

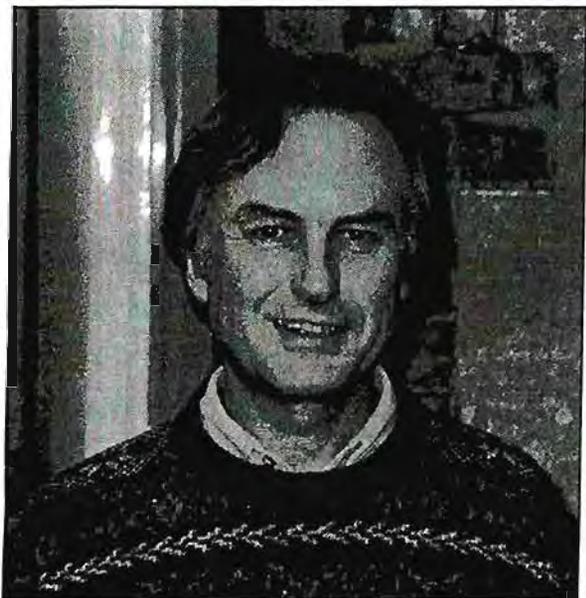
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An Interview of Richard Dawkins

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Richard Dawkins is a zoologist and Professor of Public Understanding of Science at Oxford University. Of his best-selling books, *The Selfish Gene* (1976) probably did most in bringing the evolutionary message home to both a professional and a general readership. The following interview took place in Oxford 13 December 1996.

You mention in *The Extended Phenotype* the slave-making habit in some species of ants. The slave-making ants go to the nest of another ant species to steal pupae, which are carried back. The work done in the slave-makers' nest by the slaves that hatch from these pupae is

obviously not in the interests of the slaves. Why don't they go on strike?

Because the slaves are not genetically related to anything that comes out of the nest where they are now working. Any gene that tended to make them go on strike would have no possibility of being benefited by the striking action. The copies of their genes, the copies of these striking workers genes, would be back in the home nest, and they would be being turned out by the queen, which the striking workers left behind. So there would be no opportunity for a phenotypic effect, namely striking, to benefit germ line copies of themselves.

You also write about an ant species called *Monomorium santschii* in which there are no workers. The queen invades a nest of another species, and then uses chemicals to induce the workers to adopt her, and to kill their own queen. How is it possible that natural selection did not act against such incredible deception and manipulation, which must have been going on for millions of years?

In any kind of arms race, it is possible for one side in the arms race to lose consistently. *Monomorium santschii* is a very rare species. If you look back in the ancestry of the victim-species over many millions of years, many of their ancestors may never have encountered *Monomorium*. But the *Monomorium*'s ancestors all had to succeed in killing their victim-queens. So there is an asymmetry in selection pressure. I think the easiest way to put it is to say that many victim-nests survived in spite of not having countermeasures, because they never met a *Monomorium*. But not a single *Monomorium* gene survived if it failed. So the cost of failure is much higher on one side of the arms race than the other.

Is it possible that a similar kind of asymmetry exists between human individuals?

I think that when you have arms races within a species, and I don't know why you shouldn't, say between male and female or between parent and offspring, it is possible that the cost of failure is asymmetrical. [This] means that one side is disproportionately effective. I have not really thought it through, but I do not see why in principle that shouldn't happen.

You wrote with Krebs in 1978 that cooperative signals tend to be muted and economical, while manipulative signals tend to be conspicuous and repetitive.

When there is a conflict of interest, there is an arms race between the manipulator (or signal sender) and the victim (or signal receiver). The signal sender is, over evolutionary time, evolving ever more powerful manipulative stimuli, and the signal receiver is constantly raising the barriers to whatever the stimulus is. And as the victim raises these barriers to the stimuli, whether these are sounds or chemicals or colours or whatever, this puts pressure on the signal sender to send a stronger and stronger signal. So you would expect to get very powerful signals in those cases where victim resistance is high, but in those cases where the signal sender and receiver are cooperating, where the communication is in both their interests, then it is not necessary to shout. A human example of that would be a couple at a dinner party who want to signal to each other that it is time to go. They do it in very subtle ways, like a little look at the door, or a little motion as if to stand up. So a very subtle signal is a result of de-escalation.

...

You said on BBC-Television that religion teaches people to be satisfied with not understanding. What is wrong with not understanding? People have not understood evolution for millions of years.

No, that is right, you can survive without understanding. I think it is a value judgement on my part, I think it is virtue, a good thing, to understand, and therefore if there is an ideology which actively discourages the desire

to understand, I am against it.

If two individuals or groups disagree, let's say evolutionists and religious people, then is it not an old wisdom that the truth should be somewhere in the middle?

I have always resisted the idea that when two opposing points of view are being equally strongly expressed, the truth lies in the middle. The truth can very easily lie on one side or the other. One side can simply be wrong.

But is it not a sign of bad manners to claim that you have it totally right, while the other side has it totally wrong?

No, it is bad manners to swear at people and be insulting to them in a personal way, but it is not bad manners to say, "I think you are wrong for this or this reason." There may be people who think that having strong opinions is necessarily negative, and I think it might be negative if it meant: He has strong opinions which he cannot back up.

You wrote in *The Blind Watchmaker* with capitals: COPERNICUS WRONG. FLAT EARTH THEORY VINDICATED. What did you mean?

If you have a detailed argument within evolutionary theory, where two scientist disagree about something quite abstruse and theoretically sophisticated, then creationists come along and say, "Oh, evolutionists disagree; therefore the whole of evolution must be wrong." What I wrote was that it would be equivalent to say that when people discovered that the earth is not a perfect sphere, but a slightly flattened spheroid, instead of saying "Oh, there was this minor thing wrong," you have headlines saying: COPERNICUS WRONG. FLAT EARTH THEORY VINDICATED.

George Bernard Shaw wrote that "there is a hideous fatalism about Darwinism." Why do people often think evolutionary theory is pessimistic, depressive?

I am not an authority on Bernard Shaw's psychology. Shaw is reacting emotionally to a scientific theory. He is saying: I couldn't bear it if this were true; it would be horrible if it were true, as though that meant that it was not true.

But of course something horrible can be true, something unbearable can be true. And, well, there are horrible aspects of it. There is an awful lot of suffering. Natural selection does mean death of a lot of individuals.--parasites eating you in bits from inside, predators devouring you from outside. So the force that has shaped the evolution of living creatures with all their beauty and elegance is a whole lot of rather unpleasant deaths. I could imagine finding that emotionally upsetting. But what I cannot imagine is saying: It is emotionally upsetting and therefore it cannot be true.

On the other hand, some people favoured Darwinism because it appeared to support a political idea.

Yes, Darwinism has been misused politically in this century, by Hitler and by others. Social Darwinism flourished at the end of the last century and the beginning of this century with people like Herbert Spencer and John D. Rockefeller. Rockefeller, an immensely rich and powerful man, had imported a form of Social Darwinism into his political beliefs. He really felt that the weakest should go to the war, and the strongest should survive, it was right in business, it was right in capitalism that the economically strongest and most ruthless should prevail.

Is evolutionary theory telling us this?

No! It is telling us this only if you say that what is going on there in nature ought to be true in human political and social life. What I am saying, along with many other people, among them T. H. Huxley, is that in our political and social life we are entitled to throw out Darwinism, to say we don't want to live in a Darwinian world. We might want to live in, say, a socialist world which is very un-Darwinian. We might say: Yes, Darwinism is true, natural selection is the true force that has given rise to life, but we, when we set up our political institutions, we might say we are going to base our society on explicitly anti-Darwinian principles.

This is what you favour?

Yes.

Is there any message at all coming from

evolutionary theory, telling us what we should do politically or morally?

No. The only message coming from evolutionary theory is what actually happens in nature. Now, in nature it is true that, to some extent, the strong and the most selfish survive. But that is no message for what we should do. We have to get our 'shoulds' and our 'oughts' from some other source, not from Darwinism.

Some well-known evolutionists are, or used to be, radical leftists, and you are yourself are reported to vote leftist. Yet sociobiology is often associated with right-wing sentiments. Why?

Because the opponents of sociobiology are too stupid to understand the distinction between what one says about the way the world is, scientifically, and the way it ought to be politically. They look at what we say about Darwinian natural selection, as a scientific theory for what is, and they assume that anybody who says that so and so is the case, must therefore be advocating that it ought to be the case in human politics. They cannot see that it is possible to separate one's scientific beliefs about what is the case in nature from one's political beliefs about what ought to be in human society.

Danish Society for Human Ethology

This group was founded in 1985 and now has about 100 members. That a country the size of Denmark has this many human ethologists testifies to the foothold that the field has established in Europe.

The Danish Society for Human Ethology sponsors 5 to 7 lectures per year on a broad range of topics. Recently ISHE's past President, Irenäus Eibl-Eibesfeldt, addressed the group on the history of human ethology, and in 1995 Michael McGuire spoke on evolutionary psychiatry. ISHE member Tyge Schelde has also addressed the group, describing research on nonverbal behavior

exhibited by psychiatric patients. Most meetings are in Danish, but some are in English. Current Secretary is Bjarne Westergaard, and Treasurer is ISHE member Axel Randrup.
 Home page is
<http://www.icafe.dk/sci/cirip/humanethology.html>. E-mail: arcirip@cybernet.dk. Fax: 45-46-38-46-11.

As requested by Nils Erik Andersen, copies of the *Bulletin* are being sent to the members. We look forward to future collaborations with the Danish Society for Human Ethology.

The Continuing Story of Neanderthal Man

By Johan M.G. van der Dennen, Center of Peace and Conflict Studies, University of Groningen, The Netherlands. E-mail: j.m.g.van.der.dennen@rechten.rug.nl

I wish to use a recent book by Ian Tattersall (1995) as a focus to describe the story--history and prehistory--of Neanderthal Man. Tattersall's book is the latest in a recent revival of Neanderthal studies, together with Trinkaus & Shipman (1993), Stringer & Gamble (1993), and Shreeve (1995). The study of the Neanderthals is a study of controversies, stereotypes, popular prejudices, more or less hilarious misunderstandings and, as will be seen, widely divergent extinction scenarios.

Neanderthal Man Revealed

In August of 1856, German laborers blasted out the entrance to a cave in the Neander Valley near Düsseldorf, Germany. The workers exhumed a skullcap like none ever seen before: long and low, with large ridges arching over the now-vanished eye sockets. Nearby they excavated some bones from the body of the same heavily fossilized and very robustly built individual. The workers assuming the bones to be those of a cave bear; but by great good fortune they set them aside for eventual examination by the local schoolteacher and amateur natural historian Johann Fuhlrott.

Fuhlrott, to his eternal credit, recognized them for what they were: the

remains of a previously unknown type of human. He took them to Hermann Schaaffhausen, professor of anatomy at the University of Bonn. The pair presented 'Neanderthal Man' to the world at a meeting of the local natural history society in 1857. This was the first evidence of a distinct (and now extinct) species or subspecies of human, *Homo (sapiens) neanderthalensis*, that lived during the latter part of the Pleistocene epoch, more familiarly known as the Ice Age, some 200,000 to 30,000 years ago. Neanderthal fossils have since been found throughout Europe and western Asia from the Atlantic to Uzbekistan, and from Wales to Gibraltar and the Levant. The Neanderthals probably evolved from either a late form of *Homo erectus* or a descendant of that species--either *Homo heidelbergensis* or 'archaic' *Homo sapiens* (Stringer & Gamble, 1993).

A Cossack with Rickets

Schaaffhausen came tantalizingly close to an evolutionary perspective on his fossils, but the time was not ripe for the suggestion that the Neanderthaler was anything other than an inferior, or 'savage,' version of our own species. Tattersall relates the hilarious story (pp. 77f):

Unfortunately, the heavy guns were not on Schaaffhausen's side. In Germany the life sciences were dominated at the time by Rudolf Virchow, the father of the modern study of cell biology and a doughty opponent of evolutionary thought in all its manifestations. Virchow's specialty was pathology, and pathology provided the explanation he preferred for the unusual appearance of the Neanderthaler....so he heartily endorsed the conclusions reached by...Professor August Mayer....Mayer's examination of the bones...suggested several things to him. He noted, for example, that the thigh bones and the upper front part of the pelvis were somewhat curved, as in lifelong horsemen. These characteristics, he claimed, might also have been exaggerated by childhood rickets, a vitamin deficiency disease. The left arm had been fractured and had healed badly; and Mayer claimed that this injury was the key to the unusual shape of the skull: it was the constant frown brought on by the pain of the injury

that had caused the formation of the bony ridges above the eyes! Putting all the evidence together, Mayer proposed that the remains were those of an unfortunate deserter from the Cossack cavalry that has paused near the Rhine in January of 1814, before proceeding onward to attack France.

On the other hand, Marcellin Boule (1912; see also Hammond, 1982) and Arthur Keith (1912, 1928) argued that Neanderthals were too brutish to be modern humans. Boule classified them as a separate species--*Homo neanderthalensis*--rather than as a subspecies--*Homo sapiens neanderthalensis*. Beetle-browed, bent-kneed, sloping-necked, shuffling slouches with grasping feet and inferior brains--this familiar stereotype of the Neanderthals was started by Fraipont & Lohest (1887) but was advocated most vociferously by Boule (1912). For those who held the view of human linear progress from savagery through barbarism to civilization, the Neanderthals were simply fitted into preexisting stages, pigeonholed for reference, and used to reinforce the 'evolutionary' view of human history and progress. This is most wonderfully illustrated by what is probably the first artistic depiction of a Neanderthal, a drawing that appeared on the front page of the 19 July 1873 *Harper's Weekly*: "...A more ferocious-looking, gorilla-like human being can hardly be imagined" (Trinkaus & Shipman, 1993, pp. 108f). Stringer & Gamble (1993) present various pictorial and statuary reconstructions of Neanderthal men and women, illustrating how widely these can differ: from ape-like, hairy, brutish and ferocious creatures to a somewhat stockily-built contemporary human (pp. 19f, 28f). Coon's (1939) portrait put the Chappelle-aux-Saints specimen into modern dress and gave him a shave and a haircut, suggesting that he could pass unnoticed in the New York subway.

Cave Bear Cults

Between 1917 and 1921, the amateur archaeologist Emil Baechler excavated the Drachenloch site in Switzerland. No Neanderthal fossils were found, but the Mousterian tools associated with them were, along with what Baechler considered to be evidence of Neanderthal ritual activity. Inside the cave were found the remains of many cave bears, *Ursus spelaeus*. To Baechler there was something special about the way in which these bones were disposed. He started the

notion of Neanderthal 'bear cults' (recently - once again - popularized by Jane Auel's novel *The Clan of the Cave Bear*), with bears the subject of worship and, possibly, ritual sacrifice. As Tattersall, in his sublimely illustrated book, comments (p. 95):

To a scientific milieu that was still trying to come to grips with the Neanderthal phenomenon, there must also have been a certain comfort in the contemplation of a deeply human spiritual awareness in combination with 'primitive' rituals such as those envisaged by Baechler. Familiar yet unfamiliar: these behaviors perfectly matched the equivocally human morphology of the Neanderthals. More-recent work, however, has shown that the reality of the bone accumulations of the Drachenloch was almost certainly much more prosaic than the picture Baechler painted.

Cannibal Feasts?

Another fanciful reconstruction occurred in connection with a Neanderthal specimen discovered in Italy in 1939. The discovery had been made by a workman in almost complete darkness, and the skull had been picked up and replaced on the ground by the time the paleontologist Alberto Blanc was called in. A reconstruction by Blanc showed the cranium lying inverted, a gaping hole in its base pointing straight up, within a 'crown of stones'. Tattersall's view (p. 101):

Ignoring the fact that the cave floor was covered with stones and bones, and that here was no certainty about exactly where the skull had come from, Blanc built on the tradition of Krapina [Gorjanovic-Kramberger, 1906] and the Drachenloch to spring to the conclusion that the Guattari skull represented the remains of a cannibal feast. The individual had been killed by a blow to the right side of the head; the head had been severed from the body and placed upside down in a ring of stones; the skull base had been broken open to extract the brain...; the empty braincase had been used as a drinking cup before being replaced on the floor; and the broken animal bones scattered around the cave had accumulated as a result of further sacrifices associated with this bizarre cannibalistic ritual. We know now that Guattari Cave was in fact

an ancient hyena den, and that the Neanderthal skull was simply one more of the numerous mammal bones with which it was littered.

The contemporary verdict is that the alleged evidence of Neanderthal cannibalism can be interpreted as the result of mortuary practices (as at Krapina) or carnivore activity (Bahn, 1992).

Flower People

During the 1950s new Neanderthal discoveries continued to come in. Analysis of these specimens supported the more modern-human-like picture painted by Clark Howell and Loring Brace, among others, as a reaction to the former more 'bestial' image. In 1955, both the Swiss primatologist Adolph Schultz and the French palaeontologist Camille Arambourg stated that the Neanderthals must have walked fully upright. They were vindicated when Straus & Cave (1957) published a detailed reanalysis of the La Chapelle-aux-Saints skeleton, which appeared to show the symptoms of osteoarthritic degeneration.

Around the same time, the U.S. archaeologist Ralph Solecki excavated nine Neanderthals in Iraq. One was an adult male who had suffered from a disease that withered his right arm. Solecki pointed out that this individual could not have survived without the support of his group. Suddenly the Neanderthals became caring and humane. This new persona was made yet more compelling by the discovery of fossil pollen that suggested the individual had been buried with spring flowers. Tattersall notes that the subtitle that Solecki chose for his popular book on Shanidar, *The First Flower People* (1971), eloquently reflects how dramatically the Neanderthal image was changing.

Lately, however, this 'new' Neanderthal persona has drawn heavy flak. Rowley-Conwy (1993) argued that the pollen in the Shanidar 'flower burials' could have got there in various ways, even during excavation. There is little evidence of Neanderthal burials, or that they had a religion or believed in an afterlife. So, neither the image of *H. neanderthalensis* as a cannibal, nor as a worshipper of cave bears, nor as a flower child, nor as a bent-kneed slouch has withstood the

test of time and the accumulating evidence.

Mysterious Extinction

What about the end of the Neanderthals, Tattersall's 'mysterious extinction'? Over the years two camps have disputed this issue vehemently, one favoring regional continuity and the other, population replacement. Trinkaus & Shipman (1993, p. 414) favor the continuity hypothesis:

To us, the fossils indicate that the earliest modern humans evolved out of Neanderthals (or out of late archaic peoples very like them) soon after Neanderthals had themselves appeared, about 100,000 years ago. This was not an evolutionary event that happened simultaneously across the entire Neanderthal range.

Anatomically, Trinkaus & Shipman argue, "the Neanderthals are quite similar to ourselves, having a skeletal arrangement identical to ours, brains as large as ours, and - to the best of our knowledge - the capability to perform any act normally within the ability of a modern human" (p. 412). These authors further assert, in a chapter modestly entitled "The Current View" (p. 416):

Though the evidence in different regions of the Old World records genuinely different events, nowhere is there evidence for violent confrontations between Neanderthals and modern humans (myths notwithstanding). The mosaic of local evolution, migration, admixture, absorption, or local extinction of Neanderthals was a complex process that occurred over the last 10,000 years.

Tattersall's view (p. 202) is quite different:

It is vanishingly unlikely, however, that peaceful assimilation was an overall option, with groups of the two kinds of humans [the resident *Homo neanderthalensis* and the invading *Homo sapiens* or Cro-Magnons] exchanging members when they met and going their separate ways, or joining forces. More likely, perhaps, if intermixing is to be considered at all, is a scenario of well-equipped and cunning *Homo sapiens*

descending on Neanderthal groups, killing the males - through strategy and guile, certainly not through strength - and abducting the females.

Tattersall does not even mention the possibility of a peaceful displacement scenario (as envisaged by Graham Richards and Stringer & Gamble) or a continuity scenario (as suggested by Trinkaus & Shipman). Neanderthal mass graves or other evidence of massacres has never been found, and Neanderthal females would not have been of value to the invading *H. sapiens sapiens*, as Tattersall himself admits, because rather different species probably could not interbreed.

Around 45,000-30,000 years ago anatomically modern humans (Cro-Magnons) arrived in Europe and must have coexisted with the last Neanderthals. But Stringer & Gamble (1993, pp. 193f) present a much less bloodthirsty replacement scenario:

In an area as large as Europe, with its varied environments and over a timespan of perhaps 10 millennia, many different kinds of interactions could have occurred (and probably did occur), ranging from avoidance to tolerance to interbreeding, and from conflict and economic competition to friendship and an exchange of ideas... [Very probably] there was minimal gene flow (interbreeding) between the two populations [because of] predominantly behavioral barriers that kept them distinct from one another... If the Cro-Magnons became more skilled at coping with and exploiting the European environments than the Neanderthals, the Cro-Magnon populations and ranges would have increased....the Neanderthals would have suffered from economic competition unless they withdrew to more marginal areas (such as, in this context, the southern Iberian and northern British peninsulae). If the Cro-Magnons occupied the more favourable and sheltered lowland valleys, the Neanderthals would have had to occupy higher or less-sheltered ground.... They would have suffered from higher infant mortality rates and shorter lifespans....this attrition would probably have caused Neanderthal populations

gradually to decline toward extinction. In fact, using a computer-simulated model, archaeologist Ezra Zubrow has shown...that...a Neanderthal mortality rate only 2 per cent higher than that of the Moderns could have resulted in Neanderthal extinction within about 1,000 years.

Besides direct conflict, expropriation of resources, withdrawal to peripheral regions, and interbreeding, there is one more hypothesis to be mentioned: Deadly diseases introduced by *Homo sapiens sapiens* to the which the Neanderthals (long isolated due to the climate) were not immune. This is what happened to various indigenous populations upon the arrival of Europeans: e.g., the Amazonians, the Eskimos, and the American Indians (Angela & Angela, 1993, p. 246).

How did the Neanderthals react to the advent of these modern humans? Artifacts show that the Neandertals had started to modify their tools, to borrow the Cro-Magnons' more modern technology. In France, the 'innovative' results are called Chatelperronian; in Italy, Uluzzian; in Western Europe, Szeletian.

Concluding Remarks

Three recent books on Neanderthals overlap to a great extent, but their conclusions about the end of the Neandertals are widely divergent. One proposes a continuity theory (Trinkaus & Shipman), one pictures a gradual and rather peaceful replacement scenario (Stringer & Gamble), and one paints a genocidal bloody demise of the hapless Neanderthals (Tattersall). This last theory is, however, neither novel (Boule proposed this scenario in 1912) nor very probable.

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Darlie Routier: Paradigmatic Exemplar or Error Variance or Outlier or...?

By Wade C. Mackey, 401 Lake St., Bryan, TX 77801 USA, e-mail waddmac@aol.com.

Darlie Routier was convicted of stabbing to death her two biological sons, ages five and six. Both were healthy and normal. A third son - a one year old - was not assaulted. The killings occurred on 6 June 1996 (D-Day).

Darlie Routier is 27 years old and a homemaker. Her 29-year-old husband, biological father of the slain boys, has remained a resident husband to Darlie Routier. Suggested motivations for the slayings are dissatisfaction with her financial status and the demands of motherhood.

How does evolutionary psychology/sociobiology/biocultural anthropology/human ethology handle this reality? Does Darlie Routier fit into our theoretical bundle? She kills her own progeny, not in infancy, but after 5 and 6 years of investment. She is not beginning fertility; she is probably closer to its terminus. She has a husband who, by all reported accounts, is quite supportive of her. He is quoted as saying: "We've been spit on, beat up, but they can't take our spirit away. We still have hope...We have to keep fighting" (KTVT-TV, Dallas).

If the work by Daly & Wilson (1982, 1985, 1987, 1988) is used as a template, Darlie Routier is an outlier of impressive deviancy. If the work by Mann (1996) is used as a template, Darlie Routier is an outlier of impressive deviancy. How do we address such realities?

Just as Lewis Carroll admonished everyone to beware of the Boojum, Kuhn (1973) cautioned the scientific enterprise to be chary of anomalies. How do we account for such anomalies as Darlie Routier?

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Review of Science News Stories of 1996

Science News is a weekly digest of developments in the natural sciences, with behavior well represented. Subscription rates are \$49.50 per year. The address is 231 W. Center St., P. O. Box 1925, Marion, OH 43306 USA. The year-end summary of major articles included these:

Behavior

Scientists linked a specific gene to a facet of thought--the ability to visualize and mentally manipulate parts of objects (150: 39).

Young children get an intellectual boost from parents who talk to them frequently, a practice most often observed in white-collar families (150: 100).

An immune reaction by pregnant women to the blood of their unborn babies may cause fetal brain damage that underlies some cases of schizophrenia (149: 68).

Brain-imaging studies indicated that separate neural systems handle conceptual and verbal knowledge about certain categories, such as animals and tools (149: 234, 103).

Membership Renewals

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World War II Holocaust survivors often pass on to their children a vulnerability to post-traumatic stress disorder (149: 310).

Biology

The human brain has internal stopwatches that monitor intervals of minutes to hours (149: 101).

Memory-related brain cells do not appear to die as people age (150: 150).

Worker ants produce more soldiers when threatened (149: 102); female ants kill their brothers to boost their genes' future (150: 295); and ant mating strategies may generate new species (150: 284).

A musical ability, perfect pitch, appears to be inherited (150: 316).

Paleobiology

Analysis of carbon isotopes in ancient Greenland rocks pushed back the history of life on earth to 3.85 billion years ago (150: 292).

Continental plants suffered a massive die-off 250 million years ago, coincident with a great animal extinction (149: 164).

Analysis of genes in living organisms suggested that the first animals emerged a billion years ago, far earlier than previously thought (150: 335).

BOOK REVIEWS

The Natural Science of the Human Species

By Konrad Lorenz. Edited and introduced by Agnes von Cranach, *née* Lorenz. MIT Press, 55 Hayward St., Cambridge, MA 02142 USA, 1995, \$35 (hdbk.). Originally published as *Die Naturwissenschaft vom Menschen. Eine Einführung in die vergleichende Verhaltensforschung. Das "russische Manuskript"* (1944-1948) by Piper Verlag, München, 1992, DM49.80 (hdbk.).

Reviewed by Alain Schmitt & Irenäus Eibl-Eibesfeldt, Ludwig-Boltzmann-Institut für Stadtethologie, c/o Inst. für Humanbiologie, Universität Wien, Althanstr. 14, A-1090 Vienna, Austria

In February 1948 Konrad Lorenz (1903-1989) returned from a four-year detention in a Russian war prisoners camp. He had with him a manuscript of 750 letter-format pages partly written with ink which he had made himself out of potassium permanganate, on paper recovered from cement sacks. The manuscript was written with Goethe's *Faust* as the only library background. Lorenz never published it himself, but it became the basis of *Vergleichende Verhaltensforschung* (1978; Engl.: *Comparative Ethology*); of some papers, particularly those containing his evolutionary epistemology; and of *Die Rückseite des Spiegels. Versuch einer Natur-geschichte menschlichen Erkennens* (Engl.: *The Back-side of the Mirror*), his epistemological magnum opus published in 1973, the year he received the Nobel Prize. Thus, *prima facie*, the book is an important historical document which very well illustrates an autobiographical *bon mot* of Lorenz: He repeatedly asserted that he knew all he had to say well before he was twenty, and had no new ideas after that, but had to repeat the same things over and over again in order to persuade his contemporaries--or to outlive them. He succeeded in his endeavour, but only for a short time. Indeed, today, adapted-mind theorists rediscover (or re-invent?) evolutionary epistemology without even citing Lorenz's *Mirror* (e.g., *The Adapted Mind*, edited by Barkow, Tooby & Cosmides, 1992), let alone his early relevant papers (e.g.,

Kants Lehre vom Apriorischen im Lichte gegenwärtiger Biologie, 1941, *Blatt für Deutsche Philosophie*, 15, 94-125). These modern evolutionists too have to persuade skeptical psychologists and philosophers. This is the myth of eternal return working in the social realm of science.

However, Lorenz's Russian manuscript is much more than a historical document. It is well suited even today to be read for its own merits, since it presents many of the fundamental methodological and epistemological principles of comparative ethology in a brilliant and vivid style. It shows Lorenz at his best, presenting plenty of appropriate and well-described animal examples, and integrating the vast areas of philosophy and biology with logically impeccable reasoning. [The Russian manuscript also illustrates the rule of thumb that the best books are those written starting from a huge empirical data base and vast general knowledge of theory, but without the possibility of checking in a library the correctness of the ideas borrowed from other authors, or of citing them verbatim.]

Lorenz intended the Russian manuscript to be the first textbook on comparative ethology. Its aim was to show that the morphology, behavior, sense organs, "higher" cognitive apparatus, and "minds" of all living organisms, humans included, evolved over many millions of years. Thus, all organismic structures are adapted to and represent features of the outer world. They are hypothetical but realistic models (working hypotheses), rather than ideal and deductively derived constructions of it. That humans are part of the natural world does not diminish their dignity. In contrast, the comparative approach reveals more clearly the uniqueness of humankind and makes its achievements and failures more comprehensible. In particular, the diagnosis of behavioral lapses and of shortcomings of inference, and new therapies, are needed now more than in any other period of human history.

Lorenz insists on some rules governing a successful comparative ethology. First, it has to be inductive, starting from a broad base of empirical knowledge ("*Weltanschauung durch Anschauung der Welt*"). Deduction and experimental hypothesis testing come only

after induction. The best way to get a broad knowledge base is to breed a large number of animal species in a setting as natural as possible, and to love them. Comparing species and noting the behavioral peculiarities triggered by deficiencies in rearing conditions inexorably lead the thorough observer to discover the fine-tuned adaptedness of behavior and the idea of phyletic descent. Second, comparative ethology should be anti-idealistic and materialistic; that is, it must always search for the physiological substrate of psychological or behavioral facts, and to be descriptive and systematic before being nomothetic (i.e., before searching for a general law). Third, Lorenz recommends interdisciplinarity and behavior analysis "on a broad front." That is, he demands that we ask Tinbergen's four questions, and particularly that we not mix the proximate and ultimate, and that we know the whole animal in its natural world before analysing parts of it.

The second section of the book summarizes the biological foundations of comparative ethology. Lorenz starts by defining life. Its constituents are:

- Metabolism
- Expansive assimilation
- Ectropy, the tendency to develop from the simple to the complex by fulguration, that is, by showing emergent (new and unpredictable) properties
- Integrality ("*Ganzheitlichkeit*"): any organism is more than the sum of its parts, and always a whole entity whose parts cannot be analysed without knowing the whole
- Finality or teleonomy--today, one would probably say the maximising of individual fitness
- Historicity and phylogenesis
- Mindfulness ("*Beseeltheit*"): *élan vital*, the subjective affective experience which in "complex" organisms may accompany the physical and chemical changes underlying activity of the nervous system.

Lorenz insists on the interdependency of life's characteristics and then devotes the subsequent chapters to a detailed analysis of historicity, integrality, finality and mindfulness. Here we can give only one of his numerous insights. The analysis of phylogenetic relations is hampered by

processes such as convergent evolution and the multidimensionality of phylogenetic trees. In contrast, it is simplified by Dolló's law, which says that adaptative history is never reversed. Organisms never simply lose legs and develop fins when they return from land to water, but instead refashion existing organs into "new" ones. In short, extant organisms have structures which are both quite well adapted and not enough maladapted. François Jacob spoke of "evolution's tinkering" (*Les jeux du possible*, 1981). Dolló's law is the luck of the systematist, since without it, one could never discover the (historical) way evolution shapes organs and behavior.

Section III on the "History and Methods of Comparative Ethology" is a brilliant compilation of ethological ideas and empirical deeds. It includes psychohistories of some of the major actors (Oskar Heinroth, Charles Otis Whitman, Lorenz). It is a superb stylistic trick to include this device in a book in which the notion of phylogenetic descent is of utmost importance.

Instincts had been postulated by medieval scholastics as supernatural and thus irreducible explanatory principles for inborn behavior. "Instinct thus was from the beginning one of those words which appear just at the moment when the right concepts are not at hand" (p. 264). "Instinct" stopped further causal-analytic progress. Even worse, it was at the center of the conflict between the vitalists, who mystified it ("We have instinct, but we do not explain it [*Bierens de Haan*]", Hans Driesch, Claude Bernard, Henri Bergson), and the mechanisticists, who classified it as scientifically useless and went on to study reactive behavior (Ivan Pavlov, Wilhelm Wundt, John B. Watson). Note that both schools, in their daily and practical approach to the real world, were very successful; cf. the achievements of Pavlov and Bernard. In the long run however, mechanismism won the race, as we know today.

The elements of behavior discovered by the three great mechanistic schools, Wundt's association psychology, Pavlov's reflexology, and Watson-Skinner trial-and-error learning, made the same "error" of not relying on a large inductive knowledge base. Instead, all analysed behavioral elements that stand relatively isolated within the integrated wholeness of organisms. That was their

advantage, but at the same time their dead end. None of the mechanistic schools was interested in documenting the behavioral repertoires of the animals they studied. Consequently, they overgeneralized their findings.

Comparative ethology, starting from a broad knowledge of the behavioral repertoires of many species, went beyond the above reductionisms--and invented new ones, as one has to admit in retrospect. Lorenz then gives his account of early ethology, starting with the ornithologists Heinroth and Whitman, whose pet animals were ducks and doves, respectively. They discovered independently from each other that behavioral elements may be used to systematic and phylogenetic ends in the very same way as organs. Their discovery opened the way to investigate the physiological nature of species-specific instinctive movement patterns (Heinroth's "*arteigene Triebhandlungen*") and the connection between instinct and intelligence ("The faults of instincts are...the open door through which the great educator experience comes in and works every wonder of intelligence"--Whitman, arguing against Spencer). Although Heinroth was a hardcore-empiricist, an arch-materialist and mocker of the spiritual and philosophic, he thought that animals have a subjective experience of their behavior ("*Stimmungen*", moods, affects). Animals (from Lat. *anima*, soul) should even have many more of them than man, since they have many more "instincts" (e.g., in gallinaceous birds, one mood to escape from an air-borne predator and one from a terrestrial predator). From Heinroth comes the famous dictum that "animals are emotional persons with very little understanding." (Note that the vitalist McDougall also recognized that emotions and affects are the experiential side of "instinctive" reactions, and constructed a list of 13 human instincts, published in 1908). Heinroth also described "intention movements," which in reflect preparedness to act in a near future, and which may have become ritualized and elaborated during phylogeny (e.g., by colored feathers) to become a signal.

In particular, spontaneity became the hallmark of progress in ethological theory. Wallace Craig, a pupil of Whitman, observed that (locomotor) restlessness ("appetitive behavior") grows in animals which have for a

long time not performed a particular behavior (e.g., hunting in wolves). They start searching for a situation which allows the "consummatory act". Thus, Craig formulated a simple but coherent theory of action which did not, for the first time in the history of psychology, confound the fitness enhancing function of a behavior (wolf incorporating the prey) with its subjective aim or purpose (wolf runs after, shakes to death). Lorenz himself had long since noticed the rise and fall of the threshold at which situations or releasers may trigger the performance of a behavior. He had also described vacuum activity ("*Leerlaufhandlung*") which animals show when deprived for a long time. In the thirties, Erich von Holst showed that there is a lot of spontaneous activity in the parts of the CNS that coordinate muscle groups into patterned and behaviorally relevant activity, just as cardiac muscle contracts spontaneously. Input from higher levels of the CNS or from the sense organs disinhibits, or releases, the patterned behavioral output. If there is no adequate situation or stimulus for a long time, behavior appears spontaneously, as a vacuum activity. Reflexology in all its forms--Pavlovian, associationist and behaviorist--was dead, and the core concepts of modern ethological theory were at hand. Lorenz and Tinbergen went on to make it more and more consistent and popular. Their most important achievement is probably the detailed analysis of innate releasing mechanisms (IRM).

The final pages are dedicated to the implications that basic concepts of comparative ethology have for neighboring disciplines. Particularly important is spontaneous activity of the CNS. It reduces the significance of reflexes, and magnifies regulation by inhibition and disinhibition. Releasers and key stimuli do not act directly on the automatic parts of the IRM, but eliminate somehow the inhibiting activity of the higher CNS centers.

The "Russian Manuscript" is perfectly edited by Lorenz's daughter Agnes von Cranach, who also provides personal background in her introduction. To repeat, this is more than a historical document. The "Russian Manuscript" is of a refreshing vividness, filled with sparkling enthusiasm for the scientific approach to man and animal. It can charm any reader, be it a lay person or an

old-hand ethologist.

Editor's note: This review was written as a tribute to Daniel G. Freedman, who retired from the University of Chicago in 1995.

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When Elephants Weep: The Emotional Lives of Animals

By Jeffrey Moussaieff Masson and Susan McCarthy. Delacorte Press, 1540 Broadway, New York, NY 10036-4094 USA, 1995, \$23.95 (hdbk.), \$13.95 (ppr.).

Reviewed by William C. McGrew, Departments of Sociology, Gerontology, Anthropology and Biology, Miami University, Oxford, Ohio 45056, USA.

Question: What can a psychoanalyst with a Ph.D. in Sanskrit and a science journalist add to our knowledge of animal nature?

Answer: Not much, despite their earnest efforts at an ambitious synthesis. The fault is not in the material, which is all rehashed, apart from a few interviews added by the junior author. The real problem lies in the basic assumption that knowledge is what one chooses and wishes it to be, a viewpoint that ultimately boils down to anti-science. The result is a sort of "how-I-read-a-bunch-of-natural-history-books-and-showed-all-the-biologists-to-be-wrong."

Masson's thesis is that animals (at least some of them, such as large-brained homeotherms) have feelings, but that this is denied by Science. Thus, other species have hopes, love, grief, joy, cruelty, compassion, shame, aesthetics, awe, etc., and this is well known to ordinary folk, pet owners, traditional cultures, enlightened trainers, and keepers, and a (very) few animal behaviorists. The evidence for this is to be found in accounts, impressions, and insights reported in one-off events, that is, anecdotes. However (he says), the obvious truthfulness of this thesis is obscured by a pervasive conspiracy of speciesist scholars who use the spectre of dread anthropomorphism to avoid facing up to what everyone else knows. He takes it upon himself to set the record straight, in a systematic treatment through nine chapters covering the full range of emotions, starting with fear and finishing with "the inexpressible."

The procedure is simple: Glean compelling, punchy tales from the published literature, preferably from first-person narratives by familiar names (Elizabeth

Marshall Thomas figures prominently), then group these by topic, such as sadness, rage. Recount, or infer from the behavior shown, the underlying emotion clearly present in the individual animal. Give brief mention to any scientific treatment of the topic, belittling or distorting it, preferably with selective, out-of-context quotations. Then, move on to another topic. If the basic technique sounds familiar, it is the same one pioneered by Groos, Romanes, etc. in the last century, and used regularly by popularizers ever since. The villains are those stuffy old scientists, who insist on such niceties as data.

How can such an exercise be intellectually justified? Masson's epistemology is simple: Start with a null hypothesis that assumes that all animals have the same feelings until proven otherwise. Since it is impossible to prove that any organism *does not* have feelings, the thesis can never be disconfirmed. The blinkered scientists are regularly exhorted to break through their constraints of operational definition and replicability and quantification, but no hint of how to do this is ever supplied. Instead, evolutionary biologists (Richard Dawkins in particular) are pilloried as unimaginative party-poopers who insist on such distinctions as ultimate versus proximate causation.

The irony is that most of the best indications of animal emotions *do* come from scientists. Far from denying feelings to other species, it is likely that most ethologists have strong positive views on the subject. The frustration is methodological--how to measure these phenomena, rather than just intuit them. This challenge has been taken up in captivity (e.g., Marian Dawkins) and in the field (e.g., Richard Byrne and Andrew Whiten). These and many other contemporary students of animal capacities are ignored, with only Donald Griffin & Co. being given approval. Disingenuously, outdated studies such as Wijt's work in the 1950s on drugged spiders and Harlow's in the 1960's on socially deprived macaques are presented as representative of current research. Psychological studies of animals are equated to torture, although Fouts, deWaal, etc. are selectively cited whenever it suits the authors.

In their favor, Masson and McCarthy carefully document their sources in 33 pages of

notes, and supply a bibliography of almost 300 entries, although about three quarters of these are secondary or popular publications rather than original sources. As stated above, the issue is not so much the material, which comprises many charming stories that will ring true with most naturalists, but instead their uncritical interpretation. Of course some other species have feelings, and this does have implications for our treatment of them (as argued convincingly by Masson in his conclusion), but playing fast and loose with the facts will not establish this, nor advance this cause.

Romantic Passion

Edited by William Jankowiak. Columbia University Press, 136 South Broadway, Irvington, NY 10533 USA, 1995, \$27.50 (hdbk.).

Reviewed by Glenn E. King, Anthropology Program, Monmouth University, West Long Branch, NJ 07764, USA.

This is an important book in the burgeoning study of heterosexual love. Edited and written almost entirely by anthropologists, the volume has a great deal to say about culture but says it in a way that is congenial to the evolutionary perspective on human behavior. Fourteen of the 16 contributions explicitly accept the evolutionary view or at least are compatible with it. Discussions of cultural variation and culture change underscore the durability of love in differing circumstances, including hostile ones. The overwhelming impression is that the personal experience of love and the cultural valuation of love are distinct phenomena that interact in complex ways. The ethnographic evidence for the universality and nature of "romantic passion" complements the editor's previous holocultural study (Jankowiak & Fischer 1992).

Before going into detail, I wish to consider some matters of terminology. The primary subject of this book is a rather turbulent emotion that often initiates heterosexual relationships. The contributors use different terms for it, although they are clearly discussing the same phenomenon. This is confusing in itself, but it gets worse when a term is given more than one meaning. For example, some use "infatuation" for

Jankowiak's "romantic passion," but one contributor applies it to the "puppy love" of ten-year-old children. I suggest that we discard such semantic baggage and turn to a neologism. Since several contributors refer to the prominence of Dorothy Tennov (1979) in conceptualizing the subject, adoption of "limerence" seems appropriate. Of course, the precise definition of limerence can still be argued (as Helen Harris does in the present volume), but Tennov's concept and terminology seem to provide a starting point that avoids confusion.

Another semantic issue is the meaning of "pair bond," which some contributors overextend. In the discussion of the Inuit, for example, it is used as a synonym for marriage even though traditional marriages were not usually based on personal feelings. There is no point in such a superfluous usage when the term is so valuable for designating an emotional tie that may or may not be a factor in marriage. On the positive side, most of the contributions help us to distinguish among limerence, lust, marriage, and companionate love (the calmer affection that sustains long-term relationships, whether in a formal marriage or not). Several (such as Harris) suggest more subtle distinctions on which future work can be based.

In the theory section, the first two papers present evolutionary ideas that may be familiar. Helen Fisher's contribution is essentially a precis of her book (Fisher, 1992), and James Chisholm presents an argument that has recently been elaborated elsewhere (Chisholm, 1996). Chisholm combines attachment theory with evolutionary ecology to produce a thought-provoking hypothesis in which limerence is facultative rather than obligate. It is a developmental response to childhood insecurity which results in adult behavior that is appropriate for an uncertain environment where mortality rates are high or unpredictable: more frequent sexual activity, increasing the probability of early reproduction. Chisholm contrasts this mating effort hypothesis with Fisher's more traditional view of limerence as a basis for mutual parental investment.

These two views seem quite amenable to synthesis. The relationships of Chisholm's insecure people last about five years, while the secure ones average about ten. The duration of

insecure relationships is about the same as the four years that Fisher postulated as the basic hominid adaptation. The longer relationships of secure people are consistent with Fisher's thesis and attributable to durable companionate love. This suggests to me that Chisholm's distinction is valid but that it actually arises from the impairment of companionate love in insecure people.

The anti-evolutionary position is represented by Charles Lindholm, who characterizes sociobiology as assuming that human beings are basically governed by instincts. He thinks that an evolutionary interpretation of limerence predicts correlation with marriage and a high birth rate. Since Lindholm does not understand the distinction between ultimate and proximate causation, much less the concept of environment of evolutionary adaptedness, he has no constructive criticisms for us. In his eagerness to refute sociobiology, Lindholm discards concepts of cultural anthropology, such as the distinction between ideal and actual culture: He insists that we take medieval European protestations of chaste courtly love at face value. It is no surprise that he considers sociobiology's paradigm of evolutionary success as "disconcertingly mundane," preferring an ethereal philosophy that is at odds with the rest of the book: Romantic love is an expression of deep existential longings for an escape from the self.

Lindholm's paper contains one point of possible interest for human ethology. His research on charisma indicates a connection with limerence. An evolutionary approach to his data might reveal that dominance, so often linked to aggression, may also be connected with love.

The last theoretical paper emphasizes a psychological perspective which explores the "boundaries" between the love relationship and the surrounding community. It seems that, even where love is socially approved, it requires a degree of isolation in order to flourish.

The first of the ethnographic papers is an important one by Helen Harris which puts the Polynesian case of Mangaia in a context that brings together ethology, psychology, and cultural anthropology. Drawing on several decades of psychological research (with

special credit to Tennov), Harris formulates a concise, seven-point operational definition of what I am calling limerence (Harris argues that Tennov's "limerence" occupies an extreme position in a spectrum of feelings). She then demonstrates that all seven characteristics are displayed by Mangaians. The comparative value of the book would be even greater, and some terminological problems eliminated, if the contributors had all applied Harris's framework or collaborated on something similar.

Even without such a framework, the remaining papers in the book provide solid ethnographic backing for the editor's position that limerence is a human universal. It is documented in ten distinct nonwestern cultures, as well as polygynous Mormons. Sub-Saharan Africa, which has previously provided the fewest reports of limerence, is represented by three cultures in three different countries.

The third section demonstrates how various cultural theories can coexist with the evolutionary perspective. Victoria Burbank, for example, sets out to treat "emotional discourses as pragmatic acts and communicative performances." From her study of a native Australian community, she concludes that the Hollywood portrayal of "falling in love" has become the ideology of adolescent resistance to arranged marriage and the "idiom" for changing power relations in marriage politics. However, Burbank notes that European influence may have accentuated rather than created an Australian concept of love that is similar to our own. Ritual, stories, and art point to a precontact limerence, and ethnography shows us a history of elopement and affairs that must have expressed "passionate attachment" (Berndt, 1976). Apparently limerence is an old phenomenon that can take on new meanings and functions, i.e., the political discourse of the present may be founded on emotions with an evolutionary past. Native Australian youth have used marital love to protest their Establishment in what seems to be a parallel with American hippies of the 1960s, who used nonmarital sex for the same purpose.

Many other interesting points about the interaction of biology and culture are made or implied. Spouse exchange among the Inuit is reinterpreted as a cultural adaptation to limerence in circumstances where its usual

outlets are largely absent. In Brazil culture seems to take sides between two powerful evolved systems of behavior, resulting in the mother-son relationship taking precedence over that between husband and wife (no comparison with chimpanzees, unfortunately). Limerence in Trinidad is presented as a force for gender equality, in contrast to the United States where it has been enlisted in the suppression of women.

So much of this book contributes to a modern ethology of limerence that the omissions are frustrating. I wish that there

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had been a chapter on love in nonhuman primates (cf. Smuts, 1985) and another on a hunter-gatherer population less constrained by environment and social complexity than the Inuit and Australians (cf. Shostak, 1981). But this is carping. Jankowiak's book is valuable as it stands and I recommend it to students of human behavioral evolution.

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Ethology and Psychopharmacology

Edited by S. J. Cooper and C. A. Hendrie. John Wiley & Sons, Baffins Lane, Chichester, West Sussex PO19 1UD, England, 1994.

Reviewed by Russell Gardner, Jr., Department of Psychiatry and Behavioral Sciences, 4.450 Graves Building (D28), University of Texas Medical Branch, Galveston, TX 77555-0428, Email: rgardner@marlin.utmb.edu

This volume of 18 contributions stems from a conference held in Birmingham, UK to celebrate the work of a group of scientists led by Michael R. A. Chance, E. C. Grant, J. H. Mackintosh, and A. P. Silverman who, together

with their students, brought together the fields of ethology and psychopharmacology. Editors Cooper and Hendrie represent the most productive students of these leaders. They dedicate the volume to Petr Donát from Prague (who died at age 39 years in a car accident); he organized the first Ethopharmacological Conference in Bohemia in 1991. Throughout the book, the pioneering efforts of John Paul Scott from the U.S.A. are also mentioned repeatedly.

In chapter 1, Silverman tells how Michael Chance started the ball rolling. In World War II, Chance investigated amphetamine effects on mice; amphetamine kept RAF pilots awake during long flights, but little was known about it. Amphetamine was lethal to mice but with great variability, which Chance later found to be due to whether they were housed singly or in groups; grouping made the same dose more lethal. From then on, Chance's interest in behavior had charismatic effects on many people interested in the confluence of psychiatry and psychopharmacology with ethology and evolutionary biology. He was honored in 1991, for instance, by becoming the first president of the Across-Species Comparisons and Psychopathology (ASCAP) Society, a group which he helped to found.

Returning to post-war Britain, Chance found Tinbergen's 1951 *Study of Instinct* to be a revelation. Excited about relating behavior to ecology and evolution, he persuaded authorities to support the Uffcalme Clinic Laboratory, where a mansion was turned into a clinic for in- and out-patients, with its former stables newly devoted to zoology. Like the classical ethologists, Chance and his group directly watched animals under varying conditions. Red lights during the day allowed continuous observation of nocturnal rats. Ewan Grant and John MacIntosh published a classical paper in 1963 labeling the naturally occurring behaviors of rats of varied strains using nomenclature still deployed, and also providing comparisons with other species. Studies of drugs started when colleagues developed opiate derivatives, testing of which could be done on rats whose group behavior was now known. Chlorpromazine caused similar behavioral effects, and the research program was launched with many ramifications. The resident-intruder paradigm (RIP), for instance, is now a well established (and well funded)

method for ascertaining drug effects and metabolite actions.

Chance continues to be productive, and now in his early 80s, has published an original hypothesis accounting for the location of the testes of cursorial animals outside the abdominal cavity: His concussive peritoneal pressure theory states that this protects them from sudden rises of intraperitoneal pressure secondary to running and leaping (Chance, 1996). Support for the idea stems from the protection provided by burrowing and scuttling animals, not subject to such pressures, whose testes are protected within their peritoneal cavities.

The volume considers a variety of testing systems in addition to RIP, and relates them to various neurotransmitters systems and drug classes. The elevated plus-maze, for instance, features open and closed apertures available for animal inspection; investigators observe such inspections and calculate the ratio of the two kinds of aperture. Rodgers and Cole note that only the benzodiazepine "gold standard" for anxiolytic drugs works; only with these medications does the the ratio consistently demonstrate greater exploration of the open apertures. While the GABA system is therefore clearly tapped, the adrenoceptor and serotonin-receptor agonists provided confusing data. Height turns out to be a less important factor than the animals' thigmotactic interest in staying close to the side wall. The authors note that the ethological ideal of understanding animals in their natural habitat--the idea that sparked the entire area--needs renewal; rather than going blindly from technique to technique, they argue, a more fruitful effort would be to examine the causes of confusing results.

Turning to predator defense models of anxiety, Hendrie and Weiss found that tape recorded calls of predators induced alarm in mice predictably. Their chapter provides an excellent discussion of predator-prey relationships, with a focus on ecological dimensions. Using an opiate-antagonist drug, predator calls stimulate opiate analgesia, but calls from gulls do not--despite the fact that the mice attend to nonpredators. That this is a model of panic and not general anxiety was demonstrated by differential drug effects; for instance, benzodiazepines had no effect but

chronic imipramine and experimental anti-panic drugs did. Interestingly, antagonists to cholecystokinin-B (CCK_B) were also effective, calling attention to the role of this small amino acid chain in panic disorder.

Several useful chapters deal with the roles of sex, olfaction, meal patterning, stereotypes, behavioral teratology (how do drugs affect offspring behavior?), and behavioral variability. Some models of aggression focus on maternal aggression during lactation; a new mother's aggression may be particularly intense. Alfonso Toisi provides a clinical perspective in which he justifiably laments that too little attention is paid to behavioral analysis in usual psychiatric practice--which remains remote from the animal investigations that provide initial testing for the drugs now used so extensively.

But mainly the book belongs to the RIP. The resident-intruder paradigm is extensively discussed, both explicitly and by example. Cutler and Shepherd et al., for instance, use it to examine anxiolytic drugs. They showed that leaving only the scent of a presumed resident affects the new rat entering the setting. Diazepam reduces these fear responses. The authors also showed the same effect in ferrets, a species that lives with considerable predation pressure.

The RIP mainly captivates depression researchers, whose work Mitchell summarizes. He notes the territorial advantage a resident has upon even brief occupation of a territory, giving credit to A. K. Dixon's similar work and citing K. Miczek's summary of the paradigm. Mitchell's work has focused upon the resident rat (not the intruder), finding that various antidepressants reduce aggression. According to John Price et al.'s involuntary subordinate theory (not reviewed by Mitchell), however, the intruder animal would be the one on which to focus (Price et al., 1994). Subsequent to publication of these proceedings, Mitchell has responded to questions from Price in *The ASCAP Newsletter* (Mitchell, 1996).

Other chapters on the RIP include attempts to develop "serenics" for the reduction of aggression (summarized by Mos et al.). The sophisticated work of the Blanchards (in the best chapter of the book) uses experimental manipulations such as vibrissae alterations and

anesthetization to distinguish amongst predation, play fighting, and defense. They focus on sensorimotor interactions and, of particular importance, have considered the brain systems involved, paying special attention to the periaqueductal gray structures. They note that much less is known of the brain systems for offensive aggression than of those for defence. Their primary theoretical scheme entails risk assessment (RA) mechanisms, whose normal patterning, they claim, must be understood in order to interpret the effects of drugs on that behavior. Thus, diazepam increases RA when behavior was tested against a baseline of freezing and avoidance, but decreases RA when RA is an important initial response.

This volume is an important contribution to a nascent basic science for psychiatry which would explain disorders as aberrations from normal physiological mechanisms instead of empiric behaviors without a context. This manner of study has been elsewhere been termed "sociopharmacology"--a more salutary term than those involving the disembodied "psyche" of psychiatry, psychology and psychopharmacology (Barchas, 1984). Psychiatry's basic science might best be named "sociophysiology" because the major brain systems that deal not only with predation but also with one's own kind are the same systems likely to be involved in psychiatric disorders and drug action. Few answers are available as yet to dominate our thinking on these issues and, as Troisi notes in his clinical relevance chapter, an anthropocentric, casually arrogant attitude still pervades the field. This book, however, illustrates that systematic exploration has fruitfully begun--not only of human/nonhuman contrasts, but also of comparisons addressing the questions of which brain systems are involved, and how.

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Queer Science: The Use and Abuse of Research into Homosexuality

By Simon LeVay. MIT Press, 55 Hayward St., Cambridge, MA 02142, 1996, \$25 (ppr.).

Reviewed by Vern L. Bullough, University of Southern California. Home address: 17434 Mayall St., Northridge, CA 91325, USA.

The subtitle of this book emphasizes its message: research into homosexuality and lesbianism has always had political implications, and the data have been used and misused by both anti- and pro-gay groups. Simon LeVay, a former researcher at the Salk Institute in San Diego, gained widespread public attention from a study he made of a small number of available brains from gay AIDS victims, comparing them with the brains of an available male control group of unknown sexual preference. Following in the path of earlier studies of sexual dimorphism in the brain, he found that the medial preoptic area in the control sample was much larger than in the homosexual men, in whom the size was closer to the smaller size found in females. In the subsequent publicity, LeVay found his findings condemned or praised by people from every side of the sex and gender community.

LeVay resigned from the Salk Institute and co-founded the Institute of Gay and Lesbian Education in West Hollywood; one outcome is this book. In part the book chronicles the history of research into homosexuality and lesbianism beginning with Karl Heinrich Ulrichs and Magnus Hirschfeld, the major pioneers in the field in the late nineteenth century. The other part consists of an account of how, through history, research findings have been used and misused by widely disparate groups.

The author believes that neither Ulrichs nor Hirschfeld ever quite received the respect he deserved from other researchers, probably because both were homosexual. Hirschfeld in particular, perhaps because he claimed to be studying all varieties of sexuality, was put down by many of his contemporaries. LeVay feels that the attack on Hirschfeld discouraged later researchers from following up on some of the biological factors that Hirschfeld advanced as important to a homosexual or lesbian identity. Often, even when others adopted some of his ideas, as did Freud, they were unwilling to give Hirschfeld credit. Part of the problem is that neither Ulrichs nor Hirschfeld could offer any real evidence of which biological factors were involved. It has only been in the past few decades that breakthroughs have come about.

LeVay who, like this reviewer, looks more to nature than to nurture for explanations, attributes much of the hostility toward biological explanations to the fear that if "science" demonstrated a probability of the association of certain biological factors with homosexuality, then radical interventionist therapies would be developed to "normalize" gays and lesbians. While such therapies have been advocated in the past, support for them has come not only from advocates of "nature" explanations but also from those who take the "nurture" side of the debate. Each new explanation seems to have spawned a new treatment - hormonal injections, psychoanalysis, castration, aversion therapy, various drug combinations.

Moreover, both extremes in the debate are highly selective of the data they use. Obviously, the media and the public often seize upon every new finding in order to justify their own stance. For example, Kinsey, who stated that roughly 4 to 5 percent of the male population was homosexual and slightly less of the female population, found his figures distorted to argue that one in ten males were homosexual.

Some of the researchers themselves have been less dispassionate than appropriate, ignoring data which did not suit their view. LeVay points to the historian John Boswell, a Catholic convert, who cited evidence selectively in attempting to prove that Christianity originally had not been hostile to

homosexuality. Bench scientists have been no less prejudiced. The East German scientist, Günter Dörner, believed his controversial findings pointed to a way to cure homosexuals, and he himself participated in various drastic interventions, none of which worked. In sum, all too often, research has been used to bolster the prejudice, if not of the researchers, then of their followers.

LeVay holds, and the emerging evidence is pointing in this direction, that homosexuality has a strong biological component, but that it probably involves a combination of factors - genetic, intrauterine, hormonal, and environmental. He also argues that homosexual or lesbian identity cannot be segregated from a person's being and relegated to some corner of the psyche, to be locked up and forgotten or cut away. Rather, it is more like lefthandedness or other behaviors exhibited by sizeable minorities of the population.

LeVay's book is an excellent historical introduction to this literature, summarizing what we know and what we do not know, in addition to the uses and misuses of sexual science. The end notes are extensive and the discussion is dispassionate and enlightening.

Readings in Animal Cognition

Edited by Marc Bekoff & Dale Jamieson. MIT Press, 55 Hayward Ave., Cambridge, MA 02142 USA, 1996, \$30 (ppr.).

Reviewed by Kevin Warburton, Dept. of Zoology, University of Queensland, Brisbane 4072, Queensland, Australia.

The stated aim of this book is to introduce readers to the rapidly growing interdisciplinary field of animal cognition. The outcome is largely successful: there are 24 chapters which tackle a diverse range of topics including adaptation, the aims and methods of cognitive ethology, androcentric bias, anthropomorphism, cognition and ethics, communication, concept attribution, cultural transmission, helping behaviour, the comprehension and production of language, levels of analysis, mentalism, play, recognition systems, and vigilance.

Seventeen of these essays are drawn from a two-volume collection called *Interpretation and Explanation in the Study of Animal Behavior*, edited by Bekoff and Jamieson in 1990 and now out of print. This means that, in most cases, the cited references dry up at the end of the 1980s, so that recent developments are not mentioned. Given that the editors set out to cover such a wide range, I was also surprised at the fact that there is no contribution that attempts to extract messages from the fast-expanding world of human cognitive science. While the editors maintain that the book includes most of the important topics and leading figures, no fewer than 29 of the 30 contributors hail from North America, which might encourage the impression that the Rest Of The World is an intellectual wasteland.

These grumbles aside, the book is lively and provocative, and almost all chapters provide a thoughtful, penetrating treatment of the chosen subject, rather than just a review of the literature. This ensures that the book will retain its value and makes it ideal as a text in courses in animal cognition and the philosophy of mind. Most of the essays are concise, and include well-defined concluding sections. The book is free of printing errors and is well presented.

Two themes recur throughout this volume. First, there is the increasing realisation that animals are more cognitively sophisticated than previously recognised, a realisation grounded in a body of evidence which suggests that many non-humans are "conscious, have expectations, desires, and beliefs, make assessments and choices based on fine discriminations among various alternatives, and have subjective feelings" (chapter by Jamieson & Bekoff, p. 361). The perceived cognitive gulf between humans and other animals is shrinking rapidly, as exemplified by the proposal of Sue Savage-Rumbaugh & Karen Brakke that language differences between man and ape may result from differences in information processing and memory, but not in innate linguistic structures.

Second, there is a strong and pervasive subtext which surfaces explicitly from time to time and which, by highlighting the imperfections of human cognitive abilities, serves as an ironic complement to the theme

identified above. In grappling with problems of interpretation, as opposed to the straightforward collection and reporting of "obvious" physical facts, studies on animal cognition are likely to encourage a greater awareness of the business of interpretation itself. In fact, if this book is any guide, the most important contribution of animal cognition research may turn out to be what it teaches us about the frailties of reasoned analysis and the subjectivity of the scientific process.

By way of illustration, several articles raise questions associated with uncritical or hidden assumptions, which can trap the unwary in a variety of ways. For example, in a forcefully argued chapter entitled "Do animals choose habitats?", Michael Rosenzweig maintains that ecologists often use the concept of habitat choice in a sloppy way, by equating choice with use. He suggests that evidence for true choice could come from work on exploratory behaviour, mental maps, and foraging flexibility. As another example, in an examination of injury-feigning behaviour in the piping plover, Carolyn Ristau shows that although the broken-wing predator-distraction display is an evolved, genetically transmitted behaviour, it would be wrong to assume that plovers are incapable of employing the display as an integral part of flexible, learned strategies.

Along the same lines, Bennett Galef argues that many workers have assumed that locale-specific behaviors are socially transmitted in cases where no evidence of social transmission is available. He calls for controlled experiments to establish the existence of social transmission and investigate possible methods of transmission. Further, Sandra Mitchell believes that it is dangerous to rely on comparative, adaptationist interpretations of behaviour involving species which are capable of cultural transmission, because of the need to assume that the behaviours observed in the groups concerned are strictly comparable. As a case in point she cites the use of the term *rape*, as applied to both humans and scorpionflies.

These examples show how our background beliefs and assumptions, independent of the available data, colour our interpretations of animal behaviour. At a very general level, Lori Gruen reminds us that "[t]he

way we categorize and interpret the world around us has much to do with our context - the external events that we notice and those that we do not notice" (p. 17). As a philosopher, Gruen focusses on problems of gender bias in science rather than animal cognition per se. This is one of several chapters which made me feel that the title of the book was too narrow.

Assumptions are the building blocks of biological modelling. Vigilance behaviour might seem to be a clear case of antipredator wariness, which can be modelled quite simply. Correct? Not according to Steven Lima, who I suspect has set out to induce all those interested in vigilance to give it up as an impossible area of study and leave him with a monopoly! Lima argues that conventional models of vigilance are gross caricatures of reality: they tend to rely on the many-eyes hypothesis, which may be too simplistic and usually ignore the implications of cheating. Moreover, other typical assumptions (e.g., that vigilance increases with increasing risk, that vigilance and food ingestion are mutually exclusive, and that selfish animals are less vigilant than cooperators) are not necessarily correct. Lima concludes that interpretations of vigilance are based more on human intuition than on animal behaviour, and notes that, in any case, models of vigilance are hard to parameterise and test.

So, faulty assumptions can create difficulties. However, it might be argued that an awareness of such dangers will at least encourage critical thinking. A fundamental aspect of critical analysis is the clear definition of terms. Operating along these lines, Michael Philips and Steven Austad review definitions of communication and suggest that the essence of communication is information transfer, not its causal impact. They also make clear connections between signalling, signal recognition, and social complexity, and stress that the evolution of behavior depends on the evolution of information processing.

Alexander Rosenberg is similarly concerned with definitions, but in his case definitions of play, which are notoriously elusive. He contends that because intentionality is an important characteristic of play and because the functions of play are diverse, heterogenous, and strongly dependent on environment, play cannot be explained in

terms of a single function. In the next chapter, Colin Allen & Bekoff defend the evolutionary approach by maintaining that whether or not a behaviour such as play is intentional is an empirical question and not to be decided by definitions. In fact, insisting on rigorous definitions prior to empirical research may require knowledge that can only be obtained by empirical research! This exchange highlights further problems of categorisation.

Several chapters are symptomatic of a continuing paradigm shift in animal cognition studies, away from a rigid adherence to parsimonious positivism and toward a more interpretative approach based on critical reasoning and experimentation. For example, John Andrew Fisher contends that objections to the "sin" of anthropomorphism may constitute oversimplification of the complex issue of animal consciousness. He notes that there have been economic, religious, and ideological reasons for denying animals any sort of mentality. This paradigm shift is surely for the good: after all, conscious intention is almost impossible to demonstrate unequivocally, even in other human beings, so negative evidence is easy to find. As Hugh Wilder notes, uncertainty is not peculiar to cognitive ethology, but the hallmark of all good science. Rather than being a retreat from the classical scientific approach, the shift is likely to lead to better science since its starting assumptions are more realistic and an intentionalist stance provides a better framework for developing testable hypotheses.

A good example of this is provided by Savage-Rumbaugh & Brakke in the context of language learning in non-human mammals. Early experiments on the linguistic abilities of apes tested their ability to associate signs with presented objects, but did not foster the motivation to communicate novel intentions. Evidence for linguistic intentionality and originality emerged only in later studies, in which subjects were allowed to select appropriate symbols in a natural way. These studies showed that it is not necessary to train apes to use language. Interestingly, the latter experiments were carried out in a more informed context than the earlier ones, since work on human infants in the intervening period had revealed the importance of observational learning in linguistic development.

On the current evidence, our attempts to understand animal cognition are likely to provide a valuable perspective on our own limitations, and help to counter tendencies to mindless (another irony?!) speciesism. Who knows, they may provide a path to enlightenment and humility.

ANNOUNCEMENTS

Academic Position for Observational Data Analyst

The Dept. of Education at UCLA announces a faculty position, at open rank, for a quantitative research methodologist. Requirements are skill in measurement and/or survey research methods and/or analysis of observational data. Candidates should also have a substantive area of research, such as sociology of education, educational psychology, studies of at-risk students, educational assessment, policy studies, or educational technology. The position includes teaching advanced research methods and intermediate courses in statistics and design for graduate students. An earned doctorate is required. The job also entails scholarly research and academic advising at the graduate level. Starting date is between July and September, 1997. Screening of applications will begin 15 April and continue until the position is filled. Send application letter, curriculum vitae, sample publications, and names and addresses of at least three referees to Harold G. Levine, Chair, Dept. of Education, University of California, Los Angeles, CA 90095-1521 USA, tel. 1-310-825-1342, e-mail: levine@gseis.ucla.edu. Submitted by Nick Blurton Jones, e-mail: nickbj@ucla.edu.

International Ethological Congress

Although the deadline for submitting abstracts is past, one may still register for this large and important meeting in Vienna 20-27 August 1997. Contact XXV IEC, Wiener Medizinische Akademie (WMA), Alser Strasse 4, A-1090 Vienna, Austria; tel. 43-1-405-1383-21; fax 43-1-405-1383-23; e-mail: medacad@via.at. Host Karl Grammer reports that there will be about 30 papers on humans.

ESS Meeting

The 20th annual meeting of the European Sociobiological Society will take place 7-9 July 1997 in Ghent, Belgium. The theme will be "The Sociobiology of Ingroup/Outgroup Behavior, Part II." Part I was the 1985 meeting in Oxford, which led to publication ten years ago of *The Sociobiology of Ethnocentrism*, edited by Vernon Reynolds, Vincent Falger, and Ian Vine (London: Croom Helm; Athens, GA: University of Georgia Press). Papers are welcome concerning the evolutionary dimensions of ethnocentrism, nationalism, xenophobia, and other sociobiological applications of the ingroup/outgroup concept for human societies. Studies focusing on the decline of indigenous populations, such as has occurred in some European countries, are welcome, as are papers focusing on ethnic strife and mass migration. Send abstracts of proposed papers to Prof. Dr. R. L. Cliquet, University of Ghent, Faculty of Political and Social Sciences, Section Biological Anthropology and Social Biology, St.-Pieterstraat 49, B-9000 Ghent, Belgium. Deadline for submissions is 10 June. To attend, contact Kris Thienpont, University of Ghent, same address, tel. 32-(0)9-264-42-48, fax 32-(0)9-264-42-94, e-mail kristiaan.thienpont@rug.ac.be. Registration deadline is 1 June.

International Society for Research on Emotions

This group publishes a quarterly newsletter, *ISRE*, for US\$15 per year. For information, contact the editor, Ross Buck, Communication Sciences, U-85, University of Connecticut, Storrs, CT 06269-1085 USA, fax 1-860-486-5422, e-mail buck@uconnvm.uconn.edu.

Current Literature Editor Sought

After 15 years of faithful service, Bob Adams wishes to retire from editing our Current Literature section. The job entails scanning the periodical *Current Contents* for articles of interest to our readers. If you may have an interest in this vital task, please contact Glenn Weisfeld.

ISHE Convention Dates Announced

Our next biennial meeting will be held in Vancouver, British Columbia, Canada 19-23 August 1998. President-Elect Charles Crawford is hard at work planning the event. For those who don't know, Vancouver is one of the world's most beautiful and cosmopolitan cities, and British Columbia remains an unspoiled wilderness where many large mammals still thrive. The weather in August is ideal. The Canadian dollar is worth about three-quarters of the U.S. dollar, and Charles seems to have succeeded in arranging for inexpensive accommodations and meals for us. Plan now to attend.

ASCAP Meeting

The annual meeting of the Across-Species Comparison and Psychiatry Society will take place on 4 June 1997, before the Human Behavior and Evolution Society meeting, at the University of Arizona in Tucson. The ASCAP meeting will start at 8 AM and end at 5:30 PM. Registration fee is \$20, payable by credit card, check, or money order to "University of Texas Medical Branch," c/o Frank Carrel, Dept. of Psychiatry & Behavioral Sciences, University of Texas Medical Branch, Marvin Graves Bldg., Room 1.103, Galveston, TX 77555 USA. Hotel rate is \$37 per night for a single room, \$47 for a double at the Plaza Hotel, 1900 Speedway Blvd., Tucson, AZ 85719 USA. For additional information, call Frank Carrel at 1-409-772-3475, fax -1-409-772-4288, e-mail: ascap@utmb.edu.

Evolution of Morality

The 44th annual Star Island Conference will be sponsored by the Institute on Religion in an Age of Science, 26 July to 2 August 1997. Co-chairs are Michael Ruse, editor of *Biology and Philosophy*, and Karl Peters, editor of *Zygon: Journal of Religion and Science*. This year's theme is "The Evolution of Morality." For information on this New Hampshire conference, contact Bonnie Falla, Registrar, 810 North 9th Street, Allentown, PA 18102 USA, etl. 1-610-432-8711.

CURRENT LITERATURE

March 1997

Compiled by Robert M. Adams

Interested in possibly reviewing one of the books below or some other suitable book? Please contact the appropriate book review editor (see Editorial Staff box). Submit items for Current Literature to Bob Adams (see Editorial Staff box). Please be sure that the item has not yet appeared in this space.

Abitol, M. M. (1996). *Birth and Human Evolution: Anatomical and obstetrical mechanisms in primates*. Greenwood Publishing, 88 Post Rd. West, Westport, CT 06881 USA, \$95 (hdbk.).

Allport, S. (1997). *A Natural History of Parenting: From emperor penguins to reluctant ewes, a naturalist looks at how parenting differs in the animal world and ours*. Harmony, 201 E. 50th St., New York, NY 10022 USA, \$23 (hdbk.).

Baker, R. (1996). *Sperm Wars: The science of sex*. Basic Books, 1900 E. Lake Ave., Glenview, IL 60025 USA, \$25 (hdbk.), \$14 (ppr.).

Blanchard, K. (1995). *The Anthropology of Sport: An introduction*. Greenwood Publishing, 88 Post Rd. West, Westport, CT 06881 USA.

Bowler, P. J. (1996). *Charles Darwin: The man and his influence* (reissue). Cambridge University Press, Edinburgh Building, Cambridge CB2 2UR, U.K., £35 (hdbk.), £12.95 (ppr.).

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Cappella, J.N., (1997). Behavioral and judged coordination in adult informal social interactions: Vocal and kinesic indicators. *Journal of Personality and Social Psychology*, 72, 119-131. (Univ. Penn., Annenberg, Sch. Commun., 3620 Walnut St., Philadelphia, PA,

19104, USA).

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