

CONSERVATION: ARE WE IN TIME?



HUMAN ETHOLOGY NEWSLETTER

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LOGO

The masthead was designed by H. I. Fine (1983), representing the requisite thoughtfulness of Man (portrayed at the top) regarding conservation, typified by the American bison (left bottom). The drawing was provided to HEN by permission of the artist and the Host (Douglas Candland) and Program Officer (Terry Christenson) of the recent Annual Meeting of the Animal Behavior Society in Lewisburg, Pa, June 19-24. ISHE met in conjunction with ABS this year and the program and accommodations at Bucknell University were great!

Conservation Projects:

The International Primatological Society and The World Wildlife Fund are sponsoring a two year preservation plan with primary goals of funding and implementing four programs: (1) the mountain gorilla project in East Africa, (2) the Korup National Park project in West Africa, (3) the campaign to save the Woolly Spider Monkey in Brazil, and (4) the primate conservation training program. The National Zoological Park (Smithsonian Institution, Washington, D.C.) and the Florida State Museum (University of Florida at Gainesville) have developed a training program designed to promote primate conservation by providing scientific training to nationals from countries with wild populations of primates. This training program consists of an 8-week course which complements a degree program (both Masters and Ph.D.) through the Dept. of Zoology or School of Forest Resources and Conservation at the University of Florida.

The San Diego Zoo provided a birthplace for three California Condor chicks born in April and May and marking the first time this endangered species has been born in captivity. The eggs were taken from nests in Ventura County and incubated at the Zoo where there are three other condors, all male. The scientists plan to raise the chicks for captive breeding with the hopes of saving the species according to the article in the June 1983 issue of *Audubon Action*.

PLEASE NOTE

The March (1983) issue of HEN was mislabeled Volume 4. It should have appeared as Volume 3, Issue 9.

BALLOTS

At the ISHE Business Meeting at Bucknell, it was decided to submit two mail ballots (appended) to the membership for consideration. The first concerns the location of our next annual meeting (alone in Montreal; with ABS at Cheney, Wa; with APA at Humbolt, Ca); and the second issue is with respect to the possibility of adopting *Ethology and Sociobiology* as our official journal. The alternatives in the latter case range from a 20% discount for the Society, with minimal strings attached, to a mandatory subscription included with our yearly dues. PLEASE VOTE AND RETURN THE BALLOTS IMMEDIATELY.

HUMAN ETHNOLOGY ABSTRACTS

Appreciation goes to Wade Mackey who has compiled the most recent contribution of Human Ethnology Abstracts. He has sent them off for publication and they are tentatively scheduled for publication this September in Man-Environment Systems.

ERRATA

Please note errata for the Mini Communications article entitled "Are female cercopithecines altruistic to their daughters?" which appeared in the March issue - HEN 3 (9), 1983.

Paragraph 3, last sentence: "The author's interesting results that female vervets do not differ with respect to reproductive success though they differ in other ways behaviorally may be interpreted rigorously in terms of inclusive, especially matrilineal, interests."; and

Paragraph 8, last sentence: "My discussion proposes that it is not necessary to invoke nepotism or kin selection in order to explain the author's results and predicts that sisters are not generally altruistic to one another, SETERIS PARIBUS, implying that selection acts directly on mothers, apparently favoring those who aid daughters."

WORKSHOP

Report on Life History Workshop at UCLA

The life history workshop at UCLA on May 21st was a success! The papers stimulated discussion that went on throughout the day and at the evening reception. The program and abstracts of the nine papers appear below. A workshop report will appear in a forthcoming issue of Ethnology and Sociobiology.

There were 24 people at the workshop, representing

10 different institutions in primarily the areas of anthropology, biology, and psychology. The ability of this workshop to draw people from all over the U.S. shows that a small, informal meeting need not consist entirely of local people. With a little bit of funding from the Department of Psychiatry at UCLA, and one speaker funded by giving an additional lecture in another department, plus whatever travel money participants could obtain from their home institutions, we were able to assemble a geographically diverse group. Although no registration fee was required, it is apparent that a small fee could help fund a few speakers of future workshops.

We strongly urge other ISHE members to organize comparable meetings since good success was obtained with this workshop on a topic that is relatively unfamiliar and on which little research has been conducted.

Program:

Morning Session:

- (1) Life History Strategies and Human Behavior: An Introduction. Ronald M. Weigel, Human Ethnology Laboratory, Department of Psychiatry, UCLA.
- (2) Female Choice, Male Life Histories, and Male Celibacy in U.S. Poverty Classes. Mildred Dickmann, Department of Anthropology, Sonoma State University.
- (3) Decisions About the Timing of Reproduction in Humans: An Evolutionary Life History Approach. Ronald M. Weigel, Department of Psychiatry, UCLA and M. Margaret Weigel, Department of Anthropology, UCLA.
- (4) Cross-Sectional Observational Data as Longitudinal Explanations of Human Male Strategies. Joan S. Lockard, Department of Psychology, University of Washington.

Afternoon Session:

- (1) Alternative Life Histories: The Generation of Individual Differences. Eric L. Charnov, Department of Biology, University of Utah.
- (2) Stochastic Selection and Life History Theory. Peter T. Ellison, Department of Anthropology, Harvard University.
- (3) Sex Differences in Life History Strategies. Daniel G. Freedman, Committee on Human Development, University of Chicago.
- (4) A Characterization of Primate Adolescence: Toward an

Ethological Theory of Adolescence. Ritch C. Savin-Williams, Department of Human Development and Family Studies, Cornell University.

- (5) Concluding Comments: Life History Analysis and Human Development. Nicholas G. Blurton Jones, Departments of Education, Psychiatry, & Anthropology, UCLA.

Abstracts:

LIFE HISTORY STRATEGIES AND HUMAN BEHAVIOR: AN INTRODUCTION, R.M. Weigel.

Evolutionary life history theory suggests that lifetime reproductive success is an appropriate measure of fitness for stable populations. There is an emphasis on lifetime, rather than seasonal reproductive success in recognition of (1) the decreased probability of subsequent survival due to any given attempt at reproduction, and (2) age-related trends in expected reproductive success, which may make postponement of reproduction an adaptive choice at times. Thus, life history theory focuses on the optimal schedule for allocation of reproductive effort throughout the lifetime, so as to maximize lifetime reproductive success. Considerations of the abundance and stability of available resources leads to the identification of two major reproductive strategies: (1) the r-selected strategy with rapid development, early reproduction, high fecundity, low parental care, and high adult and offspring mortality, and (2) the K-selected strategy, with the opposite traits.

In applying evolutionary life history models to humans, it is apparent that maternal age-related trends in fertility, pregnancy-related mortality, and infant viability should be major determinants of life history reproductive strategies. Social and economic factors should also affect reproductive strategies, particularly for males. Conditions of poverty should be associated with an r-selected strategy, and wealth with a K-selected strategy.

The study of child development within an evolutionary life history framework should focus on two issues: (1) what factors affect reproductive success in adults, and (2) what are the correlations between child and adult behaviors. Variation in these relationships as a function of environment should be expected.

Finally, evolutionary life history analysis is contrasted with lifespan developmental psychology and demography, with which it shares some common interests.

FEMALE CHOICE, MALE LIFE HISTORIES AND MALE CELIBACY IN THE U.S. POVERTY CLASS, M. Dickemann.

In humans, as in other vertebrates, male reproductive strategies are contingent upon control or potential control

over resources (parental investment). Thus, in human societies, relative access to and control over resources by males (and their kin) or by females (and their kin) largely determine the degree to which the male or the female exercises choice in mating, parenting and the social relations which support these reproductive functions.

Previously, I have tried to show that stratified agrarian societies are characterized by strong male kin control of resources, and consequent patrilineal control of mating at the top, and the greater female control of resources (which are in absolute terms far fewer) and mating at the bottom. The demographic consequence of this class-stratified system is an excess of non-reproductive (or removed) females at the top and an excess of non-reproductive males at the bottom.

A provisional analysis of U.S. poverty groups in these terms (based on readily available descriptive accounts) reveals increasing female mate choice in contexts in which male investment is minimal, unpredictably or non-existent, as stable families, and female-centered kin groups cooperate to exclude the most economically unstable males from access to reproduction, thus producing a celibate underclass. The way in which social space apparently operates to control sexual access will be discussed. It appears that early socialization of the poverty-class male prepares him for entry into any one of three life histories: that of the stably employed family man, that of the marginally employed, loosely bonded male, and that of the largely celibate street corner man. Passage between these life history trajectories occurs, but is largely downward.

In conclusion, some remarks will be made on the inadequacies of existing data and the methods and data necessary to a test of these propositions.

DECISIONS ABOUT THE TIMING OF REPRODUCTION IN HUMANS: AN EVOLUTIONARY LIFE HISTORY APPROACH, R.M. Weigel and M.M. Weigel

If decisions about the initiation and termination of pregnancy in humans have been affected by evolutionarily adaptive considerations of lifetime reproductive success, then such reproductive outcomes measures as maternal fertility, pregnancy-related mortality, and infant viability, all of which are a function of maternal age, must be important determinants of reproductive decisions. Other factors which should affect reproductive decisions are parity, interbirth interval, maternal health and nutrition, maternal reproductive history, economic status, and genetic factors, all of which affect pregnancy outcome.

A decision-making model is presented, in which the expected costs and benefits of current reproduction are compared to the costs and benefits of delaying reproduction, in terms of lifetime reproductive success. Implications of this model for research into the decision-making processes of women in technologically advanced societies, where contraceptive use are readily available, are discussed. A

research design is proposed, where the subjects are American women of reproductive age, chosen from obstetricians, gynecology, and family planning clinics. The data would be obtained from medical records, supplemented by interviews and/or questionnaires. Analytic methods are presented for investigating whether evolutionarily adaptive decisions are being made, and what factors (e.g., religion, education) may be responsible for making maladaptive decisions.

CROSS SECTIONAL OBSERVATIONAL DATA AS LONGITUDINAL EXPLANATIONS OF HUMAN MALE STRATEGIES, J.S. Lockard.

Two studies by Lockard and Adams (1980, 1981) utilized demographic observational data on age/sex groupings in public to propose life-history strategies of males. This approach assumed that the numbers and composition of cross sectional groups comprised of younger or older individuals could be viewed as representing longitudinal changes in group membership as a function of age. The merit of the findings in the two studies rests predominantly on the validity of this assumption. For example in the 1980 paper, the frequency and sex composition of groups below and above the ages of 18-20 year olds were utilized to explain a greater than expected number of same-sex subadult triadic male groupings regarded as "peripheral" males. Similarly, in the 1981 article, a reproductive strategy of serial polygyny was proposed as a function of the cross sectional comparison of the frequency of mixed-sex dyads where the members were of various differential ages.

Some of the limitations of this research approach are obvious as, for instance, during times when a society is in rapid flux with respect to demographic parameters or even during relatively stable times if the age differential of the projection spans many years, say from 15 to 65 year olds. However, covert problems such as subtle sampling and regional differences in age composition and density may be the more worrisome, in that their detection is less likely. An enumeration and discussion of the possible problems and subsequent data biases in using cross sectional data as indicative of longitudinal changes in group composition seem imperative to the continued application and fruitfulness of this relatively new human research paradigm. (This research was supported by the Harry Frank Guggenheim Foundation.)

Lockard, J.S. and Adams, R.M. Peripheral males: A primate model for a human subgroup. *Bulletin of the Psychonomic Society*, 15 (5):295-298, 1980.
Lockard, J.S. and Adams, R.M. Human serial polygyny: Demographic, reproductive, marital, and divorce data. *Ethology and Sociobiology*, 2:177-186, 1981.

STOCHASTIC SELECTION AND LIFE HISTORY THEORY, P.T. Ellison.

In addition to the commonly adopted, deterministic approach to life history theory there is a second, stochastic approach of equally venerable pedigree. Originated by Darwin's cousin, Francis Galton, this approach has come down to us through Fisher and Lotka to the more recent treatments of Lewontin and Cohen, Haldane, Charlesworth and Williamson, Templeton, and to a certain extent Gillespie.

The stochastic approach to life history theory does not measure fitness by an intrinsic rate of genetic proliferation, but rather by the complement of the probability of lineal extinction. That is, for a given individual in a population one does not ask how many descendants it will have, but what the probability is that, at an arbitrary point in the future, it will have any at all. Thus a process with infinitely many possible outcomes is treated as having only two, persistence or extinction.

As a measure of fitness, persistence probability differs from intrinsic rate of increase in an important way: it is dependent on all the moments of the reproductive success probability distribution and not just the mean. From the stochastic viewpoint there is all the world of difference between having two offspring with certain survival and four each with a probability of surviving of 1/2. The deterministic approach does not, and cannot, distinguish between these alternatives.

Of consequence for human life history theory, the stochastic approach suggests a reinterpretation of the selection continuum. Mean reproduction success is at a premium always, but a dichotomy exists between strategies to increase persistence by increasing the skew of a reproductive outcome distribution, and strategies to decrease the variance of that distribution.

Humans represent a strong commitment to the variance minimizing strategy (although males may pursue something of a mixed strategy), and features of human birth spacing can be interpreted in this light. In fact, from the stochastic viewpoint, variance reduction can, under certain conditions, even increase fitness at the expense of mean reproductive success. This may allow for a reconciliation of natural selection theory with human fertility behavior during the fertility decline phase of the demographic transition, a reconciliation which Barlow and Burley have found unattainable from the deterministic viewpoint.

SEX-DIFFERENCES IN LIFE-HISTORY STRATEGIES, D.G. Freedman.

Differences in gametic potential are assumed to underlie cross-culturally consistent differences in heterosexual strategies over the life span. Starting at about four, males tend to aggregate into same-sex play groups which are 2 or 3 times as large as female play groups (Dmark, 1968; Donaldson, 1973). At these ages (and thereafter), males exhibit

considerably more "over-rating" of their own "toughness" and good looks than do females, and this tendency to over-rate the self seems to fuel competitiveness, which in turn yields a dominance-submission hierarchy.

Pre-adolescent inter-sexual play tends to anticipate courtship behavior (Parker & Freedman, 1971), and when boys and girls compete, dominant girls tend to suppress their dominance while sub-dominant boys become more dominant (Cronin, 1980). With courtship, these tendencies are exaggerated in what Guttman (1973) has called the "parental imperative." Courting women, in his S-culture study, tended to suppress egoism, permitting the male full reign for his. With the advent of late middle age, however, women of all 5 cultures become more assertive, and the married men who are best "adjusted" become relatively passive and non-aggressive. The life-cycle picture thus highlights sexual complementarity until the last phase, when increasing mutuality is seen.

A CHARACTERIZATION OF PRIMATE ADOLESCENCE: TOWARD AN ETHOLOGICAL THEORY OF ADOLESCENCE, R.C. Savin-Williams.

Two fundamental issues concerning adolescence remain unresolved: are there universal characteristics of the adolescent and his or her behavior; and why is there a pubescent, pre-adult stage of life in all primates. Previous examinations of these questions are limited because they have focused on human adolescents living in advanced Western societies.

In this workshop, our approach is to consider these questions from an ethological perspective, speculating as to the nature and possible evolutionary significance of adolescence in the life course. Pertinent issues during adolescence for all anthropoid primate species include (1) the inevitability and the causes of stress, (2) developmental tasks and risks, and (3) identification of universal behaviors and developmental goals. Additional topics could include how to define adolescence, growth characteristics of pubescence, parent-child interaction, peer interactions, sexual behavior, and adult status -- all from an evolutionary perspective.

CONCLUDING COMMENTS: LIFE HISTORY ANALYSIS AND HUMAN DEVELOPMENT, N.G. Blurton Jones.

There are a few points at which developmental psychology seems to be looking for an adaptationist approach to development and parent-offspring interaction. Some of this is outlined in order to start off our final discussion period.

Some Introductory Readings on Evolutionary Life History Theory:

Gadgil, M. and Bossert, W.H. (1970). Life historical consequences of natural selection. *American Naturalist*. 104:124.

Geist, V. (1978). Life Strategies. *Human Evolution, Environmental Design: Toward a Biological Theory of Health*. New York: Springer-Verlag.

Horn, H.S. (1978). Optimal tactics of reproduction and life history. In: J.R. Krebs and N.B. Davies (eds.) *Behavioral Ecology: An Evolutionary Approach*. Sinauer.

Pianka, E.R. (1976). Natural selection of optimal reproductive tactics. *American Zoologist*. 16:775-784.

Pianka, E.R. and Parker, W.S. (1975). Age specific reproductive tactics. *American Naturalist*. 109:453-464.

Stearns, S.C. (1976). Life history tactics: A review of the ideas. *Quarterly Review of Biology*. 51:3-47.

CHANGE OF EDITOR

First announcement of a change of HEN editor starting January 1984. Bob Adams will assume the editorship for a period of at least two years beginning with the first issue in 1984. The Executive Board gave their consent a year ago at the instigation of your present editor. The rotation of this responsibility assures continual vigor on the part of the editor and new ideas to maintain the cohesiveness and communication of the membership. The changes involved will hopefully be effected in a gradual and orderly manner and will be announced in the subsequent two remaining issues of this year's newsletters.

BOOK REVIEW

GRAMMATICAL MAN: INFORMATION, ENTROPY, LANGUAGE, AND LIFE
By Jeremy Campbell, Simon and Schuster, New York, 1982

Reviewed by David Alan Munro
Laguna Beach, CA

Jeremy Campbell's Grammatical Man belongs to the genre of the single-adjective man: as naked, moral, marginal, imperial, etc. The device always goes beyond the catchiness of title to a distorting obligation upon author. But Jeremy, hoist however high on his own petard proves an able aerialist. He relies upon Chomsky for a hold on substantive theory and ultimate safety net -- and, of course, for justification of his title. He would tell us that if we had our wits about us, we would drop our search for ethnological man as probably merely partial. We should look for grammatical or communication man. Just reread your Chomsky, and you'll see.

Campbell's grand goal, however, has little to do with grammar. Under his breezy journalistic exterior, and sportive chapter titles -- e.g. "Jumping the Complexity Barrier", "The Brain as Cat on a Hot Tin Roof and Other Fallacies" -- beats the heart of a science populist. He is an exponent of a current Darwinian revision, possibly the first serious challenge to the original, 1859 theory. The unsettling question of the revisionists, an outgrowth of expanding biological science, is: "Can we read intelligence into the growth of structures facilitating walking, talking, flying, seeing, swimming, thinking and so forth?"

To be sure, Chomsky -- in either his political or scientific capacity -- might well be startled to find himself invoked on the polemic positive side of such a question. Biologists, however, have been thinking for years along these lines. Thus, Richard C. Lewontin, Harvard biologist, reports in some detail, the results of "The Dialectics of Biology Group" at its 1980 meeting in Bressanone, Italy (New York Review, January 20, 1983).

Campbell notes that it was not until Darwin that the pervasive man-as-machine thinking took over in biology, and he suggests that biology may be the first to escape this controlling metaphor. "Over and over again," he writes, "the great biologists of the last century have begun as mechanists and have ended searching for the immortal hand that framed the symmetries they studied...the embryo's development...is less like a machine than it is like a language whose elements, despite a superficial identity of morphology, take unique meaning from their context..."

Innocent cells seem to know what they're doing as they go about the business of making an arm, a leg, a brain. Furthermore, what they do not do historically seems to be under a non-Darwinian control. Jeremy, the journalist, quotes Lewontin asking, "Why are there no organisms with wheels?" And, Boyce Rensberger adding, "The fact that certain conceivable organisms are unknown [may indicate] natural laws that govern the way cells assemble themselves into specialized tissues. No one knows what the laws are..." p.174-5.

But, if we are ignorant, we at least know the revised vocabulary. Campbell notes that the late John von Neumann would replace "force, energy, work, power" in brain-state discussion with "codes, signals, messages, information." We should rethink functions, from the DNA to the synapse, as signal behavior.

This is more than a change in nomenclature. It permits a change in metaphor. And if our human fate is to think in metaphor, then the better the metaphor the better the thinking. We go from the mechanics of bits and pieces, the old reductionism, to the dialectics of biology -- a grammar, if you will, some kind of system that has rules though we do not know what they are.

Ethnological man, on the other hand, as we are just beginning to describe him, is the creature of what works, or has worked recently enough in our past to remain operative in our genes at present. As scientists, we have been trained to look backward. The dialectics of biology would bid us look forward. Possibly the full directive would add a time element: rapidly-changing species, such as Homo, are more likely to be in transition than slow-changers, like ants.

In sum, the revolt of the biologists provides us searchers with little more than a hunting license. We are told that the elusive animal may not be in the territory Darwin marked off, after all. He is over the hill, in terra incognita.

Lewontin informs that a strand of Marxism was woven into the Bressanone conference. This, no doubt, explains the use of the word, 'dialectic,' where system would have served better. He looks, however, for the scientific substance of what was said, not the political. You history buffs will no doubt readily compare the current ideological intrusion with the churchly intrusion, of the Rev. Sam Wilberforce, et al., of 100 years ago.)

Nor is Chomsky the help that Campbell so confidently expects him to be. The Chomsky/Lenneberg contribution to defining ethnological man was considerable, but defective. It has a built-in dead-end.

Both Chomsky and Lenneberg note, early-on, that imprinting in its full duckling sense is the explanation of language

acquisition (Chomsky, 1957; Lenneberg, 1967). Chomsky also cleared the air -- for all linguists aware of psychology, and all psychologists aware of linguistics -- with a devastating denunciation of behaviorism in a review of Skinner (1957), a work in which Lenneberg collaborated (1959). Now Chomsky goes on, after the regrettable suicide of Eric Lenneberg, to explore the logical possibilities which flow from the language model. For it seemed clear to him as to other psycholinguists, that if universals of language spring from an universal prewriting of human brains for language, then such prewriting must explain all other universal human behaviors.

But the Chomsky banner reads "constraints." His findings are negative. Not all imaginable languages can occur. And certainly not all possible languages do occur. Thus his constraints tell us what is not, rather than what is. No new psychology has emerged from Chomsky or the Chomsky camp, though their findings support an ethological psychology now struggling to be born.

None of this appears in Campbell. In fact, I've carefully read it all so you would not have to.

References

- Chomsky, N. 1957. *Syntactic Structures*, Mouton, The Hague.
- Chomsky, N. 1959. Review of Skinner's verbal behavior, *Language*, 35 (1), 26-58.
- Chomsky, N. 1980. *Rules and Representations*, Columbia University Press, New York. Contains a retrospective chapter on Lenneberg's major contribution.
- Lenneberg, E.H. 1967. *Biological Foundations of Language*, Wiley and Sons, New York.
- Lewontin, R.C. 1983. The corpse in the elevator. *New York Review*, January 20.
- Skinner, B.F. 1957. *Verbal Behavior*. Appleton-Century-Crofts, New York.

CHIMPANZEE POLITICS. POWER AND SEX AMONG THE APES.
By Frans de Waal, Jonathan Cape, London, 1982, 223 pp., £8.95

Reviewed by W.C. McGrew
University of Stirling, Scotland

Frans de Waal has produced the best book ever written on the social life of apes in captivity. It deserves to stand alongside Kohler's classic (*The Mentality of Apes*) on the intellectual capacities of caged chimpanzees. Whilst this

will come as no surprise to those who have been following de Waal's journal articles over the past few years, the full-length result exceeds even those high expectations. Text aside, the book would be worth its modest price for the 117 superb photographs alone---they are of the highest quality.

So, what has the author done to deserve this praise? He and his colleagues have observed for the last few years the behavior of some 25 chimpanzees in a 2-acre outdoor enclosure in the Arnhem (Netherlands) Zoo. This in itself guaranteed nothing---others have had similar opportunities before but produced little (with Menzel's work at Delta being a conspicuous exception). What happened here was patient and careful observation in the tradition of Dutch ethology plus rich interpretation in the style of Griffin's (*The Question of Animal Awareness*) newer "cognitive ethology."

The combination reveals a social milieu in which Machiavelli's prince would have felt at home. Coalitions and betrayals, privilege and persecution, the overwhelming influence of social rank---all are specifically documented. Revealing anecdotes abound, e.g. the sexually aroused but subordinate male who covers his erection with his hand in the proximity of the alpha male and a receptive female. Important empirical findings are summarised, e.g. submissive greeting is the most reliable indicator of social order. (The data behind these conclusions are to be found in the author's articles in well-known journals but are not presented in this semi-popular account).

Is it anthropomorphic? Yes, in the sense that de Waal infers intentions, motives and strategies from the acts of the chimpanzees in the same way that you or I would for fellow human beings. But his inferences are convincing for several reasons: One is that he treats his subjects as individuals and much of the book is devoted to establishing this, a la Jane Goodall. (This is one of several parallels with her in the *Shadow of Man*.) Another is that the whole project repeatedly shows ingenuity and initiative, e.g. successfully teaching a non-lactating foster mother to bottle-feed an adopted infant. Finally, and most indefinably, the author has that special empathetic insight into the mind of the chimpanzee which is shared by few (e.g. Brewer, Savage-Rumbaugh, Goodall, Kohler) but can somehow be recognised by many.

Least this review sound overly effusive, there are flaws to be found. Several times (p.18, 185) the author suggests that these findings are somehow uniquely accessible at Arnhem, in contrast to the wild. In fact, nothing described in the book could not also be studied at long-term field-sites such as Gombe or Kasoje in Tanzania. What Arnhem presents is convenience, in exchange for some costs, e.g. the fact that the captive group is unnaturally together all the time, whereas the members of a wild community are rarely, if ever

all together at once. On another front, de Waal concludes (p. 166, 172) that the chimpanzees cannot be aware of the paternity of offspring, but ample evidence now exists of kin recognition in other species, even in non-monomorous forms.

In style and format, the book is better than average. The translation into English is excellent, so much so, that the translator, Janet Milnes, might have had title-page credit. Citations are not given in the text, but a useful list of references is provided, although these are not specified by chapter. As seems to be fashionable these days, the table of contents is practically useless, with each of 5 chapters have no more than a 2- or 3-word title. Luckily, there are no prominent sub-headings within each chapter. The index is adequate but not more.

Overall, it is a book to be prized.

THE PLACE OF ATTACHMENT IN HUMAN BEHAVIOR
Edited by C.M. Parkes and J. Stevenson-Hinde,
Tavistock Publishers, London and New York, 1983

Reviewed by Peter K. Smith
University of Sheffield

This book is a tribute to John Bowlby's work on Attachment and Loss. Bowlby hypothesized that the nature of early parent-infant attachment had profound implications for later adult life. The 14 chapters in this book develop and examine various aspects of his approach and of his theory.

The first section concentrates on infant-mother attachment. Unfortunately, many researchers still use 'mother' as shorthand for 'parent' or 'attachment-figure', and the usual excuse for this and other sexist language appears as a footnote on page six.). Ainsworth gives a useful review of the work which she and her co-workers have done, including the well-known 'strange situation' assessment. This is considered further in the Main and Weston chapter (a longer version of which appears in Immelman et al's 'Behavioral Development') where the reasons for avoidant behaviour of attachment figures are considered. Hinde contributes a short but thoughtful chapter, pointing out, for example, how attachment functions to promote later independence as well as immediate proximity. He also considers the puzzling apparent difference in attachment types between U.S. and German samples.

The second section considers problems in parenting. Trowell writes about the effects of early mother-child contact on later relationships, although she is rather uncritical of the Klaus and Kennell work, and some recent and more cautious research findings are not reviewed. Dalozier reviews attachment in relation to child abuse; the review is good within its narrow brief, but does not consider related aspects

such as types of abuse or neglect, and the higher abuse levels in step-parent families. Pound contributes a short chapter on the effects of maternal depression on attachment, and Raphael on the fairly immediate effects of parental death on young children. Heard and Barrett relate attachment theory to specific reading disability.

The two chapters in the third section consider bonding in adult life. Weiss writes about adult attachments and friendships, and postulates a connection to infant attachment; a theme developed by Marris who supposes that early attachments provide a framework of meaning by which later relationships may be constructed. These two short chapters are both very interesting, but relatively short on evidence. The fourth section, by contrast, presents some very detailed evidence relating childhood experiences to adult life disorders; to maternal depression in Brown's chapter and to suicidal behavior in Adam's. Henderson considers the importance of social relationships generally in the etiology of neurosis, and Parkes provides a short final chapter on ways of helping people cope with loss. Brown's chapter is an especially thorough consideration of evidence and models, with emphasis on the effects of adult vulnerability factors as well as early loss in precipitating depression.

The book is a useful reference for recent work in the Bowlby tradition. This is not to say that the authors are uncritical of details of Bowlby's work; they often are. However, the book is conceived and edited within this framework, and thus it does not significantly embrace other approaches, such as the social psychology of interpersonal attraction, or the biology or sociobiology of imprinting and mate choice. In retrospect, it may seem that Bowlby's framework, and much research in developmental psychology, has concentrated over much on mother-infant relationships and their impact, to the detriment at the time of considering fathers, the wider social network, and the role of attachment in older children and in adults. But attachment theory can have a wider brief, as this book is beginning to show. The most relevant chapters (in Section Three) are thought-provoking but speculative. Bowlby's work has been a most significant scientific contribution; if attachment theory can be broadened to a truly life-span perspective, meeting with other paradigms in so doing, this would be a comparably important achievement.

Throwing Again: A Reply to Virginia Bruce

By William Calvin
University of Washington, Seattle, WA

(Note: no other responses will be entertained by the editor on this topic.)

I am pleased that the throwing theory for language/handedness (Calvin 1982, 1983a, 1983b) has again provided a good target for discussion. Virginia Bruce (HEN, 3(9), 1983) is dubious about my first scenario for the discovery of throwing's usefulness (braining prey to stun rather than merely to later extract dessert). I had immediately followed this suggestion with an alternative (threatening throws being converted into aimed throws as their usefulness became apparent) which, as I read more of the chimpanzee literature (Plooij 1978, Telaki 1981, de Waal 1982), seems more likely. But, so long as there are routes for discovery and development available, it may not matter which came first. Just as it may not matter whether hammering (for nut-cracking: see Boesch and Boesch 1981) came before or after throwing: using the same overarm manual-brachial sequencing machinery. Once developed, a new set of selection pressures may operate upon a secondary use (Calvin 1983a).

Much confusion occurs from small vs. big game. DeVore and Washburn (1963) note that "the hunting of small animals and defenseless young is much more likely to lie at the root of the human hunting habit." Hayden (1981) reports that "many of the present-day nonagricultural societies sampled here preferred small game, and presumably some terminal Paleolithic hunter/gatherers did too. This general emphasis on small animals, including rats, lizards, bats, mice, toads and other choice morsels, raises questions about the evolution of subsistence patterns and about the relative importance of small- and large-game exploitation." Unlike the large game whose migrations Dr. Bruce finds troublesome, small game often has a restricted range. And there are many kinds. If overhunted, they reproduce rapidly and come back (see Steward 1938). I speak, of course, of "rabbits" as simply one example of such small game.

The baboon study of Strum (1981) shows how baboons seize small game (birds and rabbits) which are hiding in the brush, having to engage in a short chase only 1/3 of the occasions. These baboons also chase small gazelles and dikdik, and sometimes hunt in groups. Surprisingly, to me, the herds do not learn to keep a distance unless repeatedly preyed upon by

baboons hunting in groups, so lone hunters still can approach them. To judge from this baboon experience, a hominoid would need only to make a cultural invention in order to eat small game.

Bipedal hominids might not be as good at the chase as a baboon, but throwing could solve the problem without a chase. Chimps throw, both underhand and overarm (Goodall 1968) but, in the wild at least, their aim is reported to be poor. This is clearly improved by culture, as zoo animals (e.g., Ardrey 1976, de Waal 1982) "throw" at visitors with accuracy.

I tend to think that right-handedness requires an additional selection pressure, and this is where female throwing comes in. I did not think it necessary to address male hunting in any detail for the HEN audience but have addressed the issue more even-handedly elsewhere (Calvin 1983). People normally do not think of female hominid hunting but I suspect that, back before male provisioning became well developed, there were female hominid hunters trying to feed themselves and their babies just as chimpanzee mothers must. The argument relies upon pacifying crying infants with the left-sided sound of the maternal heartbeat (Salk 1973). To quote myself: "Who would be the more successful hunters, the left- or right-handed throwers? For hominid men, not encumbered with infants, there might be no side preference when averaging across the population. But mothers with left-brain sequencers should be better hunters (faster throws and quieter infants) than those mothers who had to hold their infants on the right side in order to use their best throwing arm."

This says nothing about women being the first, or best, or most frequent hunters -- it just says that a little maternal hunting could go a long way towards shaping right-handedness because the infant's genes survive according to the mother's throwing skills (infant genes being selected by maternal neolamarckian objection). Even the apes may turn out to have the left-brain sequencer on the left-armed infant carry -- but (to quote myself again from the book) "unless mothers used that neural sequencing machinery for an important reason during infant carrying, there might be no environmental selection for left-brain sequencing. And thus...perhaps no selection for bigger brains and language. Did the great apes miss the bigger brain bandwagon because mothers don't throw?"

And I suppose that illustrates another failing of mine for which Dr. Bruce takes me to task -- I referred to visual-spatial functions "settling in" in the homologous areas opposite to new left brain language areas. Like "bigger brain bandwagon", I am afraid that "settle in" is metaphorical, intended to remind the reader of a pet cat trying out various areas of lap before settling in to the best spot. But there is a serious issue as well -- Dr. Bruce takes me as strongly

localizationist (a place for every function, and every function in its place). I tend to think that neural committees handle a lot of the brain's business when time is not of the essence, e.g., that there is no one place where grooming resides. Visual-spatial tasks, even in modern hominids, are not strongly lateralized (reviewed in chapter 7 of Calvin and Ojemann 1980, among other places), in the sense that even construction and dressing apraxias are seen in left brain strokes a good percentage of the time.

I did not mean to give the impression that one generation could enlarge the brain measurably -- surely the Darwinian Two-Step, that ancient dance of variations in phenotype followed by environmental selection, back and forth, is sufficient to shape a bigger brain over a number of generations.

There is surely a proximate-to-ultimate spectrum of "causes" for brain enlargements and lateralizations; throwing is not "the" cause any more than neoteny. I have tried to examine some elements of that spectrum, pointing out how throwing success in hunting could interact with a size-dependent throwing sequence, which could interact with neoteny-dependent enlargements. The throwing theory is testable at a number of points, and I look forward to the discussion shifting to data interpretations.

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! MINI COMMUNICATIONS !

Rebelliousness of the Young in Evolutionary Perspective

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(Note: any member who can use the ideas presented below to develop an empirical research project is welcome to do so).

Human beings have sexual reproduction. We do not clone. Nor do our children, in culture and personality, duplicate us. In fact, they actively fight our molding efforts. The "terrible twos" and the Sturm und Drang of adolescence are old clichés but descriptive enough (Freeman, 1983). Child socialization is a problematic process.

Need this have been so, in evolutionary perspective? Why is socialization not the almost sponge-like enculturation/absorption early culture and personality writers seemed to imagine it was (LeVine 1973)?

Had there been a strong selection advantage in children's

uncritically and docilely absorbing the totality of parental culture -- the way they absorb the totality of the grammar of the language to which they are exposed -- we would need no courses on parenting, while child abuse would be a rarity.

Ontogeny is a product of biological evolution (Blurton Jones 1982). It seems unlikely that the genetic diversity of our generalized primate ancestors would not have permitted the evolution of hominids capable of acquiring culture in a straightforward manner. Presumably (and admittedly circularly) there must have been some selection advantage in children and adolescents often being rejecting and rebellious.

Barlow and Hallett (submitted) have argued that human beings were selected for cultural editing. Occasional cultural traits seem to reduce biological fitness -- the cross-culturally common practice of not permitting the infant to nurse until the mother's milk comes in, thereby denying the newborn the immunity-conferring colostrum -- seems an undeniable example (see Barlow and Hallett, submitted, for further discussion). So too is the way in which cultures seem to arbitrarily classify some nutrients but not others as edible "foods." The early New Englanders considered the tomato to be poisonous, and some people will not eat ants even if they are chocolate-covered. Cultures seem to drift in maladaptive directions.

Given that cultures often accumulate fitness-reducing "culturgens" (to borrow the term of Lumsden and Wilson, 1981), it seems reasonable to postulate that we were selected for traits which permit us to edit and re-edit our cultures with each generation, thereby preventing them from drifting too far in maladaptive directions. Might it be that the rebelliousness of children and adolescents has been selected for in part because these are culture-editing traits?

But we must immediately clarify what we mean by "rebelliousness." In point of fact, children do learn, uncritically and acceptingly, most of the culture in to which they are born. There may be an important distinction between rebellion against particular figures -- authority figures in particular -- and rebellion against aspects of culture *per se*, even though the two types of rejection are closely linked. This paper stresses rebellion against ideas and traditions rather than against individuals. That is, it stresses cultural rebelliousness, rather than social rebelliousness.

It is, of course, possible to address rebellion against other people from an evolutionary perspective. A rather simple case could be made for rebellion against authority figures in particular being part of the usual competition for mates and scarce resources and so having been selected for. Presumably, such social rebellion would be most salient in adolescent males striving to reach high rank in the social group. But it would be worthwhile for a developmental

psychologist or human ethologist to examine the hypothesis that each instance of youthful rebelliousness against authority can be interpreted as an inclusive fitness strategy. Analyses of actual case histories might prove interesting.

But the present effort stresses instead cultural rebellion, rebellion against cultural ideas and symbols and practices. Such rebellion may range from rejecting the familial religion to insisting on eating a nutrient which local culture insists is unhealthy. Though such acts may often be symbolic of rebellion against particular individuals, they may also involve rejecting traits of one's parents' culture.

A major problem in analyzing rebelliousness as culture-editing is that it is likely to serve a variety of functions at once. Certainly, various personality theorists have argued that the "negativism" of both toddler and adolescent are part of the process of first development of a sense of self and, later, of identity. These suppositions may well be true, but rebelliousness might also be serving to edit cultures.

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Researchers interested in the study of aggression should take note of the Bibliography of Aggressive Behavior: A Reader's Guide to the Research Literature. Volume 1, published in 1977, collates studies published since 1925 and Volume 2 has organized 4,000 references published since 1975. Manuscripts should be sent to the Editor-in-Chief: Ronald Baerninger, Dept. of Psychology, Temple University, Philadelphia, PA 19122. Volume 9 contains four issues and can be ordered through Alan R. Liss, Inc., 150 5th Ave., New York, NY, 10011.

The American Journal of Anthropology is presently publishing a record of contents dating from 1964-1980 (Volumes 22-53) entitled A Topical Guide to the American Journal of Physical Anthropology by M. Yassar Iscan. This volume is also published through Alan R. Liss, Inc.

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New Journals:

Several new volumes dealing with genetics have recently been published, 6 of them from Cambridge University Press (Genetic Research, Parasitology, The Journal of Physiology to name three. For a subscription form, write to Cambridge University Press, 32 East 57 Street, New York, NY, 10022). Titles from other publishers include Theory of Natural Selection and Population Growth by Lev R. Ginzburg which is aimed at removing the barrier between population genetics and theoretical ecology and can be ordered through the

Benjamin/Dumwings Publishing Co., Inc., Menlo Park, CA; Behavior Genetics: Principles and Applications is edited by John Fuller and Edward Simmel and proposes to present summaries of "the state of the art" to interested readers from a variety of disciplines, write to Lawrence Erlbaum for a copy; Biology 83 is a new journal which scopes the biological field and can be obtained through Current Titles in the Biological Sciences, P.O. Box 368, Lawrence, KS, 66044.

A call for papers is being requested by the Editors of New Ideas in Psychology: "an international journal of innovative theory in psychology." Its aim is to integrate the current fragmented ideas and theories surrounding theoretical psychology. Receive a free specimen copy by writing to Pergamon Press, Fairview Park, Elmsford, New York, 10523 (international offices' addresses available also).

Monographs in Primatology is announcing a new series: Volume 1 - Child Abuse: The Nonhuman Primate Data by M. Reite and N. Caine. This text deals with research of infant abuse in nonhuman primates as it relates to human child abuse. The volume is available through Alan R. Liss, Inc.

Psychology Today has been acquired by the American Psychological Association with the belief of involving the organization in an opportunity to heighten the public's understanding of psychology as a science, a profession and a means of promoting human welfare. This purchase is fraught with difficulty and discordance.

A funding boost of 55% has been awarded to the National Science Foundation. Although this increase will benefit cognitive science programs, it will restore these programs to the levels at which they were funded in fiscal 1980, according to an article in the May 1983 issue of the APA Monitor. A legislative bill on infant and fetal research was also reported in the APA Monitor where a California congressman proposes prohibiting research on fetuses or infants unless it improves its chances of survival. The Congressman's proposal has already been voted down, but he (William Dannemeyer) is offering it as an amendment to the NIH authorization bill which regulates research involving any type of intervention with children.

The 2nd International Conference on Nonverbal Behavior: an Intercultural Perspective was held in May at the Ontario Institute for Studies in Education, Toronto, Ontario. Included in the agenda were papers by Paul Ekman ("Past problems and future directions in non-verbal behaviour research" and "The repertoire of facial behavior"); Marianne LaFrance ("Gender as culture: difference and dominance in nonverbal behavior"); Ray Birdwhistell ("Nonverbal behavior: past, present and future"); Mark L. Knapp ("The study of nonverbal behavior vis-a-vis human communication theory" and "Nonverbal behavior in developing and deteriorating

relationships"); William C. Donaghy ("Data collection and analysis approaches in nonverbal behavior research"); and Martha Davis ("Nonverbal behavior and psychotherapy: process research").

European Sociobiological Society: was formed at a meeting in Leusden, Holland in 1982. Its aim is to serve as a forum for the study and discussions of the part played by biological factors in behaviour, in man as well as animals, with special emphasis on evolutionary aspects. The Society will be open to scientists with an active interest in the field, irrespective of academic background. Those interested in the activities should contact Dr. J. Wind, the Secretariate, at the Institute of Human Genetics, P.O. Box 7161, Amsterdam, The Netherlands.

A symposium entitled "Darwin and developmental psychology: 100 years later" is scheduled for the International Society for the Study of Behavioral Development Biennial Meeting to be held July 31-August 4, 1983. Contact William Charlesworth, Institute of Child Development, 51 East River Road, Minneapolis, MN, 55455, for details.

The Brookfield Zoo (Brookfield, Illinois) is offering a Student Research Program for summer and fall quarters. The program involves (1) an introduction to zoos as arenas for conservation, research and educational activities; (2) a practical learning experience in research design, data acquisition, data analysis, interpretation of results and communication of research results; and (3) an opportunity for graduate students and advanced undergraduates to make, and assist in making, original scientific contributions of practical and theoretical significance. Applications and correspondence should be addressed to: Dr. J. Erwin, Student Research Program, Chicago Zoological Society, Brookfield, IL, 60513.

UPCOMING MEETINGS

August 7-10, 1983 - Annual Meeting of the American Society of Primatologists at Michigan State University in East Lansing. The scientific program has been finalized and consists of eight presented paper sessions, four symposia, one Round-Table Forum, one film, a poster session of 11 presentations and a workshop of computers. Abstracts of presentations will be published in the American Journal of Primatology. Contact either Dr. W. Richard Dukelow, Chairman (Local Arrangements Committee) at the Endocrine Research Unit, Michigan State University, East Lansing, MI, 48824 or Dr. David M. Taub, Chairman (Program Committee) at Litton Bionetics, P.O. Box 557, Yemassee, SC, 29945.

Australasian Winter Conference on Brain Research - August 14-20, 1983, Queenstown, New Zealand. This is a meeting of neuroscientists to participate in all aspects of brain research discussions and ski in New Zealand's Southern Alps. The conference involves workshops, symposia and group discussion and includes participants from North America, Japan, Europe, Australia and New Zealand. Contact G.V. Goddard, Dept. of Psychology, Univ. of Otago, P.O. Box 56, Dunedin, New Zealand for more information.

18th International Ethological Conference to be held at the University of Queensland in Brisbane, Australia - August 29th to September 6th. The Conference Secretary may be contacted at the Animal Behaviour Unit, University of Queensland, St. Lucia, Australia, 4067, telephone 07 377 1111.

International Society for Developmental Psychology, satellite meeting to be held November 3-6, 1983 at Cape Cod, Deadline for abstracts is August 15th. Write to J. Adams, Conference Coordinator, ISDP, NCTR, Jefferson, AR 72079 for details.

November 6-11, 1983 are the dates scheduled for the Thirteenth Annual Meeting of the Society for Neuroscience. The meeting is to be held in Boston and those interested in planning satellite events should contact the central office as soon as possible regarding announcements in the final program.

In the HEN March 1983 issue, we noted that Richard E. Brown of Dalhousie was organizing a symposium entitled "Paternal Behaviour" for the DAB-ASZ meeting scheduled for December 27-30 in Philadelphia. The second symposium, headed by D.I. Rubenstein and R.W. Wrangham, is "Ecological Aspects of Social Evolution." This session will offer advances in behavioral ecology in the context of naturalistic studies of single species or closely related groups of mammals and birds.

Future meetings:

1984 annual meeting of the American Society of Zoologists will be held at the Marriott City Center Hotel in Denver. Symposia will be in announced this fall and local arrangements are being made by Dennis Barrett and James Platt.

August 13-17, 1984 at Eastern Washington University in Cheney is the site of the future annual meeting of the Animal Behavior Society. Sessions are already being planned and one will celebrate the 20th Anniversary of the Society by reviewing its history, honoring founding members and presenting a slide show of past events and key people. Contact Dietland Muller-Schwarze, Chair of the Anniversary Committee, Dept. Envir. and Forest Biology, SUNY, Syracuse, NY, 13210 for suggestions. Steven B. Christophers is the host of the meeting and he can be reached at Eastern Washington University, Cheney, WA, 99004.

James G. Eise, Congress Chair, recently announced a change in the dates of the Xth Congress of the International Primatological Society. The Congress is currently scheduled for July 22-27, 1984 at the Institute of Primate Research, National Museums of Kenya.

BALLOT # 1: 1984 Annual Meeting

Marc Provost has invited ISHE to meet in Trois-Rivieres at the Universite du Quebec from the 25th to the 28th of June, 1984. Most of the cost (i.e. rooms and staff) are paid by the University during the day. If other facilities are required, such as the audio-visual staff in the evening or on the weekend, then the Society would have to pay the extra costs.

Steve Christopher at Washington State University has invited ISHE to meet with ABS in Cheney, Washington (near Spokane) August 7-13, 1984. Steve is Host of next year's annual ABS meeting.

The American Primate Society is meeting at Humbolt State College in northern California, June 11-17, 1984. They too would be delighted to have ISHE hold a joint meeting with them.

Please vote for only ONE of the following:

----- ISHE to meet alone at the Universite' du Quebec, June 25-28.

----- ISHE to meet with ABS at Washington State University, August 7-13.

----- ISHE to meet with APS at Humbolt State College, June 11-17.

(Ballot #2 is on the opposite side of this page)

INTERNATIONAL SOCIETY FOR HUMAN ETHOLOGY
 Membership and Newsletter

The ISHE was formed with the goal of promoting ethological perspectives in the study of humans. It encourages empirical research that addresses the questions of individual development, environmental, ecological and social processes which elicit and support certain behavior patterns, the function and significance of behavior, and comparative and evolutionary problems. The Society maintains an elected 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, the International Primatological Society or another major society.

Name
 Address
 Phone

Please list your field of work and research interests, disciplines, etc. below (e.g., psychologist, socialization):

.....

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