

Human Ethology Newsletter

Editor: Frans X. Plooij
Paedological Institute of the City of Amsterdam
IJsbaanpad 9, 1076 CV Amsterdam, Netherlands (20) 643321 or (2963) 4197

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Newsletter Submissions

Anything which might be of interest to ISHE members is welcome: society matters, suggestions for Forum topics, Mini Communications, Current literature and films, and material for the Bulletin Board such as announcements of meetings, sabbatical opportunities, employment opportunities, etc., should be sent to the Editor.

Suggestions for books to review, or reviews, should be sent to the nearest Book Review Editor dealing with the language concerned. A list of the book review editors is printed below.

Submissions in any legible format are acceptable as long as these are in English. Floppy disks containing McWrite files produced on the Macintosh, Wordperfect files produced on an IBM-PC (compatible), or ASCII files can be processed as well and are in fact preferred, because they lower the production costs.

Book Review Editors

William T. Bailey, English
Dept. of Psychology, Tulane Un., New
Orleans, Louisiana 70118,
U.S.A.

Ian Vine, English Interdisciplinary Human
Studies, Un. of Bradford, Bradford BD7
1DP, England.

Eduardo Gudynas and Fernando G. Costa,
Spanish/Portuguese c/o ASMER's
Regional Office,
Casilla Correo 13125, Montevideo,
Uruguay.

SOCIETY MATTERS Comments and Suggestions

Thank you very much for the constructive comments and suggestions made. As for the idea to start 'interest groups', it probably will have a short life span. No initiatives were reported, and one member commented "Interest groups reduce the flow of communication and heighten the competition for limited points of view."

A new section on "Growing points in Human Ethology" was welcomed. The commentary format was suggested: a lead article with comments by selected reviewers.

An entirely new suggestion was received from Michael McGuire, who originally got it from Randy Nesse. The idea is to devote an entire issue of the Newsletter to informing prospective students where they might train, receive advanced degrees, etc. if they are interested in Human Ethology. Such a publication could appear in both HEN and the journal *Ethology and Sociobiology* to the advantage of somewhat different groups of readers. We are working on the contents and format of such information and will report on this in the next issue. Meanwhile, I hope all of you will think about it and discuss it in your institute so that you are ready to fill out and return the forms when these arrive.

Call for Book Review Editors

As you can see on the front page, the book review editor's are presented together with their address and the language used in the books they (have) review(ed). Ian Vine is still in that list although I said he resigned. Ian is so kind to stay on until another English language Book Review Editor is found on the European side of the Atlantic Ocean.

I am still searching for a French Book Review Editor. Since the French are so keen on promoting their cultural and scientific heritage, I had expected/hoped that a pile of offers had reached me by now. Alas! Please, feel free to contact me.

New Spanish Book Review Editors

Fernando G. Costa is a zoologist, conducting research at the Division of Experimental Zoology, Clemente Estable Biological Research Institute, and is also Adjunct Professor of Ethology at the School of Humanities and Sciences, University of Uruguay. His main interest is on reproductive biology, and he has done extensive research, mainly on spiders.

Eduardo Gudynas is regional editor for Latin America of ASMER's *Man Environment Systems* journal; he also teaches at the Ethology and Ecology Area, School of Psychology, University of Uruguay, and heads the Environment and Development Group of the Franciscan Ecological Research and Promotion Center (CIPFE). His main interests are man-environment relations, related behavioral problems, and animal ecology.

Human Ethology Abstracts

John Ross has about completed Human Ethology Abstracts VII and is working on numbers VIII and IX. Hereby he places a call for abstracts from

ISHE members. Abstracts from publications, meetings and anything else that would be of general interest are welcome. Please, contact him directly: John Ross. Dept. of Psychology, St. Lawrence University, Canton, New York 13617, U.S.A.

Call for Conference announcements

Would all members be so kind to send me a copy of any conference-, workshop-, summer school-, etc. announcement they receive, together with the source they receive it from. If the Bulletin Board section is to be a success, more announcements are to be published and it is difficult to find these in one single source of information. With your help this section may become a useful source of information for all members.

FORUM

First received was the article by Manson and Wrangham commenting on the Statement on Violence, then the reply by Robert Hinde. It seemed wise to publish the Statement on Violence as well, since it may not be equally accessible to all readers.

Statement on Violence

Believing that it is our responsibility to address from our particular disciplines the most dangerous and destructive activities of our species, violence and war; recognizing that science is a human cultural product which cannot be definitive or all-encompassing; and gratefully acknowledging the support of the authorities of Seville and representatives of the Spanish UNESCO; we, the undersigned scholars from around the world and from relevant sciences, have met and arrived at the following Statement on Violence. In it, we challenge a number of alleged biological findings that have been used, even by some in our disciplines, to justify violence and war. Because the alleged findings have contributed to an atmosphere of pessimism in our time, we submit that the open, considered rejection of these mis-statements can contribute significantly to the International Year of Peace.

Misuse of scientific theories and data to justify violence and war is not new but has been made since the advent of modern science. For example, the theory of evolution has been used to justify not only war, but also genocide, colonialism, and suppression of the weak.

We state our position in the form of five propositions. We are aware that there are many other issues about violence and war that could be fruitfully addressed from the stand point of our disciplines, but we restrict ourselves here to what we consider a most important first step.

IT IS SCIENTIFICALLY INCORRECT to say we have inherited a tendency to make war from our animal ancestors. Although fighting occurs widely throughout animal species, only a few cases of destructive intra-species fighting between organized groups have ever been reported among naturally living species, and none of these involve the use of tools designed to be weapons. Normal predatory feeding upon other species cannot be equated with intra-species violence. Warfare is a peculiarly human phenomenon and does not occur in other animals.

The fact that warfare has changed so radically over time indicates that it is a product of culture. Its biological connection is primarily through language which makes

possible the coordination of groups, the transmission of technology, and the use of tools. War is biologically possible, but it is not inevitable, as evidenced by its variation in occurrence and nature over time and space. There are cultures which have not engaged in war for centuries, and there are cultures which have engaged in war frequently at some times and not at others.

IT IS SCIENTIFICALLY INCORRECT to say that war or any other violent behavior is genetically programmed into our human nature. While genes are involved at all levels of nervous system function, they provide a developmental potential that can be actualized only in conjunction with the ecological and social environment. While individuals vary in their predispositions to be affected by their experience, it is the interaction between their genetic endowment and conditions of nurturance that determines their personalities. Except for rare pathologies the genes do not produce individuals necessarily predisposed to violence. Neither do they determine the opposite. While genes are co-involved in establishing our behavioral capacities, they do not by themselves specify the outcome.

IT IS SCIENTIFICALLY INCORRECT to say that in the course of human evolution there has been a selection for aggressive behavior more than for other kinds of behavior. In all well-studied species, status within the group is achieved by the ability to cooperate and to fulfill social functions relevant to the structure of that group. "Dominance" involves social bondings and affiliations; it is not simply a matter of the possession and use of superior physical power, although it does involve aggressive behaviors. Where genetic selection for aggressive behavior has been artificially instituted in animals, it has rapidly succeeded in producing hyper-aggressive individuals, this indicates that aggression was not maximally selected under natural conditions. When such experimentally-created hyper-aggressive animals are present in a social group, they either disrupt its social structure or are driven out. Violence is neither in our evolutionary legacy nor in our genes.

IT IS SCIENTIFICALLY INCORRECT to say that humans have a "violent brain." While we do have the neural apparatus to act violently, it is not automatically activated by internal or external stimuli. Like higher primates and unlike other animals, our higher neural processes filter such stimuli before they can be acted upon. How we act is shaped by how we have been conditioned and socialized. There is nothing in our neurophysiology that compels us to react violently.

IT IS SCIENTIFICALLY INCORRECT to say that war is caused by "instinct" or any single motivation. The emergence of modern warfare has been a journey from the primacy of emotional and motivational factors, sometimes called "instincts", to the primacy of cognitive factors. Modern war involves institutional use of personal characteristics such as obedience, suggestibility, and idealism, social skills such as language, and rational considerations such as cost-calculation, planning, and information processing. The technology of modern war has exaggerated traits associated with violence both in the training of actual combatants and in the preparation of support for war in the general population. As a result of this exaggeration, such traits are often mistaken to be the causes rather than the consequences of the consequences of the process.

We conclude that biology does not condemn humanity to war, and that humanity can be freed from the bondage of biological pessimism and empowered with confidence to undertake the transformative tasks needed in this

International Year of Peace and in the years to come. Although these tasks are mainly institutional and collective, they also rest upon the consciousness of individual participants for whom pessimism and optimism are crucial factors. Just as "wars begin in the minds of men," peace also begins in our minds. The same species who invented war is capable of inventing peace. The responsibility lies with each of us.

Seville, Spain, May 16, 1986.

David Adams, Psychology, Wesleyan University, Middletown (CT) USA; S.A. Barnett, Ethology, the Australian National University, Canberra, Australia; N.P. Bechtereva, Neurophysiology, Institute for Experimental Medicine of Academy of Medical Sciences of USSR, Leningrad, USSR; Bonnie Frank Carter, Psychology, Albert Einstein Medical Center, Philadelphia (PA) USA; Jos M. Rodriguez Delgado, Neurophysiology, Centro de Estudios Neurobiológicos, Madrid, Spain; Jos Luis Daz, Ethology, Instituto Mexicano de Psiquiatría, Mexico D.F., Mexico; Andrzej Elias, Individual Differences Psychology, Polish Academy of Science, Warsaw, Poland; Santiago Genovs, Biological Anthropology, Instituto de Estudios Antropológicos, Mexico D.F., Mexico; Benson E. Ginsburg, Behavior Genetics, University of Connecticut, Storrs (CT) USA; Jo Groebel, Social Psychology, Erziehungswissenschaftliche Hochschule, Landau, Federal Republic of Germany; Samir-Kumar Ghosh, Sociology, Indian Institute of Human Sciences, Calcutta, India; Robert Hinde, Animal Behavior, Cambridge University, UK; Richard E. Leakey, Physical Anthropology, National Museum of Kenya, Nairobi, Kenya; Taha M. Malasi, Psychiatry, Kuwait University, Kuwait; J. Martín Ramirez, Psychobiology, Universidad de Sevilla, Spain; Federico Mayor Zaragoza, Biochemistry, Universidad Autónoma, Madrid, Spain; Diana L. Mendoza, Ethology, Universidad de Sevilla, Spain; Ashis Nandy, Political Psychology, Center for the Study of Developing Societies, Delhi, India; John Paul Scott, Animal Behavior, Bowling Green State University, Bowling Green (OH) USA; Riitta Wahlström, Psychology, University of Jyväskylä, Finland.

Is Human Aggression Non-biological? Problems with the Statement on Violence

by: Joseph H. Manson and Richard W. Wrangham, Department of Anthropology and Program in Evolution and Human Behavior, The University of Michigan, Ann Arbor, MI 48109 U.S.A.

The "Statement on Violence" submitted by Prof. Douglas P. Freye and 19 other scholars, and proposed for endorsement by UNESCO, is an important comment on the dangers of an "instinctivist" view of human aggression. This "instinctivist" view, as the Statement points out, promotes an unjustifiable pessimism concerning prospects for the abolition of war. We agree with the Statement on Violence that there is no justification for using biological findings in the service of violence and war. We disagree, however, with the idea that the biological basis of war is irrelevant to thinking about peace. We believe (a) that the tendency to behave aggressively in particular circumstances

has been favored by natural selection in humans and other animals; (b) that natural selection has favored particularly extreme forms of aggression in humans and very few other species; and (c) that it is easier to foster peace by understanding rather than ignoring the biological conditions that have favored violence and war. Thus, we believe that war does have a biological basis, and not just in the trivial sense that humans have the biological *capacity* to make war.

It would probably do little harm to allow the Freye et al. Statement on Violence to be adopted, and if its adoption helps to undermine philosophical justifications for xenophobia and aggression, it will serve a good purpose. However, we consider it important that behavioral scientists do not neglect the strong relationship between biology and aggression, because an understanding of this relationship is ultimately likely to provide a productive avenue for explaining, and therefore controlling, aggressive behavior.

The Statement on Violence follows traditional thinking in suggesting that if war has a biological basis, it is inevitable. We disagree with this view, because a functional analysis suggests that war is inevitable only when its initiators expect to benefit in terms of biological fitness. To explain our view we comment on the five propositions in the Statement on Violence, and briefly discuss the practical importance of regarding international war as both biologically based and potentially eradicable.

1. "It is scientifically incorrect to say that we have inherited a tendency to make war from our animal ancestors." We agree, but this does not justify the idea that war is connected to biology only through language. The issue is not "What behavioral tendencies have humans inherited from pre-humans?" but rather "What behavioral tendencies have been biologically adaptive (led to increase inclusive fitness) among *humans themselves* in the environments of evolutionary adaptiveness?" Studies of intergroup aggression among non-humans can shed light on this problem, in particular, by indicating the social and ecological conditions associated with different kinds and intensities of this behavior.

So far as is known, only humans, chimpanzees, and several ant species engage in *Exported Intergroup Aggression* (EIA), which we define as *the regular conduct, by breeding adults, of aggressive, physically damaging cooperative raids against adults in neighboring groups*. In chimpanzees, EIA has resulted in the extinction of a community, has been reported or suspected at two study sites involving five communities, and can be the principal mortality factor among adult males. We are currently exploring explanations for the apparent restriction, among the vertebrates, of EIA to humans and chimpanzees. Whatever the reasons, although "warfare" (if defined in terms of use of professional armies, extended military campaigns, etc.) is obviously "a peculiarly human phenomenon," the more inclusive phenomenon of EIA is found in other species.

2. "It is scientifically incorrect to say that war or any other violent behavior is genetically programmed into our human nature." This is true, and worth reiterating in light of the continued popularity, among the lay public, of the opposite view. However, the tendency to be aggressive *under particular circumstances* may indeed be genetically based. No one would claim that growing calluses on one's feet is "genetically programmed" - some people have doubtless lived long lives without ever growing a callus. But no one would dispute that genes make the *tendency* to grow calluses, nor would anyone dispute their adaptive

value.

3. "It is scientifically incorrect to say that in the course of human evolution there has been a selection for aggressive behavior more than for other kinds of behavior." This statement is somewhat unclear, and implies that its authors do not recognize the possibility that *different behaviors may be adaptive under different circumstances*. Chimpanzee intergroup raiding involves extremely aggressive behavior, but it has been observed to occur only when the attackers belonged to a community substantially larger than the community containing the defenders. This suggests that chimpanzees conduct EIA in response to perceived intergroup strength differentials, although a complete model of this phenomenon is likely to be considerably more complex. Eventually, variation in the occurrence and intensity of human warfare may be explained rather completely via natural selection theory. This explanation will almost certainly refer, not to strength of selection for "aggression" as a global trait, but rather to a cost-benefit model incorporating those features of the social and physical environments that cause individual contributions to varying levels of intergroup competition to be more or less effective means of increasing individual inclusive fitness.

4. "It is scientifically incorrect to say that humans have a 'violent brain.'" Like proposition 2, this is true and bears repeating, but does not address the issue of what sequences of (ontogenetic and contextual) environmental inputs *do* serve to activate the neurophysiological machinery of aggression, and why these particular sequences have this effect.

5. "It is scientifically incorrect to say that war is caused by 'instinct' or any single motivation." We agree. For instance, our own survey of the ethnographic accounts of warfare in 75 societies shows that the motivations underlying human EIA are indeed highly diverse. But it seems likely that individuals tend to have high fitness as a result of successful war, and therefore that war is ultimately caused by a unitary biological principle, although the route through which war increases the victors' fitnesses varies considerably.

Finally, some may object that although investigating war as a biological phenomenon is not necessarily a disguised attempt to justify it, such efforts are, at best, irrelevant to the pursuit of peace. We disagree. The view implicit in the Statement on Violence, and widely expressed elsewhere, is that since war is a means of advancing its initiators' (political, material, etc.) interests, the means to peace is to convince people to renounce these interests, i.e. to sacrifice perceived attainable benefits for the sake of peace. A Darwinian view of human behavior, however, suggests that war will cease only when it is no longer cost-effective from the viewpoint of self-interested individuals. The question, then, is what political, social, and economic structures will create this condition. This is an issue for careful debate by everyone concerned about the fate of our species. Those who deny the role of biology run the risk of underestimating the difficulties.

Reply by Robert A. Hinde

MRC Unit on the Development and Integration of Behaviour
(Cambridge University) Madingley, Cambridge CB3 8AA,
England

Dear Sir,

I would be grateful if you would permit a brief reply to the comments made by Manson and Wrangham on the "Statement on Violence". Although I was in fact one of the signatories, I write here as an individual, without opportunity to consult my co-sponsors.

The Statement is directed towards countering the assertions

that biological considerations imply that war is inevitable. Manson and Wrangham seem to have taken it as implying that biologists have no role to play in the understanding of violence or war - a conclusion that would certainly have surprised the signatories. Biologists have a great deal to say about the bases of human violence - though I personally believe that the relations between biological principles and modern war between industrial states is more indirect than Manson and Wrangham imply.

Manson and Wrangham believe that natural selection has favoured 'particularly extreme forms of aggression in humans and few other species'. There is nothing more extreme than killing, and that is not uncommon in animals. But this remark by Manson and Wrangham could easily be taken to imply that natural selection has favoured the use of weapons of mass destruction, and could hinder the search for world peace.

Manson and Wrangham argue that the Statement "follows traditional thinking in suggesting that if war has a biological basis, it is inevitable". This is precisely what it does not say. Rather it argues that the genes, neural apparatus etc. which make war possible do not make it inevitable.

Having set up this windmill, Manson and Wrangham tilt at it "because a functional analysis suggests that war is inevitable when its initiators expect to benefit in terms of biological fitness". The question arises, however, is their "functional analysis" ubiquitously applicable? In particular, does modern industrialised war follow precisely the same rules as tribal conflict and individual aggression? I think not, though this does not mean that biological considerations are irrelevant to its dynamics.

Manson and Wrangham agree with the Statement that we have not inherited a tendency to make war from our animal ancestors, but go on to say that this does not justify the idea that war is connected to "biology" only through language. However, this "idea" does not appear in the Statement. The Statement argues that language is important in modern war on quite other grounds, and nowhere says that the connection is "only" through language.

Manson and Wrangham note that the important questions concern adaptations among "*humans themselves*" and not our prehuman ancestors, but then advocate studies of non-human primates. I repeat, the Statement is in no way to be construed as attacking the basis of the interesting research conducted by Manson and Wrangham and their colleagues. How much studies of chimpanzees can tell us about modern war between industrialised nations is another issue.

Manson and Wrangham set up the category of "Exported intergroup aggression", as more inclusive than human "warfare", and say it "*is found in other species*". The Statement certainly does not disagree with this almost tautologous view.

The Statement argues that the action of natural selection on aggressive behaviour is paralleled by its action on other kinds of behaviours, implying that humans have the potential for prosocial as well as aggressive behavior. For reasons that are not apparent to me. Manson and Wrangham interpret this as implying a failure to recognize the importance of selection for the use of different behaviours under different circumstances. Of course cost/benefit analyses may throw light on the evolution of human propensities. They may also illuminate the behaviours of individuals in their diverse roles (politicians, generals, combatants, munition workers, etc.) in the institution of modern war. How much light they will

throw on the institution of war itself, dependent as it is on historical and many other factors, is another issue.

Manson and Wrangham complain that the Statement does not address the intricacies of the neurophysiological machinery of aggression. This was not its concern.

While human behavioural propensities can ultimately be seen as the product of natural selection, that does not mean that all human behaviour is adaptive, nor that it can be simply explained in terms of a "unitary biological principle". Intra-group and intergroup dynamics and other sociological and historical complexities need to be understood if the bases of modern war are to be understood.

I submit that the view apparently held by Manson and Wrangham that human behaviour can only be explained in terms of self-interest is either a trite truism or involves a confusion between ultimate and proximate factors. The Statement is in no way an attack on the biological research of Manson and Wrangham and others like them. Indeed I feel confident that all signatories would wish for more such research. Rather the Statement is intended to attack the misuse of biological data, and to further the most important issue in the world today - world peace.

Yours faithfully,

Robert A. Hinde
Royal Society Research Professor.

MINI COMMUNICATIONS

The objective of this section is short empirical or theoretical papers which inform and would benefit from the input of peers. If readers wish to comment, write directly to the author(s).

From an Ethologist's Journal

by: William T. Bailey, Psychology Department, Tulane University, 2007 Percival Stern Hall, New Orleans, LA 70118, U.S.A.

Writers and researchers are frequently mystified as to the functions of various characteristics or behaviors in modern times. When considering functions we need to focus on not only what happens in good seasons under optimal conditions, but rather what happens in poor seasons under sub-optimal conditions. It may tell us little, for example, that one parent can successfully raise young under good conditions or when predators are few. Can she (he) do it when times are tough and/or predators are many?

We need to stay aware that how we deal with things, approach them, may well be a function of how we label them. For instance, are some "modern" practices as "radical" as they are said to be? Sometimes "radical" means may be employed to attain "traditional" goals. Contemporary society's encouraging fathers to become intensely involved in routine childcare (radical idea) may have the traditional goal of maintaining childrearing within the family - as opposed to extensive extra-familial care. Oft times parents (instinctively?) know what's best! Compare this with Blurton Jones' findings on birth spacing among the !Kung.

There are two major evolutionary tasks: 1) survival, and 2) reproduction. Pre-reproductive individuals are faced primarily

with the problem of survival; while reproductive-aged individuals must *both* survive and reproduce. Post-reproductive survival is quite uncommon among non-humans and is unlikely to be adaptive, that is, it has not been subjected to selection. Because of its common occurrence only quite recently among humans, that is likely the case with them also.

It is frequently suggested that polygyny is the species-typical form of human reproduction; the ethnographic indexing of Murdock (1949) is the most frequently cited evidence. Yet, if one is using such evidence to determine species typicality, an important distinction must be made between numbers of cultural groups and numbers of people. First, in those cultures where polygyny is socially acceptable, only a minority actually practice it; it is not statistically normal (Colson, 1958; Irons, 1983; Welch & Glick, 1981). Second, and more important, by far the majority of humans are *not* members of the cultures surveyed by Murdock. Monogamy is the (statistically) normal and normative pattern among the millions and millions of the industrialized countries (to include China); yet, they are not considered in Murdock's work (Bourguignon & Greenbaum, 1973). What counts in determining whether or not a characteristic is typical of a species is how many individuals share the trait, not how many little groups show it. Monogamy is the human-typical practice for rearing children. It is recognized, however, that this need not imply lifelong maintenance of a conjugal bond (Wittenberger & Tilson, 1980). Williams (1975) has suggested that "if prolonged pair bonding with some possibility of divorce and adultery is the rule, the distribution of female attractiveness should lie between those of fertility and reproductive value" (see also Trivers, 1972). This is supported by the findings of Mackey (1980) concerning divorce and Lockard and Adams (1981) on adult pairings.

References:

- Bourguignon, E. & Greenbaum, L.S. (1973). *Diversity and homogeneity in world societies*. New Haven: HRAF Press.
- Colson, E. (1958). *Marriage and family among the Plateau Tonga of northern Nigeria*. Manchester: University of Manchester.
- Irons, W. (1983). Human females reproductive strategies. In S.K. Wasser (Ed.), *Social behavior of female vertebrates* (pp. 162- 213). New York: Academic.
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- Mackey, W.C. (1980). A sociobiological perspective on divorce patterns among men in the United States. *Journal of Anthropological Research*, 36, 419-430.
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- Williams, G.C. (1975). *Sex and evolution*. Princeton, NJ: Princeton University Press.
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BOOK REVIEWS

Essays in Human Sociobiology, Volume 2.

Brussels: V.U.B. Press. Study Series 26, 1986. D.fl. 55 (normal edition) or 45 (pocket edition). Distributed by European Sociobiological Society, Free University MF-F301, P.O. Box 7161, 1007 MC Amsterdam, Netherlands. ISBN 90-70289-38-5. Pp.xv + 253. Edited by Jan Wind and Vernon Reynolds.

Reviewed by Ian Vine

Interdisciplinary Human Studies, University of Bradford, Bradford BD7 1DP, England.

These 18 essays stem from a meeting of the European Sociobiological Society held in Brussels during december 1983. ESS has an enviable record in so far getting proceedings of most of its meetings published - so publicising and stimulating the growing body of European work in this area. This volume deals primarily with our own species, and although squarely within human ethology several papers make little explicit reference to sociobiological theories.

Not every chapter in such a collection could be or deserves to be discussed in a brief critical notice. But the range of topics is appreciable, including biology and ethics, bio-politics, aggression and warfare, reproduction, communication, behavioural flexibility, uncertainty and control. Philosophical, socio- anthropological and psychological disciplinary perspectives are presented, as well as more narrowly biological approaches. Several papers are of especial interest, and in themselves more than justify the publication of this volume. Others are less clear, persuasive, or substantial - but by no means superfluous.

Reynolds provides a useful and stimulating overview of his book with Tanner, *The Biology of religion*. He suggests that religions vary in whether their normative injunctions about reproductive activities are broadly 'pro-natalist' or 'anti-natalist'; and this will tend to reflect ecological as well as cultural circumstances, through their role in adaptation. In those environments where reproductive success is unreliable the maximum rate of producing offspring should be found - and be religiously encouraged or tolerated (cf. r versus K selection). While carefully avoiding crude biological reductionism, Reynolds does find some supporting evidence for this thesis.

Two predominantly empirical papers are equally though-provoking in the area of mother-infant interaction. Van de Rijt-Plooi & Plooi report observations of chimpanzees at Gombe, which reveal how critical for the normal development of independence are the subtleties of how the mother progressively rejects close contact with her growing infant. Unfortunately here, as in several papers, brevity obscures some details of the authors' interpretation. Wind's study of a large sample of children 'interviewed' with their mothers present highlights a remarkably neglected phenomenon - the common tendency of mothers to make accompanying silent mouth and face movements, especially when younger children hesitate in articulating a word. Such 'empathic' reactions surely deserve more precisely quantifiable micro- analysis in view of their potential role in facilitating language development.

To this reader, the most useful contributions are a trio of theoretical reviews. Van der Dennen provides an excellent critical demolition of 'four fatal fallacies' which have pervaded sociobiological and other attempts to explain warfare in terms of individual aggressiveness and violent

tendencies. Van der Molen complements two briefer papers (by De Winter and by Pelt) on how behavioural flexibility can be achieved through recombining basic behavioural units, as humans do through 'dual patterning' of language. He offers an ingenious and exciting attempt to integrate 'open-ended learning' with Apter & Smith's novel and important 'reversal theory' of arousal states. The appeal or aversiveness of arousing stimuli is related not to simplistic homeostatic assumptions, but to being in one or other of two bi- stable states - 'telic' and 'paratelic' (being oriented to extrinsic or intrinsic goals). Most psychological theories neglect the latter, which are seen as fundamental for explaining exploratory behaviour and how flexible learning can amplify gene- based individual differences. The new approach may help to explain the rapid and dramatic adaptive advances during hominid evolution.

In an equally elegant synthesis, Kalma gives a compelling demonstration of how a disposition to reduce uncertainty pervades the traits we show while in telic states, and is the source of many of our information-processing biases. Given a need for evolutionary compromise between quick reactions and full analysis of complex data, plus the advantages of perceived predictability and controllability of one's experiences, reliance on imperfect heuristics makes adaptive sense.

My copy of these *Essays* has already proved to be rather fragile, and page 134 is blank. But otherwise they should prove to be a worthwhile investment for most researchers in sociobiology and ethology.

CURRENT LITERATURE AND FILMS

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.

Articles, chapters, papers

- Bell-Krannhals, I., & Schiefenhvel, W. (1986). Reput et de bonne reputation. Systeme de partage du yam aux iles de Tobriand, Nouvelle Guinee, Papou. *Bulletin d'Ecologie et Ethologie Humaines*, 5, 128-140.
- Bloch, S., Orthous, P., & Santibanez, G. (1987). Effector patterns of basic emotions - A psychophysiological method for training actors. *Journal of Social and Biological Structures*, 10, 1-20. (National Science Foundation, Washington, D.C. 20550).
- Bjorklund, D.F. (1987). A note on neonatal imitation. *Developmental Review*, 7, 86. (Florida Atlantic Univ., Dept. of Psychology, Boca Raton, FL 33431).
- Blurton Jones, N.G., & da Costa, E. (1987). A suggested adaptive value of toddler night waking: Delaying the birth of the next sibling. *Ethology and Sociobiology*, 8, 135-142.
- Boysen, S.T., Berntson, G.G., & Prentice, J. (1987). Simian scribbles: A reappraisal of drawing in the

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- The possibility that "labile periods at specific ages" in the mother-infant relationship are a common feature in normal development.
 - The possibility that changes in maternal behaviour in each labile period are responsible for the phenomenon of jumps in the growing independence, provided that the infants are not pushed beyond their maturational abilities.
 - The possibility that 'regression', which precedes 'mother-infant conflict', is associated with maturational changes in the infant. Several authors associated regressive behaviour with spurts in development.
- The findings are related to Bowlby's (1969, 1973, 1980) attachment theory. The possibility is discussed that attachment takes different forms over age, depending on the proximity involved and the skills available to the infant, and that it is active and present from birth onwards.
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 Contents: R. Corluy. Preface - About the Authors; G. Pennings: "Sociobiology and ethics"; W.J. Ellos: "Sociobiology and structuralist ethics"; H. Flohr: "What is biopolitics?"; T. Vanhanen: "Evolution of political systems by natural selection"; J. van der Dennen: "Four fatal fallacies in defense of a myth: the aggression-warfare linkage"; U. Melotti: "On the evolution of human aggressiveness"; V. Fuser: "Illegitimacy and infant mortality variation in Northwest Spain"; C. Moerman: "Contrasting evolution of sexual maturity and criminal law"; W. von Raffler-Engel: "Communication between animals and man"; O. Hansen: "Sociology and biological linguistics"; R. Corluy, G. Pennings, M. Vandenbosch: "The child-ape communication project. Outline of proposed methodology."; H. van de Rijt-Plooi, F.X. Plooi: "The involvement of interactional processes and hierarchical systems control in the growing independence in chimpanzee infancy"; J. Wind: "The role of parental empathy in the ontogeny of speech"; K.W. de Winter: "Evolution of behavioural flexibility and language"; F.L. Pelt: "A model for the genetics of behavioural flexibility"; P. van der Molcn: "The evolutionary stability of a bi-stable system of emotions and motivations in species with an open-ended capacity for learning"; A. Kalma: "Uncertainty reduction: a fundamental concept in understanding a number of psychological theories".

30 people. Such a small-scale conference, with all staying under one roof during two or three days, is expected to offer the optimal circumstances for profound scientific inquiry.

The meeting takes place August 28-30, 1987, in the picturesque town of Valkenburg, Netherlands (near the Belgian and German borders), and can easily be reached by train, car, or plane. Registration fee (incl. coffee & teas only) Dfl. 25.-. Participants can stay in the "Geerlingshof" Conference Center during the meeting; prices for the three days (full board) from Dfl. 125.- up. Students, unemployed, and members of the ESS and the NVFA can attend the conference at reduced rates. For further information and paper submission, contact Dr. Machteld Rocde, Dept. of Human Biology, University of Limburg, P.O. Box 616, 6200 DM Maastricht, Netherlands (phone - 31-43-888.595, or -888.588).

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 Aug. 28-30, 1987, Valkenburg, Netherlands.

THE AQUATIC APE THEORY Fact or Fiction?

The European Sociobiological Society and the Dutch Association for Physical Anthropology organize an international conference on August 28-30, 1987, on the topic of the "Aquatic Ape Theory". This theory attempts to explain how a number of human characteristics - unique among primates but common in aquatic mammals - may have evolved during an aquatic phase in hominid evolution. Among these characteristics are management of both intake and excretion of large amounts of sodium chloride; breathholding during diving, leading to sophisticated voluntary breath control (allegedly without which human speech could not have evolved); loss of body hair; the emergence of bipedalism; our relatively thick subcutaneous fat layer; coital position; etc.

As to the size of the meeting, about six proponents are expected to participate, among whom Elaine Morgan, Mark Verhaegen, Derek Ellis and Desmond Morris, and about seven opponents, next to an active audience of about

ISHE members made the following presentations at the biennial meeting of the Society for Research in Child development, April, 1987, Baltimore, MD.

Abramson, Lauren. (Psychology, Wayne State University, Detroit, MI 48202). Developmental and social significance of face touching during interviews.

Alley, Thomas. (Psychology, Clemson University, Clemson, SC 29631). Discussion on a symposium "Aesthetic perception during infancy".

Bailey, William. (Psychology, Tulane University, New Orleans, LA 70118). Infancy to age five: predicting fathers' involvement.

Dolgin, Kim Gale (Psychology, Ohio Wesleyan University, Delaware, OH 43015). The development of ability to recognize emotions in melodies performed by male and female voices.

Dolgin, K.G. Preschoolers' and kindergarteners' ability to interpret four linguistic utterances.

- Elias, Marjorie. (Bradley Hospital, 1101 Vetran's Memorial, East Providence, RI 02915). Effects of nursing and response patterns on early infant crying.
- Fogel, Alan. (Dept. of Child Development & Family Studies, Purdue University, West Lafayette, IN 47907). Mother-Infant face-to-face interaction: Japan and the U.S.
- Fogel, A. Babies and pets: age and sex differences in children's expressions of nurturance.
- Greenberg, Mark T. (Psychology, NI 25, University of Washington, Seattle, WA 98185). Communication, cognition, and emotional development in deaf children: the Paths Project.
- Grossman, Klaus. (Psychologie, Universitat Regensburg, D-8400 Regensburg, Federal Republic of Germany). Discussion on a symposium "Quality of attachment: culture specific or universal?".
- Jones, Diane Carlson. (Psychology, Texas A & M University, College Station, TX 77843). Distinctions between friends and acquaintances among elementary school children.
- Lytton, Hugh. (Dept. of Educational Psychology, University of Calgary, Calgary, Alberta, Canada T2N 1N4). Direction of effects in child socialization with particular reference to conduct disorders: an overview.
- Moran, Greg. (Psychology, University of Western Ontario, London, Ontario, Canada N6A 5C2). Using sequential analyses to quantify maternal responsiveness in early mother-infant interactions.
- Moran, Greg. Imitation of facial movements during face-to-face play by mothers and their three-month-old infants.
- Olson, Karen L. (Psychology, Wayne State University, Detroit, MI 48202). Adult judgements of infant vocal and facial expressions of emotion.
- Platzman, Kathleen A. Psychiatry, Emory University School of Medicine, Box AF, Atlanta, GA 30322). Thirty-day follow-up of infants prenatally exposed to alcohol in a low SES, high-risk population.
- Segal, Nancy L. (Minnesota Center for Twin & Adoption Research, 75 East River Road, Minneapolis, MN 55455). The twin children of the Holocaust: a timely tragedy.
- Smith, Martin. (Psychology, Brock University, St. Catharines, Ontario, Canada L2S 3A1). Parenting as investment: converging perspectives from biology and economics.
- Thelen, Esther. (Psychology, Indiana University, Bloomington, IN 47405). Interlimb dynamics: ontogenetic and adaptive strategies for walking.
- Tremblay, Richard E. (Ecole de Psycho-education, Universite de Montreal, 750 Boul. Gouin Est. Montreal, Quebec, Canada H2C 1A6). The hidden persistence of aggressive behavior in boys: the advantage of a double follow-up.

Wille, Diane E. (Division of Social Sciences, Indiana University Southeast, 2401 Grant Line Road, New Albany, IN 47150). Prematurity, mother-infant interaction and attachment.

Structural Equation Modeling

In recent years there has been increased usage of Structural Equation Modeling by researchers in a number of the behavioral sciences. Like most innovations in methodology, more people have heard of SEM than understand or use it. In an attempt to familiarize more developmentalists with this procedure, the editors of *Child Development* have published a series of articles on SEM in a recent issue (58(1), Feb. 1987). You might find this resource useful. William Bailey.

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