

# Human Ethology Newsletter

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## Contents

<b>Society matters</b>	<b>1</b>
<b>Mini Communications</b>	<b>3</b>
Dominance, Submission and love, By Eibl-Eibesfeldt/An Ethological Conceptualization of Learning, by Jesness/The Great Encephalization, by Calvin/From an Ethologist's Journal, by Bailey.	
<b>Book Reviews</b>	<b>9</b>
The case for animal experimentation. An evolutionary and ethical perspective, by Fay/Human birth: an evolutionary perspective, by Mackey/Vaulting Ambition: Sociobiology and the quest for human nature, by Vine.	
<b>Current Literature and Films</b>	<b>14</b>
Articles, Chapters/books/papers.	
<b>Bulletin Board</b>	<b>17</b>
Defeat of Darwin/Basic Issues Forum on Territorial Rights/Call for Papers/Postdoctoral fellowships in human and primate development/Bitnet, Psychnet/Book reviewers needed/Current literature listings.	

## 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 in the column inside the backpage.

Submissions in any legible format are acceptable as long as these are in English. Floppy disks containing 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.

## SOCIETY MATTERS

### New Vice-President

Robert Adams was elected to Vice-President at the General Assembly of the ISHE in Williamstown MA., U.S.A., June 25, 1987. Congratulations Bob. There is no need to introduce him, since he served the Society so well as the former treasurer/newsletter editor.

### New French Book Review Editor

Jean-Claude Rouchouse has agreed to be the new French book review editor. He is the founder and president of the association ADRET, the French association for the development and broadcasting of research in the field of human ecology and human ethology. His main interests are man-environment relations, human ethology and town-planning, and child development. He is also teaching biology and human ethology in Massokinesitherapy schools.

### Call for English Book Review Editor

I am still looking for an English language Book Review Editor on the European side of the Atlantic Ocean. Ian Vine was so kind to stay on for another year, but the end of 1987 is approaching fast. Please, feel free to contact me.

### Questionnaire on Training Programs in Human Ethology

The Society will publish a listing of training programs in Human Ethology. A special issue of the *Human Ethology Newsletter* combined with a publication in the journal *Ethology and Sociobiology* will be produced to inform prospective students where they might train, receive advanced degrees, etc. if they are interested in Human Ethology. These programs need not be formal programs as such but might include, for instance, professors who present course work and research experiences from an ethological perspective.

If you are interested in being listed, fill out the enclosed form and return it to the editor of the Human Ethology Newsletter before November 1, 1987. In small departments, one person (the chairman, graduate coordinator, or knowledgeable faculty member) may be able to provide all the requested information. In larger departments individual responses may be more appropriate. Don't forget to provide information on each faculty member interested in human ethology and don't assume that your colleagues will provide information about you. We would rather have redundant information than no information at all.

#### Instructions for the form:

Most of the form should be self-explanatory, but some items require further explanation. The top portion of the form requests information about your department/program, and is relatively straightforward. The bottom portion requests information about you. For the item labelled "General Research area" please write in the terms *best* describing your research areas choosing 5 from the also enclosed list of *Subject areas in Human Ethology*. For the "Specific Research Specialties", indicate your specific research interests in as much detail as you wish using any phrase you wish. For the "Age Range Interest" please write in the code numbers from the *Age Range Interest* list to indicate which ages you are interested in.

Michael McGuire and William Bailey were most helpful in producing the questionnaire form.

## 1988 and 1989 ISHE Meetings

The 1988 ISHE Meeting seek to meet along with the American Association of Anthropology from November 16-20 in Phoenix, Arizona, U.S.A.

The 1989 ISHE Meeting will occur without the presence of a larger organization at Edinburgh, Scotland. Tom Pitcairn has agreed to be the host. It will begin either one day after or 6 days before the meeting in the Netherlands (approximately July 20-25, 1989) of the International Ethological Conference.

## Announcements

John Ross is editing the 1986 and 1987 Human Ethology Abstracts. The 1985 Abstracts (Volume VII) are now in print. Hereby he places a request for these years' abstracts. 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, USA. All years' back issues of the Abstracts, an excellent reference body of thousands of studies, are available for a nominal sum.

The Jemez Springs Symposium "Adult human sexual behavior with children and adolescents" was held June 29 - July 3. It was sponsored by ISHE and the Servants of the Paraclete, and it was organized by Jay Feierman. Herman Dienske promised me a report of the meeting for the December issue of the newsletter. A summary of Eibl-Eibesfeldt's paper is published in this issue.

The journal *Zeitschrift für Tierpsychologie* has been renamed *Ethology* and welcomes human ethology manuscripts. It is unusually excellent in its technical quality of printing.

## Becoming Evangelistic

In the March 1987 issue it was said that 350-400 members are desirable in order to sustain our organization. Each member was asked to try and find one colleague to join the ISHE. This plea to become evangelistic had its effect. Now we have approx. 300 members. This increase is encouraging, but far from enough. Therefore, three more actions are taken to encourage members to bring in new members.

First, from now on everyone will receive two copies of each newsletter in order to give the second to a colleague to introduce him/her to the Society and the Newsletter.

Second, from now on a "gift subscription form" is enclosed in every newsletter that enables the reader to send a subscription of the Newsletter as a gift.

Thirdly, hereby a request is made for "permanent" volunteers to take a pile of Newsletters to meetings of allied interests that they regularly attend. Thus, they can make the newsletter available at the "Book display". Please, contact the Editor.

## Dues for 3 years

At the General Assembly of the ISHE last June in Williamstown it was decided that members who send one cheque for 3 years may pay only \$25. Payment for several years is strongly recommended. It is easier for you and better for the Society.

Personal cheques from the U.S.A., and from the U.S.A. only, should be sent to Dr. Jay Feierman, Membership chair ISHE, Vista Sandia Hospital, 501 Alameda Blvd. NE, Albuquerque, NM 87113.

All other cheques should be sent to Dr. Herman Dienske, Treasurer ISHE, Primate Center TNO, P.O. Box 5815, 2280 HV Rijswijk, the Netherlands. Please, make sure that this is a pre-paid cheque. Otherwise, the bank is to verify whether your account has the money, which costs the Society no less than 4.5 dollars per cheque.

The best way is to purchase an International Money Order and send it to Dr. Herman Dienske. Preferably you should pay in Dutch guilders, otherwise this greedy bank subtracts one guilder for the currency exchange. The exchange rate varies. Although you formally have the right to use the exact exchange rate (under the condition that you have avoided the above mentioned costs), an annual dues amount of Dfl 25.- or Dfl 60.- for three years is recommended. This is slightly more than the present value of the dollar, but the dollar may go up again.

Non-Dutch who pay per giro (to the four digit number 9013, Algemene Bank Nederland, P.O. Box 50 000, 2300 HA Leiden, The Netherlands) should also send a brief note to Herman Dienske that they did so, as the giro does not inform the bank about the name of an outlandish payer. This may be a last remnant of the strong competition between the Postbank and the other banks in the Netherlands.

Please send your dues as soon as possible. If we can build up some reserves, the resulting interest will be a very welcome addition to the somewhat precarious financial state of the Society.

## 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).

### Dominance, Submission and Love: Sexual Pathologies from an Ethological Perspective

*Summary of a paper presented at the Jemez Springs Symposium "Adult human sexual behavior with children and adolescents", June 29 - July 3, New Mexico.*

by: Irenäus Eibl-Eibesfeldt, Forschungsstelle für Humanethologie am Max-Planck Institut für Verhaltensphysiologie, D-8131 Seewiesen, West-Germany.

We tend to associate sex with love and that is certainly a correct perspective for human beings. But we should be aware that sex without love or tenderness does exist and is part of our archaic vertebrate heritage.

In fish and reptiles, sexual behavior is determined by the mechanisms of domination and submission. In the marine iguana, for example, the courtship of the male consists of a threat display of the same type as demonstrated in agonistic encounters with rivals. Receptive females respond by submission, lying flat on their belly which invites copulation. This pattern of agonistic sexuality is thus characterized by a male dominance sexuality and female submission sexuality. The same pattern, in principle, is found in most of the fishes where individual interaction takes place in order to achieve fertilization. Motivational analyses of cichlids demonstrated that flight tendencies inhibit male but not female sexual responsiveness whereas the opposite holds true for aggressive tendencies which inhibit female sexual responsiveness (Oehlert 1958).

In birds and mammals this archaic agonistic sexuality is superimposed by a phylogenetically new pattern of affiliative sexuality which in some, including man, gave rise to a love relationship based upon individualized bonding.

Its origin can be traced to the evolution of maternal behavior, which gave rise to caretaking behaviors (grooming, feeding, protecting) of the mother, and infantile behaviors which release them (Eibl-Eibesfeldt 1972). These mother-child signals enable birds and mammals to act friendly toward each other. No reptile is able to act tender toward a conspecific. There is no grooming, no mutual feeding and no embracing. Only after the invention of maternal behavior such patterns of caretaking and the infantile appeals which trigger them became available to serve in a more or less derived form to establish and strengthen a friendly bond. Courting sparrows approach their mate, begging with gaping and wingfluttering, like a nestling. In addition, the evolution of maternity gave rise to the capacity to form an individual bond that is to love. Love sexuality, characterized by tender behavior and in many birds and in a fair number of mammals by an individual bond, is thus an evolutionary new acquisition.

The archaic layer of agonistic sexuality did not completely vanish, however. Males, birds and mammals display a lot and often dominance is achieved. Mounting often became ritualized

into dominance display and female postures of presenting into rituals of greeting. Thus, even male baboons greet highranking individuals by presenting (Wickler 1967).

Derived from male sexual behavior are a number of dominance displays called "Phallic displays". In quite a number of monkeys it has been observed that males sit guard to protect their group from intruders, thereby displaying their genitals. If foreign group members, nonetheless, approach, males display with erection and mounting intentions. The threat is derived male sexual behavior and mounting is often used as rank display, particularly in primates. In man, phallic displays appear in the products of apotropaic (evil repelling) art. Phallic figurines are used as guardians in many cultures. In our culture we find them in romanian and gothic churches (Eibl-Eibesfeldt 1986, Wickler 1966).

In man such linkage between dominance and sexuality definitely has a hormonal base. Mazur found that in male tennisplayers the plasma testosterone level raises significantly after victory and drops after loosing. Male medical students, which pass examination, successfully experience a similar raise of the plasma testosterone level (Mazur and Lamb 1980).

In Rhesus monkeys, loss of rankposition causes a significant drip of the plasma testosterone level. Dominance, thus is linked to male sexuality, controlled and balanced, however, in a normal sexual relation by love sexuality.

Many of the pathologies of sexual behavior observed can be explained as phylogenetic regressions to the archaic agonistic sexuality. Thus, one form of male homosexuality, which is characterized by the frequent change of partners indicating a lack of bonding and by clear exhibition of dominance, could be an expression of the atavistic form of dominance sexuality, for which submission is enough stimulant for sexual attraction. It is evident that homosexual males often seek children as their partners which are easily to be dominated. Dominance sexuality has its dominance lust and as a counterpart the lust of submission. I speculate that sadism, which often goes with homosexuality, and masochism are two forms of pathological derivations of these desires present in what we would call normal human beings. Exhibitionism seems to be a pathological derivate of the archaic primate phallic display.

There also seems a still existent linkage between fear and female sexual arousal, which hints towards the archaic female submission sexuality. Sexual phantasies of women often describe situations as arousing which involve domination by the sexual partner (Kitzinger 1984). Cleptomaniacs report that they experience sexual arousal and even orgasm during the act of stealing and the flight afterwards (Keupp 1971; Stoller 1975).

Our phylogenetic model contributes to the understanding of sexual pathologies in man, but it should not be taken as the sole explanatory principle, of course. Nonetheless, we can state that sexuality without love and affiliation is a pathological regression to an archaic reptile stage level of sexuality.

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## An ethological conceptualization of learning:

### Learning in terms of the interrelated development of basic capacities

by: Bradley Jesness, 4521 28th Ave. S., Minneapolis, MINN. 55406, U.S.A.

Every significant behavior change is now thought to involve learning. Learning and innate aspects of behavioral change are now conceived of as partners in the developmental and adaptational process (Gould and Marler, 1987). They are not even thought to be clearly separable at this point in our understanding of human behavior (Anastasi). Their partnership usually occurs in such an intimate and close time frame that they cannot be contrasted. With regard to the most significant behaviour changes, such as stage-like shifts in cognitive abilities, one cannot see the great extent to which each is involved, and it is impossible to say which is most important: Is whatever "pre-wiring" we have most important or is it what's acquired — that which involves interaction with the environment and at the same time between our basic "capacities" — that's most prominent? These are serious questions. And so are the more detailed questions: What is the initial expression of the most important innate action patterns? When do innate action patterns appear? If they are not all present at birth (AND I BELIEVE THEY ARE NOT), how do they manifest themselves as they emerge during ontogeny? AND: What are the basic capacities (if any) that have relatively constant characteristics or similar interrelationships across development? Which types of capacities most reflect that which is accrued via experience and with development and what is the nature of the changes undergone?

Learning, like other topics in psychology, concerns behaviors that have innate and species-specific characteristics. Learning is frequently said to be "constrained by innate factors", but as far as developmental questions are concerned, it is IN FACT defined in large part by such factors (Johnston, 1981). And, as such, it is involved in all the most significant behavioral changes. Learning as a topic involves the most microscopic look at behaviors, in the wider discussion of processes of significant

behavior change. Learning may be the most important topic by far, for environmentally-induced behavioral change certainly seems to be key to quality adaptation in all areas of responding.

Learning may be defined as changes in those adaptational processes susceptible to experience and due to changes in these processes occurring singly and/or in an interactive manner. There is no pure acquisition (reality does not just progressively impinge itself) and there are no arbitrary acquisitions. Acquisitions must be retained. Clearly there are innate and species-typical processes involved, and fortunately for the human behavioral sciences, general laws to be found.

It should not be surprising to find that it is impossible to discuss learning in any detail or with any generality without asking what basic processes are involved in the bit-by-bit behavioral acquisitions which characterize learning. How many types of processes are there and what are their basic natures? I will try to outline what I see as the basic types of processes, their basic character, and which aspects of the processes remain relatively constant and which change systematically, reflecting what in fact has been accrued.

First, the organism always has perceptual biases and response biases. These are interrelated and both change significantly during development.<sup>1</sup> These related processes precede cognition and cognitive processes, including the major aspect of cognitive processing — representation (to be discussed soon). The proper understanding of these processes (perception and response biases) can come only with proper definition. And, objective definition is obtained only when the environmental *and behavioral context* in which the important features of these processes occur have been specified. Behaviors (of the same organism) preceding and those following a behavior of concern must be identified. This will become more and more important with ontogeny and will be true of the other processes to be discussed as well.

In addition to having perceptual biases and response biases, in general, we have memory. Memory at first seems to be of the immediate and may thus be said to have just a short-term aspect. But with experience, the organism interacting in consistent manners with the environment will begin to respond to structure and systematic change in the environment. This shows recognition memory, and soon recall, both characteristics of long-term memory. This capacity, like short-term memory, is limited, BUT INDEPENDENTLY (Brainerd and Kingma, 1985). After some point, "processing space" for short-term memory little influences the processing characteristics of long-term memory, though it is also limited *at any given stage of development* (the matter of stages to be discussed soon)

This is not all that happens. New response characteristics emerge. As structures and occurrences are recognized, new aspects of stimuli are related or are related more consistently (i.e. reacted to in a "different way"). This is not arbitrary. This may be best viewed as determined by new "perceptual biases" and related response biases. The most significant perceptual shifts, I believe, are the first occurrence in, and that which sets into motion, a new developmental stage. Yet this kind of perceptual shift occurs only every so often with regard to any given set of related stimuli to which we respond (Fischer and Pipp, 1984). There are possibly as few as five stages of development in major response areas (Freud, 1965; Ginsburg and Oppen,

<sup>1</sup> I would say at the outset that I use an unconventional definition of "perceptual biases", but this would be misleading because I believe that common modern conceptualizations of the field of perception are arbitrarily (unsystematically) constrained.

1978; Jesness, 1985).<sup>2</sup> How are acquired behavioral adaptations guided in the mean time?

At this point we could type different sets of behavior and note the characteristics of their changes, BUT this would violate the standards we have set for the objective definition of a behavior-of-concern. We will be better off considering the basic processes we already have and look for further features of these that determine behavior change. One factor has to do with the fact that development of long-term memory takes time. And, the way it develops may show phases. Most important: There are aspects of what we recall which are *worth keeping conscious*. Consciousness requires response time and uses the scarce resources of short-term memory and much affects other responding. I would say this phenomenon of consciousness occurs for either of two reasons: (1) Further stimuli which are novel or of different varieties must be noted (and possibly, eventually recalled) and these are related to things already remembered (recognized or recalled) OR (2) things to be remembered *in much the same WAY* as past experiences (already remembered) will be encountered (i.e. similar environmental structures will be encountered (Griffin, 1981). (Some of (1) and (2) is probably related to the fact that some stimuli impinge on us via less salient sensory modalities or through less salient combinations of modalities. These aspects of stimulation could become conscious later yet may still be related to some basically similar type of relationship we know (and can remember) when it has been found through other modalities). The aspect of long-term memory of which we are at times able to be conscious is a broad, good definition of *representation*. The nature of representation will change much during development and some of that of which one is conscious as a child will become aspects of awareness or totally automatic in the older child or the adult. We still must include these aspects in our understanding of representation. We now need to ask what phases there may be in the development of representation, this important aspect of long-term memory and most important capacity in significant behavioral change involving experience.

First: *In a given type of circumstance* (or "set of circumstances") it may take time to usefully retain and represent all the necessary static and dynamic aspects of the situation. To say this in more reductionistic terms: It will take time for all the stimuli of different salience to occur a sufficient number of times, given our perceptual/response biases, and time for them to be *responded to consistently*. An entire phase of development within every stage could be related to such developments AND, as indicated before, such may well vary in timing somewhat based on the salience of the sets of stimuli involved in *different* circumstances. Second: Next, one's attending (and responding) selectively to certain aspects of immediate situations (ultimately related to perceptual/response biases) eventually may allow one to relate new things separated in space and time. This is another characteristic of memory and retention and eventually of representation. The latter may show two aspects: (1) an ability to imagine *sequences* of occurrences (the more important ones often involving your own behaviors or potential behaviors) and (2) an ability to see similarities across circumstances (Lucariello and Nelson, 1985). These two reciprocal aspects of memory development and representation

can result in there being a second phase during each major stage of cognitive development. This too, for adaptive reasons (and for adaptive purposes), takes time. I do not have the space to speculate on the details here. In any case, all changes in representation will be manifested by systematic alternations in perceptual/response characteristics.

Now, finally, I believe one must discuss stages. The processes of memory and perception and the response biases and differences in stimulus salience, all already discussed, cannot (I believe) account for the progressive, *hierarchical* nature of development (Bowlby, 1982). Development has some invariant stages (descriptively speaking) in which some problems involving representation cannot be understood or cannot be understood reliably. Furthermore, it is just such reliability or consistency which is necessary for the further development of long-term memory processes, including representation. How does one get such consistency, adaptively, AND what is the parsimonious outlook? My answer is that we have stages, defined by new perceptual/response biases, emerging during ontogeny. Such perceptual shifts within an adaptive behavioral complex can have powerful effects indeed, and especially so when it is proposed that the changes in learning also involve progressive memory developments (with phases). The perceptual biases, as indicated before, may differ from one set of related stimuli to another and thus the timing of stages may vary to a degree for different types of responses. It would also seem appropriate to look at this in terms of the timing of aspects of stages. Although what the "sets of related objects" are has not been well delineated and how the timing of developments may vary between them is not clear, there are indications of some common synchronies and some general (overall) stages seem to be defined by these (Corrigan, 1983). In any case, the perceptual biases trigger a series of effects, given some of the more consistent characteristics of memory, and these result in a new level of representation and consciousness of new problems. All this allows for another series of developmental changes, such as already described. It should be clear from the outline of ontogeny given above that a general principle applies to learning: Behavioral development involves selective adaptation and eventually consistency of response. A variety of experiences will, in the normal course of adaptation, all be encountered even as consistencies are found.

I believe one can point to two aspects of behavior (broadly speaking), spoken of above, which change most in their characteristics during development: (1) the set of perceptual/response biases operative and (2) the elaborateness and precision found in representation. The changes in these capacities are systematically related. A MAJOR CONSISTENCY throughout development seems to exist with respect to short-term memory. While this type of memory *may* vary with development by 20-30% in quantitative capacity in terms of the number of "chunks" that can be dealt with "deliberately" (increasing with development), this change does not seem tremendously significant (Case et al., 1982; Dempster, 1981). It is clearly not much that's most salient that we can process at one time even late in development. This is especially startling given the large quantitative differences over development in the detail we respond to and in the length of the sequences of responses we exhibit.

2 With reference to Piaget's theory, I should note that I consider his 2 phases of the Pre-Operational Period to be stages in the same significant sense as the s-m Period, the C-O Period and the F-O Period are stages.



"Quantitative capacity" may be *roughly* synonymous with what's often viewed as "working memory", if this is defined as that that we are conscious of in a given situation and at a given moment. But this has little to do with information processing overall. There is always awareness beyond consciousness (in the narrow sense) in significant situations and much processing of longterm memory (some of this related to representation) occurs outside normal awareness.<sup>3</sup>

Other characteristics of memory change in a manner adaptively congruent with changes in perceptual/response biases and with the changing nature of representation during each stage or phase. These changes should have less specific effects on significant learning and should be changes of a less radical nature. These changes will be definable in terms of the effects they have on responding.

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## The Great Encephalization: Throwing, Juvenilization, Developmental Slowing, and Maternal Mortality Roles in Prehuman Brain Enlargement.

by: William H. Calvin, University of Washington, 1543 - 17th Avenue East, Seattle WA 98112, U.S.A.

IN MERELY 2.5 MILLION YEARS several dozen ice ages occurred, the hominid brain enlarged three times, "Birth Canal Bottleneck" worsened, prenatal development slowed, and "Juvenilization" occurred. What follows is a proposed 3-part cycle which attempts to show how an interaction between: 1) precocious puberty, 2) re-enlarged stature, and 3) slowed development without similarly delayed parturition, leading to premature birth is particularly capable of being repeated many times, like a course which can be repeated for additional credit.

#### THE BIG BRAIN PROBLEM.

At stake here is an explanation for our peculiarly enlarged brains. Constraining the usual "bigger-is-smarter-is-better" theory is the unprecedented growth rate and the lack of other evolutionary examples. But in the same 2.5 million years, two unique processes operated: **prolific toolmaking** (but the toolkit stays static during much of the period of brain enlargement, suggesting toolmaking skill was not the driving force); and **squeeze-and-expand cycle** (ice sheets cover as much as 30% of the habitable land surface, but melt back every 100,000 years to allow recolonization of the land).

#### BIG HEADS KILL THEIR MOTHERS.

Any straight-forward tendency toward bigger heads per se would have been promptly edited out of the genome: during childbirth, big-headed babies tend not only to kill themselves but also their mothers. More drastic forms of natural selection are hard to imagine, as the fetus also kills off others with similar gene combinations (the mother, her future children, and sometimes prior offspring).

For this reason, variation in head size cannot have been the variable under positive selection. Yet big heads have happened, so there must have been a backdoor route to big heads that involved some way of escaping the "birth canal bottleneck". Indeed, big heads seem to come as part of a package known as neoteny which involves both juvenilization of adults and a halving of development rate.

#### WHY JUVENILIZATION???

Juvenilization is the phylogenetic version of what we see people attempting everyday: Adults who look and act younger than they really are (by the standards of ancestors), though not necessarily attaining true juvenile appearance and behavior. For example, adult chimps have the flatter face of a juvenile monkey.

Truncating development by precocious puberty is a standard way of achieving biological juvenilization. As sexual maturity puts the brakes on somatic development, juvenile anatomy and behavior are then carried over into adulthood (the salamander *Amblystoma* matures early during droughts, thus

3 Of course psychologists may develop awareness and consciousness of things not normally subject to such through unique and sustained observations. Obviously, much of this will be awareness, etc. of things as they are for the child during development and how this fits into the "bigger picture".

retaining its gills so as to remain in the water).

In primates, juvenile behaviors such as playfulness and a willingness to try out new foods might have been involved, but the rapidity of the hominid encephalization suggests that we must seek a route that may be *repeatedly* utilized for many cycles. Alas, most inventions in evolution are like courses which cannot be repeated for additional credit.

### Conserving Cortical Synapses.

Among the developmental changes in primate brains are a reduction in the cortical synapses with age. Synaptic density is highest in infants, declining about 30-50% until the loss slows to adult levels at puberty. The human curve is 2-3x slower than the monkey curves. If puberty stabilizes, then precocious puberty could conserve the more widespread synaptic connections of childhood that would otherwise be lost. What might have selected for precocious puberty among our ancestors? The rapidity of the hominid encephalization, once it starts, provides a clue.

### RATES OF EVOLUTION

The rapidity of encephalization suggests that we must look where natural selection is most severe. This is unlikely to be the East African Rift Valley — more likely is on the margins of the hominid niche, where prehumans were precariously adapted to conditions. The temperate zones (into which *Homo erectus* spread at least 1.5 million years ago) have an additional advantage: *an annual round of selection associated with winter*. In winter, hunting becomes critically important for a few months; while gathering can always serve as a backup in the tropics, an inept hunter in temperate zones is unlikely to raise a family. And while many skills are involved in hunting, the one in which we differ most from other primates is throwing ability.

Throwing requires exquisite timing, far beyond the capabilities of individual neurons and the best known way of achieving the requisite reduction in timing jitter is to synchronize large numbers of cortical neurons (see W. H. Calvin, *Journal of Theoretical Biology*, 1983), presumably by utilizing widespread cortical connections.

Since the developmental reduction in cortical synapses is achieved by withdrawal of collaterals, one might suppose that juvenilization would retain juvenile synchronization abilities over into adulthood. And thus improve adult hunting abilities, the ones which are so important for raising offspring to reproductive age.

**PUMPING THE NICHE: The Squeeze-the-Center, Expand-the-Periphery Cycle.**

Though selection for juvenilization might thus be strongest on the temperate zone frontiers, such regions would contain only a small percentage of the total hominid population and one might expect gradual evolution to be slow. Ice ages, however, provide a unique way in which the frontier population could expand to dominate the total population in just several ice age cycles.

Assume that about 15% of the total hominid population lives on the ice-age frontiers, and that every-winter selection for hunting skills has biased their gene pool to emphasize a degree of precocious puberty.

The meltback of an ice age takes about 5000 years, or 200 generations; this is so slow that no one probably realized what was happening (one would hardly expect an Oklahoma land rush).

But a little new land was exposed for habitation with each generation, and so *it was the neighbors, with their frontier gene*

*pool, which experienced the gradual population explosion*. Since the new land expanded the minimum-habitable land by more than 40%, one might expect that the frontier-type gene pool went from 15% of the constricted-population total to being about 40% of the new expanded-population total.

Interglacials last only about 15,000 years until halfway back to peak glacial ice mass, so advancing ice would soon push the new population into competition with the semi-tropical populations. Assuming that the former-frontier-type population held its own in the competition (and, being more highly selected, they probably did somewhat better), the squeezed population would have a markedly biased gene pool, now emphasizing frontier types.

During the long haul of the ice age, a new frontier population would be shaped by the every-winter selection for hunting ability. Upon the next glacial retreat at 100,000 years, it is their biased gene pool which would undergo the next slow population explosion.

And so one expects that this ice age pumping will quickly bias the main population in the tropics and semi-tropics to reflect the body styles most useful for frontier survival.

### RE-ENLARGEMENT: Why Aren't We Miniature Pygmies?

By themselves, hunting and juvenilization are an unsatisfactory explanation. Precocious puberty reduces stature, and we're as tall as *Homo erectus* was 1.6 million years ago, not what one would expect for repeated juvenilizations.

But juvenile status is only one of several determinants of stature, and selection for larger bodies might well have re-enlarged prehumans after a round of juvenilization via another route.

Indeed, larger bodies are most important in hunting, both for throwing distance and for a smaller surface-to-volume ratio (thus lengthening the time that it takes to reach a catastrophic internal temperature when subjected to environmental temperature stresses).

### SLOWED DEVELOPMENT: Why Wasn't Childhood Shortened?

The other glaring misfit is childhood: precocious puberty tends to shorten childhood by truncation, and our juvenile period is nearly twice as long as the apes', not half as long. Indeed, nearly all phases of our life span have doubled:

#### PHASES OF LIFE SPAN

	gestation	weaning age	menarche
Prosimians	0.36	0.3	2.0 years
Monkeys	0.48	1.1	3.9
Asian Apes	0.64	2.5	7.5
African Apes	0.66	4.1	8.0
Humans preagric. (0.73)		6.0	16.5

Human gestation would be 1.2 years if we were born as mature as the other apes (judging from developmental landmarks: suture closures, ability to sit upright, etc.).

This suggests that development rate has been halved, and the reason is not hard to discern: slowed development, but without much lengthening of gestation, is one way to escape the birth canal bottleneck for big heads.

The slowed prenatal development rate seems to carry over into postnatal development as well, as our lifespan is also about double that of the apes.

THE THREE-WAY SELECTION CYCLE: Repeating the Course for added credit

If hunting selects for precocious puberty, one might expect early sexual maturity to become more common (it's an inheritable variant: women who had early onset of menses tend to have daughters with early menarche). But stature suffers, and then bigger-body advantages might select for other variants controlling stature.

Note that the re-enlarged body will have a *relatively* larger head: bigger heads, relative to body size, is one way in which children differ from adults.

The birth canal bottleneck should have then operated upon variants in body development rate, unfortunately via maternal mortality. This will delay the age of puberty, perhaps back out to the original pre-juvenilization year. This re-positions the cycle so that another round of juvenilization, reenlargement, and slowing can occur if there is continuing natural selection at each stage.

Throwing has an impressively long growth curve: faster and farther is always better, and one expects hunting in temperate zones to continue to operate this three-part cycle via selecting for brain circuits associated with the juvenile brain.

Some differences from the apes cannot be explained by this three-part cycle (concealed ovulation, the descent of the larynx, and many others). But it does account for three of the major anatomical trends: encephalization, juvenilization, and slowing of development. And the principles utilized, such as the maternal mortality trap for pure big-head variants and the pumping power of the fluctuating niche, are all ones which any theory must address.

#### SUMMARY.

Major features of the enlargement and reorganization of the hominid brain can be accounted for by: 1) hunting-driven precocious puberty, then 2) bigger-bodies-are-better re-enlargement, followed by 3) maternal-mortality-driven slowing of development.

The Ice Ages play an essential role, via the fluctuating size of the hominid niche, by permitting frontier-type body styles to repeatedly dilute or displace the large central population which is not exposed to the frontier's selection pressures.

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## From an Ethologist's Journal

### Ethological Terminology: use, misuse, and abuse

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

I begin my comments this time by noting that, even among those who wear the label, there is an apparent lack of consensus as to what *Human Ethology* is and how it operates. I define Human Ethology as *the behavioral science which describes and explains human behavior*. To achieve this it observes behavior across a spectrum of conditions, ranging from the most unobtrusive — where only the presence of the observer encumbers the "Naturalness" of the situation, to highly controlled manipulative experiments.

Human Ethology further seeks to explain the history and occurrence of behavior. It attempts to unriddle the long range, phyletic (or evolutionary) history of how humans as a species acquired a behavior, and in addition, how a given individual developed the behavior. Finally, Human Ethology tries to determine "why". In an evolutionary sense this means establishing function — that is how the behavior, in the *environment of evolutionary adaptation*, contributes to survival and/or reproductive success. In proximal time, Human Ethology tries to determine the mechanisms associated with the occurrence of the behavior; the conditions under which the behavior occurs — when it will occur, where it will occur, under what environmental conditions (internal as well as external).

*Human Ethology* is subsumed by *Ethology*. It is distinguished as that part of ethology which has as its specific subject matter human behavior. Nothing about human ethology is incompatible with ethology — as a broader discipline not restricted in its subject matter. Nor in principle is Human Ethology incompatible with other behavioral disciplines (e.g. anthropology, behavioral ecology, or psychology) which are more limited in breadth of field.



My suggestions as to what characterizes Human Ethology studies are of course mere restatements of Tinbergen's deservedly famous four principles (Tinbergen, 1963). The statement as to how we observe behavior flows from the descriptions of his work (e.g., Tinbergen, 1968/1959, 1972).

*Alternative strategies.* The fact that alternative strategies are found within a species does not, ipso facto, imply that any individual, at any time actually can or does *choose* among alternatives. It is not a case of, "Here's the situation; I'll flip a coin to pick among alternative solutions"; but rather that individuals, who under certain conditions (e.g., life-stage, resources, environment), may execute one strategy will, under other conditions, perform the alternative.

Note also that an alternative strategy may well not be equivalent, that is have an outcome equal, to the other (the "optimal" strategy may not be open to you). But rather the alternative may be "optimising", that is produces *some* outcome and alleviates total failure. This is the "half a loaf of bread is better than nothing" strategy.

Now that I have coined one phrase, I'll keep the mind open long enough to coin another. It is the "any port in a storm" strategy — this is a case of "alternative" strategies. For example, it is expected that infants will form an intense, enduring "attachment" to their mother; that under stress, infants will preferentially seek comfort from their mother. Yet, it has been found that infants, if sufficiently stressed, will display attachment-like behaviors to fathers, strangers, and even inanimate comfort-objects. This behavior exemplifies the "any port in a storm" strategy. The "rules" are: (1) seek the optimal target of behavior, if the optimal target isn't available, then (2) try the next best. For the case of security-seeking, if stressed, do the optimal — seek comfort from mother; if she is unavailable (physically or psychologically), start working downward through a hierarchy of stress-reducing, solacing "figures" at the end of which is a nice, soft blanket!

*Altruism.* Much of the literature dealing with young (prepubertal) children's *prosocial* behavior is presented in terms of *altruism*. Yet, *altruism*, for ethologists, is defined in terms of an individual's *diminished reproductive success*. It seems quite unlikely that any social act by a young child — excluding life threatening behavior, results in reduced R.S. There flows from the definition of altruism a sense of the unusual, that altruistic behavior is other than what is normally expected to occur, that it requires explanation. On the other hand prosocial behavior by young children is hardly unusual (in a statistical sense). So call it what you may, *prosocial* behavior by young children is decidedly not *altruism*.

Learning to co-exist with conspecifics in one's group (broadly defined) is a paramount requirement for all group-living primates — to include humans; this is an aspect of social development. Young children, in their "prosocial" behavior, are learning to recognize "need", to identify those individuals (and *types* of individuals) who are likely to share with them, to help satisfy their needs, and under what conditions. I *VERY STRONGLY* recommend that you refrain from using the term *altruism* except in those cases where you can show (or at least convincingly argue) that an act potentially reduces reproductive success.

I do not believe that *reciprocal* altruism, as defined, exists. I am convinced that on closer examination alleged cases of it could be better explained. For instance, a commonly used example is that of a man who passing a drowning man, jumps in to rescue him at risk to his own life (Trivers, 1971). This quite obviously is a case of misdirected "kin-selection" (i.e. true altruistic) behavior. One need not invent "reciprocal" altruism

to explain it. The hero is behaving at a reflexive or pre-rational level. How often one hears or reads heroic individuals saying, "I didn't think about it." Under "expected" conditions (i.e. the environment of evolutionary adaptation) the drowning man would have been a member of the rescuer's small group and almost invariably a close relative.

*Social Darwinism.* There is no need, I trust, to belabor the point — this term is an insult to Darwin! If you can not avoid using it, at least have the courtesy to say "*so called* social Darwinism". For further comments see chapter 3 in Gould 1977.

In closing, I very much appreciate the comments some of you have sent me and would welcome any comments on this or future communications.

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## BOOK REVIEWS

### The case for animal experimentation. An evolutionary and ethical perspective.

Berkeley, Los Angeles, and London: University of California Press, 1986, 262 pages. By Michael Allen Fox.

### Reviewed by Brian Fay.

Wesleyan University, Middletown, Connecticut, U.S.A.

This book argues for what it calls the "middle ground" in the controversy about the use of animals for laboratory experimentation in biomedical and behavioral research. It claims to show that such use is ethically permissible but that it should be carried out in a humane way. This conclusion may seem boringly commonsensical, but there is nothing boring about the arguments which Fox puts forward in support of it.

Through a clearheaded examination of recent philosophical thinking about ethics in general and the ethics of animal treatment in particular, Fox develops what he calls the "anthropocentric view of morals". This is a view about both value and obligation. With respect to value, anthropocentrism claims that value is a relative property in the sense that something has value only in relation to a valuer. Moreover, it also claims that this valuer must be able to judge rationally that something is valuable, and also be able to articulate this judgment. Fox claims that these facts about value, together with

some facts about the nature of animals — in particular, that they are incapable of critical thinking — yield a rather startling conclusion: that no animal or state of an animal is valuable in and of itself. As Fox puts it: "Animals' pleasurable experiences are not valuable to them because animals are incapable of judging and articulating that they are" (27). Indeed, the only value animals have is instrumental in character: the use-value they have for rational creatures — humans and gods or intelligent aliens if they exist.

Doesn't this sound like a typical philosopher's trick? By a conceptual hocus-pocus, the seemingly uncontroversial belief that, for example, the state of being free and happy is intrinsically valuable for chimps, is shown to be fallacious. But why should we fall for this trick? Fox claims that valuers must be able to judge rationally that something has value and articulate this judgment. But why impose this condition? Think of the distinction between something's having value for  $x$ , and  $x$ 's thinking it has value. These are two different things — as shown by the fact that  $x$  can have one without the other (can value something without in fact knowing it has value for him or her, or think it has value when it in fact does not). But if having value can be separated from thoughts about having it, then why believe that something can have value only to a creature who can think about its value in a certain way?

Fox thinks that because animals have only instrumental value, it follows that there is nothing wrong about using them. But one might still wonder whether there mightn't be other moral grounds for not using animals. Fox's discussion of value thus leads into an analysis of moral obligation. This is the heart of his book. Schematically, Fox's argument goes like this: (1) Only autonomous creatures can be members of a moral community. (2) Only members of a moral community can have moral status. (3) Animals are not autonomous creatures. (4) Therefore, animals have no moral status. By having moral status, Fox means the possession of rights such that other moral agents have a prima facie obligation not to harm or interfere with. Thus, saying that animals have no moral status means that humans have no ethical obligations not to harm them. (I warned earlier that though Fox's position seems commonsensical, his arguments in support of it are not so). As Fox summarizes his argument: "If moral obligations are contingent on rights and their possession by certain beings, then since animals have no rights, humans cannot have correlative obligations to them. It follows that we have no duty in the strict moral sense to prevent animal suffering. Therefore, if it is wrong to inflict suffering on other sentient creatures, this must be for reasons other than the failure to heed or be bound by a moral obligation not to cause harm" (70).

To be fair to Fox, he quite carefully and clearly defines that he means by a moral community, autonomy, moral status, and obligation such that his position does not seem as wild as it might appear here.

But it still seems wild, nevertheless. To see this, consider the mental gymnastics in which Fox must engage to meet an obvious difficulty with his position: since human imbeciles and the like are not autonomous, they do not seem to possess rights either, and therefore we are under no obligation not to cause them harm. This is a position which Fox could embrace, but it would fly in the face of his preference for being in accord with informed commonsense. Thus he tries to show that, though they are not "full members of the moral community because they lack autonomy, they must nevertheless fall within the most immediate extension of the moral community and as such be subject to its protection" (61). Why? "In deciding on how we ought to look on all these classes of individuals, a reasonable position

to take would seem to be that here membership in our own species ought to count for something, in the sense in which a charitable attitude toward those less fortunate than ourselves, for whom we feel some especially close kinship, is particularly compelling to a morally mature person" (60).

But this is unconvincing. Why should only humans and not animals "fall within the most immediate extension of the moral community"? Because of feelings of close kinship? But many people feel closer kinship to certain animals than to fellow humans. And besides, why should feelings of this sort be given philosophical weight? It does no good to say that such feelings are "particularly compelling to a morally mature person" because this begs the question at issue, namely, of what does moral maturity consist. Moreover, just a few pages earlier Fox has been at pains to show that his view is different from earlier views about rights because he does not insist that it is being human that is essential to having rights, but rather possessing certain attributes; but when pushed to the wall, Fox now contends that "membership in our own species ought to count for something".

Furthermore, there seems something amiss in Fox's idea that moral obligation is owed only to members of the moral community. Why should obligations be limited in this way? Indeed, in the passage I quoted above, Fox himself says that imbeciles and others who are not members of the moral community but who fall within its most immediate extension should be "subject to its protection". Because creatures cannot participate in the making or the obeying of moral laws, it simply doesn't follow that there cannot or ought not to be laws governing interaction with them. Human history is filled with moral systems — think of Buddhism with its injunction to treat with respect all sentient life — which enjoin behavior toward creatures not thought part of the moral community.

Fox's position about moral obligation gives rise to a question about cruelty to animals. If, according to him, we have no moral obligation not to harm animals, what reason is there to treat them humanely? I found this part of the argument extremely difficult to follow. Fox seems to be saying that through empathy and the development of an evolutionary perspective it is evident that animals have interests and needs (though not rights), and that these interests and needs merit some consideration. As he writes: "Our ability to see animals for what they are ... can scarcely fail to lead to the recognition of a kind of obligation on our part, although a relatively weak one, to accord them humane treatment" (79-80). But why should our obligations extend only this far? Surely one of the most important interests of animals is not being killed or maimed; in responding to this interest, why should Fox say that our obligation is only to kill and maim them humanely, rather than not kill or maim them at all? And in what sense is this an obligation, given that Fox has said earlier that obligation accords only to those who have rights? Are there different sources of obligation? And if so, what is their relationship?

In the last half of the book Fox details lots of animal experiments thought to be cruel, purporting to show that, on investigation, they are morally permissible. Here his knowledge is wide-ranging and often first hand. But I do not think his judgments are compelling. All of his analyses take the general form: look at the good scientific results which came of this experiment. But this defense is inadequate. Fox needs to show that these experiments were the *only* or the *best* way to have obtained the valued information in order to show that causing great suffering to the animals involved was justified. He does not do this in any of the numerous cases he examines, though in the con-

asked of all proposed experiments.

Fox tells us that he wrote this book to counter the hysterical attacks by antivivisectionists and their fellow traveler philosophers who have argued for animal liberation and animal rights, and who have charged that speciesism is an evil akin to racism and sexism. He thinks such a counter is necessary so that scientific work can go forward and so that the energy committed to animal welfare can be better spent on ameliorating human need and suffering. But the book had the opposite effect on me: it confirmed in me a belief that the practice of animal experimentation is a greater evil than most people think, and that addressing it is by no means a waste of moral energy.

### Human birth: an evolutionary perspective.

A volume in the Aldine de Gruyter series: Foundations of Human Behavior, 239 pages. By Wenda Trevathan.

#### Reviewed by Wade C. Mackey

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Although this book is not aimed exclusively at human ethologists, it is certainly within our domain of interests. Trevathan, a member of ISHE, is a physical anthropologist, a discipline which traditionally concentrates on morphology and physiology in its instruction on hominids. Accordingly, when she focuses an ethological (read: behavioral) lens she will reflect our penetration into the market-place of ideas wherein physical anthropologists, among others, will search for supporting data and theory. Trevathan's effort thereby becomes a mirror by which we can view *what* we Human Ethologists have communicated to cousin disciplines and *how* clearly we have done so.

There are two main parts to *Human birth*: (1) an exposition on the anatomical and physiological aspects of the birthing process for *Homo* — in the double contexts of primates in particular as well as the zoological kingdom in general (Chapters 1,3,4,6,7), and (2) a behavioral study of the birthing process (n=110 women-neonate dyads) in a mid-wife birthing center (chapters 2,5).

*Anatomy and Physiology of Human Birthing.* Trevathan systematically weaves together the hormonal, morphological, and physiological components of the gestation and birthing processes in a complete and concise manner. The form and magnitude of the components are placed squarely within the evolutionary process. This rich and thorough presentation serves as a starting point for the development of an ethogram/biogram for human ontogeny. Chapter 7 is an excellent synopsis of the preceding material. In this chapter, Trevathan delineates five major transitions which deeply and irrevocably affected the mother-newborn relationship: (1) bipedalism, (2) secondary altriciality, (3) obligatory midwifery, (4) the agrarian revolution, and (5) the technological innovations of the industrial age.

*Behavior of Human Birth.* Trevathan next ventures to the highly emotional, often stressful environment of the delivery room. Not just any delivery room, but one staffed by midwives with a clientele primarily of poor, non-English speaking women — a "not for the faint of heart" endeavor. There (in the field) she conducted detailed observations of mother-neonate interaction. The observations were along tactile, visual, auditory, and olfactory dimensions.

differences with age, gender-of-child, and a host of socioeconomic indices as important independent variables. The specific impact of genetic information upon *Homo* is equivocal and, as Trevathan advocates, other culture-types need to be sampled in a similar way.

At this point in her analysis, Trevathan begins to leave the mainstream of her individual expertise and, like the rest of us, has to rely on the efforts of other, collateral disciples to enable her to develop a credible and substantive argument. The content of human ethology becomes a key component in her paradigm.

The extent to which the structure of Trevathan's argument may be wobbly would be paralleled by a failure of our message in crossing the boundary between academic disciplines. There are three categories for which our input may have been useful and three categories to which our message has not, as yet, come through loudly and clearly.

*First the good news.* (1) Trevathan, more so than most, recognizes both the need for cross-cultural data on a single topic and the problems with cross-cultural data. She consults the Human Relations Area Files to see how other cultures handle the birthing experience, while (2) she clearly acknowledges the difficulty in finding the degree of correspondence between the "norms of the ideal" versus the "norms of the real", and (3) she urges behavioral hypotheses (which can be falsified) to be tested against behavioral outcomes. For example, she hypothesizes that midwifery — assistance in the birthing process — is an excellent candidate for a species-characteristic trait, "I shall further argue that birth with manual (but not medicinal) assistance is *natural* in our species" (p. 86).

*Now for the bad news.* Three important arrows in the human ethology quiver include: (1) a rejection of Lamarckism and an acceptance of *differential reproduction success of different genotypes*, (2) the use of ease-of-learning to bridge the gap between arbitrary socialization traditions and instinct, and (3) the use of strong empirical/statistical techniques to identify central tendency and to isolate cause-and-effect relationships. To coin a phrase, we have not insinuated ourselves into everyone else's paradigm.

*Lamarck.* Trevathan plus the three series editors: Konner, Hardy, and Wrangham, have Lamarckian concepts sprinkled throughout the book. For example, "Once hominids began to use fire to cook food and tools to prepare it, large muscles for chewing food by older infants were no longer necessary" (p. 143), and "In fact, bipedalism may have come about in part as the need arose for the mother to carry a more helpful infant" (p. 144). Although Trevathan and other physical anthropologists who write in the same style may be aghast at the suggestion that they are Lamarckian, their writing *does* convey that concept. The rookie reader could easily be confused by the choice of words — shortcuts or otherwise.

*Learning.* Learning is unambiguously inserted in opposition to "instincts"/"biological inheritance" rather than as a potential marker of species adaptation. For example, "In fact, while a few of the very basic interactive behaviors may be instinctive, most proximity-maintaining behaviors are undoubtedly learned and are, thus, environmentally labile", and "On the other hand, most of human behavior is a result of learning, and at least in the later stages of infancy, maternal attitudes toward offspring vary extensively from one culture to another" (p. 39). If physical anthropology has not absorbed the phylogenetic linkage with learning, then there is diminished hope for family studies, education, and sociology.

*Statistics.* Whereas Jane Goodall can successfully replace

Geographic photography, the rest of us are not so fortunate. The quantitative analysis of the 100 women was somewhat erratic and uneven: degrees of freedom were not always given, "differences" between groups were discussed which at the end of the paragraph were noted to be *not* significant (p. 165),<sup>4</sup> a chi-square value was given as significant (p. 62), but upon my checking a table of chi-square critical values found it was not, a putative difference between groups was not supported by a test of significance in either the referent text or the referent table (p. 190, Table 5.6.).

Although clearly superior to the prose of Kiplingesque *Just so stories*, Trevathan's extremely hard-earned and valuable data deserved a more finely honed critique before the editors gave their "go ahead" to the printing presses. Aside from these three points whose responsibility is diffuse, *Human birth: an evolutionary perspective* will be a readable and very useful addition to individual and institutional libraries. In addition to its scholarly merit, the book is well produced with extensive and illustrate graphics.

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### Vaulting Ambition: Sociobiology and the quest for human nature.

Cambridge, Mass.: MIT Press, 1985. U.S.A.: \$25, Europe: \$33.95 or £24.95. ISBN 0-262-11109-8. Pp.xi + 456. By Philp Kitcher.

#### Reviewed by Ian Vine

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In his preface, Kitcher expresses his aim as "to explain as clearly as possible what sociobiology is, how it relates to evolutionary theory, and how the ambitious aims that have attracted so much public attention rest on shoddy analysis and flimsy argument" - hoping to "put an end to the controversies that have beset sociobiology" (p.ix). No single author can achieve to latter goal; but otherwise his success is remarkable, and especially given his evident respect and sympathy for Richard Lewontin - one of the authors of the far from impartial volume, *Not in Our Genes*.

In that book, sociobiology was pilloried as yet another worthless biological determinism - not yet scientifically inadequate, but both stemming from and functioning to support a conservative ideology that uphold political reactionaries and exploitative social institutions. The ideological stance of Rose, et al. presumably contributed to the same polemical rhetoric, biassed reasoning, over-generalization, selective use of evidence, neglect of competing hypotheses, and sheer misrepresentation that they condemned so strongly in their opponents. Since Kitcher accepts the ideological dangers of what he labels 'pop' sociobiology, and is at least as intolerant of its sloppy theorizing, wild assumptions, and cavalier approach to empirical evidence, the relative impartiality of *Vaulting Ambition* is welcome and impressive. Lewontin has hailed it as "the last word on the subject of sociobiology", according to the dust-jacket. But that it is *not* - partly, I shall argue later, because in

one sense even Kitcher's clarity of thought has been clouded by the near-hysteria of anti-sociobiologists.

Throughout this long, densely argued, but lively and clearly written volume, evolutionary arguments are dissected in detail. But its importance does not depend upon any major new critical insights. Almost all the general problems of sociobiological theorizing, and most of the points of criticism of specific hypotheses, have been identified and discussed by other authors, as he admits. What he does so well is to deploy his philosopher's skill at teasing out the logical structure of evolutionary analyses, as well as assumptions which may be only implicit or forgotten when stating the conclusion of an argument. He places no reliance upon glib but logically defective blanket criticisms which abound in anti-sociobiology - like making free with pejorative labels, setting up crudely stereotyped easy targets, or arguing *ad hominem*. He simply takes on authors like Wilson on their own terms, demonstrating that an author's professed aims and assumptions, argumentative strategy, and use or misuse of theory and data simply fail to justify the conclusions drawn. Kitcher is the paragon of rigour and thoroughness - giving each approach or claim a reasonable run for its money, before exposing each weakness which an impartial reader is typically bound to acknowledge. If his scorn for some of the most confused and irresponsible deliverances of pop sociobiology is not always concealed, it is at least held back until he has made some plausible attempt to make the claim in question philosophically and scientifically coherent.

Kitcher's biological erudition is as impressive as his analytic and expository ability. He carefully outlines the background principles of natural selection, population genetics, and other elements in various styles of evolutionary story-telling. And he gives a moderately detailed account of the basic theoretical innovations from which sociobiology grew - including the powerful addition of game-theoretic ESS analyses of adaptive strategies. Stressing the idealized nature of many of the assumptions in the basic models, and the actual complexities and uncertainties in the relations between genotype and phenotype, selection and other evolutionary factors, behaviour and inclusive fitness, and the like, he shows how cautiously the sociobiologist must proceed. He highlights both familiar and unfamiliar difficulties in constructing any convincing 'Darwinian history' for a trait as a species-specific product of fitness-optimizing selection for facilitatory gene alleles. Especially with more intelligent species like ours, it is essential to consider and discriminate empirically between rival explanations of any trait's proximate causation, adaptive functioning, and evolutionary origins. We often lack the information to do this with confidence.

Within animal behaviour, the author finds that the best research investigations of sociobiological hypotheses survive evaluation against his strict criteria of scientific adequacy - although careless and unsubstantiated adaptationist stories are not hard to find as well. But his primary target is naive extensions of the basic principles to the human case. He readily identifies the fragile logic and empirical difficulties of Wilson's original explanatory strategy ('Wilson's ladder'), and castigates Wilsonians for the typically sloppy and quite premature way in which they are prone to attribute human traits to gene selection. But he goes on to find many similar faults in the more sophisticated pop sociobiology of Alexander and others, who may not

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4 Editor's note: some statisticians recommend that procedure. That is, first report *what* you found (e.g. means), and then report the *outcome of your statistical analyses* (e.g. if differences are significant). I favor this approach. Also one has to be particularly attentive as to whether the characteristics (e.g. group means) being discussed are "different" or "statistically different". WTB

claim any tight genetic control of a trait, but are still too ready to assume that phenotypic behaviour is normally constrained in fitness-maximizing ways, whatever proximate causal mechanisms are involved. Kitcher's rather thorough tour through pop sociobiological territory takes him into most of the cherished areas for which strong explanatory claims have been made. And in the various versions of the accounts offered - for incest avoidance, sexual double standards, female infanticide and so on - he finds serious flaws which make them at worst quite implausible, and at best unproven.

In one of the most useful chapters he applies his mathematical expertise to the pretensions of Lumsden & Wilson's 'gene-culture co-evolution' approach - and effectively exposes their fancy formalizations as largely trivial or spurious in their explanatory import and superficial sophistication. Here in particular, it is apparent that the book is not - as Kitcher suggests - self-contained and accessible to all "interested readers without any background in biology or the philosophy of science" (p.ix). Although, throughout, some of the more advanced and detailed symbolic derivations of predictions are set aside as boxed 'technical discussions', readers lacking his own mathematical facility will find some analyses heavy going. Surely more of the formal treatment could have been separated off, making it easier to get a basic grasp of the accompanying qualitative arguments? Finally, Kitcher offers an illuminating if fairly brief examination of relations between our subjective autonomy of action and bio-social causal influences on our psychology, before going on to the obligatory demonstration of the confusions inherent in the Wilsonian promise to 'biologize ethics'.

This thoughtful and fairly comprehensive exposé of the errors into which human sociobiologists can and readily do fall is one which everyone concerned with evolutionary approaches to our behaviour must read with care and be ready to learn from. Anyone exploring sociobiological hypotheses about any aspect of our social behaviour could do no better than to start by noting and responding to each of the fallacies and dangers in existing formulations that Kitcher compiles here. Yet these critiques are *not* the 'last word' on human sociobiology at all. Indeed, he rarely suggests that specific hypotheses are irretrievably disposed of when their logic and typically poor fit with empirical evidence is exposed. Nor does he rule out in principle the strategies of explanation which have shown considerable promise in dealing with some aspects of the behaviour of our animal cousins. His aim is overtly just to show that the peculiar complexities of our own species demand quite a different level of caution and attention to detail than that typically shown by pop sociobiologists, who have simply neglected the canons of scientific rigour quite shamelessly in many cases.

This book may well be the definitive statement of what has gone wrong with human sociobiology, in terms which are presented rationally, clearly, and in one sense fairly. Yet its impartiality is limited by the nature of Kitcher's project, which itself presumably reflects his sympathy for much of what motivates more extreme anti-sociobiologists. Since he finds no good and defensible applications of the ideas to human beings, he gives the impression - in my view wrongly - that there is no research that is relatively free of the sins of the popularizers. More seriously, his relative silence about the biased assessments and even intellectual dishonesty of most of the radical critics gives the false impression that alternative, non-sociobiological, ways of accounting for apparently natural traits are not at least as vulnerable to a skeptical analytic critique of *their* assumptions, evidential basis, and so on. And his thorough and wide-ranging attempts to tease out every weakness in sociobiological storytelling is not matched by his own efforts

casual gestures towards remedying the imperfections of an author's reasoning, or gathering better supporting evidence from relevant literatures.

It may be fair to claim that it is sociobiologists themselves who must tackle such tasks, and persuade critics of the promise of the general paradigm. Nevertheless, Kitcher's knowledge and skill could be valuably applied to the more positive task of advocating the more promising directions for a moderate and realistic human sociobiology, recommending ways of reformulating defective hypotheses in more plausible terms. There is little systematic effort to guide the 'quest for human nature' along more fruitful pathways which still acknowledge its partial biological structuring. There are only vague gestures towards the needed integration between sociobiological models and the meticulous examination of social psychological processes which reflect the interactions between our biology, cultural environment and personal intelligence. In fact psychological data that suggest endogenous biases in how we react to social stimulation gets virtually no mention in Kitcher's volume.

In the preface, he suggests that his analysis "may even help us to envisage the *future* development of an approach to human behaviour that makes genuine use of biological insights" (p.ix). It might have really done so, had it been presented in a more explicitly constructive way. Instead, this is the definitive exercise in clearing the ground of pop sociobiological excesses, before we can aspire to a second and altogether more cautious and responsible stage of formulating and testing new theories of a more complex kind. The painstaking work of tackling this task still lies ahead - and urgently awaits the kind of explanatory framework which Kitcher hints at in an unfocussed and largely incidental way.



## 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

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## Books

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## Papers

These papers were presented by members of the Society at the annual meeting held June 21-26, 1987 at Williamstown, MA in conjunction with the Animal Behavior Society.

- Abramson, L., & Stettner, L.J. Observations on forms and functions of face touching in humans. (Wayne State University).
- Adams, R.M., & MacDonald, J.D. Interobserver reliability checks: frequency of use in animal behavior and psychology. (Eastern Kentucky University, Psych. Dept., Richmond, KY 40475, U.S.A.).
- Boatright, S.L. Chimpanzees, monkeys, mirrors and "self-recognition" revisited. (Hunter College).
- Gustavson, A.R., & Dawson, M.E. (University of Southern California) and Bonnet, D.G. (University of Wyoming). Androstrenol, a putative human pheromone, affects human (*Homo sapiens*) male choice performance.
- Brown, R. Olfactory recognition of congenic strains of rats (*Rattus norvegicus*). (Dalhousie University).
- Burghardt, G. Defensive responses and predator monitoring in hognose snakes. (University of Tennessee).
- Cox, C. Thermal load as a factor in weaning of rats. (Dartmouth College).
- Demarest, J. Dopaminergic involvement in sudden death during tonic immobility. (Monmouth College).
- Figler, M. Resident and intruder size difference effects on territorial behavior in convict cichlids (*Cichlasoma nigrofasciatum*). (Townson State University).
- Gowaty, P. Mate-guarding and nest-guarding in eastern bluebirds (*Sialia sialis*). (Clemson University).
- Heestand, J. Evolution of lateralized behavior in captive apes. (University of Washington).
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- Irons, W. Does it matter how many siblings your mate has? (Northwestern University).
- Leonard, Ch. Groom patterns and social relations in a captive group of macaques. (University of Florida).
- Lockard, J.S., Hawley, M.A., & McFarlin, T.L. Play of lowland gorillas in a naturalistic captive environment. (University of Washington).
- Miller, D. The use of vocalizations as a means of coordinating behavior in atlantic bottlenosed dolphins. (University of Connecticut).
- Miller, D. Developmental changes in whistling in atlantic bottlenosed dolphins (*Tursiops truncatus*).
- Surbey, M. The role of males in the timing of menarche in adolescent girls. (McMaster University).
- Tershy, B. Behavior and ecology of fin (*Balaenoptera physalus*) and Bryde's (*B. edeni*) whales. (Moss Landing Marine Laboratories).
- Vrugt, A.J. When the queen speaks to the nation: A nonverbal analysis.

## BULLETIN BOARD

### Defeat of Darwin

Richard E. Tremblay sent me the following piece of "old" news that he stumbled upon while searching for some information in volume 1 of *Nature* (August 1, 1872, p. 268).

*The choice of a foreign correspondent of the French Académie des Sciences has resulted in the defeat of Mr. Darwin and the election of M. Loewen, of Stockholm, who received 32 votes, against 15 given to the English naturalist. The discussion had extended over three long sittings in secret committee, the leader of the advocates of Mr. Darwin's claim being again his opponent in controversy, M. de Quatrefages, while M. Emile Blanchard led the opposition. A correspondent of Les Mondes, an eminent member of the Academy, in commenting on the result, states that not one of those who voted for Mr. Darwin shared his philosophical doctrines, and not one of those who opposed his candidature alleged as their motive the error or danger of his doctrines. "What has closed the door of the Academy to Mr. Darwin is that the science of those of his books which have made his chief title to fame — the 'Origin of Species', and still more the 'Descent of Man' — is not science, but a mass of assertions and absolutely gratuitous hypotheses, often evidently fallacious. This kind of publication and these theories are a bad example, which a body that respects itself cannot encourage."*

Richard adds that "it is easy, more than a hundred years later, to laugh at the short-sightedness of the eminent members of the French Académie des Sciences. But it should be noted that the invoked motive for not choosing Darwin was apparently lack of methodological rigor. It does seem that our definition of "science" at one point in time, however universal, can be a very bad predictor of who, in the long run, will contribute most to the development of that science. Contemporary examples are difficult to identify because of the lack of hindsight, but the controversy around E.O. Wilson and more recently around Samuel Huntington's nomination to the American National Academy of Sciences (see J. Diamond, *Discover*, 8 (8): 34-39, 1987) may be good examples of our inability to forecast what kind of science will contribute most to a field's development."

### Basic Issues Forum on Territorial Rights

Certain basic issues — such as God, the nature of humanity, the meaning of life, and the relationship between nations — have preoccupied people of all major cultures. And divided these cultures, as well.

The intent of the Basic Issues Forum is to obtain the best available thinking on these long-standing questions, in the hope that furthering such inquiry will result in truths that transcend global fragmentation, make evident our common humanity, and lead to reasoned solutions.

The second topic of the Basic Issues Forum is "The Territorial Rights of Nations and Peoples." This competition spon-

\$5,000 (1st), \$3,000 (2nd), and \$2,000 (3rd). Papers must arrive at fundamental criteria for the basis of territorial rights of nations and people, be relevant to more than one territorial situation, and have practical ramifications.

There is no restriction on eligibility, while the paper should not exceed 30 double-spaced pages and must be written in English. The criteria for judging the papers are accessibility to people with sufficient interest, intellectual rigor, originality of thought, penetration and depth, and persuasiveness. The deadline for submission is March 1, 1988.

The best papers will be presented at a colloquium at Washington and Jefferson College in October 1988. A selection of the best essays will also be offered for publication. The initial topic, "The Existence of God", attracted more than 165 essays from interested people representing various disciplines.

Interested essayists are advised to contact John R. Jacobson, Department of Philosophy, Washington and Jefferson College, Washington, PA 15301, USA.

## Call for Papers

### Southwestern Society for Research in Human Development

The Program Committee invites submissions of proposals for the biennial meeting of SWSRHD, *March 17-19, 1988 in New Orleans, Louisiana, U.S.A.* The Program Committee is interested in receiving a wide range of proposals. No single theoretical perspective dominates the exchange of ideas during the meeting. Rather, as an interdisciplinary society, SWSRHD serves the needs of professionals from various disciplines who are interested in individual development. As such, the Program Committee is interested in soliciting poster papers, symposia, and discussion sessions that reflect the full range of the life span and topics in human development. There is no limit to the number or type of submissions by any one individual. Persons submitting a proposal must be either a member of SWSRHD or be sponsored by a member. All submissions must be postmarked no later than *October 15, 1987*. For further information on the meeting and submissions contact:

Ruth Ault, Secretary SWSRHD, Psychology Department, Davidson College, P.O. Box 1719, Davidson, N.C. 28036, U.S.A.

### International Conference on Infant Studies

The Program Committee invites submissions of proposals for the *Sixth Biennial International Conference on Infant Studies, April 21-24, 1988 in Washington, D.C., U.S.A.* The Program Committee is interested in receiving a wide range of proposals. No single theoretical perspective dominates the exchange of ideas during the meeting. Rather, as an interdisciplinary, international conference, ICIS serves the needs of professionals from various disciplines and countries who are interested in infants. The Program Committee is interested in soliciting Posters, Oral Presentations, and Symposia that reflect the full range of the topics in infant behavior and development. There is no limit to the number or type of submissions by any one individual; *there are no membership/sponsorship requirements for submission*. All submissions must be postmarked no later than *October 18, 1987*. For further information on the conference or submissions contact:

Dr. Nathan A. Fox, ICIS Program Chair, University of Maryland, Institute for Child Study, College Park, Maryland 20742, USA.

(Note: my experience has been that this conference is quite open to papers coming from an evolutionary/ethological perspective. Also, if several of us are going to attend this conference, perhaps we could wet up an informal ISHE meeting. More on this in the future; let me know what you think. WTB)

### 1988 Annual Program Southeastern Psychological Association

Submissions are requested for Posters, Oral Papers, Symposia, and Interest Groups for the annual meeting of SEPA in New Orleans, LA, U.S.A. March 31 - April 2, 1988. *Proposals by non-members must be sponsored by a member.* (There are a lot of these and sponsorship should not be a big problem for anyone wishing to submit.) Deadline for submissions is *October 31, 1987*. For further information on the meeting and submission forms write to:

W. Theodore May, SEPA Program Chair, Psychiatry, University of Tennessee-Memphis, 66 N. Pauline, Memphis, TN 38105, U.S.A.

### IPSA XIV World Congress

The Biological and Politics Research Committee of the International Political Science Association will hold three panel sessions at the forthcoming IPSA XIV World Congress, August 28 - September 1, 1988, at Washington, D.C., U.S.A. One of the sessions will be on the topic "Globalization and Gender — the Biological Evidence." Individuals interested in participating in that session should communicate with Professor Glendon Schubert, Department of Political Science, University of Hawaii at Manoa, Honolulu, HI 96822, USA.

The other two sessions will deal with research currently underway or recently completed, and not previously reported elsewhere. ISHE members are cordially invited to attend. Those interested in serving as paper-giver or discussants should communicate with Albert Somit, 256 Lesar Law Building, Southern Illinois University, Carbondale, IL 62901, USA.

### Facial Behavior

On March 14-18, 1988, the Forschungsstelle für Humanethologie in Seewiesen, in cooperation with the Max-Planck Institute for Psychiatry, arranges a meeting on *Facial Behavior* in the Ringberg Schloss near Tegernsee. Organizers are Dr. Heiner Ellgring (MPI for Psychiatry, Kraepelinstr. 10, 8000 Munich 40, West-Germany) and Dr. Wulf Schiefelhövel from the Forschungsstelle für Humanethologie.

### Postdoctoral fellowships in human and primate development

### National Institutes of Health, National Institute of Child Health and Human Development.

The NICHD is seeking outstanding candidates for several positions beginning January 1988 to study all aspects of human and primate development including perceptual, cognitive, and social behaviours from infancy to adulthood. A Ph.D. or M.D.



is required with training in psychology, biology, sociology, or anthropology and statistics. Salary commensurate with NIH guidelines. Submit CV and three letters of reference to:

Stephen J. Suomi, Laboratory for Comparative Ethology, National Institute of Child Health and Human Development, National Institutes of Health, 9000 Rockville Pike, Building 31, Room B2B15, Bethesda, MD 20892, USA. Phone: (301) 496-6832.

(From published announcement)

## Bitnet/Psychnet

BITNET (Because It's Time Network) is an electronic network of over 825 computers at colleges and universities in 44 of the United States. *Also with BITNET's electronic gateways to Europe, the Middle East, Canada, and Japan, users can easily transmit research and instructional information nation- and world-wide.* If you're not sure whether you can access BITNET, contact your institutional computer center.

It would be good to have a list of members who are connected on BITNET. You can contact me (Bill Bailey) at userid: PS31AWG at node: TCSMUSA or at node: TCSMV.

PSYCHNET is an electronic newsletter and 24-hour bulletin board at the Education Psychology Department, University of Houston. This network is connected to the world via BITNET which is linked to a number of other networks. You can contact PSYCHNET as EPSYNET at UHUVMI. If your computer is DIRECTLY connected to BITNET and if you have the ability to send "interactive" commands (which most BITNET sites do) you can obtain the latest PSYCHNET Index at any time, day or night, or obtain files of interest to you within 1 to 20 minutes of requesting them, by accessing PSYCHNET directly. To get started, send the interactive command PSYCHNET HELP to UH-INFO at UHUPVM1. If you are working from an IBM or compatible mainframe computer running CMS the full commands are: TELL UH-INFO AT UHUPVM1 PSYCHNET SEND PSYCHNET HELP.

## Book reviewers needed

The North American book review editor, Bill Bailey, is *DESPERATE* for people to write reviews. You can pick your own books or we can offer you recommendations. If you know of a book you might like to review, send me the title, author, and publisher. We usually have no problem getting review copies from the publishers. *AND you get to KEEP it for your own library!* Thus wouldn't have to be something you do regularly. One every year or so would be great. If every member would do even one, we could go for 50 or so years before each would have to do another.

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