Human Ethology Newsletter

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

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Contents

Newsletter Submissions

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

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

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

Submission deadlines are as follows: the material should have reached the editor in Amsterdam before February 15, May 15, August 15, or November 15 for inclusion in the next issue of March, June, September, or December, respectively.

THE END OF AN ERA

foto: ANP

Obituaries of Niko Tinbergen and Konrad Lorenz

NIKOLAAS TINBERGEN, invariably known as Niko, was with Konrad Lorenz one of the founding fathers of modern ethology. During the 1930s they recognised that a full understanding of animal behaviour cannot come from laboratory studies alone but requires that we study animals in their own worlds.

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This may seem a modest enough conclusion, especially as there has been a continuous tradition of excellent amateur naturalists working in the field. But such a view fails to take account of the climate of opinion in biology and psychology at the time. In scientific circles then, animal behaviour was dominated by "white rat" experimental psychology. Into this arid environment, the idea that there was much of great biological importance to be derived from field studies of insects, fish and birds, came as a breath of intoxicating fresh air.

Tinbergen was largely responsible for the transformation of attitudes by his superb powers of observation, experiments and analysis of animals in the wild. This transformation has not only had major implications for biology and psychology, but has been truly popular. Ethologists have been responsible for the developments in wildlife photography and filming which now attract huge television audiences.

Tinbergen's skills in the field were acquired when he was a boy. He was born into a remarkably talented Dutch family which cultivated scholarship. His younger brother Luke was also a brilliant biologist, his elder brother Jan became an economist and won a Nobel Prize in 1969 (when Niko gained his Nobel Prize four years later, this gave the Tinbergen family a unique double). He had imaginative schoolteachers who took their pupils on summer camps to the dune coastline of the Netherlands. Here, as he himself acknowledged, he became obsessed with the natural world.

After graduating at Leiden, he joined the staff there and began to accumulate students of his own. He took them to the dunelands and here began studies on the nest-building and home-abilities of solitary wasps and the breeding behaviour of gulls. Tinbergen describes this work in his delightful book *Curious Naturalists* (1959). Its great strength and its novelty was Tinbergen's genius for simple field experiments using natural objects or models which revealed how the animals "saw" their own worlds.

Already his work was attracting outside attention and during the late Thirties he worked with Lorenz in Germany, finding in him a kindred spirit deeply involved with the social behaviour of geese and fish, and developing theories on the organisation of instinctive behaviour on which Tinbergen was later to build. He was also invited to the United States where a number of East Coast biologists welcomed his new approach.

The war eventually stopped his work and he spent the latter years of it in a hostage camp run by Dutch SS guards. This experience made for some difficulties in re-establishing contact with German colleagues after the war. It was several years before Tinbergen could bring himself to visit Germany again, although his scientific friendships, especially with Lorenz, remained unaltered.

Leiden made him a full professor but, within a few years, Tinbergen was being actively courted by prestigious American universities. However, Alister Hardy, the head of Zoology at Oxford, persuaded him to come there. This meant a demotion and a drop in salary but already, in the war-time camp, Tinbergen had resolved to spread the new ethology to the English-speaking world.

He was a splendid lecturer and many a student experienced instant conversion to ethology. Although he played a full part in the life of the zoology department he was never attracted to college life. He made no secret of his preference for a tented camp near a gull colony over the Oxford Senior Common Room.

In Oxford he rapidly built up a large research group and in 1951 his classic text, *The Study of Instinct*, appeared. Reading this book now one can still catch the excitement of those days. The physiology is naive and other parts are badly dated but the sections on function and evolution remain quite outstanding.

This was Tinbergen's real strength; he devised methods to analyse the ways in which behaviour adapts animals to their environment and to deduce how it evolved. By comparing similar displays in different species of gulls he could work out the pathways along which they had been shaped by natural selection. He pioneered the study of communication by such displays. One of his early films made with the BBC, *Signals for Survival*, links function with evolution in a most striking way. It was one of the first natural history programmes to combine marvelous photography with a clear and important biological story. Tinbergen took very seriously the responsibility of scientists to communicate their findings to a wide audience.

After retirement he, together with his wife and collaborator Lies, directed his energies to bringing an ethological observational approach to bear on the hitherto intractable problem of childhood autism. Their book, *Autistic Children* (1973), provoked much attention, favourable from many quarters but often hostile from those psychiatrists who see autism as a separate, distinct condition. The Tinbergens looked at the normal communication between child and adult and could detect elements of so-called autism in every child. The problem was then to understand why these come to overwhelm some children.

Tinbergen's students have carried his influence widely around the world. We all remember him with great affection. His clearminded and direct approach to behaviour places it in the context of the animal's adaptation to the environment. To recognise that his ideas have now become part of our way of thinking is perhaps the best testimony to his remarkable achievements.

Aubrey Manning

Nikolaas Tinbergen, ethologist, born The Hague 15 April 1907, FRS 1962, Professor in Animal Behaviour Oxford University 1966-74 (Emeritus Professor 1974), Nobel Prize for Medicine 1973, married 1932 Elisabeth Rutten (two sons, three daughters), died Oxford 21 December 1988.

This article was printed earlier in "The Independent" of London on December 24, 1988.

At the beginning of his delightful book, *King Solomon's Ring*, KONRAD LORENZ noted that the biblical account referred to Solomon speaking of animals rather than with them. The king's knowledge was based on acute observation and not on some magical ability to speak animal language. Lorenz liked the distinction but then happily pretended that he had special access to the emotions and intentions of the jackdaws, geese, dogs, fish and countless other animals he kept and loved. In part, he did this because he enjoyed playing to the public gallery. But he also sensed how much was lost by those who never treat their animals as though they were humans.

He was born in 1903, the son of a rich and famous Austrian surgeon, who had devised a way of correcting congenital deformities of the skeleton by "bloodless surgery". His father tolerated the menagerie of animals kept by Konrad from when he was a small boy, but insisted that Konrad be medically trained. Konrad obeyed, but as soon as he was qualified, worked for a PhD in comparative anatomy in Vienna and maintained his interest in the behaviour of his animals kept at the family home at Altenberg. Throughout the 1920s and 1930s, although he had no paid post, his reputation was growing and, by the end of his period, he was forging a coherent theory of instinctive behaviour with his Dutch friend, Niko Tinbergen. Together they were to become known as the founders of modern ethology, the study of the biology of behaviour.

In 1940 Lorenz was appointed Professor of Psychology at Königsberg University, but was drafted into the Army as a doctor a year later. Towards the end of the war, he was captured and spent four years in a Russian PoW camp, keeping himself in good health by eating insects and spiders. He finally returned to Austria in 1948.

Once again, he was without a job and worked from his old home at Altenberg. Many people wanted to help. One proposal was that, with his deep knowledge of geese and ducks, he should go to the Wildfowl Trust in Gloucestershire. In the end, though, the Max Planck organisation came forward and in 1951 helped him to set up a small research station in the gardens of a benign baron's castle near Munster University.

Within a few years the baron died and, since the inheritors of his estate proved to have no interest in animal behaviour, Lorenz was forced to move again. The Max Planck organisation now established a wonderful institute for him at Seewiesen in Southern Bavaria. It was on a lake where Lorenz could keep his geese. He and the geese arrived in 1955 and he stayed there as Director until retiring finally to Altenberg in 1973.

Unlike Niko Tinbergen, with whom he and Karl von Frisch were to share the Nobel prize in 1973, he was not a field worker who studied animals under natural conditions. He preferred to take animals into his home where he could more readily observe their patterns of behaviour that looked so appropriate in the world to which they had been adapted. When seen out of context, the elaborate sequence involved in, say, building a bird's nest was not easily explained in terms of a series of learned actions each triggered by a particular stimulus from the environment. This knowledge led him to contest fiercely throughout his life what he regarded as the orthodoxies of American Behaviourist psychology.

Lorenz, with his training in comparative anatomy, realised that activities could be treated like any structure or organ of the body. They had a regularity and consistency that related to obvious needs of the animal and which differed markedly from one species to the next. This insight was a crucial step in bringing the study of behaviour into the Darwinian synthesis

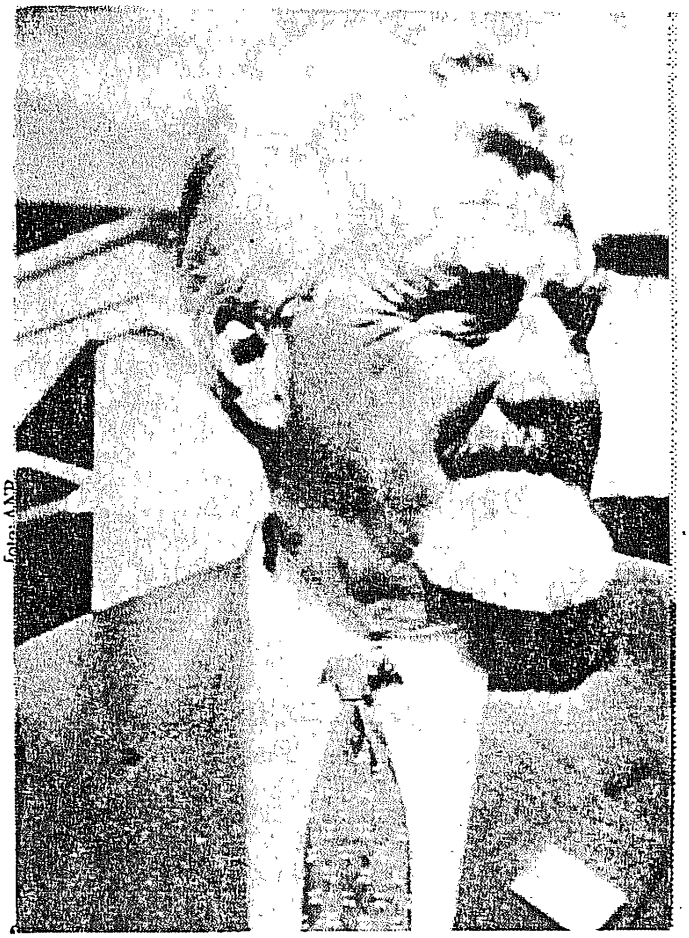

foto: AKP

that was being forged in the 1930s. Although he was very much associated with a theory of instinct, he did not deny the importance of learning. On the contrary, he gave great prominence to developmental processes like imprinting that specified what an animal would treat as its mother or its mate. Lorenz thought of instincts as inherited neuronal structures which were unmodified by the environment during development. Behaviour that was learned was "intercalated" between the instinctive elements. This notion of instinct as an unchangeable component of behaviour came under strong attack in the 1950s.

Nevertheless, the theory of instinct, developed with Tinbergen, contained enduring ideas. The notion of the sign stimulus, such as the red breast of a robin releasing an attack from an opponent, was productive in leading to the analysis of stimulus characters that selectively elicit particular bits of behaviour. Fixed action patterns provided useful units for description and comparison between species. The theory also postulated, less happily, a continual build-up of a drive for each activity. The more the drive built up, the more easily its activity could be released.

Lorenz encapsulated this idea in a famous picture of a lavatory cistern. It excited derision, but also provided a generation of ethologists with a way of integrating their thinking about the variety of influences on behaviour, both from within and without. Even so, the idea was perhaps more strongly rooted in folk psychology than it was in science and proved to be seriously misleading in many respects. In the case of aggression, for instance, "getting it out of the system" may make repetition *more* likely, not less as the theory suggests.

One by one the major ideas of Lorenz and Tinbergen that founded ethology succumbed to critical analysis and by the beginning of the 1960s any vestiges of common belief in an

ethological theory of behaviour had disappeared. People who have all called themselves ethologists pursued radically different goals and were to be found working with neuroscientists, social and developmental psychologists, anthropologists and psychiatrists, among many others. Lorenz resented the defection of what he would refer to rather contemptuously as the "English-speaking ethologists", and particularly minded that Tinbergen no longer shared his views. But the Grand Theory was in ruins and even Lorenz's views of Darwinism seemed anachronistic. His was not the enterprise society view, growing mistakenly out of modern evolutionary theory, that we are still selfish really. He had the patriarchal but equally inaccurate view that animals do things for the good of their species.

Lorenz's methods were always unconventional. He never did a formal experiment and even his descriptive observations were anecdotal. He used to infuriate his more professional colleagues by announcing, "If I have one good example, I don't give a fig for statistics". By spending so much time among his beloved geese, which treated him as one of them, he obtained insights and formulated problems that will influence ethology for a long time to come. The phenomenon of imprinting remains a focus of research interest, half a century after he first made it famous.

Apart from his battles with fellow academics, his public statements on human behaviour and morality won him notoriety as well as big sales of his popular books. When the Nazis came to power, Lorenz had swum with the tide and in 1940 shockingly wrote an article that dogged him for the rest of his life. He detested the effects of domestication on animal species and he thought (without any evidence) that humans were becoming victims of their own self-domestication. His wish to rid humanity of the impurity matched only too well the appalling Nazi ideology. Having got "our best individuals to define the type-model of our people", those who deviated markedly from such a model should be eliminated as an act of public health.

After the war, in which Lorenz was to discover with horror the full scale of what the Nazis were really up to, he would have preferred this publication to have been forgotten. So would his friends. Even his severest academic critics had their anger melted by Lorenz's extraordinary personal charm and fascination. However, Lorenz kept a high profile. He was also obstinate and his particular brand of provocative pseudo-science was to return again and again in his books and speeches. Lorenz's ideas about human behaviour were essentially those of folk psychology, which might explain the popularity of books like *On Aggression*. He held up a mirror to conventional thought. The image seen by the public was, of course, partly refracted by the animal anecdotes, but what peple saw was clearly recognisable. They liked what they knew all the more because it has a great scientist's stamp of approval.

Undoubtedly, Lorenz had great gifts as an expositor, deliberately exaggerating and simplifying, and his imitations of animals were hilariously funny. But behind the flamboyant and often assertive showman was a vulnerable, defensive person, easily hurt by criticism and easily put on the defensive. On the other hand, when he was relaxed, surrounded by friends or admirers, he was a brilliant conversationalist and a warm friend.

Patrick Bateson

Konrad Zacharias Lorenz, biologist, born Vienna 7 November 1903, married 1927 Gretl Gebhardt (one son, two daughters), died Vienna 27 February 1989.

This article was printed earlier in "The Independent" of London on March 4, 1989.

MINI COMMUNICATIONS

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

Dear Editor:

I was interested in the communication from J. Philippe Rushton on R/K reproductive strategies (December 1988 issue of the *Human Ethology Newsletter*). I would issue a caveat about some of his conclusions in that the genetic base of much of what he discusses has to remain doubtful. For example, a recent study of Black sexuality (Weinberg and Williams, 1988) has challenged the standard thesis of Kinsey et. al. (1948) that the difference in black white sexuality was due to social class standing of blacks vs. whites and that by Coleman's (1966) argument that because black women are less subject to male domination than white, it is more difficult to sustain a double standard. They would not, however, accept Rushton's thesis. Instead, they argue that the cultural setting of American Black culture gives a greater emphasis to sexuality than does cultural background of most other ethnic American groups. My own research into disease patterns and health care of Americans would also challenge the data, emphasizing that the poor, in spite of the efforts to equalize opportunities, are less prone to trust the health care system or know how to deal with it. This seems to be part of what some have called a poverty of culture and while I would not go that far, there is certainly the factor of alienation to consider (Bullough and Bullough, 1982). As the father of children of various racial identities, I can certainly accept the existence of genetic programming which is different for different racial groupings, but I think the ideas of Rushton smack of a predeterministic Calvinism which ignores all sorts of factors which sociologists, historians, anthropologists, and up to now most psychologists, have attempted to avoid. In trying to understand our biological nature, we have to avoid the sweeping kind of generalizations made here.

Sincerely,
Vern L. Bullough
SUNY Distinguished Professor
Dean, Natural & Social Sciences
State University College at Buffalo

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On acculturation and health in traditional societies: Noble savages vs. nobel savants

by: Jan Wind, Department of Human Genetics, Free University, P.O. Box 7161, 1007 MC Amsterdam, The Netherlands and Anatomical Institute, University of Groningen, The Netherlands.
Susan Weiss edited the English translation of this article.

A review of Dennett, G. and Connell, J. Acculturation and health in the Highlands of Papua, New Guinea. Current Anthropology, 29:273-299, 1988.

One of the most interesting issues in human ethology is cross-cultural comparison — a field that is, largely, the domain of social anthropologists and ethnologists. However, when the comparison concerns specific issues, such as health, it is possible that researchers in other fields can make a contribution to the discussions. As a physician with some experience in so-called "Less Developed Countries" (or LDCs), I often am somewhat surprised by statements pertaining to the development of LDCs that have been written by social scientists. For instance, I read a paper by Dennett and Connell (1988) on acculturation and health in Papua New Guinea. I hasten to say that I do not wish to specifically attack these authors with my comments but, rather, to critically analyze the opinions of a wider group of social scientists and of many other professional and nonprofessional "developers." I believe, in fact, that Donnett and Connell have assembled a lot of interesting literature and have done an impressive amount of homework as well as fieldwork. Remarkably, however, even though their paper is followed by a number of thorough comments, none of these addresses the point that I consider to be of the utmost importance, namely, that the paper is characterized by somewhat sloppy semantics leading to the misuse of the jargon of evolutionary biology and to Eurocentrism, all of which endanger the LDC cause.

Briefly and simply summarized, Dennett and Connell's thoroughly referenced paper provides a critical treatment of the "noble savage" concept of traditional exotic societies — a concept that many Westerners perpetuate — and it concludes that traditional Papuan societies were not so happy after all.

Despite the promising title, I was left with a somewhat unsatisfactory feeling after having read the paper, mainly because the authors, and some of the commentators, use the terms "acculturation" and "health" without defining them first. Only one commentator, Jenkins, notes that "acculturation" is too vague a term and requires definition. And in their reply, the authors admit that they "cannot here define them or distinguish between them and 'development' or 'westernization.'" Furthermore, terms like "adaptation," "welfare," and "homeostasis" were not defined either. Since it may seem that I am splitting hairs with my objection, I will explain why the absence of definitions has led to confusion and why it is of wider importance.

Because "acculturation" was not defined in my dictionaries, I had to distill its meaning from the paper and the accompanying comments. The sense of the term as used by the authors seems to be something like "the process of change towards a Westernized society"; in addition, the etymology suggests that the peoples concerned are assumed to finally adopt "culture" or to adapt themselves to "culture". "Health", in the authors' opinion, seems to be "the approximation towards what Western society perceives as the best possible bodily condition."

With these terms undefined, the authors use them as a basis for drawing rather far-reaching conclusions. For example, as do many among us, the authors take *our* way of life as *the* baseline, as the standard against which other peoples' welfare should be measured. But would it not be more modest — at least more scientific — if instead of using "acculturation" we spoke of "transculturation," a term that carries a lesser degree of subjective value? In addition, the use by the authors of imprecise language leads them to make subjective statements, such as, "the cost in mortality and limited life expectancy was extremely high" (p. 275). I wonder how they measured this cost. Similarly, in his comment, Schatzkin (p. 287) confuses science and sentiment by writing of "horrendous death rates."

By the same token, the use of the term "adaptive" without a definition easily leads to misunderstandings; the term confers upon the qualifications of the social sciences an aura of physical/biological exactitude — wrongly so, unless the term is defined and the level at which one is using it is indicated. While even in evolutionary biology the interpretation of the term is not agreed upon, in D' & C's paper "adaptation" in an evolutionary-biology sense is confused with "adaptation" in a social sciences or a psychological sense. Admittedly, the term can be used to describe processes occurring at a variety of levels; but laws and rules pertaining to the biology level do not automatically apply to the humanities level (see, for example, Dennett and Connell, p. 275) — nor the other way around (Wind, 1985).

To be sure, any comparison of the somatic/physical condition of Western populations with that of populations of other societies is, of course, quite legitimate — and even interesting, that is, for students of medicine. For social scientists, however, such a comparison would seem to be less relevant. In the same vein, diachronic data of mortality, natality, fertility, and morbidity figures are of interest to demographers, ecologists, and so forth. But was not the ultimate aim of Dennett and Connell and their commentators rather to assess any diachronic changes in the level of well-being that occurred during — or even that were caused or influenced by — the transculturation process?

Though I am not a social anthropologist, I wonder whether the authors have not made a basic error, i.e., equating health and the ideal society as strived for by Westerners, on the one hand, with the well-being of non-Westerners, on the other. Admittedly, to assess precisely the level of well-being is difficult, if not impossible — and *a fortiori*, this difficulty applies to unfamiliar societies. However, the authors do not even raise, much less address, the issue. Their account seems to boil down to a (somewhat distorted) "etic" interpretation. Of course, rendering etic reports is the business of an anthropologist. But would not their "eticity" increase by incorporating at least some "emicity?"

By adopting a more emic approach, Dennett and Connell could have questioned, for instance, whether what we call a high mortality rate in the very low and the very high age groups decreases well-being in a non-Westernized society as much as it does in a modern, Western society. In addition, the authors' arguments would have been more balanced had they mentioned a number of examples of pre- and non-Westernized societies in which health conditions — according to Western standards — were and are satisfactory and, conversely, some examples of (semi)-Westernized societies in which health and well-being are — again based on Western standards — unsatisfactory? If such examples had been given, would not unhealthiness and, more

important, general suffering in the latter group of societies (of course, mainly in the "Differently" . . . pardon, the "*Less* Developed Countries") be much greater at present, at least statistically, than was the case before the transculturation process? (See my accompanying figure.)

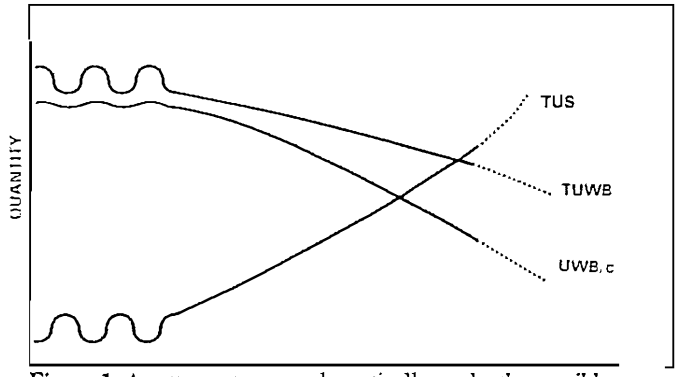

Figure 1: An attempt to very schematically render the possible course during, say, the last 100 years, of some parameters indicating some feelings, like the well-being, of populations in the typical LDCs (from Wind, 1983). TUS represents the Total of the Units of Suffering, i.e. the sum of all the individual suffering in the country concerned. UWB/c represents the average level of the number of Units of Well-being per caput of the population. TUWB means the Total of the Units of Well-being. There are reasons to assume that the total of suffering has increased since Westernization began because the average level of individual suffering has risen (due to over-population, starvation, disruption of traditional social systems, etc.) and because the population has sharply increased. Extrapolating from the present trends, the TUS is likely to increase sharply in the near future. The level of UWB/c is likely to decrease further due to the same factors, while the TUWB (that up till now had probably not greatly decreased because the lower UWB/c was compensated by for the larger number of individuals) may decrease somewhat less. The values are entirely arbitrary — the use of the curves only being to illustrate the diachronic differences of some human feelings.

Finally, I sensed additional confusing terminology. Take for instance the undefined "homeostasis": this term suggests the exactness such as often is present in the physical sciences. However, Dennett and Connell invoke Gould and Eldredge's punctuated equilibrium model of biological evolution (in which the *punctum* is something like a million years!) to explain the postcontact "evolutionary [sic] process" in Papuan societies (p. 291). But even in the humanities, and especially in the social sciences, it is more accurate to speak — as Dennett and Connell, fortunately, also have done — about constant oscillations and, in the case of the Papua Central Highlanders, about "a more or less permanent state of disequilibrium." Important questions, therefore, remain to be answered: Homeostasis of what? Of (Western) health? Of body weight? Of demographic parameters? Of well-being? And what was the size of those oscillations? And about what periods of time are we talking? Years? Decades? Centuries? Millennia? Applying evolutionary biology to human behaviour is an intriguing but a risky undertaking.

Would not the savages — noble or not — legitimately wonder whether they can, emically or etically, see us, Western anthropologists, rightly as *noble savants*?

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BOOK REVIEW

Evolution: a Theory in Crisis.
Bethesda, Maryland: Adler & Adler, Publishers; 1985, 367 pages. By Michael Denton.

Reviewed by George Kocan.
27 W 179 Galusha Av., Warrenville, IL 60555, USA. Tel.: +312-393-6401.

De dominant paradigm of Charles Darwin's day held that organisms existed as variations on a type. The lion, for example, represented one of the variations on the "theme" cat. The types, furthermore, were distinct entities, isolated from other, though similar, entities.

Darwin rejected the paradigm and proposed an alternative. He set forth a population model for dealing with the diversity of life. Organisms existed as populations, the members of which differed from each other in a statistically describable way. This assumption, enabled him to imagine that a population could eventually produce a new kind of organism, under the pressure of natural selection, over a suitable period of geologic time. In other words, he showed how a new species could develop out of an old species, as a purely natural phenomenon.

Now, more than a century later, Darwin's is the dominant paradigm. So, what is the responsibility of the modern scientist? Is it to accept Darwin as the received wisdom of the age or to treat the theory of evolution as Darwin treated typology, or as Galileo treated the earth-centered view of the universe? Michael Denton argues for the latter. He echoes Thomas Kuhn (1970), denying that science progresses by falsifying theories. Science, rather, abandons an old theory for a new one, because the replacement has somehow captured the imagination. Denton avers that the theory of evolution has become such a powerful paradigm that scientists have been unwilling or unable to face up to its serious, even fatal defects. He writes:

"once a community has elevated a theory into a self-evident truth, its defense becomes irrelevant and there is no longer any point in having to establish its validity by reference to empirical facts.... the stability achieved, the semblance of absolute truth is nothing but the result of an absolute conformism."

Denton compares evolution to phlogiston, making the point that even reputable and brilliant scientists failed to gain a proper understanding of combustion, because phlogiston so claused their imaginations.

The author of "Evolution: a Theory in Crisis" makes a persuasive case against Darwin's theory, because he sticks to biology and the other sciences. The evidence of comparative

anatomy and the fossil record supports typology, he claims. It does not support a theory of gradual change from one form to another.

Denton does concede the existence of evolution at the level of the species — microevolution. But he rejects its application to the major taxonomic groups — macroevolution. He points out that the transitional forms between the major groups, that evolutionists have been expecting to emerge, simply do not exist. They do not exist among the living forms, and they do not exist among the fossils.

He discusses at length Darwin's view that the fossil record is incomplete, and rejects the argument by citing studies which show that the fossil record is more complete than one might suspect. Of the 329 living families of terrestrial vertebrates, for example, 261 or 79.1% have been found as fossils. In spite of an explosion of paleontological activity since Darwin's time, all over the earth, and in spite or the discoveries of many strange forms, the transitional forms, by and large, have failed to appear, he insists.

The theory of "punctuated equilibrium" holds that species develop in small, isolated populations; hence, the transitional forms are unlikely to have left enough representatives to be found. Those fossils that are found represent large, central populations. Denton, however, argues that this theory fails to explain the wide gaps between major groups. Between, say, whales and bats, or between amphibians and reptiles, the fossil record should show a great abundance of long lineages, including collateral lines of hundreds or even thousands of transitional species. Yet, the fossil record has failed to yield them up.

Denton also denies that gradual mutations can account for major evolutionary changes. He relies on Cuvier's explanation that, "Every organized being forms a whole, a unique and perfect system, the parts of which mutually correspond and concur . . . . None of these parts can change without the whole changing."

Denton, furthermore, cannot see how a random process, which is unable, for example, to produce the best solution in a game of checkers, or string letters together to form a coherent sentence, could have produced a nucleotide sequence making a viable DNA.

He reviews evidence from embryology which indicates that many homologous structures apparently are not coded by the same genome. In Denton's opinion, *Archeopteryx* is a fully formed bird and not the transitional form between reptiles and birds that evolutionists claim; that the ancient fish, *Latimeria*, likewise, shows itself to be the weakest link, if at all, between fish and amphibians.

The arguments presented in the book rely on modern scientific findings and sophisticated argumentation. It is tempting to attempt refutations in this review. But, such a task would turn out to be a lengthy and boring distraction.

Rather, the book should be taken as a serious challenge to evolutionary theory, a challenge that may very well result in a reconceptualisation of the theory. Considering the unappealing alternatives to gradual evolution, special creation and saltatory evolution, Denton himself argues for a reconceptualisation. Unfortunately, he offers no hint of what such a reconstruction would look like.

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Human Nature and Suffering.
Hove: Lawrence Erlbaum Associates, 1989. ISBN 0-86377-116-5. £29.95. Pp. 424. By Paul Gilbert.

Reviewed by John Price
Department of Psychiatry, Milton Keynes General Hospital, MK6 5LD, England.

Surely one of the most significant changes during recent human evolution has been that in which social asymmetry based on coercion has given way to asymmetry based on conferral, a change from a social system in which the optimal strategy for an ambitious man is to make himself more powerful than others, to one in which his best strategy is to make himself more attractive to others. In sociological terms (Kemper, 1988), there has been a change from rank based on power (wrested from others by force) to rank based on status (conferred by others voluntarily). In Michael Chance's terms, hedonic society has replaced agonic society, and in so doing has liberated man's attention from the "rat race" to concentrate on technological and artistic achievement, reaching its highest level in the creation of new systems. This change has taken place against a background of rapid and varied evolution of affiliative behaviour, not only parent-child interaction and relations between the sexes, but also in the formation of friendships and alliances between members of the same sex. An yet, the agonic mode of coercive social relations is still with us, sometimes lurking under the surface of the hedonic mode, sometimes coming right out into the open, usually when cultural controls have broken down.

These matters, and their capacity to cause human suffering in the form of depression and anxiety, are the subject matter of Paul Gilbert's book. Dr. Gilbert, a clinical psychologist working in Derby in England, produced *Depression: from Psychology to Brain State* in 1984. That book was the best review of the psychology of depression for a decade, and this is not only an update incorporating ideas from evolutionary biology, but it covers a wider field of psychopathology than his previous volume.

One of the many outstanding achievements of comparative ethology (Eibl-Eibesfeldt, 1975) has been the demonstration of ritual agonistic behaviour as the ubiquitous vertebrate mechanism for creating and maintaining social asymmetry (in the agonic sense): and one of the many outstanding achievements of the evolutionary game theory approach of behavioral ecology (Parker, 1984) has been the rewriting of ritual agonistic behaviour in terms of resource-holding potential (RHP). RHP is a measure of fighting capacity; it is an intervening variable which is defined on the input side by measurable variables such as size, strength, weapons, previous success and allies, and on the output side by the probability of fighting rather than submitting. Each individual has the capacity to estimate his adversary's RHP and to compare it with his estimate of his own RHP, giving a decision of relative RHP, which in the simplest case may be either favourable or unfavourable. The beauty of this notation is that all the elements of ritual agonistic behaviour can be described in terms of RHP. Threat, challenge and attack are signals of favourable relative RHP, giving the message "I am stronger than you". Submission and escape are signals of unfavourable relative RHP, giving the message "You are stronger than me". Dominance display is a signal of absolute RHP,

giving the message "See how powerful I am", and containing more detailed information than the signals of relative RHP.

If we make the additional assumption that signals of relative RHP alter the RHP of the recipient, we have a system which not only leads to the formation of a peaceful society based on RHP differences, but also paves the way for making an evolutionary exit from the agonic mode of social organisation. It fits with our knowledge of human psychology if signals of favourable relative RHP, which take the form of blows, insults, criticism, etc. (called by Gilbert hostile dominance), and which cause either physical or mental pain, have the effect of lowering RHP in the recipient — we certainly know these kind of signals lower self-confidence and self-esteem in humans. Conversely, a signal of unfavourable relative RHP in the form of submission raises the RHP of the recipient. What must have occurred during the transition from ape to man is an enormous elaboration of signals of unfavourable relative RHP, which have switched from abasement of the sender to glorification of the recipient. It is ironic that for hundreds of millions of years vertebrates have been struggling to get across to each other those simple messages of "I am stronger than you" and "You are stronger than me" and to do so they have evolved an astonishing variety of symbolic forms of signalling, ranging from pushing and pulling contests to imitations of femaleness and childishness. And now human beings can simply say it. Not only can they say it more simply than animals, they can also say it infinitely more elaborately, and it is probably this power to raise RHP through words which has allowed a hedonic asymmetry of conferral to replace the agonic asymmetry of coercion (at least partly) in our social life.

Gilbert's book could be called the psychology of the development of the triune brain. How has reptilian RHP evolved into human self-esteem? Human self-esteem is a lot more than fighting capacity (Editorial, 1988). Gilbert proposes the term Social Attention-Holding Power (SAHP) for that component of self-esteem which is conferred by others. This conferral is elicited by "showing off" behaviour (quite different from display of absolute RHP) which has been reported in chimpanzees and occurs conspicuously in children. Gilbert thinks there may be a critical period during childhood when SAHP is built up by parental approval and leads to hedonic personality development, but that in the absence of such approval the SAHP system may never develop, leaving a personality dominated by agonic mechanisms.

Paul Gilbert's great strength is his capacity to integrate these ideas from evolutionary biology with existing fields of knowledge. He is as much at home with Jung's archetypes as he is with Beck's cognitive schemata, Leary's interpersonal circle and Virginia Price's Type A personality; and he relates the psychology to neuroanatomy and neurochemistry in the few cases where this is possible. Psychologists are waking up to the fact that human behaviour has evolved, that even learning is "prepared" by natural selection, and that if the duration of man's existence on earth as a vertebrate were reduced in scale to a single year, by the time language and culture came on the scene, it was already getting dark on December 31.

During those few hours of twilight on New Year's Eve society has virtually abolished ritual agonistic behaviour. It has channelled it into sport, the law courts, examinations and other forms of culturally ritualised competition; apart from anomic subcultures such as prisons and street gangs, the only arenas where interpersonal confrontations resembling ritual agonistic behaviour occur is within the nuclear family, a place where the law has neither the will nor the power to intervene. In the competition between social rule-books, those rule-books have been selected which allocate high RHP to members who are attractive to others, especially when they contribute to the achievement of group rather than individual goals. And the rules which govern interpersonal encounters have changed from those which encourage drastic changes in RHP to those which protect each participant's RHP from change, so that, for instance, Becker (1962) can conclude that the function of social rules is to protect each member's fragile self-esteem from damage. These rules are necessary because the brain mechanisms subserving hedonic behaviour and SAHP are only a thin veneer over the basic structures which have evolved under the agonic system based on RHP. Therefore it is not surprising that human nature and human suffering are difficult to understand without the perspective of evolutionary biology.

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CURRENT LITERATURE

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rious sociological deficiencies to justify a biocultural perspective. The volumes closes with an essay on "the ecological dimension of cooperation" by Vittorio Parisi. The unit of analysis here is the ecological phenomenon of biocenosis, namely, the community of organisms that occupies a particular habitat. The emphasis, both behavioral and structural, is on cooperation, and this is approached, from the viewpoint of species rather than individual fitness.)

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For a fee and reimbursement of expenses, Susan Weiss, a professional technical editor, will edit translated articles by members of the ISHE or their colleagues to improve the use of English in these texts. Address questions about this service to P.O. Box 1192, Corrales, NM 87048-1192, U.S.A.

1989 ISHE Meeting

The 1989 meeting of the INTERNATIONAL SOCIETY FOR HUMAN ETHOLOGY will be held in Edinburgh, Scotland from the 31st of July to the 4th inclusive of August 1989. This will enable members to attend the International Ethological Congress in Utrecht, The Netherlands as well.

By now everyone should have received a separate mailing from Edinburgh with more details. The venue will be the Pollack Halls of residence of the University, which are situated at the edge of Holyrood Park, very close to the city centre. The format will be to have some plenary sessions in the mornings, with workshops/papers in the afternoons. Various excursions will also be provided for some light relief. All those who wish to attend should get in touch with one of the organising committee named below. You will see that as well as normal addresses EMAIL numbers are given. It would be very helpful if those who wish to give papers or to lead workshops could send outlines of their proposals by EMAIL if possible to save typing out the outline again. **The deadline for submitting abstracts is May 1, 1989.**

EMAIL T. Pitcairn @ UK.AC.Edinburgh
Dr. T. Pitcairn,
Dept. of Psychology,
University of Edinburgh,
7 George Square,
Edinburgh EH8 9JZ.
SCOTLAND.

EMAIL I. Vine @ UK.AC.Bradford.Central.Cyber 2
Dr. I.Vine,
Dept. of Interdisciplinary Human Studies,
University of Bradford,
Bradford, BD7 1DP.
ENGLAND.

EMAIL EDRAA
U Dr. S.R. Neill,
Dept. of Education,
University of Warwick,
Coventry CV4 7AL,
ENGLAND.

Male Parenting

A symposium organized by William Bailey and entitled "Male Parenting; Evolutionary Perspectives" will be presented at the Biennial Convention of the society for Research in Child Development in April. The presentations will be:

1. Wade Mackey, "A decade after the Transfiguration; the father-myth continues".
2. Martin Smith, "Paternal uncertainty and the evolution of homosexuality".
3. William Bailey, "Affinity: The infant-father relationship from the perspective of evolution".

The discussants will be Mary Ainsworth, Thomas Alley, and Kevin MacDonald.

Geo-Cosmic Relations

The first international congress on Geo-cosmic relations (The earth and its macro-environment) will be held in Amsterdam, April 19-22, 1989. It is organized by the Foundation for Study and Research of Environmental Factors. (SREF). Topics and speakers are: Astrophysical and meteorological phenomena

(Dr. J. DeMeo, U.S.A.; Prof. Dr. W. de Graaff, The Netherlands; Dr. M. Rossignol, France; Dr. I. Örményi, Hungary; Dr. J.P. Rozelot, France; Prof. Dr. C.J.E. Schuurmans, The Netherlands; Prof. Dr. P.A.H. Seymour, United Kingdom; Dipl. Ing. E. Wedler, Germany F.R.; Dr. Ir. B.C.J. Zoeteman, The Netherlands), Chemical phenomena (Dr. C. Capel-Boute, Belgium; Prof. Dr. Ing. J. Eichmeier, Germany F.R.; Dr. F. Meyer, Belgium), Biological phenomena (Dr. Ir. J. Buis, The Netherlands; Prof. Dr. B.G. Cumming, Canada; Dr. P. Faraone, Italy; Dr. W. Grundler, Germany F.R.; Dr. R. Reiter, Germany F.R.; Dr. M.H. Webb, U.S.A.), Human phenomena (Mrs. J.S.H.J.W. Bouma, The Netherlands; Prof. Dr. S. Ertel, Germany F.R.; Prof. Dr. H.J. Eysenck, United Kingdom; Dr. M. Gauquelin, France; Dr. P.H. Jongbloet, The Netherlands; Prof. Dr. A.A. Knoop, The Netherlands) and Theoretical and background contributions (Prof. Dr. David Bohm, United Kingdom; Dr. G. Dean, Australia; Prof. Dr. A.P. Dubrov, U.S.S.R.; Prof. Dr. F. Kaiser, Germany F.R.; Prof. Dr. E. Schoffeniels, Belgium; Prof. Dr. P. Seymour, United Kingdom; Prof. Dr. P.A. Vroon, The Netherlands). Write to: SREF, c/o Dept. of Ecological Agriculture, Haarweg 333, 6709 RZ Wageningen, The Netherlands. Telephone: +31 (8370) 10205.

HBES first annual meeting

The Human Behavior and Evolution Society first annual meeting will be held at Northwestern University, Evanston, Illinois, U.S.A., August 25-27, 1989.

Abstracts of proposed presentations must be received by April 1, 1989. Decisions on the program will be made by May 1, 1989. Please use the top half of 8 1/2" x 11" paper with 1" margins. Please use the first line for the title, the second for the authors, and subsequent lines for their affiliations and a mailing address. The abstract should be less than 200 words (not including titles, names, etc.). Abstracts will be considered for both lecture and poster formats, unless a note at the bottom of the abstract page instructs otherwise.

Registration fee is $50.

Please send registration fees to:
The Human Behavior & Evolution Society
Northwestern University
1810 Hinman St.
Evanston, IL 60208-1310
+(312) 491-5402

Please send abstracts to:
Laura Betzig, Ph.D. and Paul Turke, Ph.D.
The University of Michigan
1521 Rackham Bldg.
Ann Arbor, MI 48109-1070
+(313) 936-2526

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Newsletters of the following members were returned to sender, although they paid their membership dues recently. Would anyone who knows any of them be so kind to contact them and advise them to send me their new address.

John A. Bock, 1517 Gold SE #4, Albuquerque,
NEW MEXICO 87106, U.S.A.

Membership Renewals

If the date on your mailing label is earlier than the current year, it is time to renew your membership. Renewal notices are not sent for economic reasons. No more than two warnings are given on the mailing label. Thereafter you are removed from the membership list.

Membership dues are U.S. $10.00 (ƒ25,00 guilders) per year (students U.S. $5.00) and U.S. $25.00 (ƒ60,00 guilders) per 3 years. The library rate is twice these amounts.

Directions for payment are given on the last page of this newsletter. Payment reaching the treasurer before February 1, May 1, August 1 or November 1, will be processed in time for indication on the mailing label of the next newsletter issue.

Please, report any errors, changes of address, etc. to the editor.

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

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

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Association ADRET,
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Membership and Newsletter

The ISHE was formed with the goal of promoting ethological perspectives on the study of human behaviour. 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 has elected officers and a number of committees, publishes a quarterly Newsletter, collates an annual selection of human ethology abstracts, and meets annually, either independently or in conjunction with the Animal Behavior Society, the International Primatological Society or another major society.

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Please, send this **registration form** to:
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Forschungsstelle für Humanethologie in der Max-Planck-Gesellschaft
Tel. 0 81 52 - 3 73 - 0
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Mail to:

DRUCKSACHE

Dr. John Lazarus, 1989
Dept. of Psychology
Univ. of Newcastle Upon Tyne,
Ridley Building,
Claremont Place,
Newcastle upon Tyne, NE1 7RU
ENGLAND.