone of the argument, including a definition of lying, a survey of possible failures in lying and, most importantly, a broad discussion of the different "clues to deceit" emanating from the liar's behavior. This survey of, for example, the "facial microexpressions" that distinguish the liar's behavior from the truthful one's may be the most interesting passages for the ethologist. One of the fascinating results of this survey is the fact that people do not seem to pay much attention to those behavioral features that contain the most relevant information on lying (p. 81).

The last four chapters contain more practical considerations, including a critical evaluation of the polygraph as a device in detecting lies, as well as a consideration of the importance of lie-checking in politics and public life. Basically, the practical message of the book is mixed (pp. 162-189): Although there are some reliable indicators of deceit (e.g., the fact that false smiles are more asymmetrical than true ones), those are difficult to discern. Furthermore, there is a serious problem in identifying the true emotional cause of an apparent incongruence between behavior and statement: What might be regarded as a hint at lying (for instance, signs of arousal which do not fit the statement, or changes in the rhythm of speech) can be caused by completely different reasons, including (as when applying the polygraph) uneasiness with the fact that one is mistrusted.

Hence, as in the case of the polygraph

(pp. 203ff) Ekman's approach leads to a very interesting methodological issue which might be called a "principle of behavioral indeterminacy": if behavioral scientists continue to develop further techniques for detecting true causes of behavior, the subjects under scrutiny may produce misleading behavioral clues precisely because they know about the ambivalent results of behavioral science. Hence, the application of Ekman's recipes will suffer from the same problem as the application of the polygraph: Being under scrutiny, my knowledge about the complexity and, presumably shaky basis of the methods might give rise to behavioral clues which are then misinterpreted by the researcher. Presumably, this puts a basic limit to the success of optimal checking procedures.

Ekman starts out with a very broad definition of lying which includes concealment (p. 28). This means that any kind of behavior is designated as "lying" which (a) causes the other side to adopt objectively wrong views of reality, (b) has not been announced to the other side explicitly, and (c) takes place without the consent of the other side. This broad concept of a lie is sometimes difficult to apply because there are, of course, lies that are accepted by the addressee without having been explicitly asked for consent. Ekman quotes actors as being professionals whose performance cannot be taken as a lie because of the fact that producing objectively false statements is their business. The same is true of poker players, for example. However, beyond those examples Ekman himself seems to fall into some traps of his rigorously broad definition. For instance, although he speaks about price haggling as being conducted through "authorized lies" (which means that everybody expects the other to "lie", as in poker), he continues to call this behavior lying, although we may assume that, in principle, falsifying or concealment as such has been consented to by all sides (p. 70). The same is true for his treatment of the Cuban missile crisis and the Gromyko-Kennedy meeting where we might rightfully imagine that nobody expected the other side to tell the truth (p. 268).

The problem of broad definitions can be linked back to the basic question of a culturally specific attitude towards lying. For instance, it has been argued that Chinese culture may invest the individual with the

capability of accepting a much larger difference between statement and reality than in Western culture (see, *inter alia*, Stover, 1974:242ff). Hence, a statement that is said to be "lie" in a Western cultural context might be regarded as quite normal within a Chinese one. This observation raises the point of whether Ekman is right in "demoralizing" the issue of lies by referring to them simply as objectively false statements. Is an advertisement a lie? Perhaps the answer cannot be given without referring to the general problem of legitimacy and authorization--which is a moral one, and which links emotions, behavior and perception of behavior by oneself and by others.

Treating lies as an object of natural science methodology seems to be a direct result of the US low-trust culture, which even urges people to rely on mechanical devices in order to get the truth--even though, according to Ekman, the scientific basis for putting such "trust" into a machine is shaky. So, why not trust the people instead of the machine? The same might be said about the impressive attempt by Ekman to provide "reliable" rules for detecting lies. The whole endeavour seems to be haunted by problems of definition and by the general difficulties in designing experimental settings that make behavioral science insights possible at all (on the difficulties in experimental design, see, for example, pp. 55ff).

I stress these points only because Ekman's book is directed at the reader who presumably wants to get advice on detecting lies, and not primarily at the ethologist. For me, there are many important practical issues raised by the existence of lies that are not discussed in the book (although mentioned, pp. 327ff). Many lies belong to the fabric of human social existence and might even be a precondition for sustaining overall trust. Hence, there is a real danger that the attempt to detect lies with maximum precision might paradoxically destroy general trust in a small group or in a society. Furthermore, as implied in Ray Rappoport's famous expression regarding the "evolutionary significance of lies," this raises the basic question for the ultimate standard of truth. If lies belong at least partly to the fabric of society, this could be the main explanation why people have not evolved to be perfect "lie detectors". Instead,

Ekman's data show that we are poor lie detectors, although, presumably, not "natural liars" as well. The evolutionary arms race in techniques of deceit seems to have abated long ago in human phylogeny--perhaps because there is no benefit in detecting all those clues to deceit. A certain level of deceit in a society can be the precondition for the preservation of trust, and trust is the precondition for smooth, efficient and harmonious social interaction. This is a paradox facing any standard of truth involved in defining lies (cf. my 1993 article, cited below).

But in conclusion, let me retreat from those philosophical considerations. The first edition of Ekman's book has been rightly praised as being a very valuable survey of empirical research into lying. But I feel that because of its basic methodological problems its intended audience might not be the most ideal one, namely the general reader or the professional who wants to become a well-trained lie-catcher. I would like to recommend the book as a wonderful introductory text for students of human behavior (psychologists, ethologists, anthropologists, etc.) who want to learn about the empirical methods and problems in designing experiments in which researchers try to look into the people's minds by means of observation, in particular in the laboratory. The seriousness and comprehensive approach of Ekman's empirical work be exemplary. Students who are curious about the details of that work might be stimulated to delve into Ekman's original contributions which have been published in journals such as *Psychophysiology*, *Ethology and Sociobiology*, and *Child Development*, to mention just a few.

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Chimpanzee Material Culture: Implications for Human Evolution

By W. C. McGrew. Cambridge University Press, Pitt Building, Trumpington Street, CB2 1RP, U. K.; 40 W. 20th St., New York, NY 10011 USA, 1992, £40 /\$79.95 (hdbk.), £16.95/\$29.95 (ppr.).

Reviewed by Roger D. Masters, Dept. of Government, Dartmouth College, Silsby Hall, Hanover, NH 03755 USA.

It is something of a scandal that Bill McGrew's *Chimpanzee Material Culture* is not widely known. The book is of crucial importance not only for human ethologists, but for philosophers and anthropologists (whose definitions of what is "human" may be very much challenged by McGrew's painstaking evidence and analysis). Perhaps it is the very thoroughness of the reconceptualization required in the social sciences that accounts for the silence--I'm tempted to say, the silence of fear--that surrounds this study.

These remarks may sound highly emotional, but perhaps a little outrage is in order. As with Alexandra Maryanski and Jonathan Turner's *The Social Cage*, a work with powerful implications for our basic self-understanding has been so widely ignored that one is tempted to explain the behavior in terms of self-deception rather than scholarship. Hopefully this brief description of McGrew's book will help reverse the undeserved neglect.

What McGrew has done is elegant and long overdue: a detailed comparison of some 24 African populations of chimpanzees, using the techniques of traditional human anthropology to analyze the chimp "tool kit" and behavioral adaptations. His own works are the best summary: "Chimpanzee meet the criteria of culture as originally defined for human beings by socio-cultural anthropologists. They show sex differences in using tools to obtain and to process a variety of plant and animal foods. The technological gap between chimpanzees and human societies that live by foraging (hunter-gatherers) is surprisingly narrow, at least for food-getting. Different communities of wild chimpanzees have different tool-kits, and

not all of this regional and local variation can be explained by the demands of the physical and biotic environments in which they live. Some differences are likely to be customs based on socially derived and symbolically encoded traditions" (p. i).

Most discussions of the similarities and differences between humans and chimpanzees have focused on individual behaviors. After describing the problems and the methods used in the first two chapters, McGrew compares tool-using among chimpanzees to other primate species (chapter 3). Challenging the anthropologists who define culture as a human attribute, McGrew then presents good behavioral evidence for cultural differences between chimpanzee groups (chapter 4). Chapter 5 focuses on sex differences, particularly in tool use and diet; chapter 6 compares chimpanzees and human hunter-gatherers (contrasting Tasmanian aborigines and Tanzanian chimpanzees); chapter 7 compares diet and tool use in different chimp populations; and chapter 8 looks at tool-kits, weapons use, and techniques ("chimpanzee ethnology"). The last two chapters then draw conclusions, first emphasizing the interest in considering the problem and utility of using chimpanzees as "models" in the study of human evolution (chapter 9) and then considering "what chimpanzees are, are not, and might be" (chapter 10).

This summary should give a brief idea of the daring and importance of McGrew's book. On the one hand, he demystifies our primate relatives, showing that "there is no such creature as 'The Chimpanzee'" (p. 150). This perspective can be used to clarify hominid evolution since, as he puts it (p. 199), "Chimpanzees and modern humans can be thought of as the respective endpoints that define gaps ranging from narrow or non-existent (visual acuity), through bridgeable (good-getting), to wide (written language)." Unlike guesses about early hominids, therefore, studies of the variability found among living chimpanzees give us a sharper view of the likely range of possibility ecological and behavioral characteristics of our ancestors.

The conclusion is nuanced and telling: "Chimpanzees do not have human culture,

material or otherwise.... Yet much of what chimpanzees do is so close to human that the two are indistinguishable. Some artifacts would be unattributable to species if they lost their museum labels. This similarity is of more than academic interest, for it is the best available source of knowledge about our behavioral evolutionary past. If we wish to reconstruct the prehistoric origins of human technology, then we need to use the available acts of the creatures with whom we last shared a common ancestor. Our hominid predecessors are irretrievably gone, but our hominoid cousins (just) survive. What a pity it would be to extinguish them before they could tell us all that they know" (p. 230).

In short, this one is well worth reading -- and adding to your library.

Ethology and Human Development

By John Archer. Harvester/Wheatsheaf, Campus 400, Maylands Avenue, Hemel Hempsted, Hertfordshire, HP2 7EZ, U. K.; c/o Simon & Schuster, 200 Old Tappan Rd., Old Tappan, NJ 07675 USA, 1992.

Reviewed by George Kocan, 27 W 179 Galusha Rd., Warrenville, IL 60555, USA.

John Archer takes two seemingly disparate subjects, ethology and development, and demonstrates how they not only are compatible but also are, in the final analysis, inseparable. He discusses old controversies and relates them to new ones. Thus, he reviews a broad spectrum of subject matter and brings it up to date.

For example, I found his treatment of the subject of imprinting particularly rewarding. Konrad Lorenz relied on this concept to explain following behavior in geese. During a critical period soon after hatching, goslings will tend to follow any large object that happens to be near. Under normal circumstances this, of course, is the mother goose. In his pioneering work on imprinting, Lorenz was able to imprint goslings on himself, so that they would accept him as their mother.

Eventually, the concept found other applications. Some birds acquire their species-typical song when they are exposed to it at the appropriate age. However, in the case of bird song the critical period has some flexibility. If the song is not right or something else is amiss in the environment, the developing organism has an extended period of time in which to receive the correct pattern of stimuli. In other words, the critical period concept evolved into a sensitive period.

The sensitive period idea ended up having relevance to at least two other areas concerning animal behavior: attachment and sexual identity. John Bowlby, working with children, and Harry Harlow, working with rhesus monkeys, extended the imprinting concept to primates, where it became 'attachment.' This represented a radical departure from the regnant academic ideology-behaviorism. Bowlby became impressed with the fact that infants fail to thrive when they do not have a mother to whom to attach. And Harlow demonstrated that the infant monkey's need to cling to a mother is independent of any reinforcement provided by feeding.

Since some birds learn to recognize the appearance of their mates through imprinting, the concept may have relevance to the issue of gender identity in humans, especially the curious existence of homosexuality. A possible hypothesis, according to Archer, is negative imprinting. The literature has numerous studies which show (and which support the common observation) that boys prefer the same sex as playmates--yet they seek out girls as sex mates at puberty. The evidence would seem to suggest that male homosexuals' preference for the same sex results from an excessive association with girls while young. The first sex experience, with another male, further consolidates the sex preference, thus making it resistant to change.

This is a variation on the Westermarck Hypothesis which explains why brothers and sisters, for example, do not normally find each other sexually attractive and suitable for marriage. Children which have grown up together, for instance, in a mixed-sex kibbutz, tend not to marry each other.

Archer also considers the idea that gender identity, which is established during a flexible, sensitive period, may not be

irreversible. He suggests that Money's surgical treatment of transsexuals was perhaps premature, based as it was on the more rigid "critical period" concept.

In a similar creative and incisive manner, Archer discusses aggression, play, dominance, polygyny, father absence, fixed action patterns, releasers, and cognition. He reports, for example, that Blurton-Jones questioned the results of the famous Bobo doll experiment showing a child beating a doll in imitation of an adult. The child's facial expression suggests that this was an example of rough and tumble play and not aggression. The reader, furthermore, learns that animals deprived of rough and tumble play exhibit deficits in behavior as adults.

Archer notes that where the female is able to rear young without the male's assistance, accentuated inter-male competition will occur. This will be associated with costly and risky male strategies, and the evolution of polygyny. He cites Low's prediction that the more polygynous the society, the more boys would be taught to strive and compete.

Cross-cultural research also suggests

Newsletter Submissions

Anything that might be of interest to ISHE members is welcome: Society Matters; articles; replies to articles; suggestions; announcements of meetings, journals or professional societies; etc. These sorts of submission should be sent to the editor. Book review inquiries should go to the appropriate book review editor (the British editor covers English-language books published in Europe). Submission should be in English, on paper and, if possible, on diskette. Please include complete references for all publications cited. For book reviews, please include publisher's mailing address and the price of hardback and paperback editions.

No material in the newsletter is selected by critical peer review and thus material is presented only to foster free and creative exchange of (even outrageous) ideas between scholars. The fact that material appears in the newsletter never implies the truth of those ideas, ISHE's endorsement of them, or support for any policy implications that might be inferred from them.

that father absence results in early and intense nurturance, and rearing by peers. In males, this leads to competition and aggressive displays, antagonism to females, male dominance and female subordination. 'Machismo' becomes a predominant value. Conversely, father presence in a child's early years is associated with less competition, stable relationships, and a high level of parental involvement.

The Scented Ape: The Biology and Culture of Human Odor

By D. Michael Stoddart. Cambridge University Press, Pitt Building, Trumpington Street, Cambridge CB2 1RP, U. K.; 40 W. 20th St., New York, NY 10011, USA, 1990, \$89.95 (hdbk.), \$29.95 (ppr.).

Reviewed by Hector Qirko, Department of Anthropology, University of Tennessee, 225 South Stadium Hall, Knoxville, TN 37996 USA.

This is a fascinating and informative book that attempts to cross the "epistemological boundaries," as the author would put it, separating biology, social science and art with regard to human olfaction. Stoddart argues that early hominid olfaction was attenuated in order to protect the pair bond from the competitive pressures of social living. Axillary and pubic scent organs had developed to replace long-distance, estrus-advertising cues with coitus-related "rewards." The use of odors of plants, and later of perfumes and incense (which typically contain sexual attractants of other species) arose to mask human sexual odors and persists because it stimulates unconscious, only indirectly sexual, human interest.

Chapter 1 introduces the reader to the obvious contradiction in human olfaction: While possessing a reasonably discriminating olfactory system, we generally dislike, avoid, and disguise that which it was presumably 2-4 present the foundations for Stoddart's argument through comparative physiology. He establishes the relationship between olfaction and reproduction in other species, the existence of a highly developed scent-producing system in human beings, and the connection of that system to reproduction. These chapters are technical in nature and the author provides handy summaries for nonbiologists. However, I (a

nonbiologist) found the chapters easy to follow and, ironically, better written than the rest of the book, which suffers from occasionally awkward sentence structure and confusing punctuation.

With Chapter 5, Stoddart begins to widen his perspective in his examination of human olfaction. He makes the interesting point that humans tend to regard as clean and healthy that which is, in fact, perfumed in some way. Also, he cites poets' analogies of human sexual odors and perfume to suggest that it is in the aesthetic realm that the connection between the two is best, if unconsciously, understood. Another interesting point is that humans, perhaps more than other mammals and reptiles, are born with a great deal of flexibility with regard to odor response; this seems at odds with the limited number of ingredients humans apparently choose from when concocting scents.

Chapters 6 and 7 deal with the use of perfume and incense respectively, and it is here that Stoddart most often begs what to me are relevant questions. While his historical overview of the use of nonhuman scents is informative and useful, there is no attempt to identify patterning other than that which characterizes the origins and properties of the compounds themselves. For example, we learn of the use of perfume and incense by macro-cultures such as the Romans, Arabs, Persians. However, it is likely that status is an important driver in the manipulation of scents by humans within groups--this is certainly the case with respect to visual cultural traits such as clothes and somatic art, linguistic markers, and other domains. But differential status is given little attention in *The Scented Ape*. The notion that individuals may use perfumes and other scents to mark themselves as members of particular social groups as defined by ethnicity, geography, gender, age, occupation, class, etc. is not addressed. Nor does Stoddart explore the potential relationship between these human tendencies and the use of exotic, expensive, or otherwise differentially accessible scents.

In Chapters 8 and 9 the author makes explicit his argument that Miocene hunting and increased parental care in hominids were associated with pair bond-strengthening adaptations such as hidden ovulation, extended female sexual receptivity, and olfactory desensitization. Further adaptations which tended to make sex more pleasurable included

the development of axillary organ odors. These are reasonable arguments which Stoddart bases primarily on Robert Trivers' theories of parental investment, and supports through primate studies. Again, however, some questions are not explored. For example, he makes mention of population variability with respect to axillary odor production. Are we to presume that different human groups are "rewarded" differentially with post-coital smells? And, if so, why?

In sum, Stoddart's arguments are reasonable and interesting, if sometimes frustrating. His use of biological, psychological, historical and aesthetic sources for his examination of human olfaction is the great strength of this book. While some questions remain unexplored, this is a notable effort and one well worth reading.

Animal Minds

By Donald Griffin. University of Chicago Press, 5801 S. Ellis Ave., Chicago, IL 60637 USA, 1992, 310 + x pp., \$24.95 (ppr.).

Reviewed by James H. Fetzer, Department of Philosophy, University of Minnesota, Duluth, MN 55812 USA.

No one has contributed more to the scientific study of animal mentality, cognition and consciousness than Donald Griffin, whose work on this subject—including Griffin (1981) and Griffin (1984)—virtually defines the field. The problem that confronts this discipline is the existence of the phenomena to which these studies are directed. Different views extend from the "common-sense" opinion (that animals think about their behavior and deliberate over their options) to the "behavioristic" position (that mental experiences are no more than epiphenomena that do not causally influence behavior).

Thus, if behaviorism were correct, there could be no genuine science of *cognitive ethology*, understood as the analysis of the cognitive processes that affect the behavior of non-human animals, because cognitive processes do not causally affect the behavior of human or non-human animals. And if mentality, cognition or consciousness does make a

difference to behavior, *behaviorism*, understood as the analysis of behavior strictly on the basis of the public properties of organisms and their environments, must be misconceived, insofar as cognitive variables lie beyond its scope.

To resolve this problem, Griffin provides numerous case studies that provide empirical support for the existence of the phenomena this discipline focuses upon. These case studies are drawn from three "categories of evidence":

(1) variability and versatility of behavior in response to novel situations;

(2) brain processes that might correlate with conscious thought processes;

(3) apparent communication between animals, both verbal and nonverbal.

While some examples—the piping plover feigning injury, the vervet monkey sounding alarms and the assassin bug deceiving its prey—may be familiar, they all appear to provide support for the existence of animal minds.

Chapter 1 discusses of the problem of defining animal mentality. Chapters 2 through 6 are devoted to studies of cases of type (1), Chapter 7 to type (2), and Chapters 8 through 11 to type (3). Chapter 12 then attempts to evaluate the extent to which scientific belief in the existence of animal mentality has been sustained.

The range and variety of behaviors Griffin describes provides ground for inferring that the underlying problems that confront cognitive ethology are not empirical but are instead conceptual and methodological. The definitions of "consciousness" Griffin takes to be most promising include *perceptual consciousness*, understood as the state or process of being aware of something, and *reflective consciousness*, the state or process of being aware of one's own awareness.

Griffin acknowledges the possibility of an alternative lying between behaviorism and commonsense, according to which animals are credited with *cognition* but denied to possess *consciousness*. Many psychologists, in fact, deny that cognition implies consciousness, and some claim that animal cognition is *always* unconscious. This "intermediate" conception can therefore be characterized as claiming that animals experience "perceptual consciousness",

while denying that animals experience "reflective consciousness".

Griffin clearly wants to go beyond this position by maintaining that some animals experience reflective consciousness as well as perceptual consciousness. He avoids the pitfall of assuming that animal mentality must involve the capacity to learn, which would exclude evolved, stereotypic forms of mentality. And he emphasizes the necessity for understanding human and animal mentality within an evolutionary context.

Nevertheless, the case for animal mentality might be substantially strengthened by adopting an alternative conceptual framework. If signs are understood to be things that stand for other things--for organisms, for example--then "minds" can be defined as *sign-using systems* (Fetzer 1988, 1989, 1990, 1991). Various behaviors are then activated by the presence of signs of different kinds. Signs of one kind may induce injury-feigning behavior, while others may lead to alarm calls, etc.

Distinctions can then be drawn among minds of different kinds. Minds of Type I, for example, can utilize signs that resemble (look like, taste like, smell like, etc.) what they stand for; minds of Type II, signs that are causes or effects of that for which they stand (such as things that would satiate hunger, relieve sexual tensions, etc.); and minds of Type III, signs that are merely habitually or conventionally associated with that for which they stand (such as signals that serve as warnings, which do not resemble and are neither causes nor effects of whatever they stand for, such as words in a language which must be learned).

Such an approach provides a framework for defining "mentality" as sign-using (or "semiotic") ability, where "consciousness" combines the ability to use signs (of a certain kind) with the capability of exercising that ability (by not being blindfolded, with respect to visual signs, etc.). Thus, "cognition" occurs as the effect of a causal interaction between the presence of a sign within suitable causal proximity and the ability (non-incapacitated) to use signs of that kind combined with other (including mental) states of an organism, yielding dispositions toward behavior of

various kinds, some of which may be promptly displayed but others not.

This approach implies that *cognition* always involves consciousness, not in the sense of "reflective consciousness" but in a sense analogous to "perceptual consciousness", provided awareness of other internal states is accommodated thereby. It does not claim that *animal cognition* cannot be conscious in the sense of "reflective consciousness", since various degrees of self-awareness, etc. may characterize different species. And it implies that the general criterion of *mentality* is not "self-awareness", as some theoreticians have proposed, but rather the capacity to make a mistake (by taking something to stand for something other than that for which it stands), which explains the importance of animal deception.

Methodologically, the conception of minds as sign-using systems allows hypotheses about mentality to be tested empirically on the basis of observable behavior in response to environmental stimuli, where variability under constant conditions and versatility under novel conditions provide crucial evidence. This is brilliantly illustrated by Carolyn Ristau (1991) in her study of the piping plover. And it supplies a framework that decisively vindicates the work of Griffin and his associates in their empirical studies of the animal mind.

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ANNOUNCEMENTS

Call for Research Assistance

Our Membership Chair, Nancy Segal, is seeking subjects for a study of same-age, unrelated siblings reared together. Her request: "Pairs of unrelated siblings of the same age, reared together from infancy, uniquely replicate the rearing situations of monozygotic and dizygotic twins. Study of these dyads (same-age, unrelated partners) offers a new behavioral genetic research design for examining hereditary and environmental influences on behavior. I have been collecting psychological test data on these pairs for the past few years, and would be interested in identifying more sets. If anyone is aware of families with such children, please contact Nancy Segal at: CSU Fullerton, Psychology Dept., Fullerton, CA 92634 USA, 1-714-773-2142, NSEGAL@FULLERTON.EDU. It is fine to pass my telephone number on to the families."

Human Behavior and Evolution Society

HBES held its annual convention 16-19 June 1994 in Ann Arbor, Michigan, USA. A limited number of copies of the abstract book are available for \$8 from Nancy Thornhill, Dept. of Communication, 2020 Frieze, University of Michigan, Ann Arbor, MI 48109-1285 USA. The next HBES meeting will be held in Santa Barbara, CA from 28 June to 2 July 1995.

International Ethological Conference

The 24th International Ethological Conference will take place in Hawaii 10-17 August 1995. Further details will follow. There will be two symposia on human behavior in the official program. ISHE members are encouraged to present results of observational research, especially in the areas of behavior-environment interaction and sex differences. The 1997 conference will be hosted by Karl Grammer in Vienna.

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Journal of Behavioral Medicine

This interdisciplinary journal, now in its 17th volume, furthers the understanding of health and illness through the knowledge and techniques of behavioral science. It emphasizes applications of research to prevention and treatment. It presents theoretical and review articles, experimental studies, and case studies from various disciplines, including psychology, psychiatry, sociology, epidemiology, anthropology, health economics, and statistics. Areas of interest include appetitive disorders, adherence to medical regimen and health maintenance behavior, pain, biofeedback, sociocultural influences on health, and brain-behavioral relationships that influence physiological function. The journal is published 6 times per year by Plenum Publishing Co., 233 Spring St., New York, NY 10013-1578 USA; 88/90 Middlesex Street, London E1 7EZ, U.K. Personal subscription rate is \$45 in the US, \$53 elsewhere.

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