

# Human Ethology Bulletin

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## The Fourteenth Bi-Annual Conference of the International Society for Human Ethology

19-23 August 1998  
Simon Fraser University

### *Preliminary Program*

WEDNESDAY, 19 August 1998

(Arrival)

Reception

THURSDAY, 20 August 1998

8:30 - 10:00 AM **Simon Baron-Cohen:**  
"Evolution, Autism, and Theories of Mind"  
Chair: Doreen Kimura

10:15 - 11:45 **Ethology of Mind**  
Chair: Peter Verbeek

"Attachment, Autism and Holding in Mind" -  
John Richer

"If I Look at Your Face I Have to Die! A  
Common Denominator of Early Childhood  
Autism" - Ricarda Müssig

"A Multimedia Approach to Addressing  
Mindblindness" - Karen Steiner Bell

"Is False Belief Understanding in Young  
Children Associated with Peer Interaction?" -  
Kristin Andrews & Peter Verbeek

12:30 - 2:15 PM **Applying Ethology**  
Chair: Karl Grammer

"Evolutionary Aesthetics: Landscape  
Preferences in Children and Young Adults"-  
Karl Grammer & Erich Synchron

"Signaling Benevolence: An Evolutionary  
Perspective on the Encoding and Decoding of  
Altruism" - William M. Brown, Borisu  
Palameta & Chris Moore

"Sibling and Offspring Recognition By  
Olfaction" - James Gall & Glenn Weisfeld

"An Evolutionary Based Study of Facial  
Expression Recognition" - Lisa Goos & Irwin  
Silverman

"The Cooperative (Contagious) Nature of  
Yawning: An Evolutionary Explanation" -  
Dennis K. McBride

2:15 - 3:35 **Gender and Society**  
Chair: Carol Weisfeld

"Sex Differences in Children's Investment in  
Peers" - Rosanne Roy

"Male/Female Differences in Leadership  
Appraisal" - James Schubert

"Behavioral Sex Differences in Happily  
Married Couples" - Carol Weisfeld & Margaret  
Stack

"Girls And Women in the Company of Boys and  
Men" (FILM) - Carol Weisfeld & Margaret  
Stack

3:35 - 5:35 **The Contribution of Karl von Frisch,  
Konrad Lorenz and Niko Tinbergen to Human  
Ethology**  
Chair: Agnaldo Garcia

"Lorenz's Contribution to Ethology and the Present Situation of the Field" - Irenäus Eibl-Eibesfeldt

"Tinbergen's Legacy to Human Ethology" - William Charlesworth

"Human Ethology: The Next Generation" - Karl Grammer

"Karl von Frisch on Bees and Human Beings" - Agnaldo Garcia

Salmon Barbecue

FRIDAY, 21 August 1998

8:30 - 10:00 AM **Doreen Kimura:** "Biological Contributions to Sex Differences in Human Cognition"  
Chair: Irwin Silverman

10:15 - 11:45 **Proximate Biological Mechanisms**  
Chair: Nancy Segal

"Olfactory Perception and Pregnancy Sickness: A Test of the Pregnancy Sickness as Maternal Adaptation Theory" - Farhad Dastur & Richard Brown

"Human Pheromones and the Neuroendocrinology of Behavior" - James Kohl

"Hormonal Changes Underlying Sexual Responses in Women and Men" - Astrid Jutte, Bernhard Ferro & Karl Grammer

"A Developmental Study of Sexual Dimorphisms in Spatial Behavior" - Jean Choi & Irwin Silverman

12:30 - 2:00 PM **Ethology and Health**  
Chair: Russell Gardner

"An Evolutionary Psychological Explanation of Colic" - Dennis McBride & Sasha Phillips

"Puerperal Blues and Depression - Evolutionary and Crosscultural Research on the Postpartum Period" - Wulf Schiefelhövel

"The Ethology and Sociobiology of Suicide" - J. R. Feierman & L. A. Feierman

"An Evolutionary Look at Dual Diagnosis" - J. Anderson Thompson, Jr.

2:00 - 3:30 **Finding One's Place**  
Chair: Glenn Weisfeld

"Possible Functions of Puberty Rites" - Glenn Weisfeld

"The Sexual Dimensions of Power" - N. Patrick Peritore

"Children's Reactions to Status Differentials" - Angela Waite, Rosanne Roy & Joyce F. Benenson

"Ontogenetic Priming for Social Hierarchical Functioning" - Jack Palmer, William McCown & Tricia Thronburgh

3:45 - 5:15 **Charles Crawford:** "The Biology of the Beauty Myth"  
Chair: TBA

Banquet

SATURDAY, 22 August 1998

8:30 - 10:00 AM **David Haig:** "Genetic Imprinting, Conflicts of Interest, and Development"  
Chair: Linda Mealey

10:15 - 11:45 **Fluctuating Body Asymmetry**  
Chair: TBA

"Human Body Odour, Symmetry, and Attractiveness" - Anja Rikowski & Karl Grammer

"Developmental Instability of the Digits and Predictors of Spermatogenesis, Testosterone, LH and Fecundity in Men and Women" - John T. Manning

"TBA" - Randy Thornhill & Steve Gangestad

12:30 - 2:00 PM **Socialization**  
Chair: TBA

"Relationships of Early Separation and Intimacy to Sibling Incest" - Irene Bevc & Irwin Silverman

"Implicit Early Socialization of Physical Activity Schemas" - Allen Gomes

"Could Salient Features in Mother-Infant Interactions Underlie Some Adult Affiliative Behaviors?" - Ellen Dissanayake

"Explorations of East-West Differences: The Western Canon since Goethe and the Turning Eastward" - Daniel G. Freedman

2:00 - 3:40 **Ontogenetic Constraints on the Evolution of Communication**  
Chair: Peter J. LaFrenière

"Card Sharks and Poker Faces: Development and Evolution of Deception" - Peter J. LaFrenière

"Infrastructural Properties of Vocal Communication in Ontogeny and Evolution" - D. Kimbrough Oller

"Implications of the Evolution of Laughter for Language Acquisition" - Nobuo Masataka

"Ethology in Etymology, Proverbs and Popular Phrases" - Carmen Strungaru & Wulf Schiefenhovel

Discussant: William Charlesworth

3:55 - 5:55 **New Thoughts in Evolution**  
Chair: Charles Crawford

"Does Mitochondrial DNA Proximate the Ultimate Motherhood" - Ada Lampert

"Behavioral Genomics Will Mediate Ultimate and Proximate Causations" - Russell Gardner, Jr.

"Teaching How to Answer 'Why' Questions about Behavior" - Tom Shellberg

"'Holistic Darwinism' and Humankind" - Peter A. Comins

"An Ethological Interpretation of Line Fighting" - Glenn E. King

"Unity in Variety" - Nancy E. Aiken

## POSTERS

"An Evolutionary Perspective on Pain: The Physiological Response as Proximal" - Todd Napier Levins

"Functional Significance of Swaddling as Traditional Infant-Care Practiced by Native South Americans" - Nobuo Masataka

"Sexual Selection and the Evolution of Language" - Shan Guisinger & David Schuldberg

"Invulnerables and Aspergers Anchor Interpersonal Continua" - Shan Guisinger

"Xenophobia and Mental Illness" - Cohrs U. Brune

"Availability Bias for Words Representing Human Threat: Further Evidence for Domain Specificity of Cognition" - Linda Mealey & Sean Moroney

"The Role of Fluctuating Asymmetry on Judgements of Physical Attractiveness: A Monozygotic Co-Twin Comparison" - Linda Mealey, R. Bridgstock & G. C. Townsend

## Congress Notes

Attendees are asked to bring copies of their reprints for distribution, and book authors are requested to bring copies of their works as well.

Speakers: Please remember that many non-native English speakers will be attending, so try to speak slowly, clearly, and simply.

The **conference website** has been connected to the ISHE website: <http://evolution.humb.univie.ac.at/ishe.html> or may be reached directly at <http://www.sfu.ca/cstudies/conf/humanwww/>. The URL has also been posted on the HBES website. The **conference organizer** is our President, Charles Crawford: Dept. of Psychology, Simon Fraser University, Burnaby, B.C., Canada V5A 1S6, tel. 1-604-291-3660, fax 1-604-291-3427, e-mail [crawford@sfu.ca](mailto:crawford@sfu.ca).

## SOCIETY NEWS

### Call for Nominations

It is time (actually, overtime) to elect two officers: Membership Chair and Secretary. The duties of the **Membership Chair** include promoting membership in ISHE and preparing the triennial Membership Directory. Nancy Segal has decided not to run for re-election, so the field is wide open. Some officers have suggested that we have two membership chairs, Old World and New World. This matter will be discussed at the British Columbia meeting in August. The duties of the **Secretary** include reporting on Society meetings and soliciting nominees for offices. The officers make most of the decisions for the Society, under our by-laws. Since Karl Grammer's position, Secretary, is due for election, nominations will be handled by Linda Mealey, our President-Elect. Therefore, please submit nominations to her (see Officers' Box). Self-

nominations are encouraged. There is no limit on the number of terms that a person may serve. Each term of office is normally for three years. ISHE has followed a tradition of maintaining geographic and disciplinary diversity among the officers, but there is no set rule about this.

### Membership Renewals

It is time to renew your membership for 1998 if you have not already done so. Membership is by calendar year, so dues are to be paid by the first of the year. **If the date on your mailing label is earlier than 1998, it is time to renew your membership.** For economic reasons, renewal notices are not usually sent. Those who do not renew their memberships will be removed from the membership list. **Please report errors, changes of address, etc. to the Treasurer. Be sure to inform her if you move; the U.S. Post Office no longer returns undelivered Bulletins with the recipient's new address.** Current dues and directions for payment are given on the last page. Please allow four weeks for recording changes of address or payment of dues.

<b>Financial Report for ISHE</b>			
<b>31 May 1997 - 30 April 1998</b>			
Submitted by Barbara Fuller, Treasurer			
<b>CREDITS</b>		<b>DEBITS</b>	
<b>Item</b>	<b>amount</b>	<b>Item</b>	<b>amount</b>
Balance May 31, 1997		Credit card processing fees and bank and wire charges	
	\$19,149		\$298
Member dues from June 1, 1997 through April 30, 1998	\$ 5,442	Editor's expenses for one year	\$1111
Interest earned	\$ 915	Treasurer's expenses for two years: 1997 & 1998	\$ 900
Sale of mailing list	\$ 92	ISHE Bulletins printing and mailing for 18 months	\$5548
Total credits	\$25,598	Total debits	7857
Minus total debits	- 7857		
<b>CURRENT BALANCE</b>	<b>\$17,741</b>		

\* amounts rounded to nearest whole dollar

Bank statement balance as of April 31, 1998 = \$17,741

## ISHE Website Directory

Astrid Juette has volunteered to compile an e-mail membership list for our internet homepage based in Vienna: <http://evolution.humb.univie.ac.at/ishe.html>. This will allow access to the e-mail address of any ISHE member.

### Bulletin Submissions and Duplication

Anything that might be of interest to ISHE members is welcome: Society matters; articles; replies to articles; suggestions; announcements of meetings, journals or professional societies; etc. These sorts of submission should be sent to the editor. Book review inquiries should go to the appropriate book review editor. Submission should be in English, on paper and, if possible, also on diskette (MS Word 5.0 preferred). Shorter reviews are desirable (less than 1000 words). Please include complete references for all publications cited. For book reviews, please include publisher's mailing address and the price of hardback and paperback editions. There usually is not time to consult with reviewers about editorial changes, but most of these are minor.

Submissions are usually reviewed only by the editorial staff. However, some submissions are rejected. Political censorship is avoided, so as to foster free and creative exchange of (even outrageous) ideas among scholars. The fact that material appears in the *Bulletin* never implies the truth of those ideas, ISHE's endorsement of them, or support for any policy implications that may be inferred from them.

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In order to be listed, please provide the information requested on the form below and e-mail it to: [astrid.juette@bigfoot.com](mailto:astrid.juette@bigfoot.com). The list will be continuously updated, so there is no deadline for submission. Please provide this information directly to Astrid even if your e-mail address appears correctly in the green Membership Directory, since otherwise there is no way to know if your address is still correct. If your e-mail is not working, you may send the information by post to: Astrid Juette, Ludwig Boltzmann Institute for Urban Ethology, Inst. f. Human Biology, Althanstrasse 14, A-1090 Vienna, Austria.

Information Requested (please print clearly):

Name.....

Link to your personal homepage: <http://.....>

e-mail address.....

Institute.....

Link to your institute/university: <http://.....>

### *Reply to Two Previous Essays*

## On Nomenclature and the Spread of Inaccurate Memes

By Randolph Nesse  
University of Michigan  
[nesse@umich.edu](mailto:nesse@umich.edu)

I find most debates over nomenclature tedious, but repeatedly expressed opinions tend to be those of people who have strong feelings on a matter, and thus can lead to the spread of inaccurate memes if they are not challenged. Bill Charlesworth's article in the March ISHE newsletter [*Human Ethology Bulletin*, 13 (1), 1998] is a case in point. He objects to the decision made by the Human Behavior and

Evolution Society two years ago to change its journal title from *Ethology and Sociobiology* to *Evolution and Human Behavior*, and he offers a spirited defense of the continuing utility of both "ethology" and "sociobiology." He has made these points before, in a letter to HBES members.

No one objects to the expression of such opinions. I would, however, like to object to what seems to me to be a misrepresentation of events that could foster unnecessary conflict and misunderstanding. As the chair of the HBES publications committee, it was my unenviable responsibility to carry out an assignment from the HBES council to devise a process for the Society to consider a possible change in the name of our journal. With the help of other committee members, I gathered information from the past editors, the future editors, the publisher, the HBES council and HBES publications committee. In the newsletter and the on-line discussion, all members were invited to submit suggestions for titles and their reasons. Without going into the exhausting details, we finally came, via a democratic process, to a conclusion. A mailed ballot led to an overwhelming agreement to change the name of the journal to *Evolution and Human Behavior*.

Now to my objections to Charlesworth's article. Based on what seems to me to be scanty evidence, he confidently attributes specific motives to those who made this decision. He says, "From what I gather, one reason for the journal's name change is that the label 'sociobiology' is perceived by many as a professional kiss of death--at least for a young U.S. investigator applying for a job" (p. 3).

There were a few people for whom such political factors were one reason. When they expressed their opinions in an e-mail discussion group, this led to a great digital flow of rebuttals, discussion about free speech, political trends, and all manner of somewhat related topics. Relying on this as a main source of information could easily lead to the conclusion that political factors were crucial to the decision, especially if more attention was paid to the number of megabytes spent squabbling about related issues, rather than the

number of people actually advocating a name change for political reasons. However, as the person who talked with more people for more hours than anyone else about their reasons for wanting to change the name, I can report that the great majority told me that their main reason for wanting a new title was that the old title gave little indication to a potential reader that the journal was about human behavior. I can also report that only a handful of people advocated a change primarily because of political factors. The vast majority told me that they believed *Ethology and Sociobiology* simply was not an accurate description for the content of the journal.

With regard to the word "sociobiology," Charlesworth is correct that some people (although only a handful) objected because it is so strongly associated with Ed Wilson. Even some who are great admirers of Wilson's work (and I include myself among them) did not want the title of a journal to suggest that it might be limited to publications by the followers of one great man. Others did not want to use a word that might imply that the content was limited to social, as contrasted to individual, behavior. Hardly anyone wanted to avoid the term because of objections to Wilson or his work.

Charlesworth then goes on to say, "But many respectable scientists, department chairs, journal editors, and concerned students apparently feel (as the HBES poll revealed) the both terms have become politically incorrect, dangerous, or whatever" (p.3). The HBES poll revealed no such thing. The poll revealed only that the members of HBES overwhelmingly believed their journal would be better described by a new title. The publisher and editors agreed.

Now, I don't want to accuse Bill of intentional misrepresentation because we may have talked to different people and they may have told us different things. People knew I thought political factors should be irrelevant to our decision, and perhaps some therefore did not tell me their real reasons. But, after scores of hours of discussion with dozens of people about this issue at all phases of a process that lasted several years, I can report that the

motive voiced by most participants was to find a title that accurately described the content of the journal in a way that would be as inclusive as possible and that would make librarians inclined to purchase it. I can well imagine that Bill is accurately reporting his impression based on what he heard from the people he talked to. I do believe, however, that he has been incautious about the possibility of sampling and response bias in a situation where they are expected, and where inaccurate reports on the inferred motives of others can have serious negative consequences.

One unfortunate outcome of publicity given to his position may be that Ed Wilson seems to feel, based on his [auto]biography and a speech at an HBES meeting, that the change was a personal criticism or an act of political cowardice. To my mind, this is nonsense, but I can see how people could get this misimpression. I and others have tried to set the record straight; this is another attempt. Another outcome has been to make us look foolish in the press. Still another is to create useless dissention in our shared fragile enterprise.

\* \* \*

Now for the double twist on all this. In a previous ISHE newsletter [*Human Ethology Bulletin*, 12 (2), 1997], Bruce Charlton went on at some length about "Why Don't We Drop the Darwinian Nametag?" with particular complaints about the phrase "Darwinian medicine," mainly on the grounds that it was "obsessed with authority, lineage, and the cult of a great man" (p. 8). If only he knew the hours George Williams and I spent arguing about this! Most of us with medical training have had it up to here with eponymous designations for diseases whose purpose seems to be to make Board Exams harder. So, at first I argued for "evolutionary medicine." But George soon set me straight. Evolution means, as we all know, change, change of any incremental sort, and this meaning was recognized long before Darwin. His huge contribution was to demonstrate its mechanism, natural selection. The term "Darwinian," when it is not being usurped by politicians, means explaining the characteristics of biological organisms based on the principle of natural selection.

There is another reason, recently emphasized by George, why the term "evolution" is not accurate: Natural selection is more likely, in the case of adaptive traits, to be responsible for stasis than change. As he so nicely puts it, natural selection explains why the camel keeps its hump. See his new book, *The Pony Fish's Glow*, if you have not already.

As for Euclidian geometry and Newtonian mechanics, phrases also mentioned by Charlton, these terms are used unselfconsciously in the rest of science not to glorify individual scientists but to efficiently describe bodies of knowledge. "Darwinian," applied to biology or medicine, serves the same useful function. "Evolution" is less specific and less useful. If you do a keyword search for "evolution" and "medicine" in the Medline database, you will find titles like "Polio--the evolution of an epidemic" and "The evolution of surgery in the 19th century" and even "The evolving changes in managed care reimbursement patterns." As a keyword for finding work on evolution and medicine, "evolution" doesn't cut it. "Darwinian" isn't perfect, but it is better.

It would be wonderful if we could agree on some keyword that would flag work on evolution and adaptation. Personally, I like the word "adaptationist" and I use it despite concerted attempts to taint it. But it seems unlikely that we will be able to reclaim this word in the immediate future. Perhaps "selectionist" offers an alternative.

So these are some reasons why "Darwinian medicine" is more accurate and useful than "evolutionary medicine." Nonetheless, two forthcoming edited books from Oxford University Press use the term "evolutionary medicine." I suppose I could speculate on the reasons the editors chose this term. But I won't. The memes will settle themselves out, for better or worse, and we all need to get on with our work.

## 1997 ESS Papers

The European Sociobiological Society met at the University of Ghent, Belgium, 7-9 July 1997. The conference theme was "The Sociobiology of Ingroup/Outgroup Behavior, Part II." The papers:

**Otto M. J. Adang**, Systematic observations of violent interactions between human groups. RIOV Research Centre for Public Order & Safety, P. O. Box 26, 4630 AA Hoogerheide, The Netherlands, e-mail oadang@piov.lsop.nl.

**Robin Allott**, Religion and science: sex and society forms and processes of cohesion. 5 Fitzgerald Park, BN25 1AX, East Sussex, U.K., e-mail RMAllott@percep.demon.co.uk.

**Lucio Ferreira Alves**, Marxism, Darwinism and nationalism. Universidade Federal do Rio de Janeiro, Cidade Universitaria, Aaixa Postal 68006 CEP 21944-970 Rio de Janeiro, Brazil, e-mail lfalves@gbl-rio.rio.nutecnet.com.br.

**D. Avramov & R. Cliquet**, The flaring-up of the ingroup-outgroup syndrome in Eastern Europe: the example of former Yugoslavia. Univ. of Ghent, Dept. of Population Studies & Social Science Research Methods, Sint-Pietersnieuwstraat 49, 9000 Ghent, Belgium, e-mail psy@innet.be.

**Tamas Bereczkei**, Kinship network and fertility in a Hungarian ethnic group, the Gypsies. Institute of Behavioral Sciences, University Medical School of Pécs, Szigeti u. 12, H-7624 Pécs, Hungary, e-mail btamas@cortex.pote.hu.

**Marina L. Butovskaya & Vincent Falger**, The rise of ethnocentrism in the former Soviet Union as a reflection of ingroup/outgroup kin selection paradigm. Institute of Ethnology & Anthropology, Leninski Prospekt, 32a, korp. "B," Moscow 117334, Russia, e-mail butovskaya@glas.apc.org.

**Elizabeth Cashdan**, Diversity and boundedness of ethnic groups: ecological perspectives. Univ. of Utah, Dept. of Anthropology, Salt Lake

City, UT 84112, USA, e-mail elizabeth.cashdan@anthro.utah.edu.

**John Constable**, Cultural objects and display: verse form. Kyoto Univ., Faculty of Integrated Human Studies, Sakyo-ku Kyoto, 606-01, Japan, e-mail john@ic.h.kyoto-u.ac.jp.

**Charles Elworthy**, Creating predictive models of ingroup-outgroup behaviour through evolutionary psychology and situational determinism. European Academy, Scholss Wartin, D-16306 Wartin, Germany, e-mail Elworthy@T-Online.de.

**Harald A. Euler, Sabine Hoier & Barbara Weitzel**, Grandparent-parent relations, or why do daughters-in-law often lose out? Univ. of Kassel, Dept. of Psychology, Hollaend. Str. 36-38, D-34109 Kassel, Germany, e-mail euler@psychologie.uni-kassel.de.

**Oleg V. Khrennikov, Vladimir S. Krilov & Ayder R. Kadyrov**, the problems of interethnic conflicts. Crimean Medical Institute, Dept. of Psychiatry, 333000 Simferopol, Ukraine.

**William Kitchin**, Ingroup and outgroup language: a preliminary analysis of U.S. Presidents. Loyola College, Dept. of Political Science, Baltimore, MD 21210, USA, e-mail kitchin@loyola.edu.

**Chris Knight**, Speech/ritual co-evolution: an ingroup/outgroup model. Univ. of East London, Dept. of Sociology, Longbridge Rd., Dagenham, Essex RM8 2AS, U. K., e-mail C.Knight@uel.ac.uk.

**Alexander Kozintsev**, Russian ethnic humor: xenophobia or consolidation? Museum of Anthropology & Ethnography, St. Petersburg 199034, Russia, e-mail sasha@kozintsev.spb.su.

**Kevin MacDonald**, The intellectual construction of immigration policy: an evolutionary perspective. California State Univ., Dept. of Psychology, Long Beach, CA 90840-0901 USA, e-mail kmacd@csulb.edu.

**Camilla Power**, Deceptive sexual signalling as a preadaptation to ritual: a mechanism for establishing ingroup/outgroup boundaries. Univ. College London, Dept. of Anthropology,



Gower St., London WC1E 6BT, U.K., e-mail  
ucsaccp@ucl.ac.uk.

J. P. Rushton, Genetic similarity theory and the genetic basis of ethnocentrism. Dept. of Psychology, Univ. of Western Ontario, London, Ontario N6A 5C2, Canada, e-mail Rushton@sscl.uwo.ca.

David Smillie, The evolution of sociocultural systems. Duke Univ., Dept. of Zoology, Box 90325, Durham, NC 27708, USA, e-mail dsmil@acpub.duke.edu.

Arthur M. Squires, Evolutionary hypotheses ("disciplined speculation"): origins of male ingroup behaviors. Virginia Polytechnic Institute & State Univ., P. O. Box 10098, Blacksburg, VA 24062, USA, e-mail verasqu@vtmi.cc.vt.edu.

Dorothy Tennov, Science and policy. R. R. 9, Box 251, Millsboro, DE 19966, USA, e-mail tennov@home.dmv.com.

Kristiaan Thienpont, The socio-psychological underpinnings of intergroup behaviour: bringing in the context of hominid evolution. Univ. of Ghent, Dept. of Population Studies & Social Science Research Methods, Sint-Pietersnieuwstraat 49-9000 Ghent, Belgium, e-mail Kristiaan.Thienpont@rug.ac.be.

Jan Tullberg, Separatism or unity? A model for solving ethnic conflicts. Stockholm School of Economics, P. O. Box 6501, 113 83 Stockholm, Sweden, e-mail j.and.b.tullberg@stockholm.mail.telia.com.

Johan M. G. van der Dennen, Of badges, bonds and boundaries: ingroup/outgroup differentiation and ethnocentrism revisited. Univ. of Groningen, Center for Peace & Conflict Studies, Oude Kijk in 't Jatstraat 5, 9712 EA, Groningen, The Netherlands, e-mail j.m.g.van.der.dennen@rechten.rug.nl.

Marc Verhaegen, Human Evolution: savannah vs. aquatic theory. Mechelbaan 338-2580 Putte, Belgium.

Dan Vining, On racism. Univ. of Pennsylvania, 3718 Locust Walk, Philadelphia, PA 19104 USA, e-mail vining@pop.upenn.edu.

## An Interview of Sarah Blaffer Hrdy

By Frans Roes

Lauriergracht 127-II  
1016 RK Amsterdam  
The Netherlands  
tel. (3120) 6259399  
froes@dds.nl

Few photographs drew so much attention in biology as one showing two female langur monkeys chasing a male. Sarah Blaffer Hrdy, who took the picture, is a primatologist. Two of her books are *The Langurs of Abu* (1977) and *The Woman that Never Evolved* (1981). Sarah Hrdy is currently working on another book: *Mother Nature*. The following interview took place in Tucson, Arizona, USA June 5, 1997.

### Why did you study the langurs?

In the sixties I was very much interested in the effects of population density on human behaviour. John Calhoun had just done his famous study on Norway rats. He allowed them to multiply, and he let them breed and breed until they were all crowded together, and then he began to see what he called 'social pathology'.

I couldn't study this for humans, but I heard about these langurs, so I went to India to study infanticide in langurs because I thought it was being produced by crowding. The first thing I found was that males in langur troops are usually quite tolerant of infants, instead of being infanticidal. Young infants come up and jump on the male as if he was a trampoline. They hang on his tail and swing on his tail when he is up in a tree. When he is mating with their mother, the older juveniles will bat at him and harass him. And this is very annoying behavior. I think you have to call such a male tolerant. He might get fed up and threaten them, but he doesn't kill them.

### What kind of monkeys are langurs?

Langurs are colobines. The subfamily Colobinae is very widespread; you find them in Africa as

the black and white monkeys, all over India as Hanuman langurs, and in Southeast Asia as leaf-eating monkeys and those wonderful droopy-nosed proboscis monkeys in Borneo. By far the most common of the colobines, the most terrestrial, the easiest to study and best studied, is the Hanuman Langur. They are named for Hanuman, the monkey-God, the loyal servant to king Rama in the Hindu epic, the Ramayana.

Hanuman langurs tend to live in one-male groups, though you can sometimes find them in multi-male groups. So you have one male with six females to twelve or more females, up to 59 females in the largest one-male group I know of. These are breeding units. Females stay in the same home range for their entire lives, among their mothers, grandmothers, sisters, daughters and aunts.

Territories are passed down from mother to daughter. These home ranges slightly overlap, and when females meet at the borders of their ranges you have territorial aggression, involving both the male travelling with the group and the females. The females are actually much more persistent in territory defence, and often quite aggressive. Females in intergroup encounters are extremely active, and it makes sense. They are defending the resources that are available to them to survive and reproduce. There is a photograph of two langur groups fighting one another, and the male is just sitting there as comfortable as can be while the females are skirmishing at the border, lined up at one another and slapping.

Males either leave the group at maturity, or more often what happens is that they are driven out by another male. Immediately after a father and his sons are ousted from a troop, they travel together for a while, but they usually join with other males. There is extremely little aggression among these males; you think you could not hold all these males together, that they would not get along. In fact, you cannot even detect in an all-male band a dominance hierarchy. So males are travelling together in all-male bands. There may be anywhere between two to sixteen males; these are fairly flexible groupings. These males roam around and cover tremendous

distances, and they are very hard to study for this reason.

In the morning when the resident male in a group of females wakes up, he goes to the top of a tree, and he gives these long-distance shouts: whoop, whoop! It is like "I'm here, don't even bother trying today!" And the all-male band whoops back. And you think: why are they letting him know that they are trespassing in his territory? Why are they giving him this information about where they are? Basically I think they are saying, "Do it again; let's hear a little more." I think it is information gathering, and it is in the resident male's interest to answer, because if he doesn't, they are more likely to come.

But they may come anyway. They are constantly probing and exploring. But the resident male is trying to chase them away. The resident male tries to keep them off. There is a reproductive advantage to being a resident male; he has a disproportionate probability of siring the offspring, and so there is intense competition to be that male. And the males in the all-male band will attempt to take over and to oust the resident male. When the males that come in actually attack the resident male, there is a big chance that he will fight back, and one of them will be injured. In such a climate, injury is a serious problem; it could lead to gangrene and an animal could die. So neither wants to inflict damage on the other that would then provoke a response of serious damage. They are held in check, rather like nuclear powers, by the threat of retaliation.

But if there is a skewing of the odds, such that the resident male is weak or there are so many males in the all-male band, the male band is sometimes able to drive out the resident male, and all the young males may follow later, or they all go at once. It is a very chaotic time. Everyone is fighting with everyone else, in the sense that the females are still perhaps exhibiting antagonistic behaviour toward these outsiders, and the males themselves are still fighting. And this is one of the reasons why people said that infanticide is just an accident, the infants are killed incidentally during this chaos. It is very hard to collect information when this is going on.

When these new males come into a troop, previously there were no dominance differences perceptible among them. After they take over a troop—and now you might have five adult males resident in the troop—competitive behaviour to construct a dominance hierarchy begins to emerge, and one of these males in general emerges as the new alpha male. Occasionally two or three males will stay in for a time, and you will have briefly a multi-male group.

**Are they brothers?**

We don't know. We never obtained DNA, and it is unlikely that we will get it for India because these are sacred monkeys and it is difficult for Americans to collect these data. We had a number of political problems working in India. What may be the case, though, is that if the ousted male is still very powerful, it might make sense to tolerate some competition. You'll share some copulations in order to have the alliance of males.

**You took a picture that became quite famous: two female langurs chasing a male who has just captured an infant. What actually happened?**

It is a very gripping image of a male who has an infant in his mouth. The image is blurred; you can just see the tail of the infant, its body flying off in space. And two females are charging the male to try to get that infant back. The fascinating part is that neither female is the infant's mother. They are both older females in the group. The one in the forefront, a female called Sol, was almost certainly a close relative of the infant, perhaps a grandmother, perhaps a great aunt. We estimate that females in langur groups on average are related some place on the order of 1/16 of their genes by common descent, like first or second cousins. This was a female who did not reproduce during all the years of the study where she was observed. Whether one wants to call her menopausal or not, she was for all practical purposes post-reproductive. I am assuming that both are close relatives and therefore intervening and trying to save the infant.

**What about the mother of the infant?**

The mother of the infant herself was a young

female at the peak of her reproductive career—much more cautious, sitting on the sidelines, letting the older females protect the infant. In fact there are other cases where a mother, for example, allows her infant to drop from a tree. The male will rush to it, and it is not the mother who goes down to save it; it is again these older females.

**Why these older females instead of the mother?**

What I believe is going on is that females with a lower reproductive value have a different cost-benefit ratio for taking chances on behalf of relatives. This can be seen in other contexts. So, for example, McCarthy and Bugos, when they collected data on maternal infanticide among the Ayoreo Indians in South America, found that very young mothers and mothers with high reproductive value were much more likely to give up and not go on with an infant under bad circumstances. Whereas an older mother will go ahead, no matter what, because she is getting near the end of her reproductive opportunities.

So, in other words, females are making very different decisions based on where they are in their life history, and this is an important point when we come at a problem with preconceptions about what is maternal behaviour. Biologically, what is a good mother? Well, the truth is there is no one good mother, no one good solution. As post-Enlightenment Western human beings, we have very set ideas about what is a good mother. But in biological terms, maternal investment is very facultative. One of the things that I would like to do with my work is to expand our understanding of concepts like maternal behaviour and female nature to encompass a much wider range of what females biologically are about.

**Why is the male trying to kill the infant?**

As one male emerges as the new troop leader, he will start to stalk females with young infants, and disproportionately the targets of his assaults are six months and under. Before a male is ousted, a langur male is extraordinarily tolerant of offspring, many of whom are likely to be his own. It is only when males enter the

troop from outside the breeding system, and encounter females that they have not mated with, that they exhibit this aggressive behaviour. The male's behaviour is very goal-directed. He will stalk an infant for hours or days. He looks in different ways, in every direction except at the mother-infant pair, pretending not to be interested while moving closer. Then suddenly he grabs towards them.

The behavior of the females also tells you something. Females without infants don't particularly avoid this male. Females with young infants are moving away. They are very, very skittish of him. The fights that happen after the male has taken over, between females and the male, tend to be when the male has attacked someone's infant. And other females then come back and fight. In other words, it does not appear to be a male attacking females in an effort to hurt the females. Rather, the females attack the male because he is trying to hurt an infant. When you are watching this, no one who has ever seen this happen has ever come away questioning the idea that this is goal-directed behaviour on the part of the male. It is not some incidental by-product of social disturbance or chaos.

The sexual selection hypothesis that I proposed is based on Darwin's theory of sexual selection. You have competition between males for access to females, and in some cases female choice. Males are in competition with other males to breed with females. If a male has taken over a group of females, he has only a brief window of opportunity, 27 months on average. If a female weans the offspring she already has, if she continues to lactate, she will not ovulate again for perhaps a year. By that time the male could be driven out by another male. So he is basically trying to subvert female choice. He is cancelling female choice. By eliminating the offspring of the last male who sired her infant, the new male creates a circumstance whereby the female is going to be under pressure to ovulate again soon.

**Why don't females simply refuse to mate with an infanticidal male?**

Because females are in competition with other females for representation in the next

generation. If a female refuses to mate with an infanticidal male and therefore waits, say, two years before she breeds again, she leaves fewer progeny in the next generation than will her sister who does go ahead and breed. The second point is that if she is in a population where male take-overs are happening, her son, once grown up, will be at a disadvantage in competition with other males if he is not infanticidal. So females are under tremendous pressure to make the best of this appalling situation by breeding with an infanticidal male.

**How do females defend their young?**

The most obvious thing, of course, is that they try to stay clear of the male. Mothers threatened by infanticide are much more restrictive of their infants. A female with an infant may leave the group and travel apart. If she has an infant approaching the age of weaning, she may try to leave that troop with the male and the all-male band.

Furthermore, females can sort of preemptively reduce the probability of infanticide if they have bred with a male before he comes into the troop. It is the mother who is the cue to whether or not he attacks or tolerates the infant. I have seen females kidnap infants from other groups and bring them back to their own, but so long as the female is familiar, the male does not attack these little strangers. So I hypothesized that females solicit outside males in addition to her resident male as a defensive strategy in case one of these comes in. In this way she manipulates information available to males about paternity.

**Why was your interpretation of infanticide in langurs so controversial?**

Part of the controversy that has come up is that people have assumed that I am talking about a gene for infanticide--a male either has it or he doesn't. I have never said anything about it; I don't know. The same male is infanticidal in one context and not in another. And I know that some strains of mice are more infanticidal than others, so we have good reason to think that this is something that is inherited. But even in an infanticidal strain of

mice, such a male may only engage in infanticidal behaviour 70% of the time when he has an opportunity to do it; so clearly context matters. The fact that the mechanism is still unknown was one problem.

But the other, larger issue was a reluctance to accept that something so obviously detrimental to the species or the group could be adaptive for individual males. When my langur research was published, some people drew a parallel with human child abuse and the stepfather phenomenon. It is structurally similar, but the rationale for it I now believe to be quite different. A human male who kills his stepchildren is usually not enhancing his reproductive access to that female.

I agree completely with Martin Daly and Margo Wilson that the issue here is best explained in terms of competition for resources. The stepfather who assaults an infant belonging to his mate's previous partner is responding to the demands that an infant is making on him, on the mother, and on the household resources.

Infants, of course, have been selected to be basically insatiable in their demands, and you have a situation that is more nearly comparable to another kind of langur behavior. Langurs are infant-sharers: females take and carry other females' infants. The only time you see infants abused by female langurs is when a female takes an infant and is subsequently no longer motivated to carry it. A female who has taken an infant that is not her own and grows tired of holding it, and no other female comes up to take it from her--she tries to push it off herself. For an infant not being held by a female is tantamount to death. If it is on its own, it is subject to predation. Infants are selected to cling like glue to whoever has them. So you have an allomother--a female other than the mother--pushing the baby off, the infant trying to stay on, and this is when you see langur females sitting on the baby, pushing it against a rock, this kind of abuse.

This is much more relevant to what is going on in human child abuse than is infanticide by adult male langurs. It is a stepfather who is not motivated to invest as much in the infant as the infant is asking for. He is pushing it off,

and that is when he is losing his temper and inflicts mortal damage on the infant.

**Why is it the females that stay in their natal group, instead of the males?**

Most Old World monkeys, as among most mammals, if they are social, are matrilineal. The advantages to a female of having her relatives around are very great. It is usually males that disperse because females, probably to avoid inbreeding, exhibit a preference for mating with novel males. So a male who remains in his natal troop is at a breeding disadvantage with other males, because he is often not the preferred male. I believe that he moves to a new troop to advance his breeding options....

There are, however, circumstances when males can't afford to move, as when they are threatened by male alliances in competing groups. When fathers, brothers and sons remain together, as among chimpanzees, it can force females to move instead. But it is very costly to a female to lose the social support of her relatives. If her mother's feeding grounds are particularly good, an advantaged daughter may refuse to move, as Jane Goodall has reported for Flo's matriline at Gombe. Once patrilocality emerges, though, females are at a real disadvantage. Control of feeding grounds and resources falls increasingly under patrilineal control.

The key phrase here is being able to control the resources. If parents in a patrilocal situation are aware that their sons are going to have allies around while their daughters will not, they risk territory and resources passed on to daughters being diverted to her mate or husband's patriline. So parents [exhibit] bias toward sons in intergenerational transmission, because their sons are better able to protect and maintain it. I am obviously thinking here of human examples.

**Why are a few primate species patrilocal?**

Yes, why do males sometimes stay? Why are there male patrilocal groups? Well, let's think about where we find them. We find them in species like chimpanzees. Chimp males have a special problem because they are in competition

with the males in competing communities, males who will come in and, if they have the chance, wipe them out and take their females. How can they protect the resources they have, which consist of the females...? They need alliances with other males, and the best allies are relatives. You can depend on them the best; a relative has a different cost-benefit ratio for helping you versus competing with you than does a strange male. So males stay with their fathers and grandfathers and brothers in order to protect themselves, the females they want access to, and the offspring they produce from other males. The males won't leave because if a male goes off, he'll be killed by the males of competing communities.

So the female leaves, and as Anne Pusey suggests, she uses her sexual receptivity as a passport to enter new communities. She tries to settle there. She may lose her first infant, it may be killed by the resident males, but her later offspring will probably get to survive. Mariko Hiraiwa-Hasegawa has noted that there is a bias in terms of males attacking sons in chimpanzees. It is one of the few cases outside of humans where you have sex-biased infanticide. The sample sizes are still quite small; I think it is 12 cases of infanticide in chimps, and 9 of the 12 are males rather than females killed by males. And you have to think, why on earth is this going on? I think that the males who control the community, this brotherhood of males, have much more to lose by allowing another male's son to grow up in their community. A female, if she grows up in their community, might leave and go off and breed elsewhere, or they might have a chance to breed with her. But if a male that is not related to them grows up and joins the brotherhood, they will be sharing the resources of their community, the females in it...with this unrelated male. And this is a tremendous dent in the inclusive fitness of these males to tolerate an alien male among them.

**What is your opinion about the idea that, in some distant human past, females were dominant over males?**

I think it is feminist invention, a psychological antidote against myths of male dominance. It is an effort to invent an alternat[iv]e reality. But the archaeological or ethnographic

evidence is just not there to support it. For instance, fertility figures, they say, look as if people worshipped females, so females must have ruled. But all this proves is that somebody in those societies was fascinated by female fecundity. These statuettes tell us nothing about political power.

Hunter-gatherers are often held up as being egalitarian, but according to my reading of hunter-gathering monographs--and I read quite a few of them--they are more egalitarian than most, but, even so, males are dominant. I don't find even among hunter-gatherers a very convincing case where females are dominant or even completely equal...What tends to be overlooked in some feminist mythologies about how "we are regaining a matriarchal past" is that this is an illusion, and it is dangerous. I think we need to understand that this is an experiment, and that it is fragile. Rights that so many now take for granted no one familiar with our evolutionary history would sensibly take for granted.

## BOOK REVIEWS

### *Next of Kin: What Chimpanzees Have Taught Me about Who We Are*

By Roger Fouts (with S. T. Mills). William Morrow & Co., 1350 Avenue of the Americas, New York, NY 10019, USA, 1997, \$25 (hdbk.).

Reviewed by W. C. McGrew, Anthropology & Zoology, Miami University, Oxford, OH 45056, USA.

Who is the most famous living ape? Arguably, it is Washoe, a 33-year-old female chimpanzee (*Pan troglodytes*), who dazzled the world in the 1970's with her apparent fluency in American Sign Language. This book is the story of her life, so it is a biography. Playing Boswell to her Dr. Johnson is Roger Fouts, an experimental comparative psychologist, now a professor at Central Washington University. But he is more than an amanuensis--*Next of Kin* is his life story too, so

it is also an autobiography. But the book is more than this: With the assistance of a skillful science journalist, Stephen Mills, it is a chronicle of the rise and fall of "pongolinguistics" (or at least a strand of it) and an indictment of research on captive apes in the USA. This is a big load for any book to carry.

Washoe's story is the most straightforward. Captured in the African wilds as an infant, she ended up in the psychology laboratory of Allen and Beatrice Gardner at the University of Nevada (in Washoe County). There she was reared like a human baby and taught Ameslan, the American gestural language of the deaf. Her successes were eye-opening. The Gardners passed her on to Fouts, and she moved to Oklahoma, where she joined other chimpanzees in the laboratory of William Lemmon. It was there that signing chimpanzees garnered fame, if not fortune; along with Lucy, another home-reared chimpanzee, there seemed to be a constant flow of media attention, as well as scientific publications. Also, in Oklahoma, Washoe became a mother at 10 years of age, first by birth (but the infant died), then by adoption (her son Louis, who is with her still). Since 1980 she has lived in a group of five chimpanzees at the Chimpanzee and Human Communication Institute at Central Washington University where, as a matriarch, she continues to be active in non-invasive psycholinguistic research. There are many wonderful stories told, though much is left out too, for example, the time that she bit off the finger of a certain famous neuroscientist.

Fouts came from humble beginnings and stumbled into comparative psychology almost by default. He was the proverbial person in the right place at the right time, when the Gardners needed a graduate student to take charge. He was astute and hard-working, and once he and Washoe were linked, they never looked back. Much of this story is that of overcoming odds, as a trailblazer in a new field that challenged received wisdom.

Perhaps the frustrations of these struggles were redirected into male-male rivalry, for he is highly critical of the two authority figures in his career, Gardner and Lemmon. The latter is virtually demonized beyond recognition but is no longer alive to

defend himself. Scientific rivals are assailed as well, especially Noam Chomsky and Herbert Terrace, a reminder of how bitter was the debate in the early 1980's over the bases of human language.

Somewhere in the process, a compassionate campaigner for ape rights emerged, and the transformed Fouts is signalled by the subtitle of the book. By the end of the book, the reader may disagree with some of his views, but no one can doubt his commitment to his little band of apes, nor to the species *Pan troglodytes* in general.

*Next of Kin* is not the place to seek an even-handed, comprehensive account of the history of ape language studies, though these form the backdrop for everything. Such a book remains to be written, by a detached outsider who knows the species both in captivity and in nature. Here, other workers are given short shrift (Patterson, Rumbaugh, Savage-Rumbaugh) or omitted altogether (Premack). Sometimes this leads to skewed conclusions, such as claiming that most other researchers worked only with single apes, which fails to credit (for example) Sherman and Austin's productive partnership in Atlanta. However, Fouts and Mills together present a compelling insider's recounting, warts and all, of a genuine scientific saga of meteoric rise and just as precipitous crash, when the Columbia group of critics Bever, Petitto, Seidenberg, and Terrace weighed in and toppled the field.

Fouts is at his weakest when he writes about chimpanzees in nature. According to him, they are terrestrial, weigh up to 200 pounds, and typically live to be 50-60 years old in close-knit families in communities of up to 150 members. Females have full sexual swellings at 10 years and typically go on consortships of weeks to months, while males at 6 years challenge their mothers, yet give unconditional affection to white-tail-tufted infants. This is stuff and nonsense. More disappointing is his use of the demeaning diminutive word, "chimp," which at best has the connotation of "chump."

Fouts is at his best in his ringing defense of the fight for rights and welfare for captive apes. He argues eloquently and movingly the case for their personhood, and

the final third of the book is truly thought-provoking. Most important, Fouts practices what he preaches: He and his family have made a lifelong commitment to the well-being of Washoe and her crew. Finally, it is this rare and precious bond that carries this multifaceted book.

### *Handbook of Evolutionary Psychology: Ideas, Issues and Applications*

Edited by Charles Crawford and Dennis Krebs. Lawrence Erlbaum Associates, 10 Industrial Avenue, Mahwah, NJ 07430, 1998. \$140 (hdbk.) retail, \$75 if ordered directly from the publisher.

Reviewed by Linda Mealey, School of Psychology, University of Queensland, Brisbane, Australia 4072.

We are lucky to have Charles Crawford as President of ISHE. That Chuck is a man ahead of his time was demonstrated by the forward vision of his 1989 *American Psychologist* article with Judith Anderson and the product of his previous collaboration with Dennis Krebs (and Martin Smith), "Sociobiology and Psychology: Ideas, Issues and Applications" (1987). This "new edition" of the afore-mentioned book (similar title, same publisher) is equally forward-looking. Anyone who is interested in evolutionary theory as applied to humans and who wants to know where the "cutting edge" resides should pick up this hefty "handbook."

The organization of the book follows its title: first come "ideas" (5 chapters), then "issues" (5 chapters), then "applications" (11 chapters). The first section starts with an overview by Crawford, followed by updates on: kinship (Hudson Kern Reeve), mate choice and sexual selection (Geoffrey Miller), life history (Bobbi Low), and models of cultural evolution (Maria Janicki & Dennis Krebs). These chapters serve to introduce newcomers to the basics of evolutionary theory, but should not be ignored by long-time practitioners, as they also report the newest twists and insights in what is still a young, dynamic field.

Although chapters in this first section should not be skipped entirely, it is not necessary to read the chapters, or even the sections, in the order presented. The ease of skipping back and forth from one section to another is one of the positive attributes of a well-edited book, and the editors have done well in this regard: chapters neither repeat basic information nor depend upon what appeared elsewhere.

I went first to the middle section on "issues." There one can read about the key contemporary controversies within the field of evolutionary psychology. First, J. M. Bailey addresses the relationship between evolutionary psychology and behavior genetics. The issue is the role of human variation. To what extent is it adaptive? To what extent is it genetic? To what extent is it facultative? This is perhaps the biggest controversy in the field at the moment. See also Buss & Greiling (in press), Mealey (in press), and Segal & MacDonald (in press).

Also competing for the "biggest controversy" title is the issue of the role of reproductive success in prehistoric and modern societies, otherwise known as the "Darwinian anthropology" versus "Darwinian psychology" debate. See Symons (1987, 1989, 1992) and Turke (1990). Laura Betzig takes on this issue with a chapter entitled "Not Whether to Count Babies, but Which"---reminiscent of Crawford's (1993) article "The future of sociobiology: Counting babies or studying proximate mechanisms?"

The other three chapters of section two are all related to one or both of these two key issues. Philosopher Harmon Holcomb, III discusses the role of hypothesis testing and methodological issues in relation to evolutionary questions. Andrew Wells addresses issues surrounding the new, oft-used metaphors of psychic "modularity," "mental computation," and "cognitive architecture." And Crawford presents an updated version of his 1995 talk to the Human Behavior and Evolution Society on the role of the modern environment in creating maladaptive behavior; see also Mealey (1997).

Section three, being the largest and



most diverse, has something for everyone. Like those in section one, the chapters here provide up-to-date summaries of the literature, making them useful for newcomers and specialists alike. The chapters most closely reflecting current interests of ISHE members are, I think, Michelle Surbey's chapter on human development, Randy Thornhill's chapter on "Darwinian aesthetics," and Irwin Silverman & Krista Phillips' chapter on sex differences in spatial skills. The other chapters are, however, equally good: Dennis Krebs on the evolution of morality; David Buss on human mate selection; Martin Daly & Margo Wilson on family violence; Christopher Badcock on the relationship between Darwinian and Freudian paradigms; Douglas Kenrick, Edward Sadalla & Richard Keefe on social cognition; Neil Malamuth & Mario Heilmann on sexual aggression; Yuwa Hedrick-Wong on the evolution and development of socio-political states; and Derek Bickerton on language. The range of topics that appears in this section illustrates the extent to which all areas of psychology have been included in research using the evolutionary perspective.

There are three areas of direct interest to *Bulletin* readers that might have been included in this type of handbook but were not. I refer to the rapidly expanding literature on theory of mind and Machiavellian intelligence, our various psychological threat- and cheater-detection mechanisms, and the evolutionary status of mental "disorder" and "disease." Perhaps these topics will appear in the next "edition."

This book can play a role in advancing the evolutionary approach--but only if it gets good marketing and scholars are informed of the dramatic cost saving by ordering the book directly from the publisher (see address above). Even then, the price is a bit steep for graduate students and, as far as I know, there are no plans for a paper-bound version. I suggest that if you can't afford your own copy, you have your local university library order a few copies. It's definitely worth having around.

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## Officers of the Society

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Charles B. Crawford  
 Department of Psychology  
 Simon Fraser University  
 Burnaby, B. C. V5A 1S6 Canada  
 tel. 1-604-291-3660  
 fax 1-604-291-3427  
 e-mail: crawford@sfu.ca

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 Dept. of Psychology  
 Queensland University  
 Brisbane 4072, Australia  
 fax 61-7-3365-4466  
 e-mail: lmealey@psy.uq.edu.au.

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 fax 43-1-31-336-788; e-mail:  
<http://evolution.humb.univie.ac.at>

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 tel. 1-303-315-8929  
 fax 1-303-315-5666  
 e-mail: barbara.fuller@uchsc.edu

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 Department of Psychology  
 California State University  
 Fullerton, CA 92834 USA  
 tel. 1-714-773-2142  
 fax 1-714-449-7134  
 e-mail: nsegal@fullerton.edu.

## *Darwin's Black Box: The Biochemical Challenge to Evolution*

By **M. J. Behe**. The Free Press, 1230 Avenue of the Americas, New York, NY 10020 USA, 1996.

Reviewed by **William Charlesworth**, P. O. Box 18, Stockholm, WI 54769 USA.

Black boxes are boxes scientists can not or will not look into. One of Darwin's black boxes was the living cell. Because 19th century scientists did not have the equipment to study cell mechanics and function, Darwin had to limit his theory to more macro phenomena. Today, with help of x-ray crystallography, nuclear magnetic resonance techniques, and electron microscopy, knowledge of cell mechanisms and their functions has grown enormously. The box is now open and evolutionary scientists, like biologists in general, have to look into it.

Behe's book says, in effect, look carefully, then tell me if you still believe that cell mechanisms and their functions evolved as a result of chance as claimed by current evolutionary theory. In Behe's opinion, molecular biology has clearly demonstrated that each cell is an "irreducible complex" system, i.e., "a single system composed of several well-matched interacting parts that contribute to the basic function, wherein the removal of any one of the parts causes the system to effectively cease functioning" (p. 39).

Given this, Behe asks how reasonable is it to account for cell machines and their myriad of functions by evoking the traditional Darwinian forces of mutation and natural selection. His answer is that it is very unreasonable: the gradual trial and error process required would take a preposterous amount of time to just to get started. Darwin's theory simply cannot account for the molecular origin and structure of life. Consequently, the theory has to be reconsidered.

So, what would account for the complexity? Behe's answer is "intelligent design." He defines design as "the purposeful

arrangement of parts," quickly noting that making an inference of design does "not require we have a candidate for the role of designer" (p. 196). An intelligent agent or agents will do, but what "intelligence" is, precisely, is not discussed.

Much to the surprise and chagrin of many, Paley's ghost has risen once again, informing us that the watch found on the beach points to a watchmaker, not to the lawful and random natural processes that created the beach sand and pebbles. As can be imagined, most neo-Darwinists find this claim archaic, naive and unparsimonious--in short, very unscientific.

In Chapter 1 the black box is opened, revealing an astonishingly complex world of biochemical entities and processes--astonishing in its infinitesimal complexity, completely unknown before the 1950's when the synthetic theory of evolution reached its high point. As an example of such complexity, Behe gives an account of how vision most likely evolved in terms of the eye's molecular mechanics and functions. In his mind, this account is vastly superior to the superficial comparative accounts of Darwinists. Precise knowledge of the mechanics of how things work as they do is always superior to descriptions and analogies.

Chapter 2 ("Nuts and Bolts") goes into the inadequacies of Darwinian theory in accounting for the evolution of beetle bombs. Citing Dawkins' oversimplified account, Behe points out that no one really knows how the bombs evolved. There are just too many--"tens of thousands of different types of molecules"--involved to speculate on "the mutations that might have produced them" (p. 41).

Chapters 3 and 4 go into the complex nature and function of proteins, the complicated construction and function of cilia which cells use to swim, and blood coagulation with its breathtaking cascade of biochemicals. "Faced with such complexity beneath even simple phenomena, Darwinian theory falls silent" (p. 97).

In Chapters 5 and 6, Behe covers measles, the vesicular transport systems involved in body defenses, and the complexities

of the immune system--none of which, he asserts, can be accounted for by the gradualistic argument of Darwinians. Such complexity "dooms all Darwinian explanations to frustration" (p. 139).

Chapter 7 on "Road Kill" hits again the supposedly unrealistic assumption of gradual stepwise evolution. One mutation at a time is a myth passed on by compliant textbook writers. "Like a groundhog trying to cross a thousand-lane highway, there is no absolute barrier to putting together some biochemical systems gradually. But the opportunities to go wrong are overwhelming" (p. 142). Scepticism and agnosticism are more in order than blithe assumptions.

Chapters 8 through 10 go into such disparate topics as the lack of reference to evolution in biochemistry textbooks (a blatant sign that evolution theory is actually not very useful in biochemical research), the nature of "intelligent design," and arguments for and against a theory of it. Both of the latter are disappointingly incomplete.

Chapter 11 is the big one. It is entitled "Science, Philosophy, Religion" and goes into all three as superficially as one can in 22 pages. In concluding the section on the vicissitudes of scientific theories, Behe notes that "Now it's the turn of the fundamental science of life, modern biochemistry, to disturb [current orthodoxy] . . . other centuries have had their shocks and there is no reason to suppose that we should escape them. We will endure the opening of the black box" (pp. 252f).

An appendix on the chemistry of life closes the book.

As recent presses releases, magazine articles, and conferences amply show, Behe's book has created something of sensation. It is looked at by religious creationists and other non-believers in evolutionary theory as a lethal blow to Darwin. On the other side, most neo-Darwinians look at it as another dangerous, unscientific attempt to bring God into the creation-of-life picture.

Behe points out he is not a creationist but believes strongly that the data from

molecular biology point to something bigger and more complex than chance mutation and natural selection. He believes that the latter two forces play a role in evolution, but a more restricted role than orthodox Darwinians believe.

As I see it, the positions held by both Behe and Dawkins have belief-laden, metaphysical stances to them. By themselves, such stances need not undermine scientific research--unless they close off inquiry into legitimate areas of scientific research. The history of successful science is replete with believers and non-believers in invisible forces.

Actually, Dawkins seems to have the tougher position to hold because his allegiance to Darwin compels him to believe that chance and physical laws are sufficient and necessary to explain evolution. However, claiming that an hypothesis is totally sufficient to account for complex phenomena has a bad history in science. Reality wrecks such hypotheses annually. On the other hand, postulating that an invisible designer is an adequate explanation for the origin of life has no credible history in science because no acceptable empirical evidence for it has yet been discovered.

Hopefully, readers inclined toward Dawkins will take time to read Behe. To ignore Behe is to suggest that there is an unbridgeable discontinuity between our rapidly growing knowledge of microbiological processes and our present knowledge of that wide and wonderful range of macrophenomena upon which evolutionary theory is built.

Overall, this book is interesting but its structure is awkward. It is written for the layperson but not as well as it should be. In a disjointed way it covers everything from molecular behavior to metaphysical issues. A subsequent edition ideally will involve major revisions.

In my opinion, molecular research poses no mortal threat to Darwin's achievement. It just puts the achievement into a new light. There are levels and levels of reality and it is conceivable that a convincing theory coming out of current molecular biology will, itself, some

day have to be revised by further developments and another level like small particle physics.

### *The Evolution of Love*

**By Ada Lampert.** Praeger, 88 Post Road West, Westport, Connecticut 06881, USA, 1997, \$49.95 (hdbk.).

**Reviewed by Dorothy Tennov,** RD #2, Box 251, Millsboro, DE 19966, USA.

The story of evolution is a wonderful story, however incomplete and speculative it may still be. In some ways Ada Lampert, Senior Lecturer in the Department of Behavioral Sciences at the Rupin Institute in Israel, tells it well. Her exposition gives the reader a 'feel' for 'the slow progression' of species through extinct ancestors over unimaginably long periods of time.

In keeping with the title (this is not the first time 'evolution of love' has been used in a book title), this survey ranges across the span from the Big Bang to the present. Along the way, she recounts some of the accumulated lore that has sprung up:

Sexual reproduction won out over asexual forms due to its causing greater genetic variance in offspring. Sex strategies appropriate to the microscopic sperm and giant egg differentiate themselves early in life. With a longer period of extra-uterine helplessness in mammals, survival depended on parental care. Infanticide by males is virtually the rule in mammalian species and is not unknown in *Homo sapiens*. Care of offspring by fathers (who have so little investment in the young) is very new and still rare. Love is new; fish don't have it. Devices that help to keep fathers around include the hiding of oestrus and year round sexual activity (so unlike other species in which the time of fertility is loudly declared). Maternal love was the first love; it 'cradles' all other loves. Falling in love is a reconstruction of infancy. Children who grow up together don't desire each other sexually as adults. The sons of cold and unaffectionate mothers fall in love with cold and unaffectionate women because those are the traits that have been imprinted on their brains.

'Limbic' love (dazzling, sweeping, excited) does not augur well for the future happiness of a marriage couple. Through an unconscious phenotypic signaling mechanism, people who are genetically similar are attracted to each other, an important aspect of mate selection. The mother-infant separation findings of the Harlows demonstrate the need for maternal contact with offspring. The aim of anxiety is to guard the infant, and the goal of depression is to avoid separation.

Much of what Lampert presents is accepted by the majority of evolutionary scientists, some of it is still at the 'proto-science' stage, and some is stated ambiguously in terms metaphorical at best. Although in so short a book (116 pages) full treatments can hardly be expected, the discussion is too often scant and uneven. Moreover, the research on which it is based has been questioned, amended, or even overturned by subsequent theories and findings.

For example, Lampert does not mention that published criticism of genetic-similarity theories of mate selection has led many to reject this notion. Lampert's recommended end-of-chapter readings that refer to mate choice by genetic similarity predate 1989, the year in which an article and commentaries on the theory appeared in the journal *Behavioral and Brain Sciences*.

When its proximate mechanism is unknown and evolutionary writers have nothing more to go on than a conjecture about how a trait might be related to reproductive success, they often retreat to nonquantitative evasions. For example, they say that A 'reflects' B, or B is 'strongly rooted' in A, or the existence of A 'affords a feeling of B,' or A 'could be explained' by B. Lampert uses many such intimations of causality, some of which are closer to the poetic than to scientific discourse. For example, she remarks that repetition of rewarded actions "reflects" our passion for habits.

Lampert frequently gives little indication as to which aspects of her discussion a source refers to. And an absence of references all too often leaves the reader clueless, which certainly limits the usefulness of the book. In

addition, the editor missed a number of grammatical errors and awkward wordings.

The many theories and speculations that circulate concerning human evolutionary development have become familiar to readers of this journal. Of established findings, there isn't much here that is new. It is hard to imagine what readers the publisher and author had in mind. The limitations of the scientific references, including the acceptance of outdated and at times idiosyncratic thinking on important and controversial topics, make it unusable as a reference work or as a textbook. Lampert's *The Evolution of Love* might be of greatest value to the nonscientist or to scientists in nonbiological fields as a means of describing how evolutionists think about human behavior. Perhaps for such a readership, evoking the image of human evolution does not require accuracy of all details.

Despite her proclaimed allegiance to materialism, her style is metaphorical rather than operational. It can be criticized for vagueness, but it can also be enjoyed for the images evoked. I quote two sentences, the first and last in the book: *The Evolution of Love* begins with "The theory of evolution has perhaps become the strongest and most central tool of thought for those struggling to explain the world," and it ends, "Evolution has made every person into a lamp, so that as long as we go on loving, we will not sit in the dark."

To summarize, in *The Evolution of Love*, Ada Lampert discusses topics current in evolutionary psychology and anthropology, human ethology, and sociobiology that she conceives of as at least loosely related to the development of human love. Unevenness in coverage is probably inevitable in a brief monograph, but failure to provide references keyed to specific assertions made in the text is a more serious shortcoming. Furthermore, the jury is still out on many of the issues discussed, though the naive reader would never know this simply by reading Lampert's account.

A book similar in scope and subject but brought up-to-date in its details, one that put research findings and ideas from relevant literature in a single volume, could be a useful resource. Perhaps the focus could also be

broader:ed to include the entire range of human affiliations from sexual love to cooperation and altruism.

**Author's Note:** I wish to express appreciation to Don Brown, Glenn Cochran, Peter Frost, Linda Mealey, Frank Miele, Elaine Morgan, Don Symons, Karen Tracy, and Joao Sousa who kindly and helpfully responded to my questions regarding some aspects of this review.

## ANNOUNCEMENTS

### *ASCAP Award Winner*

The winner of the 1998 Aaron T. Beck Award, presented by the Across-Species Comparison and Psychiatry society, is Bruce J. Ellis, Dept. of Psychology and Human Development, Vanderbilt University, Nashville, Tennessee, USA. His winning essay is entitled "Psychosocial antecedents of pubertal maturation in girls: Parental psychopathology, stepfather presence, and family and marital stress."

### *Cape Cod Course*

The 19th Cape Cod Institute will feature a course entitled "Healing the Moral Animal: Lessons from Evolution." It will be held 20-24 July 1998 in Eastham, Massachusetts, on the eastern shore of Cape Cod. Sponsored by Albert Einstein College of Medicine, the course will comprise these programs:

John K. Pearce on evolutionary theory and our mental health care, natural selection and human capacities, our presumed original environments, physical and psychological adaptations for survival, mismatch of our evolved traits with themselves and with our culture, evolutionary theory and treatment.

Robert Wright on moral impulses as biological adaptations, deception and self-deception, biology and social status, the Darwinian analysis of moral issues and public policy, can what's natural be pathological?, inner city violence, child abuse, serotonin and aggression, is knowing thy (Darwinian) self therapeutic?, biology and Buddhism.

Russell Gardner on mood disorders, an expression of evolved psychological features, anxiety, depression, mania, obsessive-compulsive disorder, and sociophysiology and evolution: considerations for treatment.

James Brody on psychological adaptations, familial action patterns, the behavioral family history, mode jumping in children and adults, uses of the psychological adaptations and familial (behavioral genetics) models.

James Brody on differences in male and female reproductive interests, strategies for deceit and cooperation (manipulation and enablers), children and altruism/competition, interventions, issues with managed health care especially for emotional distress, conflicts with psychological adaptations.

One may register on-line at [www.cape.org/1998/](http://www.cape.org/1998/). Fee is \$435 for the first program (\$285 for graduate students and medical students), \$250 for each additional one. Programs last all morning and up to early afternoon. Makes checks payable to "Montefiore-Cape Cod" and mail to Cape Cod Institute, Albert Einstein College of Medicine, 1308 Belfer Bldg., Bronx, NY 10461, USA. Mastercard or VISA charges can be faxed to 1-718-430-8782. Complete catalogues from Dr. M. H. Peters at the address above.

### *Human Nature Website*

*Human Nature* is a quarterly journal that publishes articles on biosocial factors underlying human behavior. Information on all previous articles and on forthcoming ones, as well as instructions to authors and subscribers, can be found at <http://www.unm.edu/~humnat/>.

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