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REPLY TO A BOOK REVIEW

Morgan's Aquatic Ape Theory Is Fringe Science

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

During the last few decades, anthropology and biology have been attacked with considerable public effect by proponents of fringe science and pseudoscience whose topics range from ancient astronauts to creationism (see Feder, 1996 for a brief partial survey). Thus it is especially disappointing to me, as an anthropologist and primatologist, to see the "aquatic ape theory" of Elaine Morgan (1993b, 1995) enthusiastically endorsed by a legitimate behavioral scientist in the *Human Ethology Bulletin* (Tennov 1996).

The history of pseudoscience teaches us that we should be skeptical when a person who is "not a formally trained scientist" flatly contradicts highly trained experts who have done original research (Does anyone remember Robert Ardrey?). Alarm bells should ring when that person accuses scientists of avoiding "awkward facts" that contradict their views. This is the typical rhetoric of the pseudoscientist seeking the support (and usually the money) of a gullible public against the conspiratorial scientific establishment (e.g. Ardrey, 1961; von Daniken, 1971). Actually it is the pseudoscientist who habitually ignores evidence, as well as distorting the views of the mainstream. These characteristics are readily apparent in Morgan's work as conveyed by Tennov's review.

Savanna Hominids and "Savanna Primates"

To Morgan the scientific establishment is represented by savannah theorists who ignorethe "awkward fact" that "other savannah primates," such as baboons and vervets, "moved from forest to grasslands" and yet did not become bipedal and hairless. The truly awkward fact here is that baboons and vervets are not literally "savanna primates" because they did not "move" from one place to another. Rather, some populations of each species expanded their savanna activities to varying degrees. Baboons and vervets characteristically live in "savanna-mosaic" habitats and their home ranges always contain trees (Richard 1985). Only hominids completely lost touch with the trees, a drastic step that we should expect to result in some unique adaptations, possibly including hair reduction and bipedalism.

Hair Reduction

Though supposedly "ignored," reduction of the hominid pelage has been under consideration for a long time. Schultz (1931) approached the matter through a survey of diverse primate taxa but was unable to reach a conclusion. Reanalysis by Schwartz and Rosenblum (1981) yielded a strong negative correlation between relative hair density and total body surface area. They concluded that a sparser coat is necessary for release of metabolic heat in large primates. They inferred that the pelage of our (relatively large) hominid ancestors must have been substantially reduced when they still lived in a somewhat forested environment. Besides being a contribution to the discussion of hair reduction, this hypothesis reminds us that the intepretation of human evolution has options other than savanna and water (*contra* Morgan,

1993a).

Schwartz and Rosenblum see the initial adaptation for heat control as setting the stage for a further reduction of hair in response to savanna conditions. Their hypothesis provides a specific explanation for the difference between hominids and "savanna" monkeys. Dense hair is a "principal mammalian defense" against radiant heat, a defense that has been maintained by vervets and baboons. Early hominids, larger and more sparsely coated, adapted to savanna heat in a different way: they evolved thermal sweating, which was facilitated by further loss of hair. At this point Schwartz and Rosenblum link their views with earlier theorists who had reached similar conclusions about thermal adaptations to savanna without considering antecedent conditions (Montagna, 1972; Newman, 1970).

More recent writers have continued to elaborate the thermal explanation for hair reduction in the savanna (e.g., Folk and Semken, 1991; Wheeler, 1994). Others have offered sophisticated scientific criticisms (e.g. Queiroz, 1996) rather than simplistic comparisons between early hominids and "savanna primates".

Bipedalism

Allegedly, terrestrial tool use cannot account for bipedalism because "hominids walked on two legs long before they used tools." This statement can only be based on the relatively late appearance of *stone* tools. Early hominids might very well have used organic tools (of wood, bone, etc.) that left no trace in the archeological record. That this actually occurred is suggested by the diverse tool using and manufacturing activities of contemporary chimpanzees (McGrew, 1992; Schick & Toth, 1993).

"Concerning the need for free hands for carrying ... chimps and gorillas manage quite well to hold food in one arm while walking with three." The fact is that apes "manage quite well" with a few objects over short distances. Anyone who has watched "banana madness" in a Jane Goodall film knows that carrying a substantial quantity of food for any significant distance is not so easy for apes. If selection pressures on early hominids favored regular carrying of heavy and/or awkward burdens over long distances, the ape mode would probably not have been adequate.

"Unless instantly efficient, developing bipedalists would have been vulnerable to predators." Recent evidence indicates that the earliest hominids lived in open forest or woodland habitats (Hunt, 1994) where they had ready access to trees as a refuge from predators. That they used the trees is demonstrated by fossils that combine bipedalism with arboreal traits such as long arms and curved hands (Hunt, 1994; Johanson, 1996). Predation need not have been a major problem in the terrestrial origins of bipedalism.

Given the probability that early hominids used organic tools (see above), they may have used weapons such as stout branches to protect themselves as they ranged farther into the savannas. Transport of weapons, like other carrying, may have favored further evolution of bipedalism. It is unlikely that effectively large and heavy weapons could have been conveyed any significant distance by the ape mode of carrying. Perhaps even more important was the *use* of these weapons. Clubbing, thrusting, and throwing are all more effective when performed in a stable bipedal stance (Dart, 1953; Hewes, 1961; King, n.d.). As with tools in general, chimpanzee behavior supports the plausibility of clubbing and stabbing weapons in the repertoire of the earliest hominids (Kortlandt, 1972; King, pers. obs. at Holloman AFB).

We should be aware that other terrestrial explanations for bipedalism exist. Rodman and McHenry (1980) argued that bipedal walking was an energy efficient way for early hominids to cover long distances between dispersed food sources in savannas. This important hypothesis has been criticized by some mainstream scientists (Steudel, 1994) and carried forward by others (Isbell and Young 1996). The bioenergetic interpretation can be linked to thermal adaptations such as sweating and hair reduction (Wheeler, 1994). The aquatic hypothesis is not the only one that "combines and makes sense of disparate and previously unconnected facts."

Finally, Hunt (1994) reminds us once again that there are more possibilities than savanna and water. He suggests that bipedalism originated as a postural adaptation to the collection of small arboreal fruits in open forest and woodland habitats.

The Hemorrhoid Fallacy

Morgan discusses the "scars of human evolution," which is neither a new idea nor even a new phrase (see Krogman, 1951). Few if any of these "scars" provide specific support for the aquatic theory. The odd position of the human larynx, for example, *might* have started with an aquatic preadaptation to language, but the more parsimonious view is that it evolved on land in direct response to the growing importance of language in human evolution.

Other "scars" are simply byproducts of bipedal anatomy and posture, regardless of how bipedalism evolved. These include birthing difficulties, lower back pain, varicose veins, and hemorrhoids. Morgan's attempt to use these in support of the aquatic hypothesis indicates a poor grasp of evolutionary theory:

> If the initial stages of bipedalism were carried out in the low-gravity medium of water, these effects would have been less in evidence in the early stages, enabling the transformation to be a smooth and gradual process (Morgan 1993a:199).

As Robert Trivers emphasizes, reproductive success is central to evolution by natural selection (Roes, 1996). "Smooth and gradual" processes are not germane unless they facilitate reproductive success. Irritations are not germane unless they interfere with reproductive success. Hemorrhoids are not likely to interfere with reproductive activity and so are not likely to have been a factor in the location or conditions of hominid evolution.

Conclusion

Properly handled, the "Aquatic Ape Theory" might be one of the weaker entries among several viable theories of human origins. However, Morgan's presentation is no better than fringe science because it depends extensively on selection of evidence, loose reasoning and misrepresentation of opposing views. It covers errors with a glib style and makes an emotional appeal based on opposition to an allegedly inept scientific establishment. Encouragement of such discourse by real scientists seems self-defeating.

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naked animal: A speculative review. *Human Biology, 42,* 12-27.

Queiroz do Amaral, L. (1996). Loss of body hair, bipedality and thermoregulation. Comments on recent papers in the Journal of Human Evolution. *Journal of Human Evolution, 30,* 357-366.

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SOCIETY NEWS

Impressions of the Vienna Congress

By Susan Stewart, 29897 Red Cedar, Flat Rock, MI 48134 USA

I became interested in human ethology and sociobiology in 1991 when I took an evolution and behavior course at Henry Ford Community College, Michigan, with Tom Shellberg. I have since earned a bachelor's degree in Psychology as a Natural Science at the University of Michigan. My knowledge and interest in ethology continue to expand as I prepare to enter a master's degree program in biology.

Prior to the 1996 ISHE conference in Vienna, I had attended one ISHE conference (Toronto in 1994) and five Human Behavior and Evolution Society conferences, all of which were intellelctually stimulating. The Vienna conference maintained this quality but I also found it to be more exciting than the others. The warm atmosphere induced by the evening social activities complemented the interesting talks heard during the day.

Vienna is a very large, old city with beautifully detailed buildings, palaces, and lush green city parks. The mass transit system was overwhelming at first, but once I became familiar with it, it was very useful.

The welcoming reception on Monday evening was held at the University, which is located near the center of the city. The reception gave participants a chance to

socialize with past acquaintances, eat wonderful sandwiches, drink cocktails, and meet new people. The highlight of the evening was hearing the life history of Konrad Lorenz, which was included in the opening address given by I. Eibl-Eibesfeldt.

The Tuesday evening reception at the City Hall offered the enjoyment of viewing a splendid building, socializing, and hearing an interesting speech. The mayor was unable to deliver the speech as planned; however, the deputy mayor stood in most capably. What struck me was how knowledgeable European officials (in Vienna anyway) are on the subject of ethology and how they incorporate it into their politics. Throughout the conference, I realized how far behind the U.S. is with regard to ethology and general knowledge of evolution. What puzzles me about the U.S. is that you can turn on the television to the Discovery Channel or open a popular magazine such as *Time* and see or read about topics on evolution, natural selection, and sociobiology. However, a college student majoring in biology may never be required to study and understand the fundamentals of evolution, let alone read about sociobiology or ethology. Hopefully, societies such as ISHE and HBES will help change this situation.

The exhibition of orangutan paintings and a commentary by the artist's trainer added an entertaining twist on Wednesday evening. This was followed by a keynote address by Robin Baker on the topic of copulation, masturbation, and infidelity in humans.

No activities were planned for Thursday evening, which was nice because it gave people a chance to see some of the city without feeling guilty about missing events. The Friday dinner at a Heurige, a traditional wine tavern, was an excellent gathering that ended the week with delicious Viennese food. Outdoor terraces and adjoining restaurants seemed common in Vienna; however, the Heurige dinner was in a private garden with beautiful flowers and plants on every side. The outdoor setting added a special touch to the evening.

The entire conference was very well planned and organized. Everyone working at the conference was friendly and eager to help. I thought allowing the poster authors a brief opportunity to introduce their work was a great idea. Hard work goes into making posters and sometimes they do not receive the recognition they deserve. I am looking forward to attending the 1998 ISHE conference in Vancouver, and I hope the organizers are as creative at planning social activities as the Vienna ones were.

Young Investigator Awards

By William Charlesworth

At the ISHE Congress in Vienna last August, the first Young Investigator Awards were presented. The Society's original intention was to make the award to a single investigator. This intention still remains. However, the judges in Vienna discovered that all the paper presentations were very good; hence the task of selecting a single paper was virtually impossible. The judges therefore decided to rank all papers and select the top three. Fortunately, the top three, as John Richer aptly noted, "reflected the diversity of effort that characterizes human ethology as a scientific discipline." The winners were:

Irwin Geertz, Department of Biological Psychiatry, Academic Hospital Groningen, The Netherlands: "Nonverbal interpersonal attunement predicts course of depression."

Guido Kempter, Gerhard-Mercator-Universität Duisburg, Lab. Interaktions-forschung, Duisberg, Germany: "Modelling natural human movement."

Barbara Niedner, Forschungsstelle für Humanethologie in der Max-Planck-Gesellschaft, Andechs, Germany: "Female courtship behavior in singles' bar: Woman the sexual gatherer."

The awardees will receive (1) Society membership for one year, (2) 50% off the registration fee for the 1998 ISHE meeting, and (3) the costs of a book to be selected by the recipient (providing it is not the *Encyclopedia Britannica* or *Der Neue Brockhaus*).. Congratulations to the awardees and thanks to all who entered the competition.

Vienna Congress Program

Workshops

W. Charlesworth, C. Crawford, J. Richer & W. Schleidt, organizers. Epistemology. See Charlesworth, infra, for address.

V. Filova, M. Fieder & K. Grammer, organizers. Digital image analysis and the structure of human behaviour. IBL-Sistemi, Zelezna cesta 18, 61000 Ljubljana, Slovenia.

M. S. Magnusson, organizer. Behavioural real-time symmetry, the T-pattern family, and THEME search. See Jonsson, infra for address.

Papers and Posters*

G. Attili & P. Vermigli. Fatherhood certainty, paternal investment and children's communicative competence. Universita dell'Aquila, Dipartimento di Culture Comparate, Paklazzo Camponeschi, L'Aquila, Italy.

K. Atzwanger, Katrin Schäfer, Christa Sütterin, & Kirsten Kruck. Environmental aesthetics in urban spaces. Ludwig-Boltzmann-Institute for Urban Ethology, Althanstr. 14, A-1090 Vienna, Austria.

K. Atzwanger & Alain Schmitt. Walking speed and depression: Are slow pedestrians sad? See supra.

R. R. Baker. Copulation, masturbation and infidelity: State of the art (KEYNOTE). School of Biological Sciences, University of Manchester, M13 9PT, U. K.

J. Beaudichon & M. S. Magnusson. Real-time patterns in children's interactive problem solving. Laboratoire de Psychologie, Université de Paris V, France.

M. Bechinie & K. Grammer. The dark side of love: About breaking up romantic relationships. See Atzwanger, supra for address.

J. Bensel. Early crying--developmental constant or civilizatory artifact? Forshungsgruppe Verhaltensbiologie des Menschen, Albertstr. 21a, D-79104 Freiburg, Germany.

B. H. Bichakjian. the compexity criterion in linguistics. University of Nijmegen, Department of French, P. O. Box 9103, Nijmegen, The Netherlands.

D. Lenti Boero. The evolutionary psychology of political communication. Corso di Laurea in Psicologia, Facolta di Magistero, Via Saffi 15, 61029 Urbino, Italy.

T. Bouchard. Twin studies and human behavior (PLENARY). Dept. of Psychology, University of Minnesota, Minneapolis, MN 55455 USA.

N. Bouhuys, E. Geerts, & P.-P. Mersch. Reduced perception of facial expressions predicts persistence of depression. Dept. of Psychiatry, Academic Hospital Groningen, P. O. Box 30.001, 9700 RB Groningen, The Netherlands.

G. Brand & J..-L. Millot. Behavioural modification in depressed children: Head and trunk movements before and after treatment during clinical interviews. Laboratoire de Psychophysiologie, U. F. R. des Sciences et Techniques, Route de Gray, 25030 Besançon, France.

M. Brüne. Is the acute neuroleptic-induced akathisia a displacement activity? Zentrum für Psychiatrie und Psychotherapie, Universitätsklinik, Alexandrinenstr. 1-3, D-44791 Bochum, Germany.

M. Bujatti-Narbeschuber. *Homo*-specific symbol-syntactic paigniotic-kolymbetic ethology. Museum of Natural History, Dept. of Anthropology, Burgring 7, 1014 Vienna, Austria.

M. L. Butovskaya & A. G. Kozintsev. Gender differences in social strategies of Russian urban primary school children. Institute of Ethnology & Anthropology, Leninsky prosp. 32-A, Moscow 117334, Russia.

S. Carter. The influence of hormones in behavior (PLENARY). Univ. of Maryland, College Park, MD, USA.

C. Casagrande, M. S. Magnusson & H. Montagner. Organization of dyadic interactions in infants. Laboratoire de Psychophysiologie, Université de Franche-Comté, route de Gray, 25030 Besançon, France.

W. Charlesworth. Cooperation and competition among kindergarteners. Institute of Child Development, 51 E. River Rd., University of Minnesota, Minneapolis, MN 55455 USA.

C. Crawford & S. Walters. The FCST: A measure of female-female competition stress. Dept. of Psychology, Simon Fraser University, Burnaby, B. C. V5A 1S6, Canada.

J. Czerlinski & D. G. Goldstein. Attractiveness: Some cues weigh infinitely more than others. Center for Adaptive Behavior and Cognition, Max Planck Institute for Psychological Research, Leopoldstr. 24, 80802 Munich, Germany.

G. Dimitrov. Evolution, brains, and arts. Kliment Ohridsky University of Sofia, 1000, 15 Ruski Bd., Sofia, Bulgaria.

R. Dunbar. Brains, groups and the evolution of language (PLENARY). Department of Psychology, University of Liverpool, P. O. Box 147, Liverpool L69 3BX, U. K.

I. Eibl-Eibesfeldt. Vienna's contributions to ethololgy (OPENING ADDRESS). Forschungsstelle für Humanethologie in der Max-Planck-Gesellschaft, von-der-Tann-Str. 3-5, D-82346 Andechs, Germany.

C. Elworthy. The Milgram experiments and evolutionary psychology. Free University of Berlin and the European Academy, Schloss Wartin, D-16306 Wartin, Germany.

G. Fenk-Oczlon & A. Fenk. Co-evolution of cognitive functions and natural language. Institute of Linguistics and Computerlinguistics, University of Klagenfurt, Universitätsstrasse 67, A-9020 Klagenfurt, Austria.

M. Fieder, K. Grammer & G. Ronzai. Averageness and symmetry: The assessment of female beauty. See Atzwanger, supra for address.

Bettina Fischmann, K. Grammer & J. Dittami. Hormones and female self-presentation. See Atzwanger, supra for address.

C. Franz & I. Weiche. Competition and social bonding among female gorillas *(Gorilla gorilla)* and bonobos *(Pan paniscus)*. Karl-Franzens-Universität, Institute für Zoologie II, Universitätsplatz 2, A-8010 Graz, Austria.

S. Frey. Non-semantic approach to non-verbal behavior: Cartographic methods (PLENARY). Universität Duisburg, Germany.

A. Fürlinger. Hunting or following each other: Origins of mimetic skills? Isbarygasse 13, A-1140 Vienna, Austria.

E. Geertz & N. Bouhuys. Nonverbal interpersonal attunement predicts course of depression. See Bouhys, supra.

D. Wilson Gilbert & C. C. Weisfeld. A sociobiological analysis of conflict in blended families. 5445 El Camino, Columbia, MD 21044 USA.

D. G. Goldstein, G. Gigerenzer & G. F. Miller. The adaptiveness of recognition. See Czerlinski, supra.

K. Grammer & M. Fieder. A neural network approach for the classification of body movements (FINAL ADDRESS). See Atzwanger, supra.

W. Hale, J. Jansen & N. Bouhuys. Behavioural social support predicts depression outcome. See Bouhuys, supra.

E. G. Hammerstein. "Heimat"--attachments and return to the native place: On the behavioral biology of migrants. Kurfürstenstr. 97, D-12105 Berlin, Germany.

G. Haug-Schnabel. Routh-and-tumble play as a contact strategy. See Bensel, supra.

M. Heilmann. When does selection favor stupidity, self-deception, biases? Dept. of Psychology, University of California, Los Angeles, CA 90095 USA.

A. Hejj. Dating like on the African savannah: An evolutionary psychological contribution to human mating behavior. Institute of Psychology, University of Munich, Leopoldstr. 13, D-80802 Munich, Germany.

K. Jaffe & A. Briceño. Human axillary odors and sexual selection: Theory and data. Depto. Biologia de Organismos, Universidad Simón Bolivar, Apartado 89000, Caracas 1080A, Venezuela.

G. Jonsson. Self-esteem, friendship, and verbal and nonverbal interaction. Human Behavioral Laboratory, University of Iceland, Masholar 6, 111 Reykjavik, Iceland.

A. Jütte. Cognitive and physiological responses of men to female pheromones. See Atzwanger, supra.

J. Kamaryt. New approaches to human aggression. Institute of Philosophy, Academy of Science of the Czech Republic, Jilská 1, 110 00 Praha 1, Czech Republic.

G. Kempter. Modelling natural human movement. Gerhard-Mercator-Universität Duisburg, Lab. Interaktionsforschung, Lotharstr. 65, D-47057 Duisburg, Germany.

Z. Klein. The semantic gestures in the Czech population. Psychiatric Center, 181 03 Prague 8, Czech Republic.

A. G. Kozintsev & M. L. Butovskaya. Quasi-aggression in apes and the origins of humor. Museum of Anthropology and Ethnography, St. Petersburg 199034, Russia.

K. B. Kruck. Behavioral patterns in stranger's talk. See Eibl-Eibesfeldt.

C. M. Lay, N. L. Segal & D. L. Christian. Ethological analysis of social interaction in twin children. California State University, Dept. of Psychology, Fullerton, CA 92834 USA.

A. Letzer. On the biology of reassurance. See Bensel, supra.

K. Lind & K. Wermke. Ontogenetic development of pre-speech vocalizations. Department of Human Ethology and Chronobiology, Institute of Anthropology, Humboldt University (Charite), Berlin, Germany.

T. Ljungberg, A. J. Lindqvist Forsberg & K. Westlund. Conflict resolution in preschool children. Div. of Ethology, Dept. of Zoology, University of Stockholm, S-06 91, Stockholm, Sweden.

B. Löhr & Renate Siegmund. Development of sleep-wake and feeding rhythm in German infants. See Lind, supra.

G. F. Miller. Culture as courtship: Darwinian demographics of public cultural displays. See Czerlinski, supra.

R. Müssig. Symmetry, mate selection, good Gestalt and the frontal animal scheme. Neustadterstr. 7, D-71687 Karlsruhe, Germany.

E. Nagy & P. Molnar. Origins of emotions. Institute of Behavior Science, Semmelweis Medical University, Nagyvarad sq. 1089, Budapest, Hungary.

B. Niedner. Female courtship behavior in singles' bars. See Eibl-Eibesfeldt, supra.

A. Oldenquist. Group egoism, group selection, and nationalism. 176 Walhalla Rd., Columbus, OH 43202, USA.

J. Richer. Autism: Inside and out. Paediatric Psychology, John Radcliffe Hospital, Oxford OX3 9DU, U. K.

G. I. Ronzal. Physical correlates of female beauty. See Atzwanger, supra.

F. K. Salter & K. B. Kruck. Family resemblance and mother's facial beauty. See Eibl-Eibesfeldt, supra.

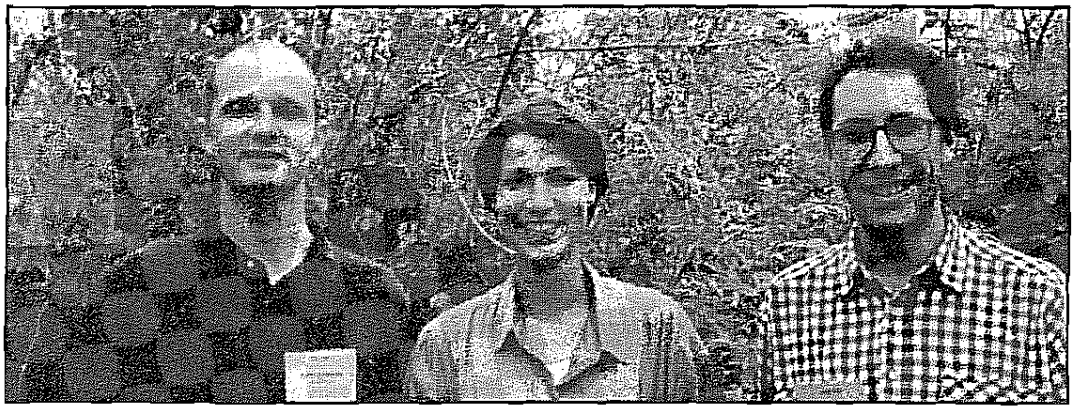
W.Schiefenhövel. Sexual behaviors in Melanesian societies--evolutionary models? See Eibl-Eibesfeldt, supra.

W. M. Schleidt. Tonic communication: A RE-view of human nonverbal behavior. Robert-Hamerling-Gasse 1/22, A-1150 Vienna, Austria.

A. Schmitt. Reconciliation among kindergarten children. See Atzwanger, supra.

R. Siegmund, M. Tittel, & W. Schiefenhövel. Time pattern in activity behavior of Tribriand Islanders. See Lind, supra.

M. Schleidt & J. Kien. Segmentation in behavior and its connection with brain functions. See Eibl-Eibesfeldt, supra.



Congress organizers Klaus Atzwanger, Katrin Schäfer and Alain Schmitt.

K. Sigmund. Games evolution plays: Mathematical basics of cooperation (PLENARY). Universität Wien, Austria.

P. K. Smith. Children's rough-and-tumble play: Observational and reported data (PLENARY). University of London Goldsmiths' College, Department of Psychology, New Cross, London SE14 6NW, U. K.

K. Solokowski. The influence of emotions on the persistence of conscious perceptual representations. Psychologie im F6 3, Bergische Universität, Gesamthochschule Wuppertal, Gausstr. 20, D-42097 Wuppertal, Germany.

F. L. Stricker & B. Chiarelli. Hormonal development and pheromonal perception in man. Dip. Biologia Animale, via Accademia, Albertina 17, 10023 Torino, Italy.

C. Sütterlin & K. B. Kruck. Environmental influence on sociality between strangers. See Eibl-Eibesfeldt, supra.

E. Synek. Human landscape preferences. See Atzwanger, supra.

C. Tafforin & R. Campan. Social and individual behavior of crew members in confinement experiments. Laboratoire d'Ethologie et Psychologie Animale, Université Paul Sabatier, 118 route de Narbonne, 31062 Toulouse, France.

H. G. Walbott. "Morphing" as a research tool to study the process of emotion recognition. Institut für Psychologie, Universität Salzburg, Hellbrunnerstr. 34, A-5020 Salzburg, Austria.

B. Wallner & J. Dittami. Sex appeal: A useful strategy in social life of barbary macaque females? See Atzwanger, supra.

C. C. Weisfeld & G. Weisfeld. Some correlates of marital satisfaction. Dept. of Psychology, University of Detroit Mercy, 8200 W. Outer Dr., Detroit, MI 48219 USA.

G. Weisfeld. Research on emotions and future developments in human ethology (PLENARY). Dept. of Psychology, Wayne State University, Detroit, MI 48202 USA.

K. Wermke & W. Mende. Evolution strategies of complexity: Development of pre-speech sounds. See Czerlinski, supra.

J. M. van der Stelt. Mother-infant acquisition of speech communication. Institute of Phonetic Sciences, University of Amsterdam, Herengracht 338, 1016 CG Amsterdam, The Netherlands.

J. P. van de Sande. Some links between cognition, situation and evolution. Rijks Universiteit Groningen, Psychologisch Instituut Heymans, Grote Kruisstraat 2/1, 9712 TS Groningen, The Netherlands.

J. P. Vieille & J.-L. Millot. Social behaviors of young children in a wading pool. See Brand, supra.

L. Ziegenhorn. Social status, affect, and sex in human adolescents. See G. Weisfeld, supra.

*Some papers were not delivered but are listed in case written versions are available.

Treasurer's Report
July 1995 to June 1996

Prepared by Barbara F. Fuller, ISHE Treaurer

CREDITS		DEBITS	
Source	Amount	Source	Amount
Balance July 1995	$12,637.33	Fee to IRS for tax-exempt status	$ 150.00
Member Dues thru 31 May 96	4, 925.00	Fee to accountant for application for tax-exempt status	100.00
Sale of mailing list	185.00	Misc. expenses for W. Charlesworth	50.00
Interest on certificate of deposit	333.15	Misc. expenses for B. Fuller	19.82
Transfer of European bank account containing pre-1994 dues	4, 728.50	Credit card company charges	218.37
	$22,808.98	Printing of Sept 95 *Bulletin*	360.00
		Mailing of Sept 95 *Bulletin*	324.00
		Printing of Dec 95 *Bulletin* and Membership Directory	1,377.00
		Mailing of Dec 95 *Bulletin* and Membership Directory	611.00
		Printing of Mar 96 *Bulletin*	380.00
		Mailing of Mar 96 *Bulletin*	338.00
		Printer for *Bulletin* preparation	998.00
		Computer for *Bulletin* preparation (partial payment)	400.00
		Balance of expenses payment for attendance by G. Weisfeld at 1995 ESS meeting	500.00
			$5,826.19

FINAL BALANCE = $16,982.79

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 (Linda Mealey, the chief book review editor, covers books in English). 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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Report on HBES Meeting

The Human Behavior and Evolution Society met in Evanston, Illinois, USA 26-30 June 1996. Local hosts were William Irons and Jack Beckstrom of Northwestern University. They assisted the program committee of Linda Mealey (Chief Book Review Editor of the *Bulletin*) and Michael Bailey.

An acknowedged highlight of the program was an invited address by Karl Grammer, ISHE Secretary. His talk concerned human sexual pheromones. Men's sweat contains androstenol, which oxidizes to androstenone. Women find the former steroid pleasant, but the latter has the unpleasant smell of urine. Why should mature males excrete a pheromone that repels women? As Karl expressed it, why should men go around stinking? The apparent answer is that the repellant effect does not apply to ovulating females. Thus, the male repels nonfertile females but not fertile ones, who are apparently attracted by androstenol. Androstenone also induces ovulation. From the woman's point of view, this pheromonal system serves to draw her to men when she is fertile but not otherwise. Karl speculated that men may be able to detect this heightened female interest, thus reducing the female's concealment of her fertile state. According to one theory, a woman conceals her time of ovulation in order to extract favors from men in exchange for sexual access throughout her cycle. But presumably she also has an interest in being inseminated when fertile, a goal that may cause her to betray her menstrual condition.

Karl also studied vaginal copulines, another pheromone. These seem to enhance and level men's ratings of women's attractiveness at all stages of the menstrual cycle, thus increasing women's opportunities for mate choice. However, exposure to ovulatory copulines raises men's testosterone levels, so that ovulation is detectable by men. Again, this may be advantageous to women, not only in attracting men when she is fertile, but also in inducing male-male competition, including sperm competition.

In a series of observational studies at discotheques, Karl found that unmated women not taking contraceptive pills concealed their ovulatory state in that their dress did not vary with stage of the cycle. Perhaps they did not wish to attract men sexurally when they were fertile but unmated. Pill-taking, unmated women showed the most sexual signalling, as measured by exposure of skin and tightness of clothing. They may have felt protected by their contraceptives. Mated but unaccompanied women not on the pill exposed more skin at mid-cycle than at other times, for reasons that we hesitate to suggest.

This research report demonstrated the power of observational research that combines proximate and ultimate levels of explanation. It seems to have moved inquiries into sexual strategies onto a more complex but more promising level than had prevailed previously. For further details, see Holden (1996a).

Another notable invited address was Vincent Sarich's on race. An emeritus anthropologist at the University of California at Berkeley, Sarich argued that racial differences are real and appreciable, being comparable with the morphological differences between the common chimpanzee and the bonobo. Further, they are rather recent, with most racial variation no older than about 20,000 years. These facts suggest that racial variations reflect rather strong selection pressures. In some cases, these morphological variations may have synergized with cultural differences, which abound in humans. In short, as David Rowe has pointed out, the causes of individual and group variation are the same, so how can we deny the latter?

Steven Emlen (Section on Neurobiology and Behavior, Cornell University, Ithaca, NY) presented a talk on the relation of animal reproductive behavioral principles to studies of human families. He cited avian evidence for a pattern of increased helping at the nest when territories are scarce or when the family has a

high-quality territory. Cooperative breeding is very common in birds and mammals. Numerous avian and mammalian studies suggest further that degree of consanguinity predicts amount of altruism in families. For example, in the striped-back wren, sons compete with their fathers for sex with their stepmothers. Evidence from human family studies was interpreted in light of these general principles.

In the keynote address of the meeting, Harvard University's Edward O. Wilson chided HBES for bowing to political pressure and voting to rename the 15-year-old journal*Ethology and Sociobiology*. He argued that the name change, to *Evolution and Human Behavior*, was coming just when the battle to recognize the importance of biological factors in human behavior has been won. He faulted the group for caving in to critics who still regard sociobiology as racist and determinist. HBES members have offered a number of reasons for the name change, including a desire to escape the political heat. For additional information, see "Sociobiology" to history's dustbin? (1996).

HBES has also had some problems concerning the degree of their control over the content of the journal. After a review by John Hartung of Kevin MacDonald's book *A People that Shall Dwell Alone: Judaism as a group evolutionary strategy* was criticized as anti-Semitic, Hartung strove to publish an "addendum." The journal publisher, Elsevier, refused to publish it, however. The resolution of this sort of issue for the future is unclear, since details of the new agreement reached between HBES and the publisher were not revealed. For additional information, see Holden (1996b). MacDonald's book was reviewed by Harmon Holcomb in the June 1996 issue of the *Bulletin*.

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BOOK REVIEWS

Primate Behaviour: Information, Social Knowledge, and the Evolution of Culture

By Duane Quiatt and Vernon Reynolds Cambridge University Press, 40 W. 20th St., New York, NY 10011 USA, 1995, $59.96 (hdbk.), $24.95 (ppr.).

Reviewed by N. Patrick Peritore, Dept. of Political Studies, University of Auckland--Tamaki Campus, Auckland, New Zealand. Email: p.Peritore@Auckland.ac.nz

Quiatt and Reynolds are anthropologists utilising a Neo-Darwinian and socioecological approach to discover the emergence of human social organisation out of primate cultural transmission and social knowledge. Their thesis is that "primate behaviour is cognitively organised" and that the evolution of human institutions can be found in "information-based patterning of social knowledge."

The book is billed as a synthesis of "cognitive primatology" and displays a certain textbook quality with at times a frustrating lack of decisiveness regarding alternative concepts. The first three chapters air standard Neo-Darwinian concepts--reproductive success, reciprocal altruism, kin selection--and does not come down to central theses until page 92. Thei authors' basic thesis is that primate social behaviour involves recognition and manipulation of relationships, defined as repositories of information. Both observers and observed use relationships as "cognitive constructs connecting the neural system with patterns in the external world" (p. 92). Information in the minds of primates is used in decision-making; humans are different from apes only in their use of language for information processing and in their institutionalisation of social relations.

There is nothing really extraordinary in this thesis, and it is not terribly illuminating. It rests on the intellectually sloppy notion of distinct "levels of analysis", each with its own methodology and methods, drawn from social science (p. 160). This metaphysical assumption of hierarchy and attendant methodological assumption of "levels of analysis" means that biology must proceed at two levels: philosophical conceptualisation and parallel empirical research. This reduplication has stopped social sciences from becoming science, and could block advance in ethology. It is unnecessary because the models and methods of biology apply equally to any "level of analysis" or organisation, or else they are deficient or misplaced. As George Williams said, "It matters little what taxonomic rank is reached by the diverging lineages. Subspecies can replace each other, and so can orders and classes. The same graphic or mathematical models are applicable at all taxonomic levels" (Dawkins and Ridley 1985 :7).

Quiatt and Reynolds are canny critics of methodology, and throughout the text place valuable caveats regarding the applicability of information models to primates, the problems of observation and testing of such concepts, and so forth. They do not seem to realise that these caveats actually vitiate their conceptual framework.

Thus, they admit that "institutionalisation of social relationships" and "information as a central feature of communication" are "conceptualisations of behaviour which may be problematic where non-human primates are concerned" (p. 111). They propose to "beg the question" as to whether information is a "substantive attribute or "central feature" of communication or whether a signal has content apart from the observed pattern in an interaction. They leave such issues to philosophers, who rarely know any biology. But these questions are central to defining a communications model of primate social interaction. If social knowledge is "information stored and accessed exosomatically by individuals in groups" (p. 111), and culture is "processing information in the social domain", then the old scholastic question arises: is "information" *in rem* (the essence of communication, i.e., realism), *ante rem* (a construct imposed by the observer, i.e., idealism), or *post rem* (a construct imposed on inductive data, i.e. , nominalism). Without clarity on these methodological issues, Quiatt and Reynolds cannot specify a working method

for extracting field data and testing the hypotheses developed in their book.

Cognitive primatology arises from a shift in perspective away from behaviourism to a cognitive modelling of animals as strategists. Social cognition is "The application of intelligence to the review of social information and the exploitation and management of social relations toward attainment of short- and long-term goals" according to Quiatt and Reynolds (p. 141). This model presupposes a range of concepts which cannot be operationalised: intelligence, exploitation, management, goal attainment, intentionality and so forth. Quiatt and Reynolds recognise "how difficult it appears to be to make reliable inferences concerning mental processes and conceptual abilities from observed overt behaviour." (p. 133).

They say, in relation to animal language projects, "the extent to which primates or other animals learning to solve such problems may use concepts familiar to human beings is not at all clear. Attempts to study directly and with similar experimental rigour a subject animal's grasp of concepts such as number have not been particularly enlightening" (p. 135). They recognise, in connection with Cheney and Seyfarth's study of vervet calls, that "it is by no means clear what interpretation should be given to this consistency of response...to the calls of other species" (p. 147). In terms of recognition studies, "we can only guess about their [primates] ability to objectify their own behaviour" (p. 151).

Quiatt and Reynolds reject cost-benefit and rational choice models because "there is nothing to be gained by introducing ideas of conscious intention, purposeful action, and the kind of reflexive thinking that we ordinarily associate with the making of human decisions..." (p. 156), although such imputations are required by an information model. They recognise "how difficult it may be for even the most practised, most responsible, and most articulate observer of primates in nature and in captivity to establish a reliable evidenciary base for assertions concerning the revelation of intention embodied in action" (p. 173). They recognise the extent to which "the account foreshadows interpretation and the intentional language of interpretation bleeds back into the description of events...." (p. 172). Yet they insist that informational analysis is "more productive than intentional analysis" (p. 173). On their own account of methodological difficulties in positing mental events in primates, this assertion, and their model, cannot stand. Their informational model simply adds unnecessary analogy to inductive accounts of observed behaviour.

If primates "act like" information processors, the authors believe that we can then explain the evolution of social cognition, social life, and culture. In fact, even if there were a perfect match between data and concepts, which there is not and probably never will be in principle, this paradigm is flawed by two problems.

First, it is anthropomorphic. As they said, "To explore the question of animal awareness from a comparative evolutionary standpoint, with human beings as the final referent species, makes sense to us as evolutionists, as anthropologists, and, inevitably, as human beings...." They continue, "We intend, however, for reasons that we trust most readers will agree are good and sufficient, to beg the final questions: Are other animals capable of reflexive self-awareness? Is the behaviour of other animals intentional? Do other animals think as we do?" (p. 166, my emphases). To beg these questions is to admit that it is futile to posit humans as the "reference species" without having a clear, concise, and operationalisable theory of our mental processes to apply as a measure to animals, in order to rank them by their proximity to our language-based mentition. If, as I believe to be the case, we have no such finished theory of human mental processes, then why talk about primates in terms of information processing, intentionality, mental processes at all? There is no model to apply from the top down, and there is no possibility of inductively approximating our "emic" self-account from primate observations. This program is futile and wastes valuable field research in asking questions which ultimately reveal nothing about the origin of human competences, and are incapable of revealing the cognitive world of other species.

Second, it is unclear what relationship an information or cognitive model would have with Neo-Darwinian explanation. Again, Quiatt and Reynolds point this out quite sharply, in saying "However, when comparing and interpreting the meaning of behaviour, we need to bear in mind that similarities and dissimilarities in behaviour observed across species do not in themselves tell us much about similarities and dissimilarities in adaptation, ontogeny, proximate causation, or cognitive function" (p. 151). Quite so. Comparative behaviour is a methodology-intensive enterprise, requiring the most rigorous conceptualisation and testing of assumptions.

Quiatt and Reynolds, with all their field experience, would have done a great service in writing a methodology text instead of this ambitious programmatic text. Neo-Darwinian proximate and ultimate causation can proceed without any imputation of conscious motive or intentionality at all, even, and perhaps especially, in the case of human beings. Our cultural-linguistic self-accounts are mainly noise masking unconscious motivations of reproductive success/kin selection. Baker and Bellis, in their brilliant study of human sperm competition, put the case this way: "[I]t seems likely that physiological programming has a more powerful influence than any conscious rationalisation. Bearing in mind the benefits of self-deception and the deception of others, we suspect that subconscious programming is actually likely to be a more accurate indicator of adaptive function than is conscious rationalisation" (1995:185). As J. S. Kennedy said, "[A]lthough we cannot be certain that no animals are conscious, we can say that it is most unlikely that any of them are. Science does not deal in certainties but in order to keep going it must adopt working hypotheses.... It is in that spirit that anthropomorphism is treated here as a definite mistake. In point of fact, the hypothesis that animals are conscious is not a scientific one, since it cannot be tested" (Kennedy 1992: 31-32). Instead of treating humans as an exceptional "referent species," we may best proceed in comparative studies by treating Neo-Darwinian motivation as entirely unconscious, and taking our own "emic" accounts no more seriously than as mere data for "etic" accounts.

In summary, Quiatt and Reynolds' *Primate Behaviour* is a solid and competent summary of cognitive primatology. But this field is flawed by anthropomorphism and by a lack of conceptual and methodological clarity and testability. It poses an informational model of social processes which provides only the weakest explanation of human behaviour, an untestable analogy with animal behaviour allowing generation of illicit teleological hypotheses which would waste a great deal of field research if adopted. These deficiencies probably should not be laid at Quiatt and Reynold's door, as they are shared by large segments of the ethological community, but it is time for debate about such models. Wild primates are rapidly disappearing, and field studies cannot indulge in theoretically misplaced efforts.

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A Dynamic Systems Approach to the Development of Cognition and Action

By Esther Thelen and Linda B. Smith. MIT Press, 55 Hayward St., Cambridge, MA 02142 USA, 1994, $ 50 (hdbk.), $25 (ppr).

Reviewed by Wolfgang Schleidt, Robert-Hamerlingg. 1/22, A-1150 Vienna, Austria.

This book is founded on the premise "that while the endpoints of human development are complex and unique, the processes by which we reach those endpoints are the same that govern development in even

simple organisms, and to some degree, even complex, nonliving systems" (p. xiii). It is dedicated "to showing that behavior and development are dynamic at many levels of explanation, in particular, that phenomena described at the level of behavior are congruent with what is known about the brain and how it works."

Esther Thelen moves in the forefront of organismic developmental psychology. With her seminal papers (e.g., Thelen, 1979, 1985), she has established an outstanding reputation for her research in human locomotion development. By means of patient observations, clever experiments, and sophisticated analysis, she explores what makes a human baby move and kick, what moves the babies legs and arms, and, ultimately, how the growing human being reaches competence in upright gait. Linda B. Smith, akin in spirit and approach (e.g., Smith, 1989), has began exploring the growing infant's cognitive skills. What for Thelen is understanding and explaining the competence of upright gait is for Smith understanding and explaining the competence of grasping an object with eye and hand.

With today's concern for the relevance of the human quest for knowledge, this work would not--could not--be considered to be in the forefront of science without a modern theoretical framework: the dynamic systems approach (patron saints: L. von Bertalanffy, 1968 and C. H. Waddington, 1977; management: Prigogine & Edelman, 1984). According to Thelen and Smith, researchers such as Sherrington (1906), Gesell (1946), Piaget (1971), Lorenz (1937), and Prechtl (1986) may have contributed interesting observations, but they are only suppliers of brick and mortar for the grand design that deserves the attention of professional psychologists. However, some very important researchers in the field of the development of action and cognition--the "grand designers"--are identified by name only in the introduction (e.g., P. B. Baltes, L. von Bertalanffy, P. H. Wolff), and others are not even mentioned (N. G. Blurton Jones, E. H. Erikson, D. G. Freedman, J. P. Hailman, V. Hamburger, S. Harnad, B. Hassenstein, D. McFarland, R. Spitz, E. von Holst).*

Sherrington (1906) is credited only for having influenced (misled?) psychologists and neurophysiologists alike by suggesting the existence of "a clear distinction between sensory and motor pathways and between central and peripheral processes" (pp. 187f). Gesell (1946) is cited only for having been the first to use an organismic systems approach as a "powerful root metaphor in developmental psychology" (pp. xix-xx), for having "evolved a sophisticated theory that acknowledged both the dynamic and nonlinear nature of developmental processes" (p. 5), and for having described "23 stages and 28 substages of prone behavior" (pp. 3, 7)--that's all. Lorenz is mentioned once (between Hinde and Gottlieb) in the context of "sensitive period" (p. 116), and Prechtl is cited for reporting "alternating leg movements in utero" (p. 89); both have done a lot more than this for our understanding of dynamic systems and of the development of cognition and action.

Such courtesy references stand in sharp contrast to those referring to Piaget, whose name appears on 37 pages. Most of Piaget's classic books are listed among the references, and selected examples are discussed, in some cases in great detail. Piaget (1971) is included among those credited for their organismic systems approach, which provides a "powerful root metaphor in developmental psychology." Thelen and Smith find it sad, however, that he failed to find the superior explanation for his observations that they themselves so effortlessly construct with the help of Waddington's epigenetic landscape paradigm, chaos theory, Edelman's (1992) theory of neural group selection, and basic thermodynamics. In spite of such critisism of Piaget's explanations and his different conceptual approach (conceived within a very different Zeitgeist), he is not used as a mere straw man, but is accepted as someone who thought of many theoretically profound, clever answers to questions that developmental psychologists tend to worry about, and who designed experiments that can be repeated conveniently and reinterpreted according to the principles of the dynamic systems approach.

Here is the core of my critique: The claim of Thelen and Smith that they "are not seeking reductionist explanations, but harmonious ones. . ." and are grounding their "thinking about process and mechanism in a developmental theory of the brain" (p. xiv) rings hollow. I see a fascinating field and a

rich body of facts forced into the vague set of concepts of a highly simplified dynamic systems approach. Much of what we have learned from years of research in classical physiology, evolutionary biology, ethology, control theory and cybernetics has been pushed aside. Instead, highly selective observations and experiments are interpreted in a spirit of "thermodynamic reductionism," which is presented as a panacea for dealing with the complexity of biological systems. The role of evolution in shaping development (and development shaping evolution), to me especially striking for human development, is ignored.

This new model follows an old track, namely trying to explain complex biological systems by one single "physical law." Julian Huxley coined the term "nothingelsebuttery" for such attempts (Lorenz, 1981). A few years ago I analyzed sociobiological "nothingelsebuttery" and was able to show that a minimal number of components on at least four different levels of organization and their interactions are necessary to build a working model of evolution (Schleidt, 1981). What I miss most in the Thelen and Smith book is a discussion of all the parts and their interactions on all levels of organization, from physics and chemistry through all levels of biology, that make human development tick, not just the contribution of selected parts of physics with a sprinkle of neurology.

The beauty and merit of the book by Esther Thelen and Linda B. Smith lies in its quest to deal with the enormous complexity of human development, both in the domains of action and cognition. For my taste, however, their attempt to present "thermodynamic reductionism" as an explanation for all features of human development is too narrow.

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*** A reading list containing exemplary papers by these scientists is available from the reviewer on request.**

Exiles From Eden

By Kalman Glantz and John Pearce. W. W. Norton & Company, 500 Fifth Avenue, New York, NY 10110, USA; 1989, $32.95 (hdbk.).*

Reviewed by Donald F. Klein, M.D., Department of Therapeutics, New York State Psychiatric Institute, Unit 22, 722 W. 168 St., New York, NY 10032, USA.

This interesting book, published in 1989, suffered from being ahead of its time by antedating the current wave of interest in evolutionary psychology. It is a well-written, ambitious attempt to use evolutionary biology as a basic framework for the rational practice of psychotherapy. Over and above their substantive suggestions, their social purpose is to refute those biologists (citing Nancy Andreasen, author of *The Broken Brain*) who think that if something is wrong with a person's behavior, then there must be something wrong with his brain, which implies brain treatment and not talk treatment. The authors agree that the "spectacular successes of biological psychiatry (psychopharmacology)...have indeed created a revolution in psychiatry." They assert, however, that "psychotherapy, *while not a cure for major mental illness or organic disorders*, will prove to be the treatment of choice for much of the psychological distress now seen in clinical practice." A factual basis for this theoretical assumption is not presented, however. There are also no references to the important, if controversial, literature on just how specific psychotherapy is (see Jerome Frank).

The authors' major argument is that the human species evolved primarily as hunter-gatherers but that while our social and physical environment have radically changed, our genes have not. They further assert that this mismatch creates "much of the psychological distress that psychotherapists are called upon to treat." Again, this is more anecdote than evidence.

Since our brain builds environmental models, the authors claim, we don't need a broken brain to develop a defective model. "In a confusing environment, a perfectly good brain can generate a perfectly terrible model. And this model will not be improved by drug therapy (although drugs can certainly improve, temporarily at least, the outlook of people with emotional disorders). Hence the need for psychotherapy." They conclude by arguing that there is a lot of good in many different types of psychotherapy but that helping the patient understand the nature of the mismatch between her genes and her current environment provides a useful practice.

As an example, they discuss the inappropriate triggering of the fight or flight response, which is appropriate for a physical danger but nonetheless may be triggered by a boss who doesn't like one's work. Other topics include a useful gloss upon simplistic Social Darwinist views, emphasizing the genetic predispositions for helping others, the argument that current child-rearing practices conflict with our natural predispositions, and, in their discussions of sex, a disputation of the social constructionist view that gender is only an artificial, socially-determined category unrelated to biology.

Although a good read, and at times provocative, the authors never squarely face the issue of how to, or if you can, distinguish between poor models based on confusing environments and actual brain malfunction. The major problem with the erroneous model approach is that the models seem inflexibly refractory to ordinary relearning by new experience. One must explain why such "errors" persist in the face of disconfirming experience. The authors offer two explanations for this. One is the prepotent force of early modeling and learning, i.e., the hypothesis that what one has been exposed to as an infant and child is fixed. Although commonly held, this belief seems a post hoc hypothesis generated to deal with inflexible maladaptation rather than a factually based premise. Indeed, this explanation actually contradicts the other proffered explanation, which is that people learn to avoid disconfirming experiences because of anticipatory anxiety which incites avoidance. This explanation seems reasonable in the context of simple phobics who react with fear or disgust, presumably on the basis of either early experience or parental modeling, or perhaps prepotent innate aversions. The important issue is that in this area exposure therapy really works. The subject is forced to learn that he is wrong by direct experience,

extinguishing anticipatory anxiety and avoidance. However, if that's true, then the effects of early learning are not so fixed, and indeed may be reversible by rather simple procedures.

The authors implicitly argue that it is only the major mental illnesses, and diagnosable organic brain disorders, for which medication is appropriate and that everything else is in the realm of evolutionarily informed psychotherapy. For instance, they clearly state the value of "[l]ithium for manic-depression, neuroleptics for the symptoms of schizophrenia, tricyclics and MAOI's to control depression, anxiety and panic, etc." But how can we infer if a particular patient's anxiety stems from a gene-environment mismatch, for which psychotherapy (or education?) is appropriate, or from a broken adaptive circuit? Particularly relevant here is the omission of the syndromal approach to differential diagnosis. If an emotional or behavioral malfunction is embedded in a recognizable syndrome with a predictable course, and if it is further known that this particular syndrome responds well to specific medication, as demonstrated by appropriately controlled clinical trials, wouldn't that indicate that such patients may well have a medically treatable organic disfunction?

Further, I believe that emotional reactions due to defective mechanisms extend far beyond the major mental illnesses. Such emotional manifestations as applause hunger, emotional liability, explosive rage, separation anxiety, social anxiety, and rejection sensitivity often are manifested disfunctionally, and are not the evident outcome of a gene-environment mismatch. My reasoning is not based upon a direct physiological method for diagnosing malfunctions of the emotional regulatory system, since this is as yet far beyond us. Rather it is based upon experience in psychopharmacology, where we frequently see remarkable, but fairly subtle, changes in emotional reactivity due to medication, without any apparent changes in the cognitive structures that supposedly elicit these emotional reactions. This point of view is supported by my belief that the major psychotropic agents are normalizing in their effect, perhaps via correction of cybernetic derangements, and have little effect upon ordinary emotionality (Klein, 1988). In other words, antidepressants do not make a normal person elated, nor do they mollify the realistically unhappy.

Unfortunately, this belief, which has much clinical support, has never really been put to a conclusive test. What is needed is a large scale double-blind, placebo-controlled trial of various psychotropic medications on normal people with measures attuned to the range of emotional responses that occur during ordinary life, as well as to the sense of ordinary self-esteem and life enjoyment. One could even make this more interesting by stratifying the normals into those with and without a family history of mental disorder. This would test the possibility that apparently similar emotional reactions within the range of normal emotional regulation may be either physiological or pathophysiological.

Because this model implies a sharp distinction between pathological and normal emotionality, the authors' terminology which slides back and forth between terms such as distress and depression is imprecise. Many of the examples of "depression" appear to be simple unhappiness. An example of the problems in dealing with the distinction between normal and pathological emotionality occurs with their description of Samantha. It is stated that "Her extreme sensitivity and emotional outburts destroyed all her work and personal relationships," and that "She reacted emotionally to instruction and criticism." Further, her emotional pathology is explained on the basis of a "limited sense of self and low self esteem." But since many others with low self esteem are not sensitive to rejection or indulge in emotional outbursts, this explanation is inadequate. Indeed, patients with marked sensitivity to rejection (hysteroid dysphoria) occur within the syndrome of atypical depression and are particularly likely to benefit from monoamine oxidase inhibitors, as demonstrated in several placebo-controlled trials. Perhaps the authors have the wrong end of the stick. Rejection sensitivity may be a specific emotional disregulation that has, as one of its many consequences, low self-esteem rather than the other way around. This is open to experimental analysis.

To sum up, the authors presciently address important issues regarding brain, behavior, pathology and treatment. Answering the questions they raise and questioning their conclusions will spark an important part of our research agenda.

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*** We apologize for the tardiness of this review. Unfortunately, the book is now only available from John Pearce, M.D., 247 Lakeview Ave., Cambridge, MA 02138, USA for $30.**

The Scent of Eros: Mysteries of Odor in Human Sexuality

By James Vaughn Kohl and Robert T. Francoeur, Continuum Books, 370 Lexington Ave., New York, NY 10017, 1995, $24.95 (hdbk.).

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

The Scent of Eros reminds me of two other recent popular books on similar topics: Diane Ackerman's *Natural History of the Senses* and Helen Fisher's *Anatomy of Love* . This is not an endorsement of *The Scent of Eros* , as I disliked both the other books, and for the same reasons disliked *Scent of Eros*. There are, however, many people who liked Ackerman's and Fisher's books (both sold very well and Ackerman's book was made into a public television mini-series. Since these same people will probably also like *Scent of Eros*, I will try to outline both my reasons for disliking the book, as well as the reasons why others may find it worthwhile.

First the negative: Jim Kohl, the first author of *Scent of Eros*, is "a scientist at Partell Medical Center in Las Vegas"; Robert Francoeur, the second author (read "editor"), is a professor of human sexuality at Fairleigh Dickinson and New York Universities, and has a long, distinguished career as a teacher and an editor. Having met Kohl and having used some of Francoeur's previous editorial products for use in my own course on human sexuality, I had expected this to be an effective team and really looked forward to learning a lot from the book. As it turns out, however, Kohl's career has nothing to do with research in either olfaction or sexuality; rather, he is an enthusiastic layperson who got hooked on a topic and used his initiative and library skills to do a search of the literature - which he then marketed to a variety of experts and educators (via the Society for the Scientific Study of Sex). Francoeur picked up on it and, essentially, the book is Francoeur's edited version of Kohl's literature search. While it may make for exciting reading by other laypeople, for those of us who teach about olfaction and sexuality and therefore do literature searches on these topics, there is nothing new here. I cover most of this material in my undergraduate sexuality, animal behavior, and biopsychology courses; most of it is easily available in a variety of places.

Moreover, like Fisher's and, particularly, Ackerman's book, *Scent of Eros* is a collection of quotes, anecdotes and "factoids" strung together in a weakly organized fashion. Some of the "factoids" are up to date and based upon scientific investigation, but there is an overabundance of quotes from very old and second-hand anthropological reports and literary sources, as well as from magazines (such as *Newsweek, Vogue, Discover*, and *National Geographic*), newspapers (the *New York Times*, the *Los Angeles Times*, the *Las Vegas Review-Journal*), and popular (non-technical) books.

Additionally, of the 275 published pages of the book, the first 22 consist of acknowledgements, a forward, and an introduction by Francouer (which frankly reads as an apology for things to come). The last 85 comprise the endnotes, an unnecessary glossary (with words such as "embryo," "axon," "inbred," "menses," and "dopamine"), a bibliography that almost entirely repeats the endnotes, and an index. This leaves only 169 pages of text, of which four chapters are only tangential to the thesis: Chapter 7, "Deep in the Womb," addresses conception and prenatal development. Chapter 8, "From Genes to Behavior and Back," gives a superficial summary of the "nature/nurture debate." Chapter 10, "Natural Opiates, Infatuations and Bonding," discusses not

pheromones but the roles of various hormones and neurotransmitters in sexuality. And chapter 14, "The Healing Power of Aromatic Oils," is a New-Agey, nonscientific plug for aromatherapy). This leaves a total of 110 scant pages which somehow address odor and sexuality.

Lastly, the referencing style (endnotes organized by chapter) is such that one is not always sure where a particular "fact" came from: sometimes there is a full citation; sometimes there is just another supposed fact or opinion elaborating, but not verifying, the first; and sometimes there is no reference at all.

So why would some people like this book? One: the reading is not difficult. Two: most people do enjoy the anecdotal material. Three: for laypeople (and some academics), the material will indeed be new. And, four: after all is said and done, this *is* an interesting topic! People who enjoyed Fisher and Ackerman for those reasons are likely to enjoy Kohl and Francoeur, too. If you are looking for an introduction to this topic, it might be a good place to start. My personal suggestion, however, is Stoddart's *The Scented Ape* (reviewed by Hector Qirko, 1994), which is just as readable but longer, better organized, better on referencing, and better on evolutionary aspects.

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Breastfeeding: Biocultural Perspectives

Edited by Patricia Stuart-Macadam and Katherine A. Dettwyler. Aldine de Gruyter, 200 Saw Mill River Rd., Hawthorne, NY 10532, USA, 1995, $65.96 (hdbk.), $31.95 (ppr.).

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

Breastfeeding is viewed by the authors as a biocultural phenomenon, that is, a biological process whose practice is culturally dictated. The editors of the book consider themselves to be biological anthropologists; each wrote two of the fifteen chapters. They are not, however, dispassionate scholars. Both are strong advocates of breastfeeding, and in fact the book is dedicated to Derrick L. Jeliffe and Niles R. Newton, both known for their commitment to promoting maternal and child health through breastfeeding. In compiling the book, their purpose was to emphasize the dangers of ignoring the "bio" factor of their biocultural analysis and to urge all who have contact with the infant's mother to encourage her to breast feed and to delay weaning until the child is approximately four years of age.

The editors and their various authors (physicians, anthropologists, nurses, and a historian) emphasize the importance of breastfeeding as a preventive for breast cancer and as a way of transferring immunity to various diseases to the infant. In their

enthusiasm to convey the "biological" imperative of breastfeeding, they conveniently forget that just because some areas of the world nursed their infants for up to four years, this might not be the most desirable length. Dettwyler, a mother of three, believes so strongly in the four-year period that she nursed her children for that length of time even though historically such practices have been rare. While she admits that she is an upper-middle-class college professor who can control many more aspects of her life than can the average woman, she seems to ignore the implications of this in terms of her own advocacy.

Thus, although the authors adopt what they say is an evolutionary approach, they tend to ignore the changing role and status of the nursing mother. They also ignore technology. For example, although it was possible for the biological mother to turn over the feeding of the infant to a wet nurse in the past, artificial feeding was a very difficult and hazardous enterprise until development of the rubber nipple, acceptance of germ theory and of the need for sterilization of feeding equipment, and recognition of the importance of pasteurization of milk, all of which occurred near the end of the nineteenth century (Bullough, 1981). This was followed by the development of infant formula, a topic which the authors do investigate and toward which they are hostile, particularly regarding its use in underdeveloped areas of the world.

Valerie Fields, a historian, presents data from the nineteenth century about the rise in infant mortality in the New England towns where the mothers worked in mills and had to use artificial means of feeding the infant. She also gives information on time of weaning across various historical periods and presents convincing data on the importance of breastfeeding and delayed weaning. These historical data probably are of great value for underdeveloped countries, where large segments of the population live at minimal nutritional levels. However, one questions whether these data can be applied to advocate breastfeeding and delayed weaning in advanced industrial countries.

The contributions are wide ranging. Dettwyler herself presents an interesting chapter on beauty and the breast, concluding that there comes to be greater sexual emphasis on the female breast as breastfeeding declines. She goes so far as to argue that the biological function of the human breast has been denied its biological function as the physiological link between the mother and infant, at least in the United States, and instead has come to be viewed as an erotic aspect of being female. Interestingly, Dettwyler would deny that women in general receive sexual pleasure from breast feeding. She states that whatever pleasure they experience derives from maternal-child bonding. For a person who emphasizes biological factors, she seemingly wants to deny the factors involved in sexual arousal itself (Masters and Johnson, 1966).

In other chapters, Penny van Esterik discusses the politics of breastfeeding from an advocacy perspective, Patricia Stuart-Macadam writes on breastfeeding in prehistory, Sara A. Quandt examines the sociocultural aspects of lactation, Michael W. Woolridge looks at baby-controlled breastfeeding, and Allan S. Cunningham argues for breastfeeding as an adaptive behavior for child health and longevity. Other contributions include James J. McKenna and Nicole J. Bernshaw on breastfeeding and infant-parent co-sleeping as adaptive strategies, Peter T. Ellison on breastfeeding and fertility, and Marc S. Micozzi on breast cancer. Sheila Kiyzinger, Ruth A. Lawrence, and Doren Fredrickson each provide a ten-page commentary on different aspects of breastfeeding.

In sum, the result is a scholarly advocacy book for breastfeeding in a biocultural context. One can disagree with some parts of it, as I do, and argue that practices change as society evolves, but how far can we alter our biological predispositions? This is a question that the book raises. I believe there is more room for change than the editors and contributors do, but, thanks to this book, it will be difficult to overlook many of the biological aspects in any future discussion.

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ANNOUNCEMENTS

Society of Ethnobiology

The Society of Ethnobiology is devoted to the interdisciplinary study of the relationships between living organisms and human cultures worldwide. It oversees publication of the semi-annual *Journal of Ethnobiology*. The Society's annual meeting will be held 26-29 March 1997 at the University of Georgia. The conference will provide a forum for scholarly presentations and interaction among anthropologists, biologists, native peoples, and others who are interested in zooarchaeology, ethnobotany, archaeobotany, ethnozoology, nutrition, linguistics, pharmacology, geography, and ecology. For information, contact LaBau Bryan, Dept. of Anthropology, University of Georgia, Athens, GA 30602-1619, tel. 1- 706-542-1433, fax 1-706-542-3998, e-mail anthro@uga.cc.uga.edu.

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