

Human Ethology Newsletter

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Membership Renewals

If the date on your mailing label is earlier than 1986, it is time to renew your membership. Renewal notices are not sent for economic reasons.

Report any errors, changes of address, etc. to the editor.

CONTEST!! PRIZES!!

This Newsletter needs some spice. Book reviews, announcements of meetings, and lists of references are enlightening, but an informal newsletter should be much more. It should and could, for example, be a place to express ideas which would not be published in the more conservative journals.

So . . . a DARING IDEAS CONTEST! Send your far-out ideas, with whatever documentation you can master, to the editor. One to three typed pages should be about right. Prizes will be awarded based on originality and some semblance of sanity. First prize is a one year free subscription to HEN. Second prize, two years.

Let's hear from you, and put some new controversy into the Newsletter.

Human Nature Revisited

(The following is a response by David Munro to John Ross' comments in the previous issue of the Newsletter. This is a continuing reaction to Ian Vine's Forum commentary on human nature.)

To impute imprinting as explanation in the affairs of young humans seem to require, for John Ross, "a leap of faith" he is unprepared to make. But I have long thought that Noam Chomsky and the late Eric Lenneberg cleared that hurdle for us all some 20 years ago. Lenneberg made a single reference to imprinting (in *Biological Foundations of Language*, 1967, p. 175) as the happy solution to the mystery of the sudden appearance of language - readiness because it "postulated nothing that would be extraordinary in the realm of animal behavior."

This made good sense to me, after struggling through Bloomfield's painful behaviorist explanation of language acquisition. It also explained Montessori's early insight, which may never have been followed up, that children mature through "sensible" periods of receptivity to specific subject-matter. And, after reading Hess' *Imprinting*, I asked myself, "Wouldn't it be surprising, indeed, if this ubiquitous behavior **did not** appear in the human makeup?"

Now that would require a leap of faith for the agile Professor Ross.

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Human Nature: Continuing the Discussion

Territoriality and Property - Two Natural Human Traits

I am glad, that my remarks (HEN 4/7:7) - rude as they were - triggered the hoped-for response, namely focusing this intriguing debate on its prerequisite: the clear and comprehensive definition of the traits which eventually will make up the desired model. For this re-start Ross (4/8:1) well chose territoriality, and I a priori agree with him, that the concepts of privacy and property should be kept separated therefrom. However, I fail to understand, how one can

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SEE YOU IN TUTZING!

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adopt a definition which restricts "territory" to spaces "defended by a group - not an individual" and simultaneously negate that people are a territorial species: behold the defense of human group territories, from Bushmen's and other tribes' in their integrated habitats to its horrid contortion - the wars of political and/or national states! All this was conscientiously analysed by Eibl-Eibesfeldt (Krieg & Frieden, 1975:V). On the other hand, territories held by individuals and pairs abound in nature; furthermore, territoriality though species-specific must not be uniformly constant (just remember the European robins and starlings!). I therefore dare suggest to define, for the present purpose, "territory" in the same sense of Brown (Evolution of Behavior, 1975) et. al. as an "exclusive and defended homerange, sustaining at least one individual, pair or group for at least one season".

Homes were aptly defined by Hediger (1942) as "the places of maximal relative shelteredness". In integrated habitats, such as of singing birds, farmer's homesteads or pastoralists' tribal areas, these are usually the territory's center; not so, if the habitat is a differentiated one: in the multi-species colonies of herons, the exclusive nesting sites are separated from the not at all exclusive feeding grounds; in human industrial society, the residential habitat is evermore separated from the occupational one. **Individual space** may be established anywhere, also by otherwise non-territorial creatures and serve a variety of objectives. In between are specific combinations, such as the nesting spaces of the pairs making up breeding colonies, the Uganda Kob's stamping grounds, or human workbenches, office space, or hotel rooms at that. All these are not territories *sensu strictu*, but they are exclusive patches of living space, defended by similar stereotyped actions, and these should be included in the category of "territorial behaviors" (Ref. again to Eibl-Eibesfeldt (1975:IV), P. Van Den Berghe, O. Koenig, Desmond Morris, et. al.)

But than people not just mark and defend their territorial assets - ritualisedly or straightforward, always and ever: they may use their real estate and any other kind of resource they control, to exchange them, for something equivalent (or even better) - and no animal ever would do that. This uniquely human tradeability is what sets off "property"; the answer to Ross' question "Is my house a territory or property" thus is: "both—depending on the level of analysis". But even if people, besides all their "tendency to gather as many possessions as possible" would just "do a lot of (not always fitness enhancing) marking of areas they wish to call their own" - does this not simply strengthen the argument, that human territoriality is indeed a real category, namely one which - in a sense of, i.a., Old Master Lorenz - may become autonomous and eventually be carried out for its own sake?

How to plot the relationships between territory and property, and between then and privacy and status - remains to be seen, as do the definitions of these latter two and many other traits. Perhaps, to expediate matters, somebody is able and willing to organize a symposium or workshops on "Defining Human Nature"?

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Book Review

The Innocent Anthropologist. Notes from a Mud Hut, by Nigel Barley. Harmondsworth: Penguin Books, 1986. Paperback, 190 pp., £3.95.

Reviewed by W. C. McGrew, Dept. of Psychology, University of Stirling.

Anyone who has done field-work, especially in the tropics, will know that reports appearing in scientific journals and scholarly monographs only scratch the surface. Whatever the subject matter, from laterite to levirate, it is what gets left out that stays in the memory, long after mere data have faded. Sometimes, however, field-workers write up their extra-curricular experiences too, and these often add down-to-earth detail to the otherwise rarified results of research.

Barley has done this most amusingly, in an account of his first year of field-work, among the Dowayo of northern Cameroon. The subtitle is indicative: The hitherto armchair anthropologist coping with a very different setting. He chronicles from start to finish: From taking up the casual challenge of a colleague to go to the field, through to the culture - shock of returning to the 'real world' of the U.K. The latter is especially well-done, as it is the more alarming of the two transitions. One expects to make adjustments when leaving home, but coming back is not supposed to be so hard.

In between, Barley relates a string of adventures, many of which will be recognizable to old Africa hands, at least. (This reviewer cannot say, through lack of experience, whether these episodes generalise further afield, but I guess that they do.) Frustrating delays, tangles with officialdom, initial misunderstandings with subjects, small but crucial triumphs, bouts of illness, rewards of acceptance, bitter-sweet departure — all are here. What makes the account stand out is Barley's ability to laugh at himself, to not take himself too seriously.

Along the way, a lot of useful anthropological knowledge is passed on too. Given the shortish length and reasonable price, it is an ideal book to be recommended to undergraduates as vacation reading. For anyone interested in the cross-cultural study of human behavior, it is a good way of finding out how ethnographic data gets got.

Book Review

Myths of Gender: Biological Theories about Women and Men, by Anne Fausto-Sterling. Basic Books. New York, 1985.

Reviewed by George Kocan
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The dust jacket of this book promises, "a solid scientific refutation of the myths about gender differences that have dominated so many of our assumptions about human

behavior." In my view, Fausto-Sterling has not so much given the reader a solid refutation of anything as much as a study in how to advocate strongly held personal beliefs by pooh-poohing unwanted evidence. The author is a developmental geneticist, but her cause is feminism, and her strategy is to go over offending research results with a sledge hammer. Feminism is not expounded and defended with the citations of its most important findings. No. The value of feminism is assumed and equated with justice, equality, affirmative action and socialism. That any decent and intelligent person would have a different outlook is just not considered. So, feminism prevails by default.

Research Bias

Fausto-Sterling's general theory is that researchers who work in the area of behavioral sex differences are biased by their culture. Since modern culture is patriarchal, it should be no surprise to learn that much of the research supports the traditional sex stereotypes. That is, she assumes that sex stereotypes have been produced by culture and not the other way around.

Maccoby and Jaklin (1974) for example, reviewed a mass of literature on sex differences and came up with several categories for which the evidence favoring sex differences was strong: aggression, verbal skills, mathematical ability and visual-spatial skills. But when Fausto-Sterling looked at their conclusions, she found that they did not measure up to the "highest standards of science".

Maccoby and Jaklin profess allegiance to feminism, fully acknowledging that researchers in the past were working under the influence of sex stereotypes, and arguing that much of the literature did not justify the conclusions in favor of sex differences. Their review was to be different. It would comply only with the highest standards of science. And yet, their results favor a sex difference hypothesis. And now comes Anne Fausto-Sterling to tell the world that she is a bona fide feminist dedicated to the highest standards of science and that Maccoby and Jaklin, scientifically speaking, muffed the ball.

What is one to make of all this? I, for one, am not going to announce that my scientific standards are higher than anyone else's. I am certainly not going to claim any sympathy for feminism, either. I hope to answer some of the criticisms brought against those approaching human behavior from a biological and evolutionary point of view.

Brain Laterality

Fausto-Sterling criticizes the work that shows that women's brains are less lateralized than males. In particular, she attacks Jerre Levy's research thusly, "Let us suppose . . . that male and female brains do lateralize differently and ask what evidence exists to suggest that such differences might lead to variations in performance of spacial and verbal tasks. The answer is, quite simply, non whatsoever."

I suspect that Fausto-Sterling did not look into this issue personally but relied on fellow feminists' reviews of the brain literature (e.g. Kimball, 1981). In male neurological patients, there is a strong association between the nature of psychological defects observed and the side of the brain that has been injured: verbal functions are disordered with left

hemisphere lesions, and non-verbal functions (spacial skills) are disordered with right hemisphere lesions. In female neurological patients, this association is much weaker than in males.

Using a tachistoscope, if written words are briefly flashed in the right eye, the typical right hander is more efficient in identifying the stimulus as compared to left eye presentations, and vice-versa, if non-verbal (e.g. shapes) information is presented. In such experiments the direction of asymmetry is the same in men and women; however, the magnitude is greater in men than in women. (Levy, 1981; Spinger and Deutsch, 1981)

The above findings hardly support Fausto-Sterling's contention that no evidence exists to favor a sex difference in laterality hypothesis.

Testosterone and Aggression

Fausto-Sterling devotes a full chapter to hormones and aggression. Her conclusions are predictable. She lists six studies in which aggression was compared to testosterone levels in men. According to her mode of reasoning, "the weight of the evidence . . . suggests no reliable correlation." Some studies show a correlation between aggression and testosterone and others do not.

In my judgement, the evidence supporting a positive role for testosterone in aggression is remarkable. Both behavioral and psychological variables appear conceptually unified by this hormone.

Prisoners having a history of aggression (e.g. convictions for violent crimes) during adolescence exhibited higher testosterone levels than less aggressive prisoners (Kreuz and Rose, 1972). I agree with Fausto-Sterling that this does not favor the hypothesis that testosterone acts as a proximal cause of aggression. But the research is consistent with the hypothesis that testosterone organizes the brain during a critical period of development and subsequently activates aggression during puberty, when other secondary sex characteristics appear. Therefore, one cannot rule out testosterone as a cause of aggression in men.

Research which Fausto-Sterling somehow missed, however, does show a positive proximal relationship between violence and plasma levels of testosterone. Ehrenkranz et. al. (1974) found significant differences in plasma testosterone between aggressive and non-aggressive prisoners. They also found that aggressive prisoners scored higher on the Buss-Durkee Hostility Inventory than did the non-aggressive inmates. The results not only show a positive relationship between behavior and a hormone but also a link between behavior and a psychological variable, hostility. Fausto-Sterling questioned the validity of equating violent behavior with psychological variables through such a broad concept as aggression. In my view, the evidence as a whole is consistent with the hypothesis that testosterone positively influences aggression. On the other hand, all the evidence should be negative to support the null hypothesis.

Sociobiology

Fausto-Sterling also goes after sociobiology, arguing in effect that any evolutionary reasoning being applied to human behavior is doomed. "Human sociobiology is a theory that inherently defies proof," she concludes after

discussing Wilson's (1975) concept of a biogram and the putative existence of human universals. She, of course, is not asserting that sociobiology has been disapproved. Her argument relies on the philosophical principle that a truly scientific proposition must be testable. Because sociobiology is not testable, therefore, it is not science.

My answer is that sociobiology is the application of evolutionary concepts to social behavior. All evolutionary and biological concepts are applicable to human social behavior, because there is no *a priori* (save the dictates of feminism) or empirical reason to separate human organic matter from non-human organic matter.

Furthermore, the principle of falsifiability, it seems to me, is not directly applicable to the level of a theory *in toto*. A theory pulls together disparate facts and generalities into a nomological net (Cronbach and Meehl, 1955). Hypotheses are deduced from the theory, and it is they which are tested. It is a lot to expect for one to produce a critical experiment to test an entire theory like evolution.

Whatever criticisms she makes against sociobiology apply to the theory of evolution. But she stops short of an outright denial of evolution, as do other critics of sociobiology (Sahlins, 1976; Lewontin, 1979). Nevertheless, I think that a forthright denial is necessary. Evolution is the issue. If evolution has any validity it must apply totally to the human animal. The universality of its application is intrinsic to this concept. Otherwise, one is compelled to argue that human existence represents a special case — a special creation. Some cultural anthropologists have come very close to taking just such a position. Ashly-Montagu (c.1970) asserted that human evolution had taken a quantum jump which somehow emancipated mankind from the laws of evolution.

She discusses other topics in a similar manner. She talks about the human smile but neglects to mention the work of Freedman (1974, 1979) and Ekman (1969, 1975) She addresses rape, PMS, polygyny, dominance and biological determinism. I wish to conclude by saying that I disagree with the author on almost every point. I think most biologically orientated students of behavior will disagree also. However, her effort has merit — in the same way, perhaps, that an automobile accident has merit, if it motivates one to learn from others' mistakes.

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Current Literature

Material for this section of the Newsletter should be sent directly to the editor. A sentence or two of summary would increase the value to readers.

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Papers

- Papers presented at the 55th annual meeting of the American Association of Physical Anthropologists, April, 1986. Abstracted in *American Journal of Physical Anthropology*, 1986, 69.
- Clarke, M.R. Interactions of male howling monkeys with immatures in a free-ranging social group.
- Gibson, K.R., & Parker, S.T. Extraction: a non-sexists interpretation of early hominid foraging patterns.
- Hoff, C. Influence of maternal body size on early postnatal growth and development.
- Quandt, S.A. Strategies for collecting reliable dietary intake data for infant growth and nutrition research.
- Reid, R.M. Kin structured migration and inbreeding on the American frontier.
- Tierson, F.D., & Orland, J.M. Forming a social unit among captive lowland gorillas.
- Walker, A.C. *Homo erectus* skeleton from West Lake Turkana.
- Wright, P.C. Diet, ranging behavior and activity pattern of the gentle lemur (*Hapalemur griseus*).
- Galdikas, B.M.F. (1985). Adult male sociality and reproductive tactics among Orangutans at Tanjung Puting. *Folia Primatologica*, 45, 9-24.

Papers

- Papers presented at the Biennial meeting of the Southwestern Society for Research in Human Development, San Antonio, Texas, March 6-8, 1986. Session on Men and Children:
- Draper, Thomas, Robertson, L.B., & Sermon, T.S. (Brigham Young University). Men with children: the limits of male fears of nurturing.
- Bailey, W. (Tulane). Father's knowledge of development and his involvement with his preschool child.
- Owen, M.T., Margrand, N., Cox, M., & Gossett, J.T. Timberlawn Psychiatric Research Foundation). Antecedents of parental involvement with infants.
- Bailey, W. (Tulane). Daddy is and daddy does: paternal involvement in terms of parents' personalities and attitudes.

Bulletin Board

L.S.B. Leakey Foundation General Research Grant

The L.S.B. Leakey Foundation supports research related to human origins, behavior and survival. Priority is given to studies in the areas of human paleontology, archaeology and environmental studies of the Miocene Pliocene and Pleistocene; into the behavior of the Great Apes and other Old World Primate species; and into the ecology and adaptation of living hunter-gatherers. Eligibility is limited to applicants who are doctoral candidates or engaged in post-doctoral research. Potential applicants are encouraged to submit a Petition for Grant Application. On invitation of the Science and Grants Committee, projects falling within the range of priorities of the foundation will be asked to submit a formal application. Deadlines for submission of the formal application are: March 15, June 1, September 1, and December 1. For further information and application forms contact: Kathleen F. Galvin, PhD, Grants Officer, L.S.B. Leakey Foundation Center 1-7, Pasadena, CA 91125.

It appears from the reference lists in many recent articles on infant-mother attachment that many researchers are not aware that John Bowlby has published a revision of *Attachment and Loss. Vol. 1. Attachment*, his seminal treatise on the development of infant-mother relations. The second edition is dated 1982.

For those who might be looking for a stat text by a Human Ethologist, see Ted Horvath's recent publication *Basic Statistics for Behavioral Sciences*. Little, Brown & Company, 1985.

More Meetings

Animal Behavior Society: 8-13 June, 1986, Tucson, AZ, USA; 21-26 June, 1987, Williamstown, MA, USA.
American Society of Zoologists: 27-30 December, 1986, Nashville, TN; 27-30 December, 1987, New Orleans, LA.

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Bulletin Board

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American Society of Primatologists

The next annual meeting of ASP will be June 28-July 6, 1986 at the University of Texas at Austin. Program Chair is Dr. Joyce Sirianni, Dept. of Anthropology, SUNY Buffalo, Buffalo, NY, USA 14261.

Newsletter Submissions

Yes, please send anything which might be of interest to ISHE members: announcements of meetings, comments relevant to human ethology, suggestions for Forum topics — conspicuous by their absence lately, sabbatical opportunities, employment opportunities, anything.

Suggestions for books to review, or reviews, should be sent to European Editors William McGrew (Dept. of Psychology, University of Stirling, Stirling FK9 4LA Scotland) or Ian Vine (Interdisciplinary Human Studies, University of Bradford, Bradford, West Yorkshire, BD7 1DP, England) or to American Editor William Bailey (Dept. of Psychology, Tulane University, New Orleans, Louisiana 70118).

Submissions in any legible format are acceptable.

Newsletter Editor Wanted

The current editor's term expires at the end of 1986. Please send nominations, or an indication of your own willingness to serve, to the current editor or to any officer. It's a fun job, stimulates interactions with a lot of very fine people, and requires little in the way of resources.

Membership

The Newsletter mailing list now numbers 280. There are 50 European members, 8 in Asia, 1 in Australia, 2 in South America, 24 in Canada, and 190 in the U.S.

Jay Feierman is Membership Chair; send him your ideas for expanding the Society (Vista Sandia Hospital, 501 Richfield Ave., Albuquerque, NM 87113). And be sure to copy and send the membership blank from last issue with your reprints and correspondence.

(Members: Please photocopy this application and send it to prospective members. A sample issue of the *Newsletter* is available on request. You may also wish to send a copy with each reprint you mail out.)

INTERNATIONAL SOCIETY FOR HUMAN ETHOLOGY

Membership and Newsletter

The ISHE was formed with the goal of promoting ethological perspectives on the study of human behavior. 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 has elected officers and a number of committees, publishes a quarterly *Newsletter*, collates an annual selection of human ethology abstracts, and meets annually, either independently or in conjunction with the Animal Behavior Society, the International Primatological Society or another major society.

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