

Human Ethology Newsletter

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MEMOIR

Light Thoughts on the Origins and Nature of Human Ethology

By: William R. Charlesworth, Institute of Child Development, University of Minnesota, Minneapolis, MN 55455 USA

Like rivers, histories of scientific disciplines have many tributaries. The human ethology tributary I know best begins in a paper given by Eibl-Eibesfeldt at a 1965 conference in Minnesota. Eibl was invited by the originators of the conference--Eckhard Hess, a one-time-student of Lorenz, and several child psychologists interested in strengthening interdisciplinary connections. In his paper Eibl (1967) argued that the concepts of fixed action pattern, IRM, releasing stimuli, spontaneity, and play that ethologists found useful were also of great importance for every student of human behavior (p. 141). As far as I can determine, this melding of ethology with human research interests was a first in the area of child behavior and development. A year earlier, Detlev Ploog (1964) made an analogous move aimed at establishing connections between psychiatry and ethology. In a very scholarly paper, written in German and hence virtually unknown to English-speaking readers, Ploog laid the foundations for a comparative/behavioral approach to psychiatric phenomena: his suggestions covered topics ranging from brain mechanisms and instinctual behavioral stereotypies to social behavior and structures.

As Eibl started moving his program forward, Dan Freedman, working in Chicago, was establishing novel links between evolutionary theory and human infant behavior, as well as pioneering an



evolutionary approach to research on the life cycle. At the same time, others such as Ambrose, Bowlby, Blurton Jones, von Cranach, Crook, Esser, Ekman, Hutt & Hutt were also in the process of establishing connections between ethology and psychology. In short, by the end of the sixties, a variety of tributaries were feeding into the slowly widening river channel of human ethology.

By 1972, as a result of informal contacts among Chicago, Eibl's group in Seewiesen, and Minnesota, a small group of somewhat innocent, self-labeled human ethologists held the first international meeting at the University of Minnesota. Attendance consisted mostly of German, Canadian, and American students. It was a modest beginning to say the least, but it did lead later to two much larger, more sophisticated meetings. The first was held in Percha/Starnberg (Eibl's first research station); the second immediately followed in London under the sponsorship of Nick Blurton Jones. Both meetings were very well-attended and, despite much healthy

disagreement on about nearly everything, it became apparent that a substantive scientific enterprise was in the making.

But more than meetings were taking place in 1972. That year Blurton-Jones' *Ethological Studies of Child Behavior* appeared in print. This collection of very promising papers launched a serious commitment to do two things most human ethologists liked to do back then--develop objective methods for observing, categorizing, and organizing behavior, and talking about their subject matter in terms of evolutionary theory. In the foreword to the volume, Tinbergen gave the newly emerging discipline a boost by stressing the need for psychology ("not yet really a science") to build its foundations on "the observation and description of . . . natural phenomena" (p. vii), undertaking, in the process, the work of building ethograms, a labor-intensive program of research so productively engaged in by him and Lorenz.

During this same year, Bill McGrew's (1972) volume, *An Ethological Study of Children*, also appeared; it was a methodological tour de force demonstrating ways to meet the challenge posed by the task of observing and categorizing preschoolers' behavior. Also, at the time, John Bowlby's work on attachment was awakening child psychologists and psychiatrists to the value of taking evolutionary theory seriously. In summary, things were on the move but much of the activity critics claimed, was at the level of "ethologizing." Human ethologists reputedly were over-speculating on the evolutionary origins and functions of human behavior, and wildly extrapolating from animals to humans when they should have begun building human ethograms and discovering novel phenomena.

As a personal note, when I met Eibl, I had grown tired of testing children for Piagetian cognitive structures. I had come to Piaget via general developmental psychology. About a decade earlier, I had been introduced to comparative/experimental psychology by Bob (W.R.) Thompson and ended up working in his rat laboratory at Wesleyan University (Connecticut). Other professors, at that time, did not share Bob's biological leanings, so using the term "instinct" in some classes was a misdemeanor quickly to be corrected by appropriate extinction methods. I realize the weaknesses (operational and conceptual) of the term, but they did not seem to me any more pronounced than the weaknesses of the term "learning." In addition to comparative animal research, Thompson was also well into behavior genetics with John Fuller even though genetics was unpopular in psychology at the time.

As an occasional champion of unpopular causes, I was motivated to extend the biological approach to the study of children when I went to Cornell. When I arrived, I quickly discovered that environmentalism was in strong command. Interestingly, though, animal work was always recognized as a possible source of hypotheses about human behavior, especially if it had anything to do with critical periods for learning. Harlow's work on the effects of social deprivation on rhesus monkeys quickly captured everyone's attention (and devout allegiance) in child development. I found this curious because other animal analogues usually got short shrift if they suggested that instincts were lurking somewhere within them. What was also curious was that Lorenz was condemned by a sizable segment of the faculty as a reactionary nativist. The same faculty, though, enthusiastically acknowledged his imprinting studies which, it was obvious (to me at least), were classical examples of a gene/environment interaction rather than unmitigated genetic determinism.

Also at the time, it became apparent to me that caging and depriving monkeys was not scientifically superior to studying them in their natural habitats. After two years of experimenting with pregnant rats at Wesleyan, it was refreshing later to hear Eibl describe his warm and humorous relationship with his polecat. It struck me that a significant difference between ethologists and comparative psychologists at the time was that the former viewed their research subjects as friends to understand while the latter viewed them as research objects to manipulate. Recognizing individuals for what they are (as well as what their peculiar environments require of them) seems to me a much more interesting and humane way to study and deal with humans (and animals) than conceptualizing them solely as objects to be used to test hypotheses.

Of course, psychologists have been studying individual differences since the 19th century, but their data have been mostly test scores (reaction time, intelligence, personality, etc.) and hardly ever observational data connecting such differences with differences in success and failure in everyday adaptation. Studying individuals adapting to their environments is very different from testing them; it is also a lot more difficult.

As I got to understand ethology better, a number of its features struck me as very interesting. The major one was that, for a human ethologist perhaps more than for any other behavioral scientist, daily experience and scientific scholarship can never be totally separate. The former feeds the latter with a steady stream of fresh ideas and potential data; the

latter controls the former and keeps it from becoming a subjective, unproductive morass. But what really makes this happy symbiosis distinctively ethological is evolutionary theory: it is always lurking in the background suggesting that what happens today on a daily basis may be a very old story with a predictable, long term outcome, or, maybe, a new story with a significant but unknown end. How can one lose?

Another feature of ethology I find attractive was best expressed by the mother of Barbara Pym (modern British author) when she presumably was giving Barbara tips on studying people as potential characters in her books: Mother said, "See what you can find out without asking." Those of us who work with infants or young children understand such advice so well. Asking children questions can be frustrating and perplexing, as well as hilarious (especially when asking gifted children). Asking adults questions, especially questions having to do with resources and inclusive fitness matters, can frequently be an unproductive enterprise.

Establishing human ethology as a branch of ethology, as we all know, has not been free of impediments. Accepting a biological (and especially an evolutionary) approach to studying human behavior has frequently released a whole range of accusations--genetic predeterminism, reductionism, over-simplificationism, sexism, racism, the especially pernicious iam of telling too many adaptationist stories, etc. Much of this criticism is understandable when it comes from those unfamiliar with how science operates and the difficulties ethologists face when doing research on subject matter that is both complex and virtually always out of control. It is less understandable when it comes from other ethologists. Robert Hinde (1979), for example, has noted that "carving up science along phyletic lines smacks of a regression to nineteenth-century science" (p. 645) and that "human ethology comes near to being a contradiction in terms" (p. 646). Hinde's main worry seemed to be that human ethologists would not only lose the comparative approach that proved so useful to ethology in general, but also be very tempted to attribute more causal status to evolution in accounting for human behavior than warranted.

These worries have, of course, not materialized--at least not on any counter-productive scale. Nor have the concerns materialized of those who try to pigeonhole human ethologists as rather simple-minded biological determinists. A cursory look at several issues of *Ethology and Sociobiology* should dispel such concerns. The contribution of this journal to the field of human behavior has been

refreshing, to say the least. Its editors get good marks for tolerance towards new ideas, support for young researchers, and an unpaternalistic attitude toward readers. If any corrections of speculative flakiness or poor methodology are necessary, they will emerge sooner or later. One of the redeeming features of science is that it eventually catches up with shoddiness. In the meantime, I see no problem in letting a thousand different species of memes bloom; it is better than a thousand of the same species, as some totalitarian editors would prefer.

As far as I can determine, human ethology to date has had two broad, slow-burning impacts on psychology--(1) it has strengthened the conviction that naturalistic observation is a powerful method for creating an empirical foundation for the behavioral sciences (primate field studies, for example, have helped immensely in this respect), and (2) it has strengthened the conviction that evolutionary theory is a useful conceptual tool to guide such a method. Actually, field observation had already been a tradition in some behavioral sciences. There is a tradition of it in child development albeit virtually uninfluenced by Darwin's main theoretical contributions (Charlesworth, 1986).

As for textbooks, I know that introductory child psychology textbooks now mention ethology, some half-heartedly, some slightly incorrectly, most with strong mention of Bowlby and attachment theory. None of them views (and correctly so) ethology as a major contributor to thinking about children. I say "correctly so" for various reasons, the main one being that most contemporary research in child development is still heavily based on assessment (test, interview, questionnaire) and experimental methods not directed toward addressing issues concerning proximate and ultimate functions of behavior (in that order). Other behavioral science disciplines, such as psychiatry and political science, seem to be more open now to ethology than ever before.

Future historians of human ethology are going to have difficulties defining just what human ethology really was in the 20th century. They may ask what human ethologists did to justify themselves in forming a unique sub-discipline. More specifically, they may ask what features distinguish human ethologists from other behavioral scientists. They may also want to know what a human ethologist would have had to do to be guilty of professional heresy.

I have no easy answer to such questions. Surely, an answer should include an honest attempt to deal with Tinbergen's four questions as well as an

emphasis on issues of survival and reproductive success. As for professional heresy, surely it will be collecting data only by telephone or from TV shows. One thing future historians will agree on for sure is that 20th century human ethologists collected a lot more data than they could ever explain.

Actually, future historians would probably get a better understanding of human ethology by doing a content analysis of the abstracts of the Society's international conferences or of issues of *Ethology and Sociobiology*. If they do so, they will discover that human ethology is like a vast scattering of mosaic stones, some glossy, some small, some well-shaped, some lying on far fringes. I doubt that any historian will come away with the image of human ethology as a homogeneous matrix of concepts, methods, and researchers. That it is not.

As for the future, there will always be work for human ethologists. The reasons are obvious. Taking high-resolution snapshots of human behavior is a job that will never end--the Yanomami, English preschoolers, fitness maximizers, fecund males and females, patients in mental institutions, Bushmen foragers, conflicting parents and children, stratified societies, cute babies, r and K strategists, cooperating competitors, and sexy bonobos will be just as interesting in their new adaptations tomorrow as they are in their adaptations today.

The motto of my graduate course on ethology (inspired by Gary Larson's cartoon "The Far Side") is "Follow the duck, not your theory of the duck." This means that, when in doubt, stay on the empirical trail, not on your theoretical rocking chair. Besides, when one is on the trail, one is never alone: there are always some persons--parents, nurses, police officers, school teachers, insurance salespersons, children, bus drivers, secretaries, short-order cooks, bowling champions, sweater knitters, organic gardeners, boxers, and store clerks who know better about what goes on with humans in their own micro-niches than anyone else in the world. It is these proto-ethologists who help keep the science of human ethology growing. They also provide humor and common sense, vital elements that sustain ethologists as they slog through the long fields of human behavior.

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Membership is by calendar year, so dues are to be paid by the first of the year. If the date on your mailing label is earlier than the current year, it is time to renew your membership. For economic reasons, renewal notices are not sent. No more than two warnings are given on the mailing label; thereafter you are removed from the membership list. Please report any errors, change of address, etc. to the editor.

Current dues and directions for payment are given on the last page.

Newsletter Submissions

Anything which might be of interest to ISHE members is welcome: Society Matters, suggestions for Forum topics, essays for the Growing Points feature, Mini-Communications, Announcements of meetings, journals or professional societies, etc. These sorts of submission should be sent to the editor. Book review suggestions should go to the appropriate book review editor. Submission should be in English, on paper or on these disc formats: ASCII (preferred), Wordperfect (IBM), or Microsoft Word (MAC).

No material in the newsletter is selected by critical peer review and thus material is presented only to foster free and creative exchange of (even outrageous) ideas between scholars. The fact that material appears in the newsletter never implies the truth of those ideas, ISHE's endorsement of them, or support for any policy implications that might be inferred from them.

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ETHOLOGICAL METHODS

Improved Resolution of Videotape Images

By: **Jay R. Feierman**, Presbyterian Behavioral
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Albuquerque, New Mexico, 87112 USA

I recently started using some computer equipment that makes almost photograph quality laser prints from good videotape taken with a camera and acceptable quality laser prints even from video taken off television. The equipment has lots of potential for human ethology. I wanted to put something in the newsletter about it and then got the idea to show its use with the enclosed sequence (Fig. 1, next page). It depicts a beauty contestant. The displays themselves may be of interest to readers. There are "smiles" on frames b and n, a "neck presentation" on e, an "eyebrow flash" on d through g, a "play face" on i, a "coy" look on i, a "kiss intention" (lip purse) on j, a lateral "head angulation" on the neck to the right on e, to the left on h, and then to the right again on k. My wife jokingly called e "begging face" for food. K is a "teeth clench" or "bite intention," which is perhaps an ambiguity signal along with the more friendly displays. There are three "eye battings" on b, f, and j with a rapid blink rate of 1/second. That's 11 displays in only 3 seconds! When I was on my sabbatical at Max-Planck there were people interested in the first 30 seconds of an initial potentially romantic encounter. I can think of no context in which a young woman would exhibit more social displays/second than in the final few seconds of a beauty pageant interview when she knew that the camera is on her face and she is trying to impress the judges. I have thought about doing the same type of analysis for all of the contestants who made it to the final interview and then seeing if there is any correlation with the score that contestants get on the interview and the number of displays/second they exhibit!

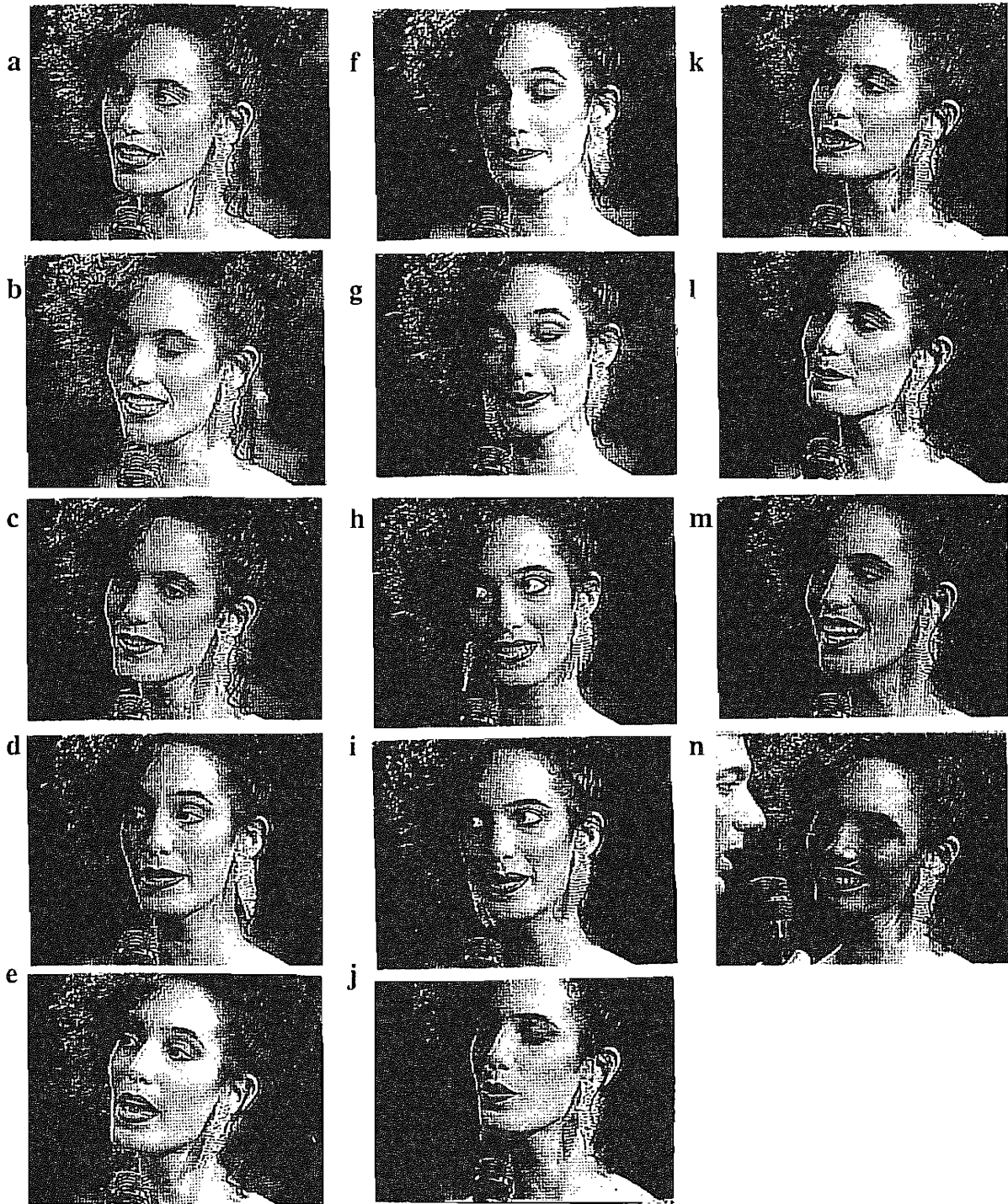


Figure 1. The last three seconds of the beauty pageant interview. Frames 0, 4, 19, 18, 23, 25, 27, 31, 37, 44, 59, 64, 72, 90 of the sequence at 30 frames/second. Images taken from cable TV onto 1/2 inch VHS videotape. Taken from VHS and digitalized into PC with VGA Framegrabber (\$665.00, Tel. 1-800-VGA-GRAB). Printed on a Cannon LBP-8 Mark III (but also works with HPII or III) with a XLI LaserPix (\$1299.00, Tel. 1-617-932-3449).

Editor's Note: Considerable resolution was lost in the printing process.

BOOK REVIEW

The Evolution of Social Systems, by John Paul Scott. New York: Gordon and Breach, 1989. P.O. Box 786, Cooper Station, New York, NY, 10276, USA or P.O. Box 90, Reading, Berkshire, RG1 8JL, UK. ISBN 2-88124-358-4 (\$49.00 paper).

Reviewed by Daniel G. Freedman
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John Paul Scott has been an outstanding contributor to the field of biopsychology for five decades, starting with the publication of his dissertation under Sewall Wright on the developmental genetics of guinea pigs (Scott, 1938). Scott became interested in the social organization of sheep while a professor of zoology at Wabash College, Indiana (Scott, 1945), work that he was to continue with goats at the Jackson Memorial Laboratories in Bar Harbor, Maine. There, however, his primary charge was the study of the genetics of social behavior in dogs, an undertaking that remains truly unique in the history of mammalian genetics (see Scott and Fuller, 1965).

His current book is probably Scott's most important theoretical contribution to date. Among other things, it demonstrates his deep understanding of biological complexity, his mastery of systems-theory thinking, and his appreciation that gene combinations rather than single genes make the difference in speciation. The book provides a well documented antidote to the gene determinism displayed in Dawkins (1976) and in Williams (1966). The latter has been influential to the point of dogma, and Scott's well argued systems position makes more sense of the ecological world than does Williams' view, shorn as it is with Occam's razor.

The wide ranging nature of the book can be discerned from some of the chapter headings: The nature of genetic systems; Processes of change in genetic systems; Genetic evolutionary theories and the use of appropriate models; On theories of organic evolution and their relevance to modern genetic-systems theory; On current theories of the evolution of social behavior and social systems; The evolution of cooperation and competition; On eco-evolution.

To focus on but a few issues, it was most helpful to read Scott's discussion of the importance of inbreeding in the evolutionary process, a dynamic rarely raised to center stage in current literature. Hamilton (1964) mentioned its potential importance in his brief allusion to "viscous populations," but Scott, carrying forward Sewall Wright's message, re- impresses us with its power.

One of the most valuable chapters for me was the one that reviews current theories of the evolution of social behavior. It is a totally level-headed critique of current theories and models. Interestingly, Peter Corning (1983), who is a political scientist, gets the highest marks from Scott, in part "because the author has trained himself in both the social and biological sciences and speaks with a great deal more authority concerning the evolution of human social organization than do more narrowly trained scientists (p. 150)." It is typical of Scott to approve such a cooperative unification of the social and biological sciences rather than a subjugation of the social by the biological. To me this smacks of wisdom.

My only quarrel with Scott, ironically, stems from his hopefulness that man might overcome the strong biological predispositions that make up ethnocentrism (e.g. Freedman, 1984), and it thus seems more a wish than a possibility. But it speaks to a deeply felt moral position regarding the evil of warfare, and my objection is stilled by my admiration for the man.

Scott is avowedly in the tradition of Sewall Wright, while Dawkins and Williams are just as clearly in the tradition of R.A. Fisher. I know of no other book on social evolution that systematically makes this helpful, if not critical, distinction. It thus deserves wide dissemination and discussion, and should be read especially by budding sociobiologists lest they take too seriously the single-minded message of the selfish gene.

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species comparisons. To accomplish these comparisons, different methods are required. We expect that work in natural history biology, combined with cellular-molecular wet lab research, will emerge as a comprehensive basic science of psychiatry. Both top-down and bottom-up analyses are needed. Indeed, this must happen if we are to explain psychiatric illnesses as deviations from normal processes, something not done now. Compare this approach to pathogenesis in psychiatry to pathogenetic formulations of diseases in internal medicine.

The aims as stated at the onset are: (1) free exchange of letters, notes, articles, essays or ideas in whatever brief format; (2) elaboration of others' ideas; (3) keeping up with productions, events, and other news; and (4) proposals for new initiatives, joint research endeavors, etc.

All of the above four aims have been realized through the interest of investigators, clinicians and others from England, Canada, Denmark, Belgium, South Africa, Germany, Australia, Italy, Crimea, Hungary, Switzerland, Russia and Mexico. Some of these investigators have been briefly curious, and some have continued as subscribers. Book excerpts and summaries have been published, along with citations of new articles, many essays, and responses to others' essays and abstracts from relevant published articles as in *Science* and *Nature*. Published reactions to the Human Behavior and Evolution society annual meetings have elicited some spirited exchanges. Contributors have included psychiatrists, ethologists, psychologists, physiologists, sociologists, engineers, historians, philosophers, psychoanalysts, anthropologists, former patients and still others not formally and specifically educated but interested and knowledgeable. Topics have ranged from sociobiological interpretations of psychoanalytic theory to how spinal fluid from patients with psychiatric illnesses that is injected into socially interactive rats affects them.

Often "basic plans" are at issue: these are biologically driven neuronal programs that emanated from remote ancestors to whole varieties of descendants, such as the yielding response discussed at length by John Price, who saw the lowest ranking chicken in the peck order as exhibiting an ancient survival response that is also exhibited by the depressed patient. I have been interested in mania as an example of an ancient program that has a normal human counterpart in charismatic leadership and an animal counterpart in alpha dominance.

ANNOUNCEMENTS

The Across-Species Comparisons and Psychiatry (ASCAP) Newsletter

The Across-Species Comparisons and Psychiatry (ASCAP) Newsletter is an informal monthly publication now in its fourth volume issued by Russell Gardner, Jr., M.D., an academic psychiatrist from the University of Texas Medical Branch (UTMB), Galveston, TX 77550-2774, Phone: (409) 761-3474, Fax: (409) 761-4288. The following is his account of the founding, philosophy, and subsequent history of the *ASCAP Newsletter*.

I was interested in how evolutionary mechanisms result in bodily processes that might go awry in psychiatric disorders. I corresponded with others similarly curious when a new word processor provided the opening for a newsletter. I sent a trial balloon to these colleagues to see if they desired a newsletter; they did. The project has continued, gaining friends. Funded first by UTMB, subscribers now pay \$18 per calendar year for the 12 monthly issues.

ASCAP's philosophy is that a mature understanding of human behavior rests on cross-

There are connecting links to the *Human Ethology Newsletter*. Ethologists who have observed behaving patients have integrated their information with those who work clinically or conceptually. An important ethologist and regular contributor, Michael Chance, has been interested in the "atmosphere" of primate groups, i.e., friendly, playful, cooperative (hedonic) vs. competitive, tense, authoritative (agonic).

We hope to implement Dr. Chance's hedonic atmosphere in the *ASCAP Newsletter*: friendly, playful, and not uncritical, but with a long distance give-and-take that is fun overall. "Play is work for mortal stakes," said the poet Robert Frost, and we hope that our work provides groundwork for data collection. Certainly there have been other outcomes: a publisher is interested in the collected materials and I have been asked to write the next chapter on sociobiology as it applies to psychiatry for the sixth edition of *The Comprehensive Textbook of Psychiatry*.

But collaborative research remains a most important aim. A working group convened during 1990 in Boston, New York and Los Angeles and anticipates a fourth meeting near London on July 7 and 8, 1991, when Dr. Price will be able to come for the first time. This will be a two-day, open-format session in which we hope to work out data-gathering plans that would involve several nations, with an emphasis, at this point, on collecting data on the hypothesized overlap between the psychiatric disorder of depression on the one hand and the ethologically appreciated concept of defeat on the other. Contact me if interested either in the *ASCAP Newsletter* or the results of the London planning meeting.

New Journal: *Evolution and Cognition*

Publisher's Blurb: Recently, there has been increasing interest in understanding cognitive phenomena in animals and humans as bio-functions, as particular features of organismic behavior. This interest goes together with findings in different disciplines, e.g. developmental psychology, behavior science, evolutionary biology, neurobiology, and others. Evolutionary epistemology has been established as a comprehensive (evolutionary) theory of cognition and knowledge, i.e., a theory which explains cognitive phenomena as results of

evolution and which describes evolution itself as a cognition process. This theory has strongly influenced discussions in the traditional fields of epistemology as well as in several scientific disciplines and the humanities (from mathematics to linguistics and jurisprudence). Also, genetic epistemology - the description and explanation of the development of cognitive abilities in the individual - has influenced discussions in many fields of scientific research. These and some other conceptions (e.g. constructivist psychology) show that the study of cognitive phenomena requires a broad disciplinary background and that a science-oriented epistemology is needed.

Evolution and Cognition is an interdisciplinary forum devoted to all aspects of cognition, be it at the level of animals or at the human level. Particularly, evolutionary explanations of cognitive phenomena are discussed, since organisms are to be considered as products of evolution and their cognitive capacities as results of evolutionary processes. Besides, the impact of the study of cognition on our understanding of evolution is considered of importance.

To this end, *Evolution and Cognition* welcomes papers on different aspects of the relation between evolutionary processes and cognitive phenomena. Original papers as well as review articles will be published; the editors will pay attention to papers reporting empirical research work as well as to articles including important theoretical implications. Each issue of the journal will also contain some critical book reviews.

The Editorial board is represented by scholars covering different disciplines and advocating different views; it has representatives from different parts of the world. Submitted manuscripts will be subjected to peer reviews and will be sent to three reviewers.

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1991, Vol. 1 (2 issues): DM 115,-,oS 800,-,plus carriage charges, US \$89.00. The first issue will be published in June 1991.

CURRENT LITERATURE

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