HUMAN ETHOLOGY NEWSLETTER

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CALL FOR NOMINATIONS

The International Society for Human Ethology has a formal structure consisting of an eight person executive board, elected by the membership. The executive board was initially organized so that at least one representative from each of the following areas be on the board:

Animal Behavior	Anthropology
Psychology	Other Social Science

Other appropriate fields would certainly include political science, communication,or other similar field. Nominations might also come from the natural sciences of biology or ecology. The only stipulation is that that nominee have an active research interest in the theory and methods of ethologv as applied to the study of human behavior.

This structure was initially established at the human ethology meetings held in conjunction with the Animal Behavior Society at Pennsylvania State University in 1977. An election was held in the winter of 1978 with the provision that the four nominees who received the most votes would serve for three years and the next four highest recipients of votes would serve for two years. In subsequent years the nominees will be elected to two year terms. The procedure insures that only half of the executive board is elected during any given year and there is always some continuity.

Current members of the executive board are I. Eibl-Eibesfeldt, C. Travis, W. Charlesworth, W. McGrew, J. Lockard, D. Omark, G. King, R. Simons. Lockard, Omark, King, and Simons will fulfill their terms this year and four positions on the board will thus become vacant. In the past we have found that the call for nominees and the subsequent election is a good opportunity to discover the breadth and energy reflected among the membership in general of our society. You are encouraged to nominate yourself and others who would be interested in serving on the Board.

A nomination should contain the name and address of the nominee, a clear interest in serving if elected, and the major research area or degree area of the nominee, the date, place,and degree type. The nomination should also include a one sentence statement about prior research interests and/or activities. Send nominations to Dr. Joan Lockard, Dept. of Neurological Surgery, Univ. of Washington, Seattle,Washington 98195 USA ,by Sept. 1.

NEWSLETTER EDITOR

Although there are no bylaws pertaining to the tenure of the newsletter editor, I strongly suggest that a maximum three year term be observed. I have almost completed my third year as editor of the newsletter, a responsibility that has been very rewarding. It is time to consider a new editor who can develop new directions for the newsletter. This is a call for nominations to the executive board for the selection of a newsletter editor. Please do not hesitate to nominate yourself.

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Newsletter Editor

A nomination should contain information about the candidate, including degree area, previous experience in editing if any, research interests, facilities, and a confirmed willingness to assume the position. A nomination might also include a statement of policy or perspective on the newsletter or the Society. Send your nominations to Cheryl Travis, Department of Psychology, University of Tennessee, Knoxville, Tennessee, U.S.A. 37916. Nominations will be forwarded to all members of the executive board in early August. Therefore, all nominations should be received by no later than July 30. Board members will consult about nominations and potential candidates; the decision will be announced in the fall issue of the newsletter.

My decision to retire as newsletter editor reflects my concern for the continued growth of the Society. New features, new perspectives, and new contacts will necessarily accompany a new editor. My own interest in human ethology remains undiminished, and I look forward to the increased time that I will be able to devote to research and more informal colleagual relations. I will certainly continue to serve as a member of the executive board for the remainder of my term.

Cheryl Brown Travis

HUMAN ETHOLOGY ABSTRACTS

Human Ethology Abstracts III is now available. The reference is Man-Environment Systems, 1979, 9, (2 & 3), 55 - 164. Reprints can be ordered from ASMER, P.O. Box 57, Orangeburg, N.Y. 10962 at a cost of $3.00. There are over 500 citations, thanks to the diligent efforts of Bob Adams (Eastern Kentucky University) who was responsible for the editing.

Human Ethology Abstracts IV is now in process. Larry Stettner (Dept. of Psychology, Wayne State University, Detroit, Michigan 48202, U.S.A.) is editing the fourth collection of Human Ethology Abstracts. Citations should follow the American Psychological Association format and should be approximately 150 words. Major topics include general ethology, sociobiology, social organization and sex roles, methodology, agonistic behavior, social spacing, territory, and crowding, courtship, sex, and reproduction, communication, infant behavior, child-child interactions, and applied human ethology. The deadline for receipt of abstracts is July 30. You do not have to be a member of ISHE to submit an abstract. Abstracts may be based on unpublished manuscripts , currently under review for publication, papers presented at conferences, published papers, technical reports, and books.

reviews

QUANTITATIVE ETHOLOGY edited by Patrick W. Colgan. Wiley-Interscience, 1978.

This book came out of a symposium on "Quantitative Methods in Ethology" held at the Animal Behavior Society meeting of 1975. It is an edited collection of papers concerned primarily with aspects of data analysis relevant to ethological problems. Some novel approaches are suggested.

You might come to this book with a problem in mind or because you have an interest in some particular technique of analysis. Problem

* FORUM *

Spring Forum
A Critique of Human Ethology

Contributors to the spring forum were asked to address the problems of human ethology as an emerging discipline, paying particular attention to the weaknesses of theory and method, and suggesting future directions for the field. The contributors have addressed the topic with vigor, and each merits several readings and continued discussion. Additional insights or opinions on this topic are welcome and will be included in future issues to the extent that space is available.

Barnett criticizes the exclusive use of ethological methods which ignore human language, while recognizing the value of ethological methods in general. He also points out that humans occupy an incredibly diverse range of habitats, each with its own ecology. Therefore, evolutionary concepts which are helpful in explaining the behavioral adaptations of other species do not impart much to the understanding of human behavior. He finally argues strongly for the necessity of a genotype-environment interaction, and pointedly calls for the discarding of concepts like instinct and innate along with deterministic models. Mackey calls for a change of perspective in our theories and in our methods. His thesis is that human behavior can be understood only within a social context and that our theories and methods should focus on relational systems. Eibl-Eibesfeldt calls for a conservative interpretation of events which conveniently support sociobiology, such as the oft cited infanticide of langurs or the outcomes of computer simulations. He is particularly concerned about the overgeneralization of concepts such as kin selection, pointing out that in fact it probably cannot apply to human societies marked by elaborate marriage rules. Larsen also argues that the use of concepts such as altruism, selfishness, and fitness have been distorted to such an extent that much of the theory surrounding these terms has become vacuous. McCracken points out that evolution does not operate on teleological principles and that determinism is not in fact a part of the concept of evolution as biologists understand it. He further suggests that we should not focus solely on the individual but should consider the possibility that in some circumstances the group may be the unit of selection.

HUMAN ETHOLOGY AND COMMON SENSE *

S.A. Barnett, Zoology Department
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'Human ethology' may signify: (i) applying ethological *methods* of observation or experiment to human beings; (ii) interpreting human behavior by the *principles* of biology; or (iii) making methodological *presumptions* when studying human behavior.

(i) By ethology I mean the *science of animal behavior*. In ethological research the observer is detached and objective; typically findings are detailed, quantitative, and repeatable; they may be physiological, or at the level of behavior only. References to thoughts, feelings, intentions and so on are usually ruled out. Ethological methods may be used to describe non-verbal acts, such as pupillary dilatation in response to a social stimulus, blushing, looking down when an acquaintance approaches, staring ... They have been fruitful in studies of the signals of infants, such as sucking, smiling, and crying; observations on the interactions of mother and infant have been of practical value. The method has, however, severe limitations. We daily, without difficulty, pass immensely complex information among ourselves; but there is no ethological account of our principal means of communication, language. Our non-verbal communication too is, except in infancy, carried on in the context of language.

(ii) Relevant biological principles are (a) evolution by *natural selection*, and (b) the laws of *genetics*.

(a) Evolutionary concepts provide a framework in which to examine how extant species fit their ecological niches. But our own species is not adapted to one niche, but adaptable to many. Hence we do not fit such a framework. Aspects of human life, ranging from whipping schoolboys to the authoritarian personality, have been 'explained' by their supposed resemblance to the conduct of carefully selected species of Primates: the implication is then of an effect of common ancestry. Findings on other species can, however, lead to no valid conclusions on *H. sapiens*; but they can (by analogy) suggest testable hypotheses.

Attempts have also been made to account for our customs and beliefs by the supposed action of natural selection. The method is to observe what we (or some of us) do, and then to show that, on the basis of a few simple presumptions, what we do is also theoretically possible. This method often entails unacknowledged changes in the meanings of key terms, such as 'kin' and 'altruism'.

(b) The principles of genetics are an essential component of any biological account of man. All our characteristics, behavioral and other, are influenced by both our genotype and our environment, and by the interaction between them. Hence it is never appropriate to say of any feature that it is genetically, or environmentally, determined: only *differences* between individuals may be properly so described. This elementary but neglected principle arises from the familiar distinction between the genotype and the phenotype: genetically identical individuals may develop in very different ways in different environments; and individuals in nearly identical environments may differ greatly if they are not genetically uniform. Hence one should never ask whether certain features or propensities are 'inherited' or 'innate'. Our behavior, the inner processes that make us act, and our other features are not passed on, through successive generations, like items of property or names. Biological transmission is wholly different from legal inheritance, because every feature is developed anew by each individual. What we inherit are genes, not characters.

These principles provide one reason why the concept of instinctive or innate (or genetically preprogrammed) behavior is being discarded. Even the apparently fixed, species-typical behavior patterns of animals develop: experiments may show them to be more or less labile during development. More formally, every characteristic varies, and for each there is both a genetical and an environmental component of the variance. This, though a truism, causes immense difficulty. The practical implication is the importance of paying attention to ontogeny.

(iii) Two central methodological presumptions are (a) the principle of *reduction*, and (b) that of *determinism*.

(a) Reductionism, in its most extreme form, states that the body is nothing but an assemblage of molecules, atoms or fundamental particles. Everything we are and do is then, in principle, explicable in terms of physical science. The most obvious objection is that, if we are nothing but atoms, then there is no human being or body to explain. Every level of analysis, from that of the electron to that of, say, social organisation, has its own validity. None should be rejected.

(b) Determinism may be regarded as a necessary presumption in science. Much scientific work consists of trying to establish causes. (On the other hand, all such statements are of probabilities.) But determinism is not a scientific principle: it is part, not of physics, but of metaphysics. It is also one that everybody, except a few insane persons, rejects in ordinary affairs. In practise, we presume that we can sometimes make choices, independently of our evolution, our genes or our physiology. Determinist systems such as those of conditioned man (*H. skinneri*) or of sociobiological man (*H. egoisticus*) are based on the single-minded application of concepts which are far too simple to interpret the human condition.

We should continue to ask the eternal question, 'what is man?' and we should refrain from answering merely by saying, 'he *was* an ape'.

For a longer, documented argument, see *Modern Ethology: The Science of Animal Behavior*, to be published by Oxford University Press, New York, in 1981.

The Ethology of Social Relations ✶

Wade C. Mackey

In the 1980s, human ethology and its sibling disciplines will enter into the market place of ideas and world-views. The clearer and more defined the contribution human ethology is prepared to offer, the more ears will be poised to listen. Let it be assumed that among human ethologists, there is a general concensus that primary foci for human ethology are the understanding and predicting of human social behaviors as examples of species-characteristic behavior.

What then become "units" of analysis for these foci? I will argue that the best indicator of human social behavior is _human social behavior_. Accordingly, the more this mosaic is exchanged for other versions, which include research upon nonhuman species, research in controlled environs or research on symboling systems (e.g., language)--regardless of their own intrinsic value and interest--the less understanding and predictability of human social behavior will occur.

A basic problem facing human ethology emanates from the very legitimate position that "culture transcends biology" (Sahlins 1976). A rebuttal that humans are culture bearing animals does not address this problem. The burden of proof of a postulate falls upon the shoulders of the proponents, and it is our responsibility to demonstrate the genotypic-information predisposes or orients or canalizes human _behavior_. Assertions are going to generate more credence when buttressed by appropriate evidence. It is our responsibility to demonstrate as unlikely the alternate hypothesis that human social behavior is limited by genotypic-information _only_ due to anatomy and physiology. Work with nonhumans (_inter alia_ primates, canids, rodents)--as useful as they are to form theories and to build models--cannot address the problem of the appropriatness of generalizing from nonhumans to humans. The validity of the generalization is the very item being questioned. Research upon human social behavior via _human social behavior_ obviates the problem and removes whole categories of potentially unanswerable criticism.

I would also argue that perfect knowledge of how types of hormones and types of neurons interact within each type and between types will not result in predictability of _human social behavior_. That such knowledge is clearly valuable and should be encouraged across the spectrum of the disciplines is not in question. However, knowledge of the configuration of the internal environment of an _individual_ tells us nothing of the interaction configuration _between_ individuals. To develop the validation of neuro-hormonal models would still require the collection of data upon human social behavior. Any achieved correlation between the internal environment and the external behaviors then runs into the quagmire of the determination of causality. Because the lack of perfect knowledge may well prove to survive intact throughout the 1980s, the more parsimonious index to human social behavior would still be _human social behavior_.
An attempt to gain perfect knowledge of cultural rules or symboling systems (an "emic" analysis) is a theoretically impractical mode of studying cultural behavior (an "etic" analysis) (Harris 1974). Cultural rules are often more recognized in the breach than in the practice, and for many rules, there are other rules to allow an antithetical behavior. Ethnographers, individually or in corporate sense (Human Relations Area Files), tend to have an emic orientation, i.e., explore the subjective reality of the informants. Parametric data are usually not available and, when present, are difficult to compare across cultural boundaries. Statements about society's "expected behaviors" imply a subjective norm which may tell us little about the actual incidences or exceptions. Again the problem is magnified when subjective "norms" are attempted to be compared across cultural boundaries. Because of the unknown correspondence between subjective ideation and objective behavior, if behavior is the focus of the inquiry, then the behavior--not the ideation--becomes the optimal unit of analysis.

Even when behavior is placed as the point of equilibrium for maximizing predictability, a second tier of problems emerges. To christen a particular behavior pattern as a candidate for species-characteristic behavior, the alternate hypothesis of the behavior as purely a consequence of socialization traditions must be shown to be untenable and thereby should be abandoned. In other words, it is necessary for the thematic behavior (the candidate) in question to be found in a number of ecologically distinct societies which have operated independently from each other for many generations (i.e., Galton's Problem ameliorated). In addition, the verdict must be rendered that the trait is not an adaptive social formula, highly functional, which is deeply enmeshed in the socialization traditions of the society. Fire-making and weaving are widely found traits which undoubtedly are kept extant by, and only by, socialization traditions. The ontogenetic genesis and maintenance of other traits, e.g., suckling, are going to be more equivocal. The project which--to my mind-- most satisfies the methodological rigors to identify species-characteristic traits is Ekman's (1973) work on facial expressions. His research is cross-cultural, in diverse ecologies, and the individual facial gestalts appear to have no systematic functional relationship with the environment. Another example is Whiting and Whiting's (1975) Children of Six Cultures series which, because it addresses more generalized complex behavior, exchanges the precision of Ekman's method for a wider range of social applicability.

To encapsulate, let it suffice to proffer that an opportunity exists for human ethology to fill a unique slot in the sciences: the understanding and prediction of human social behavior by studying human social behavior--using ethological techniques and evolutionary theory--in diverse, extant cultures. The consistencies which are distilled become candidates for species-characteristic behavior. No other discipline has such a paradigm.

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Sahlins, M.
1976 On the Use and Abuse of Biology. Ann Arbor, Mich.:U. of Michigan Press.

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1975 Children of Six Cultures. Cambridge, Mass.:Harvard U. Press

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Too Many Jumping on the Bandwagon of Sociobiology *

I. Eibl-Eibesfeldt

Because Sociobiology implies that a wide range of animal and human behavior is regulated genetically and that people behave only according to self-interest, the debate around sociobiology has tended to be more emotional than scientific. Those who are enthusiastic about the idea look for positive evidence supporting sociobiology, and if they find any, indiscriminately jump on the bandwagon. Often too, those who oppose it react emotionally and consequently, unlike with other new theoretical developments, sociobiological issues are rarely evaluated by examining both sides of the coin as fairly and as objectively as possible, in other words by scientific procedure. A good example of this can be seen in recent work with the langurs.

Male Hanuman langurs are said to kill as a rule nursing babies after taking over a harem; quite a number of scientists reported this. It's one of the frequently cited examples used by sociobiologists to illustrate the principle that individuals act in a way which maximizes their reproductive success, even when it means acting destructively against members of their own species. Sociobiologists use this as evidence to argue that there exists no species interest, and that group interest, which may play a role, certainly does not count when it interferes with individual interest. Alas, Cristian Vogel (1979) just published a paper which should caution against an all too ready acceptance of such theses. The data are much less solid than they appear at first glance. Vogel and his coworkers have studied the Hanuman langurs over an extensive period, and in the area from where the baby-killing had been reported, they observed many takeovers but no single case of babymurder! Scrutinizing the references in the literature, Vogel found that only three cases of baby-killing had actually been observed, --all by the same author, and in all cases the killer was the same male. Two of these killings occurred after takeovers, one six months later. All of the other 40 cases reported were found to be based on speculation. For example, the authors found a dead baby with wounds and recorded it as killed by a male, or they found wounded ones, or just found a baby missing and jumped to the conclusion that it was killed by a male. Also, a large number of cases were reported second-hand by "informants". Vogel and his group found that dogs harrass the langurs, and this can easily explain the number of wounded and dead reported. And it would fit better to the fact, that nearly half of the cases attributed to baby-killing would not fit the predictions of sociobiology, since the young killed were either already too old, or the "killing" took place too long after the takeover to affect the new leader's reproductive success. In other words--a frequently quoted example can certainly not be used as supportive evidence any more. In an effort to jump onto the bandwagon of sociobiology, too many hastily present evidence nowadays. A critical review and reconsideration seems timely. Too much indeed is at stake: the reputation of the behavioral branch of biology as a natural science!

The use of computer simulation and other mathematical models is sometimes very rewarding. Giving x points to the winner in a ritualized fight, and y to the loser in contrast ot x' and y' in a nonritualized encounter demonstrates that one does not need to assume a species or group interest in order to explain, for example, the evolution of ritualized fighting. But, it only demonstrates that ritualized fighting can evolve via selection at the level of the individual, not that it did. Certainly a simulation model could also demonstrate how ritualized fighting could evolve at the level of the group or species and the point is, that both alternatives must be examined thoroughly to see which provides a more plausible explanation.

In addition, even if individuals were acting in pure "self-interest" without consideration of a species or group interest, such behaviors could evolve by individual selection and thus one could expect an equilibrium in a population of ritualized and ruthless fighters. Maynard-Smith spoke in this context interestingly enough in finalistic terms of an evolutionary stabile strategy, as if implying some sort of interest on a higher (species?) level, even though this is hotly denied to exist by most sociobiologists if not consistent with the "selfish interests" of genes. There are, of course, other ways to explain, why the ruthless cannot win. In my 1955 paper on the ritualized fighting of the Galapagos Marine Iguana, I mentioned that a ritualized fighter changes to nonritualized fighting the moment the opponent does not obey to the rules of the tournament. Since the nonritualized fighter therefore meets only others who pay back with the same currency, he runs the greater risk. It's exactly what Maynard-Smith called retaliator in one of his models. From my comparative knowlege of ritualized fighting, by the way, the retaliation seems to be the rule. But the other models have to be considered and the whole approach is inspiring and stimulating. We should not, however, accept them uncritically as if they were already pure truth and, in particular, as if it were proven by these models that group- or species interest does not count or is of minor importance, and that, accordingly, selection on these higher levels would play an insignificant role, if at all.

Kin selection is another topic in sociobiology which is grossly exaggerated. In the first place, it is curious that, although Hamilton (1964) in his pioneering work with kin selection stated correctly that the coeficient of relatedness 'r' is the probability that individuals have inherited a particular rare gene from a common ancestor (since all members of a species share the vast majority of each other's genes), that the most commonly used formula used in studies of kin selection today is a fallacy. That is, that I share 50% of my genes with my children, 25% of my genes with my grandchildren etc. and thus should scrifice my life to save 2 of the former or 4 of the latter. The situation becomes quite different if one considers that in reality I share 99.5% of my genes with my children and perhaps 98-99% with others in my breeding population (gene pool). Animals then are said to act so as to serve the survival of such genes, by favouring their offspring and close kin even to the extent of ruthlessly raging against other species members which are no' "blood relatives", although they share 95-99% of that animal's genes. The Hanuman langurs were quoted as one importan proof. Based upon my knowledge of gregarious mammals I had cast doubt on the interpretation by the sociobiologists and argued that among the langurs we might be confronted with pathological behavior (I.Eibl-Eibesfeldt, 1978), as indeed seems to be the case as Vogel's publication shows.

What other evidence then do we have in favor of the thesis of kin selection? For gregarious mammals the lion might fit, provided that they really behave in the way attributed to them, for which I now have my doubt. Yet if so, from my knowledge of gregarious mammals, I think that they must be an exceptional case. Certainly the principle of kin selection must be carefully examined in man, who, in each generation reshuffles 50% of his offspring. Spouses marry in from outside and are defined as kin, as are members of their families, and offspring are released to marry non-kin. What an individual contributes as reproductive success is mixed into the gene-pool of the group. The marriage rules insure that nothing like kin selection can take place. Nepotism is counteracted by them, and history teaches us indeed that groups succeeded or failed to survive, thus acting as units of selection. Looking at the size of populations in which man lived for most of his history, we will see that they were fairly small indeed. Populations characterized by a distinct language, count among hunters and gatherers often some hundred individuals only, and in the neolithic horticulturists of New Guinea comparative units are made up of several thousands--which then already tend to separate into warfaring competing units. Certainly in face of the rules of exogamy everyone in such small population is closely related to other group members. Any mathematics trying to calculate the payoff of altruistic acts by the degree of blood-relationship must fail, as Hawkes has demostrated in New Guinea. The observeable fact, that we are emotionally inclined to aid those more fullheartedly that others, that live with us in the family, finds its simple explanation in the need of parental attachment for offspring survival. The resulting inclination for nepotism may be considered an unwanted sideffect, since it is counteracted culturally. The hastiness by which unproven theses are accepted, as if they were proven, as reflected in a number of contributions e.g. in the recent book of Chagnon and Irons, is indeed astonishing, if not appalling. Sociobiology has its undoubted merits. As a branch of ethology it boosted evolutionary thinking in the USA. and its models inspired quantitative studies in the field of behavioral ecology. There is, however, the danger that too many bandwagon-jumpers spoil the promising new start. Wilson--I leave it open whether he meant it serious or not--insisted that sociobiology will cannibalize related fields--indeed it has cannibalized the imagination and creativity of a number of scientists.

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Hawkes, (1955) Der kommentkampf der meerechse (Amplyrhynchus cristatus). Zeitschrift fur Tierpsychologie, 12, 203-219

Vogel, C. (1979) The Hanuman-langur (Presbytis entellus): A key example regarding the theoretical concepts of sociobiology ? Verhandlungen der Deutschen Zoologischen Gesellschaft, 73-89, G. Fischer, Stuttgart.

[1] I wish to thank Dr. Polly Wiessner for her kind help in revising the manuscript

SOCIOBIOLOGY, FITNESS, SELFISHNESS, AND ALTRUISM

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Wilson (1975) has claimed that "it is precisely through the deeper analysis of altruism that sociobiology seems best prepared at this time to make a novel contribution (p. 150)." It is the purpose of this brief essay to argue that rather than making a novel contribution, Wilson and others under the banner of sociobiology have succeeded only in producing an erroneous conceptualization of altruism and its opposite, selfishness; a conceptualization that is furthermore dangerous because it appears to give scientific support for a particular philosophical and moral position when in fact there is no such support for such a position. The crux of the argument is that the sociobiological definition of selfish behavior is not the same as the ordinary meaning of that term, and in fact that what the sociobiologists are referring to is not selfish at all in the ordinary meaning of the term. For them to use the term selfish in the way they do is to misuse it and to mislead others as to the implications of biology for the description of human behavior. Scientists are of course entitled to define the words they use as they wish, but not usually in such a way as to be totally at variance with their ordinary meanings, nor is it then justified to use the words equivocally in contexts that imply the ordinary meanings.

Wilson (1975), following Hamilton (1964), defines altruism as behavior that decreases one's own personal fitness in order to increase the fitness of others, and conversely selfish behavior is behavior directed toward increasing one's own fitness at the expense of others. The definition of selfish behavior is the key to the sociobiological error. The ordinary, dictionary definition of selfish behavior is that it is behavior that is to the benefit of the individual, that enhances his wealth and security, and thus his survivability. Wilson has added to this basic meaning the notion of fitness, so that selfish behavior is not just behavior that is to the benefit of the individual, but to the benefit of the individual's fitness, i.e., behavior that enhances his survival and reproduction. This shift from a focus on the individual to the individual's fitness is crucial. Fitness is ultimately defined in terms of relative reproductivity. The individual who reproduces more is, all other things being equal, more fit. Given a choice between continued survival and reproduction, reproduction is the more important factor in determining biological fitness. The salmon, for instance, does not survive after reproduction: survival is literally sacrificed for reproduction. Survival is justified only as a means to the end of reproduction. Selfish behavior in Wilson's terms, then, becomes behavior that increases an individual's reproductivity.

In what sense is behavior that increases an individual's reproductivity selfish? In the ordinary sense of selfish it is not. It does not increase an individual's own advantage or survivability. In fact reproductive behavior is often if not always detrimental to the continued survival of the individual, as in the case of the salmon mentioned above. Nor is behavior directed toward the welfare of one's offspring generally considered selfish. In general having offspring requires time and effort that may be better spent, from a selfish point of view, preserving and enhancing one's own life. Among humans, successful bachelors and childless couples are much more able to enjoy the pleasures of modern life than are couples burdened with children. A truly selfish individual acts to his own advantage regardless of the effect on others, including his own offspring, which for the vast majority of organisms are of no benefit to him.

If reproduction is not selfish, i.e., for the benefit of the individual, then to whose benefit is it? In an implicit recognition of this question Wilson (1975) and others (esp. Dawkins, 1976) have also argued that selfish behavior is not behavior that is of benefit to the individual, but behavior that is for the benefit of the genes. In this way they can avoid making the clearly false claim that reproduction is for the benefit of the individual, and yet still maintain that neither is it altruistic or for the benefit of the group or species.

It is really immaterial whether one speaks of individual genes or complete organisms; whatever it is called, what is produced by the process of reproduction is the benefit itself, the contribution the individual makes. Just as a gift cannot at the same time be the recipient of the gift, so the product of the reproduction whether it be called genes or offspring, cannot at the same time be the beneficiary of the genes or offspring.

One can only conclude that reproductive behavior is for the benefit of the next generation of individuals, or more generally for the benefit of the group to which the individual belongs, and ultimately for the good of his species - i.e., reproduction is an altruistic behavior in the ordinary, everyday sense of that term. At the level of genes one can say that reproduction is for the benefit of the gene pool, but "gene pool" is only a technical term for the species. Behavior directed toward increasing one's fitness, i.e., one's relative reproductivity, is therefore altruistic behavior; it benefits the group rather than the individual. Wilson's (1975, 1978) designation of this behavior as selfish is thus a complete distortion of the usual meanings of both selfishness and altruism.

This conclusion should not be taken as a denial of natural selection as a process that operates solely at the level of the individual. Williams' (1966) massive attack on the notion that selection occurs at the level of the group is completely convincing. Selection takes place entirely at the level of the individual, but always for the benefit of the group and species.

The ultimate import of these remarks is that biology does not, in spite of what Wilson says, justify the view that human behavior is based essentially on self-interest. Indeed, if anything the biological theory of evolution through natural selection supports a view of life in general, and human life in particular, as basically altruistic. In a "blind", "irrational" act of reproduction each individual makes its altruistic contribution to the continued existence of the species through the production of the next generation. Other forms of altruistic behavior, such as in the social insects or among humans, are simply extensions and generalizations of this basic act of altruism. Selfishness in the ordinary sense is biologically as well as morally justified only insofar as it is necessary to ensure the existence of altruistic behavior. A totally selfish individual is simply not viable or biologically meaningful. Far from being based essentially on self-interest, life from a biological point of view is based essentially on altruism.

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RESPONSE TO
SOCIOBIOLOGY, FITNESS, SELFISHNESS, AND ALTRUISM *

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I agree with Larsen's (1980) criticism that the use of loose terminology in dealing with complex phenomena can easily lead to confusion. However, in the context of their discussions I generally find Hamilton (1964), Wilson (1975), and others' (eg. Barash 1977; Dawkins 1976) use of the concepts "selfishness", "altruism", and "benefits" intelligent and coherent. This is not my opinion of Larsen's essay. The reasons are that most sociobiologists and evolutionary biologists speak a common (although, sometimes loose) language, and they are starting from the same entirely mechanistic principle. Larsen clearly stumbles over the language, but the main fallacy in his essay is that he does not appear to grasp the principle.

The basic principle is that evolution has no purpose (see Gould 1977 for elaboration on this). Organisms vary, some of this variation is inherited, and heritable variation arises randomly. On average, organisms which vary most strongly in the direction favored by their local environments will survive and reproduce better than those not as favored. Thus, the genes of the most favored individuals increase in frequency in the next generation. That is all. If environments change (as eventually they always do) selective pressures may change and allele frequencies within the gene pool will change. Other than leading to better adaptation to ephemeral, local environments, evolution has no direction. It is completely mechanistic.

In the context of this understanding of how evolution works, Larsen's contention that individuals reproduce <u>for the benefit of the group</u> is deterministic and metaphysical. Individuals do not, as he contends, altruistically reproduce for the benefit of the gene pool. The structure of the gene pool is merely the result of natural selection. I also disagree with Larsen that "reproductive behavior is behavior <u>directed toward</u> increasing the relative frequency of one's own genes in competition with the genes of others". Reproductive behaviors are rather the <u>result</u> of successful reproduction in past generations.

The operation of this mechanism is apparent for a single genetic locus with more than one allele. However, the questions that concern sociobiology (including human sociobiology) arise from the fact that a genetic locus exists and interacts with the environment only in concert with many other loci that comprise the individual. Therefore, I again disagree with Larsen and maintain that the important issue is the level at which selection takes place, and that the question of "benefits" is really a tautology. Williams (1966) buried Wynne-Edward's (1962) group selection hypotheses, but he did not entirely bury group selection. Since individuals usually have shorter durations than groups, and individuals frequently move between groups, it is likely that selection occurs most commonly on individuals. But, in some circumstances, the group may be the unit of selection (Williams 1966; Maynard Smith 1976). Furthermore, mutualism, reciprocity (Trivers 1971), nepotism (Bertram 1976), or "trait groups" (D. S. Wilson 1977; 1979) may all influence competitive and cooperative interactions among individuals. None of these issues will be resolved by essayists, they will be understood only by gathering empirical data from natural populations.

I share Larsen's concern that the use of sociobiological theories to support certain human moral and philosophical positions is unfounded and perhaps dangerous. Much of the theory certainly needs further study, and abuses through inference have been common, but the theory itself is not the major problem concerning human sociobiology. The important scientific issue involves distinguishing heritable, genetically determined social behaviors (if they exist) from behaviors that result from the complex cultural effects of our past and present (Cf. Caplan 1978). In conclusion, I suspect that Larsen's essay is in fact more a defense of a philosophical position than an inquiry into the order of nature.

Barash, D.P. Sociobiology and Behavior. New York: Elsevier, 1977.

Bertram, B.C.R. Kin selection in lions and in evolution. In P.P.G. Bateson and R.A. Hinde (eds.), Growing Points in Ethology. Cambridge: Cambridge University Press, 1976.

Caplan, A.L. (ed.), The Sociobiology Debate. New York: Harper and Row, 1978.

Dawkins, R. The Selfish Gene. New York: Oxford University Press, 1976.

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Hamilton, W.D. The genetical theory of social behavior. J. Theoret. Biol., 1964, 7:1-32.

Larsen, G.Y. Sociobiology, fitness, selfishness, and altruism. Human Ethology Newsletter, 1980-28:

Maynard Smith, J. Group selection. Quart. Rev. Biol., 1976, 52:277-283.

Trivers, R.L. The evolution of reciprocal altruism. Quart. Rev. Biol., 1971, 46:35-57.

Williams, G.C. Adaptation and Natural Selection. Princeton: Princeton University Press, 1966.

Wilson, D.S. Structural demes and the evolution of group-advantageous traits. Amer. Natur., 1977, 111:157-185.

Wilson, D.S. Structured demes and trait-group variation. Amer. Natur., 1979, 113:606-610.

Wynne-Edwards, V.C. Animal Dispersion in Relation to Social Behavior. Edinburgh:

SUMMER FORUM

?? Does the Concept of Behavioral Precursors Do Away with the Concept of the Innate??

This topic has been proposed by Eibl-Eibesfeldt and will be edited by him. The following comments are offered as a general statement of a position on the problem which may be further supported or attacked by contributors to the forum.

It has become fashionable practice in behavioral publications to mention in at least one sentence that the nature-nurture issue is outdated, antiquated, or even dead; as is shown , for example, by the number of comments on a target article by Eibl-Eibesfeldt in the Behavioral & Brain Sciences (1979). But if anything, these statements prove the opposite. It is still an issue and a burning one for those who wish it dead, since they have not come to grips with it. This is obvious in their arguments. Gilbert Gottlieb argued against the value of the concept of the innate by mentioning the possibility of precursors of such behaviors, which might be acquired during an early ontogenetic stage-- an idea orgiingally brought forth by Z. Y. Kuo, promoted by Daniel Lehrman and answered by Konrad Lorenz. Lorenz emphasized that the issue concerns itself with the specific adaptedness of a structure or a behavior pattern and how this adaptedness came about.

The deprivation experiment in this context was found to be a most important tool to decide whether adaptedness was the result of phylogeny or ontogeny, since every adaptation depicts certain features of the environment, like a casting depicts a mould. And, in order for such copying by adaptation to occur, an interaction between the organism and the environmental feature which its adaptations depict must have taken place at one time. Adaptation means knowledge which exists only in relation to something to be known. The organism must somehow have "informed" itself in order to adapt. In referring to a specific adaptation , one can deprive an animal of the information required for the specific adaptation to take place. One can, for example, raise a bird in social isolation, and should one find, that it finally produces all those songs characteristic for the species, then this would be taken as proof, that the song or, more specifically, its particular patterning is a result of phylogenetic adaptation and thus innate.

But what if one should find that precursors of these behaviors are learned? Would our statement about the innateness of the song be changed, if one were to prove, for example, that the breathing movements of the bird--which certainly are a precondition for any singing to occur-- are learned during ontogeny? It would not, since these adaptation refer to another level of integration !

In analogy: By learning a language one certainly acquires all necessary precursors for reciting a poem, but definitely not the knowledge of a single poem. This patterned information must be acquired in addition.

The emphasis of the importance of ontogenetic precursors is quite right, since it is of interest to learn how the process of self-differentiation takes place, but it does not in the least undermine the concept of phylogenetical adaptations in behavior -- in short of the concept of the innate -- which continues to be the basic concept of ethology .

Eibl-Eibesfeldt (1979) Human ethology: Concepts and implications for the sciences of man. The Behavioral and Brain Sciences, 2, 1-57

Manuscripts and commentary for the summer forum should be submitted to Eibl-Eibesfeldt by air mail no later than August 1. Essays should be limited to 1,000 words if possible and prepared single spaced, photocopy ready, with a minimum of references that require formal citation at the end of the comment. As a precaution against international mail problems contributors may also wish to file a copy of their essay with the newsletter editor, Cheryl Travis. All comments should be submitted to the following address.

continued on p. 15

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FALL FORUM

Evolutionary Biology and Political Authority

Fred Willhoite has suggested the title of the fall forum and will serve as the special editor for the issue. The call for commentary asks that contributors address themselves to the issues, theories, applications, etc. discussed in any of the following four articles, listed below. Essays should be submitted to Fred no later than Sept. 15 so that any necessary revisions can be accomplished in time for the fall deadline of the newsletter. Essays should be submitted in photocopy ready form, according to the general style of the newsletter.

Articles:

Alexander, R.D. Natural selection and societal laws. in H.T. Engelhardt, Jr. & D. Dallahan (Eds.), Morals , Science, and Sociality (Hastings Center, 1978).
Barner-Barry, C. An observational study of authority in a preschool peer group. Political Methodology, 4 (1977), 415-447.
Chagnon, N.A. Is reproductive success equal in egalitarian societies? in N. Chagnon & W. Irons (Eds.) Evolutionary Biology and Human Social Behavior (Duxbury, 1979).
Goodall, J. et al. Intercommunity interactions in the chimpanzee population of the Gombe National Park. in D. Hamburg & E. McCown (Eds.), The Great Apes . (Benjamin/Cummings, 1979).

Comments should be limited to 1,000 words if possible and should rely on a minimum of formal references that require full citation at the end of the essay. Mail essays unfolded with protective covers to Fred Willhoite, Coe College, Cedar Rapids, Iowa, USA 52402.

* CONFERENCES *

ABS HIGHLIGHTS
Colorado State University

Invited Paper Sessions:
- Female Social Strategies   Sam Wasser, Univ. of Wash., organizer
- Structure and Evolution of Carnivore Social Systems   Marc Bekoff, Michael Wells , Univ. of Colorado, & Peter Waser , Purdue, organizers
- Laterality   Ira Perelle, Mercy College, organizer

Invited Addresses:
- Keynote Speaker: Dr. R. A. Hinde, Ethology and the Social Sciences
- Banquet Speaker: Dr. V. Geist

continued on p. 17

INFORMATION ON HOW TO PREPARE COPY FOR THE NEWSLETTER IS ON THE LAST PAGE, WITH THE SUBSCRIPTION & MEMBERSHIP FORM.

APPLICATIONS FOR INVITATIONS, XVIIth INTERNATIONAL ETHOLOGICAL CONFERENCE:

Canadian Ethologists: Individuals interested in being considered for an invitation should write for an application form to: Dr. Jacques Bovet, Département de Biologie, Université Laval, Québec, Qué., Canada G1K 7P4. Invitations will not be issued to individuals who do not apply. Deadline for submitting applications is September 1, 1980.

U. S. Ethologists: The U. S. Committee of the International Ethological Conference welcomes you to submit an application for an invitation to the XVIIth meeting to be held from September 1-9, 1981, at the University of Oxford, Oxford, England. There is a selection process since each country has a limited number of slots assigned. Please follow the instructions. Application packets must reach M. Bekoff by August 1, 1980.

The following materials must be included in your packet:

(1) Ten (10) copies of your application form (below). (All committee members vote.)

(2) Two (2) self-addressed, stamped postcards (or envelopes), one to notify you that your application has been received, the other to inform you of the disposition of your application (whether you were on the first list of invitees sent to Oxford, your place on the waiting list, etc.). This information will be available after October 10, 1980. If you wish to supplement your application you may include a brief resume (less than 1 page), a letter of reference or introduction, a brief abstract of your thesis, etc., but you must send 10 copies of any supplementary materials.

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U. S. Ethologists Application Form for the XVIIth International Ethological Conference, Oxford. England

Name: Ph.D. (where, when, expected):

Complete Address and Telephone Number:

General fields of interest:

Supplementary information attached? Yes_______ No_______

Application and supplementary material should be sent to: M. Bekoff (USECC Secretary), Univ. of Colorado, Dept. EPO Biology, Box 334, Boulder, CO 80309 by 1 August 1980.

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The International Ethological Congress is held biannually. In past years it has been held at Bad Homburg, Germany, and British Columbia, Canada. Participants are selected by various international committees; only those applicants selected by the committees receive an invitation. Most people who are invited to attend are also given some opportunity to present their research, either in poster sessions or paper sessions, but this is not guaranteed. The U.S. committee has attempted to select both established scholars and scientists and to also include some applicants who rank as promising young professionals or advanced graduate students. Human ethologists have not attended this conference in great numbers, but anyone may apply and perhaps some ISHE members will be interested.

Workshops:

Human Ethology Tuesday June 10 8-10 pm

Collection, Management & Analysis of Complex Social Interaction Data
Thursday June 12 8-10pm

ETC.

Paper sessions, Poster Sessions, Social Hours, Film Sessions, Field Trips

BOOK REVIEWS

handling data in the form of durations, intervals, latencies, and sequences of behavior; grouping similar kinds of behavior together or searching for other structural properties in data; discovering which variables are most important for discriminating among populations. Some of the techniques discussed are information theory; hierarchical cluster analysis; multidimensional contingency table analysis; multidimensional scaling; factor analysis and principal componenet analysis; multivariate analysis of variance and discriminant analysis; systems analysis; and modelling. All you have to do is to match the problem to the technique...et voila!

The authors and editor appear to have given considerable thought to ways of interpreting complicated concepts for the non-mathematician. Tables, graphs, and diagrams set in terms of ethological examples have been used liberally. They are very helpful.

HANDBOOK OF ETHOLOGICAL METHODS by Philip N. Lehner. New York: Garland Publishing Co., 1979.

This is a 'how-to' book. It was designed for any ethologist who has puzzled over how to observe, how to record observations, how to analyze data, or how to interpret the results. It might serve as a text for a field study or lab course.

Lehner has found a useful level of technical detail. In the section on data collection equipment, for example, he discusses photographic equipment in 16 pages. The discussion covers still photography and film, black-and-white and color. It gets down to specifics of types and brands of camera. There is a lot of information ('The Knoica 35AF features an automatic focusing mechanism.') sprinkled with down-home advice ('You should choose a camera that is ruggedly built and handles well.'). But the reader is not overwhelmed by detail. More technical sources are referred to for further information. A short section on metronomes for timing of field observations includes schematics for constructing your own.

Roughly two thirds of the book is devoted to research design and data collection. The remainder is concerned with analysis. In this section, again, the level of detail is less than that of a text on statistics. However, enough information is given (including tables) to carry out a number of statistical procedures. The book devotes particular attention to descriptive statistics of interest to ethologists, such as rates of behavior, sequence analysis, and spatial patterns.

In general, this book can be recommended as one which delivers on its promise.

BIOPOLITICS: SEARCH FOR A MORE HUMAN POLITICAL SCIENCE. Thomas C. Wiegele, Westview Press, Boulder, Colorado, 1979.

The thesis of this short book is simple and is one that perhaps all human ethologists would applaud: that political science must begin to include considerations of human biology in its analyses of human political behavior. Wiegele begins with a brief chapter on the need for a biological perspective in political science and follows it with a chapter on the various approaches followed by the handful of political scientists who have already begun to work with biological concepts and data. The remainder of the book, except for a summary concluding chapter, deals with how Wiegele and others relate such biological-ethological subjects as dominance, territoriality, altruism, circadian rhythms, stress, handedness and birth order to such political science concerns as political elites, international relations, conflict, charisma and more.

Wiegele is not, however, writing for human ethologists, but for his fellow political scientists. Many human ethologists will be dismayed by Wiegele's failure to include the works of such biologists and ethologists as W. D. Hamilton, Lionel Tiger and John Maynard Smith, who have themselves already tackled substantive issues directly relevant to political science. Many more will be concerned about Wiegele's oversimplification of ethological concepts and his over-readiness to see their parallels in human political phenomena. Inspite of their objections however, human ethologists must credit Wiegele for attempting to branch out and include the findings and theories of a new field that should be relevant to his own. Moreover, there is considerable value in this book for ethologists who wish to acquire a quick overview of current issues in political science - if only to see if they can go further in synthesizing than Wiegele has.

James Chisholm
Laboratory of Human Development
Harvard University

HUMAN BEHAVIOUR AND ADAPTATION. V. Reynolds and N. Blurton Jones (Editors). London: Taylor and Francis, New York: Halsted, 1978.

This book presents the proceedings of a two-day symposium held in Oxford in early 1977. It was organised by the editors for the Society for the Study of Human Biology. In fact, the 14 papers split evenly into two separate symposia, each with an introduction by its respective organiser. The common theme is adaptation: in one case, the long-term ecological aspects of cultural adaptation (Blurton Jones); in the other, the short-term physiological aspects of individual adaptation (Reynolds). Each editor tries bravely to weave his day's papers together, but as is so often the case with colloquia, they remain a somewhat motley collection. Furthermore, one seaches hard for any given links between the two main areas, although there is one big exception (see below). Faced with this fragmentation, this reviewer finds it impossible to discuss the volume as a whole and so will touch upon its constituent papers.

In the ecologically-oriented first half of the book, Durham leads off with a rather unfortunately named paper on the "coevolution" of human biology and culture. Much of it has appeared in his other papers, and large chunks are to be found verbatim in at least two other, similar anthologies. Vayda's review of recent advances in ecological anthropology is out of date, being a revision of an earlier paper published in 1975.

It still provides a wealth of references however (N=108). McFarland presents a clear theoretical article on optimisation in animal behaviour, but only the final section is new and just begins to tackle the special case of Homo sapiens. Rappaport's paper on the adaptiveness of ritual is abstruse, containing such sentences as: "Certainty is one of the grounds upon which unquestionableness stands, the other is the acceptance intrinsic to the performance of that which is certain." (p. 90). Packer's brief paper on the dangers of thinking in group-selectionist terms when considering the structure of primate society is apt; several of the other contributors seem unaware of the problems involved. Unfortunately, the paper is sketchy and makes no mention of either human behaviour or adaptation. Harris' article on the subsistence activities of forest-dwelling Australian aborigines is a fascinating reconstructive account. However, it arrives at certain conclusions which seem most unlikely, e.g. that aboriginal culture maintained its population density at levels well below that which food resources and technological skills would have allowed.

As mentioned earlier, the truly masterful paper of the book is Blurton JOnes and Libly's attempt to test the adaptiveness of birth-spacing by Kalahari Bushmen women. If one has time to read only one paper in this collection, this is the one. They bring together such factors as food requirements, energetics of load-carrying, time spent foraging, and heat stress to show convincingly that the observed birth-interval of 4 years is highly likely to represent maximization of reproductive success. The approach is original and provocative and should serve as a model for much future research. It leads logically into the second half of the book.

Reynolds reminds the reader of the importance of homeostatic adaptations to day-to-day stresses, and the papers in the latter section exemplify this. Hutt and Hutt argue convincingly for the utility of heart-rate variability as a sensitive indicator of arousal. Helevuo and Reynolds present disappointingly inconclusive data in an attempt to link behaviour of children in free-play with excretion of catecholamines. They compare normal and autistic children, but the study suffers from methodological problems. The most notable papers in this section are those of Montagner and his colleagues, also on the relationship between children's behaviour and endocrinal rhythms. After many publications in French, these are the first in English. The ethological analyses of behavioural sequences are detailed and well-illustrated and lead to useful profiles of seven types of child, e.g. dominated-aggressive. The second paper is full of figures of circadian rhythms of corticosteroid excretion, but the reader is frustrated by the lack of knowledge of what goes on in the children's lives outside the kindergarten. The final three papers (by Theorell, Carruthers, and Johansson and Lundberg) deal straight-forwardly with the psycho-socio-physiology of stress.

The book is well-produced and contains a useful author-index. The subject-index is less complete, e.g. there is no entry for altruism although the topic arises in several places. I doubt if many individuals will wish to buy it, but it should be in every university's library, if only for the Blurton Jones and Sibly article.

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Current Contents

Current Contents is a relatively new section of the newsletter. All subscribers and other researchers in the field are invited to notify the editor (Cheryl Travis) of recent publications or papers presented at meetings which would be of interest to ethologists. You may or may not include key words for identifying the content of the paper ,at your discretion. The goal of Curent Contents is to alleviate the publication lag between presentation and proceedings or between acceptance and publication in a journal. Because Human Ethology incorporates many disciplines, it is also helpful to centralize lists of pertinent articles and papers. An annual update has been conducted for the past three years by means of the Human Ethology Abstracts; see the notice in earlier pages of this newsletter.

● Barnett, S. A., Cooperation, Conflict, Crowding and Stress: An Essay on Method, Interdisciplinary Science Reviews, Vol. 4, No. 2, 1979.

In this review I discuss, in an ethological framework, some of the principles of method that lie behind concepts such as those of altruism, crowding, dominance, stress and territory; and I try to replace the illusion that ethology can solve the problems that face us with a statement of what ethology can actually do. I hope in this way to contribute to theuse of exact and rational methods in the sceince of behaviour.

● Mackey, Wade C., Taikio College, Parameters of the Adult-Male--Child Bond, Ethology and Sociobiology 1:59-76 (1979).

Proxemic relationships between adults (men and women) and children (boys and girls) were investigated in ten cultural areas on five continents. All data were collected via naturalistic observation in places of public access with equal access by gender during daylight hours.

● Mackey, Wade C., South Dakota State University, Some Indicators of Fathering Behaviors in the United States: A Crosscultural Examination of Adult Male-Child Interaction.

In this study, the adult male-child dyads of five countries (United States, Ireland, Spain, Japan, and Mexico) were examined at the proxemic level. It was found that American men (compared to American women) do not associate or interact with children much differently than men (compared to the respective women) in other countries. Of special interest were the findings that American men do associate with children in large numbers when the societal norms allow them access to the children and that American men interact with children at levels consonant with adult female-child dyads. These findings challenge the idea that American children are particularly deprived of nurturing behaviors from the father figure.

● Wicker, Allan W., Ecological Psychology: Some Recent and Prospective Developments, American Psychologist, Sept., 1979, 755, Vol. 34, No. 9, 755-756.

Recent and prospective developments in ecological psychology include streamlined descriptive surveys of community and organization behavior settings; the study of intersetting linkages and of the life cycles of settings; applications of manning theory and research on stress to work settings, particularly to settings that serve the public; extensions of manning theory by drawing on the literature on small-group performance and job enrichment; and the development of a technology for improving human environments.

INTERNATIONAL SOCIETY FOR HUMAN ETHOLOGY

Membership and Newsletter

The ISHE was formed with the goal of promoting ethological perspectives in the study of humans. An ethological perspective encourages empirical research which addresses the questions of individual development, environmental, ecological and social processes which elicit and support certain behavior patterns, the function and significance of behavior, and comparative and evolutionary problems. The society maintains an executive board and a number of committees, publishes a quarterly newsletter, collates an annual selection of human ethology abstracts, and meets annually in conjunction with the Animal Behavior Society.

Membership to the Society and subscription to the newsletter is $5.00, and payable on a calendar year basis each January; this is true regardless of when you joined the society during the previous year. Make checks payable to the International Society for Human Ethology. Checks must be drawn on U.S. or Canadian banks; otherwise send U.S. currency. The expense of processing other payment forms usually exceeds the cost of the subscription. Please make sure that the mailing address for your subscription is printed clearly below.

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NEWSLETTER MATERIALS SHOULD BE PREPARED PHOTOCOPY READY, SINGLE SPACED WITH HALF INCH SIDE MARGINS. STANDARD BOND PAPER SHOULD BE USED, NOT ERASABLE, AND ELECTRIC TYPEWRITERS ARE BEST USED IN ORDER TO OBTAIN THE CLEAREST POSSIBLE COPY. THE DEADLINE FOR SUBMISSION OF MATERIALS IS JANUARY FIRST, APRIL FIRST, JULY FIRST, OCTOBER FIRST. SEND MATERIALS , UNFOLDED, TO DR. CHERYL TRAVIS, DEPT. OF PSYCHOLOGY, UNIV. OF TENNESSEE, KNOXVILLE, TENNESSEE, USA 37916.

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