Human Ethology Bulletin

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ARTICLES

A Remembrance of Thorleif Schjelderup-Ebbe

By John Price, Odintune Place, Plumpton, East Sussex, BN7 3AN, UK.

Some years after first reading Schjelderup-Ebbe's contribution to Murchison's *Handbook of Psychology* (1935), I became curious about the man and, realising he might still be alive, persuaded a Norwegian speaking friend to telephone him. It was not difficult to get his number as he was the only Schjelderup-Ebbe in the Oslo telephone directory. The telephone was answered by his son, Dag, who told me that his father had died four years previously. However, he accepted an invitation to spend a weekend with us later that year, and the following interview took place at that time. Dag himself was aged 60 and was a Professor of Music at Oslo University; he had published a book on Grieg and was writing one on Johan Svenson. He played bridge and chess for Norway, was a great traveller, and would be invited to make up bridge teams on cruise ships and transatlantic liners. Dag was delightful company. He visited us again and taught us to play duplicate bridge. We corresponded for awhile, and I was able to show him my paper (with Leon Sloman) in *Ethology and Sociobiology* entitled "Depression as yielding behaviour: a model based on Schjelderup-Ebbe's pecking order". Dag said would have pleased his father if he had known his work was still found useful. Dag was an only child and unmarried, and so there will be no more direct descendants.

In addition to the material related below, Dag told us that his father had written several novels and children's stories; he was obviously a talented man of very wide interests. It was clear that he made all his observations on chickens between the age of six and taking his Bachelor degree; he was never allowed to do postgraduate work in Oslo. It was also touching that the man who had first described the adverse effect of despotic behaviour in chickens should be so thoroughly crushed by the biological hierarchy of his university. Dag said that in spite of being passed over for recognition in so many ways, his father was at least able to tell his family that he had been made a Fellow of the Royal Society of London; but when, hoping to get some more information about him from the citation, I made enquiries about this, it turned out that he had been made a Fellow of the Royal Society of Arts of London, an honour which can be achieved by the payment of a modest sum of money. But his family never saw through this deceptive fluffing up of his feathers.

An interview with Dag Schjelderup-Ebbe, January 1986

The interview started with extensive data on the family background which are omitted here, but can be obtained from the author. Dag's responses to my inquiries were tape recorded and transcribed as follows:

"My father had a very sheltered life. He had tutors and one of his tutors happened to be my

mother's uncle, and she heard about this uncle of hers who was a schoolteacher and who had this very gifted pupil, and she never knew that she was going to marry this wealthy gifted child where he went to give lessons. As a child she learned about this brilliant and talented and wealthy boy whom her uncle used to go tutoring, and strangely, she was going to marry him later. Then of course my mother came from completely different circumstances - very poor people.

"When my father was in his teens he started to feel the dominance of his mother, he started to revolt from her dominance and he got interested in girls and more or less got engaged to a young girl, about 1914 or 1915, and she was half British, and she went to London, and then the war broke out and she couldn't come to Norway - she was the one my father should have married - and sometimes, unfortunately, he came to London and visited her. My father was not very faithful to my mother. He had several liaisons which made my mother very unhappy. He was very egotistical, just very self-centred. And when I discovered this in my teens it made me very antagonistic to my father, it made me want to be very different from him.

"And now we come to - how he got to be - some tragedies in his life. Fortunately my father was a typical rationalist. He rationalised everything and he had an optimistic nature. In spite of difficulties he had he was not melancholy, he was able to see the good points, a cheerful kind of person. The bad things that happened to him had to do with his scientific work.....

"I will mention how he became interested in chickens. My grandmother wanted to do everything pleasant for my father. She had a house in Oslo, but it was hot in Oslo in the summer so when my father was six they rented a house fifteen miles away and the family there had a yard of chickens, and my father as a child of six became terribly interested in chickens, terribly interested, it was a whole new world for him. He started to identify the chickens and give them names. The next summer they came back and there were the same chickens and some new ones there, he recognised them, and then he persuaded his mother to let him have his own chickens. She bought them for him, so they rented some other house, and he had ten or twelve chickens. And he kept those. In the winter they were boarded out in a barn and he went to visit them and in the summer, he was so happy, he could have them all to himself, and that continued, they had all generations, and he had names for each one of them. He knew them all and he was not interested in how many eggs they were laying, which he noted. But when he was nine or ten he had a notebook which I have seen, in which he began to write down his discoveries about the hierarchy among them, that there were these triangles, and, a strange thing, one of my father's laws, is this hierarchy, triangular, quadrangular, any kind of angle. Chicken A may be the master of B and B may be the master of C, then you would think that A would be the master of C, but by some quirk it's possible that C may be the master of A. It works in all kinds of rotations depending on when the chickens first met, how it happened then, or if a chicken gets sick it is reversed. He started to write that down."

JP: He was nine?

"I'd say ten or eleven. He wrote it down in a book and it fascinated him. He tried to read about it but nobody had ever thought of that. Then he took his matriculation exam for university and of course he was to study science. You had to take a major and several minors. It was very difficult then, it's easier now. So my father took chemistry as a minor, also botany and mechanics as minors, and zoology as his major. And the professor of zoology - there was only one - she was the first woman professor in Norway - this was after the liberation of women in Norway. Kristine Bonnevie was her name. Her only interest - she was a brilliant scientist in her field - was the anatomy of some sea creatures - some kinds of little crayfish - she was the world authority on them. She would take my father out in a little boat to canvass for them and he got terribly sea-sick, and she had absolutely no interest in my father's interest in chickens. She wanted him to become - she recognised his good mind and he had a tremendous memory - he used to know the Latin names of all animals and all plants and all chemical formulae, just stuck in his mind - she had no interest in his interest, but he majored in zoology under her.

"And then the unfortunate thing happened. She was a woman, very radical, probably an

early socialist - she was very domineering, high in the hierarchy, there were small professional circles in the university, proud of her position - and then there was an article in the student paper, called *Minerva*, making ridicule of her. It was written by a man who became one of our foremost novelists, his name was Sigurd Hoel, he studied zoology at the time. And some enemy of my father told her that my father was author of this anonymous article, and she believed it. My father was absolutely innocent; he hadn't written it. But he later heard that she had believed that he was the author. It was well written, my father was a good writer and at that time, he had written children's stories and beautiful poems for children, so he was a good writer, and she was sure it was him. So she started to dislike him. And after he had passed his exams he wanted to get a job, and he went to her and asked to become her assistant and she said, 'No, your work is quite different. In any case, you don't need the job, you have money. We have so many talented young scientists who have no money, they need the job, you don't need it.' And that was a crushing blow to him. He said to himself, I can't continue in Norway; I have to go abroad.

"That's about the time he went to see his father and married my mother. So he broke away from the University and had no chance of getting a job in Norway from that. She was always his antagonist and would always work against him. So he did some work at the University of Lund in Sweden, and then he went to Germany to get a PhD. The German PhDs are not as good as those in Scandinavia or England. He got it at a place called Greifswald. It's said that the conductor of the train says "Here's Greifswald, get out and get your doctorate." Compared to what he had done in Norway it was easy. But he worked - his treatise in the University of Oslo had been on birds, on chickens - but here he wrote an important paper called *'Gallus domesticus* in its daily life', which really made a name for him already in Germany. Then he went to Leipzig with my mother and he studied there with a very famous psychologist called David Katz, and he published quite a few things in various publications - he had already published something on the language of chickens when he was a student - he published in various German and Polish journals - and he got well known, and in about 1955 he got a letter from Konrad Lorenz - he of course became the world's authority on ethology - and this letter - I have seen it but cannot find it - and Lorenz said, your work has been a great influence on my work - my father was before him. But Lorenz worked on geese, I think, and became very famous. And another man in Holland - Tinbergen - in 1937 my father was invited to Holland, to the university, and was well received, I think by Tinbergen. Then in 1972 when my father was very old the Nobel prize was given to three scientists for their work in ethology, Lorenz, Tinbergen and another man. I was upset about this, I said to him, You should have had this. After all, he founded the word pecking order which you find in any newspaper all over the world - he called it hackordnung - he coined the word which now is in everyday use - my father is the one who discovered it before Konrad Lorenz, so I think it's a bit unfair.

"Anyway, my father came back to Norway about 1925 and settled in Norway forever. Then another tragic thing happened. He had no chance of getting anything in Norway because of this woman, who just didn't like him. He wanted to get a Norwegian doctorate, so as his minor had been in botany he went to the professor of botany and said I want to do some work in botany. And the botanist said, 'Yes, you have a good mind. You have to do some really scientific work on this, and we have here a whole batch of seeds that are 100 years old, and we want somebody to try to make them grow, and to prove that seeds in the pyramids can survive for 2,000 or 3,000 years.' And so my father had to work for two years, and he had these seeds and did a tremendous amount of work on that, and he wrote a large book - published by the Norwegian Academy of Sciences in 1936 - on the possibility of old seeds to grow. It was absolutely different from anything that he'd ever done, it was a huge book - 400 big pages - very, very scientific, and he thought he would get a doctorate from it. It was a pioneering work in this field, it was OKed for a PhD, which was the thing he absolutely wanted.

"But then some enemies of his said, 'He's not the one we want at the University of Oslo, and we'll prevent him from getting his PhD.' To get a PhD in Norway - I have got one - you have to

defend your thing, sometimes they can wreck it, that's very rare. You have to give two lectures, one on a prepared subject and one on your own - that has never been done except in my father's case. The one they gave him was completely OK. But my father took a chance - rather a silly topic for his own lecture. Its title was 'On Fragrance Intensity in Angiosperms', and he published it in English later and sent it to lots of scientists. It was on the fragrance intensity of smells of flowers - he got very good responses from people he sent it to. But they had decided he was not going to get this doctorate, and they said, 'No, this was absolutely unscientific, we won't give him the PhD.' That had never happened before and it never happened after. If they pass your thing and it's OKed .....That was a terrible shock to him, he was sure he was going to get it. It destroyed him, actually, but he recuperated. It was a terrible shock, that he didn't get this for that work he had done for two years."

JP: You heard this from him?

"Oh yes."

JP: Was it the professor of zoology?

"She intrigued against him. But of course this was in botany. They said, he has made a name for himself in Germany, he's not the kind of person we like here. So they found a way of preventing him from getting his doctorate. It was most unfair."

JP: This was the only time in Norway....?

"Yes, the only time, it's unheard of. He protested, but they said, 'We can't accept it.' It was a chance, it was something he'd discovered about the fragrance intensity. They found that as a good reason. A bit later he translated it into English and sent it to various scholars in America and got good responses from them, that it was of scientific merit, but it was too late. He tried to take it up with the university on that ground, but they refused, they said, it's so long ago....it's not possible.

"Then his great triumph. Of course he was very well known in Norway. To enter the university of Oslo you had to take something called preparative exams which include logic, history of philosophy and psychology - there's a huge volume written by a relative of my father called Harald Schjelderup - a textbook of psychology - with several pages devoted to my father's work - and so all students in Norway had to learn about it. And of course there are often articles about my father in Norway, so he was quite well known in Norway. Then of course the great thing for him was that he got an honourary Doctorate of Science from the University of Copenhagen in 1956. He was very proud of that. Not very many people get that. So that was a great triumph for him. But that was in Denmark. He really felt downtrodden by -justifiably so - by the people who didn't like him. He was a likeable person, he was not offensive."

JP: Did he do any observations on chickens in Germany?

"No he had done all his work then - he did it on other fowls there - on crows, he had some crows. And then he wrote some huge books on insects."

JP: Did he have chickens when you were a boy?

"No. Here is a paper from 1913, called 'The voices of Chickens: a contribution to the psychology of chickens.' That's his earliest work, he classifies their language, quite brilliantly, I think - I read it - he was 19. When my father died I found stacks of his notes, I burnt most of it."

JP: He did all his studies on chickens when he was a child?

"Yes, he observed them while he was a child; after that he wrote it down. For his thesis for his zoology degree. She had to accept it, but she didn't like it; she liked anatomy, not psychology."

JP: What year did he take his degree in Zoology?

"1917."

JP: Did he talk about human hierarchies?

"Just generally; he said these things apply to a more refined degree in human society, but not in any detail."

JP: How did it (not getting his doctorate) affect him?

"I remember - I was ten or eleven, it was 1936 - he typed it - we had a new typewriter - I helped him, so when it happened he couldn't believe it - 1937, I think. And of course I talked to him very sympathetically."

JP: He was pecked by his mother and by his professor?

"That of course created a sort of inferiority complex, also a very self-centred thing. He felt that he was unjustifiably denied his rights, and that came to the exaggeration, the opposite, that he had to become recognised in certain ways, so he was very much occupied in establishing his importance in so many fields, and making that known. So he would correspond, he was a very diligent writer. He didn't have to teach. I don't think he would be a good teacher at all, so he had time to write, and he produced 100 different works. And he wrote a lot of poetry which he thought he was good at but he wasn't. So that kept him alive, doing this. He worked hard, but he branched out into other fields, into botany, and these insects. He found the same things among the insects."

JP: Was he a professor?

"He always wanted to be a professor. There is a place in Paris - Université Nouvelle de Paris, I think you pay them some money or something...he paid them some money and they made him Professor Extraordinaire de Sociologie - in 1931. But of course a teacher in high school in France is a professor. So he got it anyway and he was very proud of that."

JP: Did he study in Paris at all?

"No. He spoke fluent French. He had a French nurse or tutor as a child. He spoke fluent German and pretty good English, enough to read Julius Caesar; it was his favourite book."

JP: He was academically isolated?

"Yes. He had a friend who was a psychologist - Ingvar Raknes, interested in psychoanalysis - and other offbeat people who were outsiders. He took to them and they took to him.

"My interest in music comes from both sides. One of his mother's sisters, she was a pianist, quite a good one, and there are famous musicians and composers by the name of Schjelderup. And on my mother's side, she was a good singer, and her sister was an excellent singer, not a professional but very good, and her uncle was a professional singer and a singing teacher, so I got it from both sides. But it skipped my father; he hated music. He could play the piano but absolutely unmusically as a child; and he hated to go to concerts. And, I tell my students this story (I don't say it's my father, but it's a good story which proves the unmusicality of somebody), I tell them, a relative of mine, this happened, after the war we lived on a hill near a school, and the school band came and played a song that was very popular - a song called "Norway, in red , white and blue". And then my father said, "How wonderful to hear our National Anthem again." Yes, he couldn't hear that it wasn't the National Anthem. And when we sang Christmas songs, my father would sing all the words but he would sing on one note. And my mother and I looked at each other and laughed, and he thought he was singing the song. So the musicality just skipped him completely. But he had a sense of rhythm. I came back once before the war and my father was listening to jazz music on the radio - I got a shock - to write poetry you have to have a sense of rhythm, but he hated classical music.

"He wrote things on mathematics which were published. And he studied this awfully difficult thing called mechanics, so he was quite versatile. He learnt all those chemical formulas and remembered them to his death, and the name of any kind of animal or flower in Latin - he was very good at Latin. He spread himself out too much; he was too much a romanticist. I have lots of almanacs, for every year, and he would write down lots of ideas and poetry. He had ten huge scrapbooks in which he would paste anything that referred to him in clippings. I gave it to the university library. He wanted me to do that. He spent a lot of time on it."

Major Publications

Schjelderup-Ebbe, T., 1922, Beitrage zur sozialpsychologie des haushuns. *Z. Psychol. 88*:225-252.

Schjelderup-Ebbe, T., 1935, Social behaviour of birds. In *Handbook of Social Psychology*, C. Murchison (Ed.). Worcester, Mass.: Clarke University Press, pp. 947-972.

Schjelderup-Ebbe, T., 1975, Contributions to the social psychology of the domestic chicken. In *Social Hierarchy and Dominance. Benchmark Papers in Animal Behaviour Volume 3*, M. W. Schein (Ed.). Philadelphia: Dowden, Hutchinson & Ross.

Guarding of Females and Internal War

By Frans Roes

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Male potential reproductive success in many species exceeds female potential reproductive success. This results in female reproductive capacity being scarcer for males than vice versa, and therefore most species show more intense male-male than female-female competition.

One tactic in the competition between males is the sequestering and guarding of female mates. Daly & Wilson (1988: 521) remark that the utility of this male tactic increases, (relative to alternative tactics like maximizing copulatory contacts) in species with biparental care, since parentally investing males can be fooled about paternity.

Apart from male parental investment, group structure may also influence guarding by males. If a species is characterized by groups with only one male, the logical thing to do for this male is to chase off other, intruding males. But if groups are multi-male, multi-female, fighting all other males might be difficult if not impossible. Male guarding behavior therefore sometimes shifts in part to controlling the behavior of the females themselves. This controlling behavior may include threats of violence against the female mate, and actual violence.

Primate male violence against the mate as a tactic of guarding is known in baboons and vervet monkeys (Smuts, 1987: 406). During intergroup encounters females from the resident group are chased and attacked in an attempt to minimize contact between them and males from other groups. Breeding male hamadryas baboons persistently herd their females away from bachelor males. "Whenever a female strays too far from her male, he will threaten her by staring and raising his brows. If she does not respond instantly by moving toward him, he will attack her with a neckbite (Kummer 1968). The neckbite is usually symbolic - the male does not actually sink his teeth into her skin - but the threat of injury is clear" (Smuts, 1992:4). A hamadryas troop may contain as many as 236 individuals (Stammbach, 1987: 115).

In humans, male paternal investment and group size are unparallelled among primates, and therefore one would expect extreme guarding by males. I am not sure if humans are unique in this respect, but humans may indeed be the only primates in which males sometimes kill their own mates.

Disputes over women are sometimes perceived as the principal cause for violent conflicts between males. The most common explanation (Chagnon 1988:986) for the initial cause of fighting among Yanomamö Indians of Amazonas is simply: "Women". Conflicts between males are especially likely to arise when a male has an affair with another's wife. It would seem, therefore, that if females are prevented (by their males) from contacting other males, such conflicts would be less likely to occur. In other words, the hypothesis put forward here is that male guarding of female mates correlates with a decrease in male-male conflicts.

Data from the *Standard Cross Cultural Sample* (SCCS) were used to test this idea. The SCCS contains data on 186 mainly non-Western societies, selected to represent the known cultural types of the world. As the independent variable, supposedly measuring the extent to which women are guarded by their males, the variable "Frequency of extramarital sex - female" was chosen. The values of this variable are: (1) Universal, (2) Moderate, (3) Occasional, and (4) Uncommon. As the dependent variables, supposedly measuring male-male conflicts, "Conflict (social or political) in the local community" and "Frequency of internal war" were chosen. Here are the correlations:

	Conflict in local community	Frequency of internal war	
Frequency of extramarital sex - female	-.1370 32 p= .227	.4617 49 p= .000	(Coefficient) (cases) (1-tailed)

"Conflict in local community" shows no significant relation with the independent variable. Assuming that the frequency of extramarital sex by females is determined in part by male guardmarital sex - female" and "Frequency of internal war" could, however, be considered some support for the hypothesis that male guarding of females correlates with a decrease of male-male conflicts. One may even speculate that the guarding of females, both in humans and hamadryas, facilitated large group formation, as larger groups probably tend to split as a consequence of male-male conflicts over females.

If the hypothesis about the relation between male guarding of females and frequency of male-male conflicts indeed holds, what seems to be in need of an explanation is why modern societies are such obvious exceptions.

Alice Fuldauer, who wrote a book about spousal homicide, put me onto the above subject when she asked if male animals ever kill their female mates.

References

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BOOK REVIEWS

The Left-Hander Syndrome: T he Causes and Consequences of Left-Handedness

By Stanley Coren. New York: Vintage Books, 1993, $12.00 (ppr.).

Reviewed by W. C. McGrew, Dept. of Sociology and Anthropology, Miami University, Oxford, OH 45056 USA.

What might a human ethologist find interesting and useful in this book? After all, every introductory textbook tells us that handedness is one of those cut-and-dried subjects (like imprinting?): Right-handedness (about 90% of any population) is said to be both universally and uniquely human. All cultures have it in species-specific form, and all other species, even our closest relations, do not have it - instead they are individually lateralized or ambilateral. So, what's to know?

First, both of these generalizations are suspect, at least based on the evidence summarized by Coren, and second, ethology is remarkable for its *absence* from the whole field of laterality research.

Like most authors of recent books on handedness (e.g., Corballis, Bradshaw & Rogers), Coren has a chapter on handedness in animals. What he reports is the standard story (à la J.M. Warren) that whether mice, rats, cats, or monkeys, only some individuals in any group or population or species show consistent preference for one paw or hand. (Those that show no consistency are presumably random operators). Of those that do show consistency, there is more or less 50:50 division into left- or right-siders, or so goes the story. (In fact, there is suggestive species-level footedness in several parrots - recall that these birds use their feet as manipulators). Coren mentions, but then ignores, a major challenge to the received wisdom, mounted in 1987 by McNeilage et al. in *Behavioral and Brain Sciences.* Right or wrong, they rekindled interest in the subject, and primate ethologists have since been collecting data to test MacNeilage et al.'s "postural origins" theory with enthusiasm.

What about the universality of human right-hand dominance? Coren correctly points out that no culture has ever been found to be left-handed. But cross-cultural analyses show much more variance than is generally realized: Some cultures are virtually 100% right-handed, while others are only about 75% so. Most societies fall in between, at about 88-92% right-handed. Whence come this variation? A prima facie hypothesis is that cultures vary in terms of their adherence to left-handed "taboos". (Coren remarks that in all societies that distinguish between right and left on imputed grounds of good and bad, the left hand is *never* good). However, there is no ethological evidence of a correlation between strength of preference and degree of taboo, and there are some data to show that when the left-hand is forbidden specifically for, say, eating or writing, such prohibition does*not* generalize to other activities.

By now the reader of this publication will be asking for the ethological evidence on the matter, that is, for data on spontaneous hand use over the variety of everyday activities in daily life, as performed by both sex and all ages. The answer is that there is virtually none. As Coren's huge body of research makes clear, most data on handedness (or any other aspect of laterality: footedness, eyedness, earedness, etc.) are collected by administering questionnaires to undergraduates. Validity is either neglected or dealt with by asking subjects to perform a few, simple tasks in the classroom, e.g., handwriting. Tasks of multiple function or multiple difficulty or basic independent variables such as posture or object manipulation are never naturally observed, but only artificially elicited. The complexities of bimanual tasks, e.g., redundant versus complementary, are ignored. In summary, laterality of function is an untapped area for ethological research.

However, there are plenty of other reasons to read this witty and well-written book. Coren and his colleagues were the ones who found that left-handers live much shorter lives - a finding that engendered controversy, not surprisingly! This finding is well-discussed and convincingly defended. The fashionable myth of "right-brain-thinking" is debunked, in a chapter entitled Psycho-Neuro-Astrology. Most importantly, the last four chapters are

devoted to applied matters, that is, to the hazardous and persecuted life of an invisible minority, the left-handed. As a right-hander married to a left-hander, this reviewer was aware of some of the issues of handism, but there is much to be faced up to, e.g., safety implications of utensils and machinery designed for the dominant right-handers.

Biology and Cognitive Development: The Case of Face Recognition

By Mark H. Johnson and John Morton. Basil Blackwell Press, Walton Street, Oxford OX2 6DP, UK and 200 Madison Ave., New York, NY 10016 USA, 1991.

Reviewed by Linda Mealey, Psychology Department, College of St. Benedict, Collegeville, MN 56321 USA.

The authors open the preface to their book explaining that they "struggled to balance two different themes: the general theme of how biological factors can be usefully brought into studies of cognitive development, and the specific theme of the developmental mechanisms that underlie face recognition in the human infant" (p vii). The book addresses both themes well and, as the preface later notes, a reader interested in only one or the other theme can skip certain chapters and experience no loss in fluidity of the text. For those interested in both themes or those interested in reading about face recognition as a specific example of the general theme, I can report that the "struggle" of the authors has resulted in an outstanding product: the results of a variety of studies from cognitive psychology, ethology, and neuroscience have been integrated into a complete and satisfying model of the development of face recognition.

Chapter one convincingly explains the authors' rationale for their eclectic approach to development. Chapter two then presents a brief descriptive summary of research on the development of face recognition in infants. This is followed by a long and thorough chapter on the visual aspects of filial imprinting in fowl, much of which is based on research by the first author. Chapter three demonstrates that in fowl, at least, there are two separate brain mechanisms devoted to two different sub-routines that subserve the imprinting process. It is hypothesized that the development of human face recognition is, likewise, a two-step process subserved by two different brain mechanisms. Differential rates of maturation of these two brain areas can, according to the authors, explain the otherwise confusing U-shape that is seen in infants' preference for face stimuli as they age. Chapters four and five describe the model as it pertains to humans, and provide evidence in its favor. Chapter six, the final chapter, puts the model and research supporting it into an ethological framework, and suggests that other topics in cognitive development might be studied the same way.

With fewer than 150 pages of text and many boxes, insets, and figures, this monograph can be read in three or four hours. Still, it is not a book designed for leisurely reading, nor is it for the interested layperson. While the material is lucidly written, the topic is technical and the content dense. It is intended for academics interested in (a) cognition or cognitive development, who will find that the book provides a good philosophical argument and a strong practical example for using an integrated approach, and (b) the neural mechanisms underlying face recognition. Graduate students, in particular, will find it enlightening, especially those who study child psychology but who have not been exposed to much ethology.

Membership Renewals for 1995

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Biology and Cognitive Development is the third of a new Blackwell series on cognitive development (the first: *Autism* and the second: *Language and Thought in Normal and Handicapped Children*). If the other volumes in the series incorporate as much ethology as this one, they will collectively be a welcomed and valued addition to an unnecessarily sparse literature.

The Chosen Primate: Human Nature and Cultural Diversity

By Adam Kuper. Harvard University Press, 79 Garden St., Cambridge, MA 02138 USA, 1994.

Reviewed by Harmon R Holcomb III, Dept of Philosophy, University of Kentucky, Lexington, KY 40506-0027 USA.

From 1985 to 1993 Kuper served as editor of *Current Anthropology*, a forum for debate over how to reconcile evolutionary theory with human cultural variation. Focusing on this problem in his new book, he undertakes "a fundamental rethinking of some of the central questions in the anthropological tradition" (p.viii). His reading of history is so engrossing that the book is hard to put down.

With Kuper as our guide, we meet a host of figures: Darwin, Huxley and Wood on criteria for counting a species in the genus *Homo* ; Dubois, Dart and Wood on the Piltdown Man fraud; Johanson and Richard Leakey on Lucy's skeleton; Washburn and the revisionists on Man the Hunter; Binford and the archeological tradition on the Neanderthals and the evolution of human culture and language; Harris and Rappaport on utilitarian and meaning analyses; Galton and Pearson on eugenics; Burt's intelligence testing theory and his duplicitous results; Lorenz, Fox, Tiger, Wilson and Trivers on evolved human nature; Changon's ethnography of the Yanomamo; Cavalli-Sforza on population movements; Westermarck, Freud, and Levi-Strauss on incest avoidance; conservatives and feminists on the universality and value of the nuclear family; Mead and Freeman on Samoans and cultural relativism; Mead, Gewertz and Errington on male-female differences; Hobbes and Evans-Pritchard on civil society and government; and finally, Malthus and world-system theory on population growth and our future.

The main guiding question of interest in these issues is whether there is a Darwinian account, not just of human origins, but also of human nature and of all the ways of life humans have tried out over the last 150 millenia (p.1). Darwin's views are somewhat consistent with all three basic positions on human nature that have been taken, in various versions, during the history of anthropology (Chapter 1).

First, "the biological party" holds that human beings are just another primate species: that human behavior is a modification of the habits of other apes; that there is a universal, genetically transmitted human nature; and that culture follows biological needs and instincts. Second, the "culture school" holds that humans, being members of various cultures, are unlike any other primate species. Human behavior is the product of cultural development independent of our evolutionary heritage, and freely varying cultures transcend the bounds of any postulated universal human nature. Third, the "interactionist view" holds that feedback between nature and culture occurs; for example, the development of the brain yields language and tools which in turn stimulate further brain development. As organisms having brains and cultures, the course of human history results from the interaction of both modes of inheritance.

Some readers will be disappointed that Kuper refrains from offering his own positive solution. What people need, Kuper says, is not another big idea, but a healthy dose of skepticism and a guide to the literature of big ideas by someone who has none. His project is to "explore the great questions about human origins, human history, and human nature, sketch the investigations they have inspired, and review the answers currently on offer in the human sciences. There are often no sure answers, and it is essential to appreciate why this so, to grasp the difficulties, to maintain a sophisticated skepticism" (p.18).

While this call for skepticism and attention to difficulties is to be applauded, Kuper's attitude about big ideas is self-undermining. Instead of a Big Idea, Kuper gives

us an undeveloped Big Critique.

His account of recent work on human origins is a case in point. European archeology had portrayed Neanderthals as our ancestors, even as proto-Frenchmen. In 1989 Binford claimed that the transition in Europe from "paleoculture" to a fully modern "culture" occurred much later than had been supposed (between 45,000 and 30,000 years ago). The great spurt of cultural creativity corresponded to the development of language. Neanderthals had a culturally sterile language but, being physically indistinguishable from ourselves according to the fossil record, had the physical capacity for language. They were displaced in a relatively short time (say, 10,000 years) by other humans (our ancestors) originating, as Darwin thought, in Africa.

Kuper's interpretation is that "The hiatus between the evolution of modern humans and the development of culture leads to a conclusion that is of pivotal importance for this book. Physical evolution and cultural development did not march hand in hand. The physical capacity for culture had been in place for millennia before modern human culture began its explosive development" (p.90).

Although Kuper doesn't say why this conclusion is of pivotal importance, the lesson seems to be that physical evolution does not determine cultural development, and that this puts limits on the extent that Darwinian, biological approaches can explain human behavior. He echoes the standard refrain, "But once cultural development got into its stride, it operated at a rate quite foreign to the slow, blind mutations of biological evolution" (p.90). On his analysis, "A choice presents itself, therefore, between two contrasting views of the human condition. The biological measure draws attention to continuity with other primates; the cultural measure, however, shows a sharp break at the start of the Upper Paleolithic ... Each of these two contrasting views has its own particular scales and poses distinctive questions. The biological history of the human species is measured in hundreds of thousands of years, and is programmed, perhaps, in our genes" (p.91).

This critique has its own big assumptions, which most Darwinian scientists would find uninformed and unacceptable. It is wrong to assume that "evolutionarily significant traits" are restricted to traits that can't culturally vary, i.e., that are "physical" or "caused by genes" or "invariant over short time spans of intergenerational change" or "not learned by individuals" or "invariant upon changes in the physical and social environment" or "universal among species members" or "not developmentally contingent on input from other species members".

Perhaps the simplest reason why these sorts of assumption are wrong is one of consistent application of methods. Once you examine biological studies of nonhuman species closely, you'll find many that don't obey these restrictions. If so, it doesn't make sense to use these restrictions to guide human evolutionary studies. Kuper's critique is true of a caricature, false of Darwinism today. (For a fuller demolition of such misconceptions, see Alexander (1979), Crawford and Anderson (1989), Holcomb (1993), and Barkow et al. (1992).)

Much Darwinian research uses the criterion of differential reproductive success as a way to understand the goal structure of human behavior. Doing so does not require us to think that humans are either like or unlike other primates in particular behaviors, and to think so confuses explanatory principles with features of the behaviors explained. We find population-level variations among humans in behavioral acts, tactics, and strategies for surviving and reproducing. We treat culture as the social environment to which we adapt, and adaptations work on various time scales in order to match regularities in social and natural environments. Rather than match up the genetic/learned dichotomy with a biology/culture dichotomy, we seek to show how variations reflect the cumulative effects of past selection, and only under special conditions posit physical or genetic changes that match up to cultural changes. The fact that humans are organisms having minds and cultures is taken into account by current Darwinism at the start, as variable conditions in terms of which evolutionary principles apply to us.

There is, then, some overstatement in Kuper's statement that "sociobiology leaves aside the problem of variation" (p.148). One source is the assumption behind the claim that "the potential of human sociobiology is obviously not as great as it may have seemed twenty years ago" because "it will not replace the other human sciences" (p.148). The aim, Darwinian anthropologists typically insist, is

not replacement, but to incorporate evolutionary approaches into existing human sciences.

Kuper endorses Washburn's judgement: "Human ethology might be defined as the science that pretends humans cannot speak" (p.148). He appeals to adaptable symbolic communication, technological inventiveness, and social exchange as explanatory factors, which are conceived as sources of cultural variation. The conception is circular, since "culture" is defined in those very terms (p.90). Missing here is the idea of evolution by natural selection operating on psychological capacities presupposed in learning, symbolic communication, and social exchange. He derives the variable content of human behavior from culture, not from domain-specific information processing capacities. His point that the physical basis of human speech existed 60,000 years (2,000 generations) before full blown modern culture suggests that it took some time for the mind to evolve and become adapted to other humans, and that "the missing link between biology and culture" is evolutionary psychology.

Kuper (p. 92) pictures a watch whose second hand measures daily affairs of individuals (everyday explanation), whose minute hand measures the vicissitudes of particular communities over the course of a few generations (cultural explanation), and whose hour hand measures the unfolding of our destiny as a species (biological explanation). On one hand, the watch metaphor faces a reductio ad absurdum. Biologists would be surprised to learn that changes of nonhuman individuals over their lifetimes and changes in variation within a nonhuman population over several generations, by parity of reasoning, are beyond the scope of biology! On the other hand, the watch metaphor suggests that there is no incompatibility between cultural and biological approaches. Kuper's ultimate point is that "A great deal of confusion has resulted from failing to distinguish between these different histories..." (p.93), implying that the three positions in terms of which he organizes his own book--the biological party, the culture school, and the interactionist view--are all conceptually confused.

In sum, Kuper's sophisticated skepticism delivers an attack on a caricature of Darwinism, his refusal to generate his own big ideas serves to perpetuate his own unexamined big assumptions, and he frames his whole book in terms of a triad of positions with which he disagrees. Because of these flaws in its analysis and guiding vision, the book does not help us answer the main question of whether Darwinian theory can explain human cultural variation. The book is useful as a guide to the history of thought on the topic. It is very well-written and interesting, accessible to a wide audience, deceptively subtle in its simplicity, and full of essential information everyone interested in Darwinism and anthropology should know.

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Fremdenfeindlichkeit. Biosoziale Grundlagen von Ethnozentrismus

By Anne Katrin Flohr. Westdeutscher Verlag, Postfach 58 29, D-65048 Wiesbaden, Beiträge zur sozialwissenschaftlichen Forschung, Bd. 124, Germany, 1994, 271 pp., 46.00 DM (ppr.), in German.

Reviewed by Alain Schmitt,
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Ethnocentrism is universal in (historical) time and space, may hitchhike on any event, is persistent and tends to destructivity. This is the message to biologists of Anne Flohr's *Xenophobia: Biosocial fundamentals of ethnocentrism.* Her message to

(German) political scientists, to whom the book is explicitly addressed (biopolitics is only *in statu nascendi* in Germany), is that ethnocentrism is rooted in our nature. She argues that there are biological and psychological theories and data in favor of this thesis, and that political action may prevent or change ethnocentric behavior if policy makers learn to integrate both its biological and cultural causes in their thinking. For example, one suggestion is to split rather than to lump ethnic groups, when in doubt). Flohr's aim is to stimulate interdisciplinary research. The main dilemma that separates those going back to human nature and those looking at historical, cultural and other contextual variables is that "naturalists" cannot explain individual cases, and that "culturalists" are unable to explain the patterns common to all cases. One has to combine both approaches to resolve the dilemma.

What factors determine ethnic identity? Individuals belonging to one ethnic group are characterized by sociocultural similarities (language, behavior, etc), social and spatial isolation as a group, feelings of belonging together, and having a common history and a group identity. Biological similarities may also be of importance; both in Germany and in the US, coloured immigrants are discriminated against. The single most important element is belief in a common ancestry. Ethnic membership is typically not chosen, but given by birth.

Ethnocentrism is a tendency to have cognitive-perceptual and emotional biases in favor of one's own group and against members of another group (out-group). Out-groupers are perceived as inferior, subhuman, primitive. Whereas there is inherent hostility against and distrust in members of the out-group, in-groupers are met with loyalty (xenophobia is a broader term which means the negative response to a stranger, even if he/she is a member of the in-group). The fact that 90% of the current UN member nations are multi-ethnic, many of them ruled by ethnic groups, clans or families basing their power on nepotism, clearly reveals the political importance of dealing with xenophobia and ethnocentrism.

The first half of the book makes the following claims: First, ethnic conflict is ubiquitous and phenomonologically variable (e.g. India and the death of the Ghandis; Hitler's anti-Semitism; the former Yugoslavia; Ireland and the IRA; Black and White power and Chinatowns in the US; clan feuds in Africa; Israel and Palestine; Ancient Greece and its wars with various "barbaros"). Second, ethnocentrism is relevant to most ethnic conflicts. Third, current political theories are inadequate; most of them consider ethnocentrism as an abnormality, not as a universal disposition. A particularly convincing example is that political theory was completely unable to predict both the disintegration of the communist bloc and its consequences. Whereas political scientists predicted a quite peaceful state of the Eastern world after the fall of the Berlin wall, there are today more than 70 ethnic conflicts in the former Soviet Union alone.

In the second half of the book, Flohr collects evidence on the possible biological foundations of ethnocentrism and xenophobia. We are adapted to living in small, face-to-face kin groups. Thus, we have a quite limited amount of sympathy to distribute among our fellows. We tend to prefer kin over non-kin, and hence to be nepotistic and xenophobic. This behavior results in genetic advantages if performed in kin groups (inclusive fitness theory, genetic similarity theory). Ethnocentrism thus appears to be a generalization of nepotism (our psychological make-up often leads us to immoderate generalization), with culturally determined similarities within an ethnic group acting like genetic markers.

But, Flohr continues, ethnocentrism may also be a generalization of our tendency to negatively react to any stranger, an adaptation to strengthen the mother-infant bond when the infant becomes capable of actively inspecting the surroundings. This is supported by the fact that shyness toward strangers develops in all human infants between 6 and 10 months of age, whether they had bad experiences with a stranger or not. Moreover, there are many animal examples of xenophobia (cave: many of these can be interpreted as nepotism).

A third possibility is that in-group favoritism is based on cost/benefit thinking and reciprocity. We have a much higher chance of future interaction with a member of the in-group than with a member of

the out-group. This makes out-groupers candidates for exploitative behavior, thus leading to mistrust between strangers or members of different groups.

Furthermore, many of our psychological characteristics strengthen any ethnocentric tendency in place. These characteristics include our tendency to think in simple dualities (black and white thinking!), to confirm rather than to falsify existing cognitions (we collect data supporting our prejudices), and to better recover well known facts from our memory (availability heuristic; we first compare new information with our prejudices and only then with more exotic stored information). Thus, we are caught in a net of self-fulfilling prophecies when we deal with new information. In everyday life, people, especially strangers, are the most arousing sources of new information, and thus potentially the most likely object of the many centrisms of our mind. Whether or not these characteristics are phylogenetic adaptations is being debated. Most investigations have used Caucasian (student) populations; thus even the transcultural stability of the results has seldom been demonstrated.

The book is up to date, is based on very thorough investigation, and is written clearly and with convincing logic. As far as I know, no important modern political, psychological and biological theories and bodies of data are omitted. All chapters are concise overviews of the most important findings, theories and missing links. Although it is intended to be an instigation to further multi-disciplinary research, one problem diminishes its utility as a scientific tool: there are 436 (!) footnotes containing mainly references. An important but to me unknown fraction of these references is not repeated in the list at the end of the book.

Overall, *Xenophobia* is worthwhile reading for everyone (lay person, student and researcher) interested in biology, psychology, and political action.

Membership Renewals for 1995

It is time to renew your membership for 1995 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 the current year, it is time to renew your membership.** For financial reasons, renewal notices are not usually sent. Those who do not renew their memberships will be removed from the membership list. Please report any errors, change of address, etc. to the editor. Current dues and directions for payment are given on the last page.

Newsletter Submissions

Anything 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 (the British editor covers English-language books published in Europe). Submission should be in English, on paper and, if possible, also on diskette. 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.

Newsletter 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 newsletter never implies the truth of those ideas, ISHE's endorsement of them, or support for any policy implications that may be inferred from them.

ANNOUNCEMENTS

European Sociobiological Society

The theme of the 18th meeting of ESS will be "The Darwinian Heritage and Sociobiology." The venue is Christ's College, Cambridge, UK, 3-6 August 1995. Registration deadline is 31 May. For information and registration form contact R.M. Allott, 5 Fitzgerald Park, Seaford, E.Sussex BN25 1AX, U. K. , tel./fax 44-323 492300., E-mail rmallott@percep.demon.co.uk.

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International Ethological Conference

The 24th International Ethological Conference will take place in Hawaii 10-17 August 1995. Further details will follow. There will be two symposia on human behavior in the official program. ISHE members are encouraged to present results of observational research, especially in the areas of behavior-environment interaction and sex differences. The 1997 conference will be hosted by Karl Grammer in Vienna.

Politics and the Life Sciences

Many ISHE members are familiar with *Politics and the Life Sciences.*, the international journal of the Association for Politics and the Life Sciences. Published semi-annually in February and August, *PLS* is a peer-reviewed, multidisciplinary journal with readers in more than twenty countries. Its mission is to advance knowledge of politics and promote better policy making through multidisciplinary analysis that includes the life sciences. Membership in APLS includes a subscription to the journal, a semi-annual newsletter about the association, conferences and other developments; a yearly membership directory; and other occasional mailings. Membership dues for 1994 are $35 for individuals ($20 for students). For additional information about the journal, membership, or submitting a manuscript, contact Gary R. Johnson, Editor, *Politics and the Life Sciences,* Lake Superior State University, Sault Ste. Marie, MI 49783-1699 USA; tel. 1-906-635-2757; fax 1-906-635-2111, e-mail pls@acs.saultc.on.ca.

Sexual Orientation Meeting

A meeting on the **Biological Basis of Sexual Orientation and Sex-Typical Behavior** is scheduled for 25-27 May 1995 in Minot, North Dakota. Contact Dr. Lee Ellis, Minot State University, Divison of Social Science, 500 University Ave. West, Minot, ND 58707 USA, tel. 1-701-857-3241.

International Twin Congress

Richmond, VA is the site of the Eighth InternationalTwin Congress, to be held 28 May -1 June, 1995. Contact Eighth International Twin Congress, Dept. of Human Genetics, Virginia Commonwealth University, Box 980003, Richmond, VA 23298-0003 USA. The Congress will be followed by the meeting of the *Behavioral Genetics Association*, 1-3 June.

Patricia Draper

ISHE member Patricia Draper is President of the Society for Cross-Cultural Research. The official journal for the Society is *Cross-Cultural Research*, formerly *Behavioral Science Research*.

Delwart Foundation Award

The $10,000 award of the Jean-Marie Delwart Fondation in Human Ethology and Cultural Anthropology was won by Tim Ingold, University of Manchester, for his work on the field of social anthroplogy and especially for his analysis of the relationships between technology and social relations in modern society.

Mailing Labels of ISHE Members

Sets of mailing labels of the membership, over 500 in number, are for sale for legitimate scholarly purposes. The rate is $0.35 each, or about $175 for the entire list. This is a good means for publicizing a book or journal. ISHE members pay the reduced rate of $0.25. For information, contact the editor.

Observational Research Software

A new software package for observing, coding, timing and analysing series of events has been marketed, "The Observer for the Mackintosh." For information conact Gonja J. J. Hikspoors, Noldus Information Technology, P. O. Box 268, 6700 AG Wageningen, The Netherlands, tel. 31-8370-97677, E-mail noldus@rcl.wau.nl.

ISPPM

The International Society for Prenatal and Perinatal Psychology and Medicine will hold its Eleventh International Congress 11-14 May 1995 in Heidelberg. For information, contact A. and J. Bischoff, Friedhofweg 8, D-69118 Heidelberg, Germany, fax 62 21/80 42 77.

HBES-L

The electronic bulletin board of the Human Behavior and Evolution Society has a new address: HBES-L@LISTSERV.ARIZONA.EDU. Operated by Gene Mesher, this service is kindly extended to "kindred sprits" of HBES on a trial subscription basis. Listserve commands should be addressed to: LISTSERV@LISTSERV.ARIZONA.EDU.

CURRENT LITERATURE

March 1995

Interested in possibly reviewing one of the books below or some other suitable book? Please contact the appropriate book review editor (see Editorial Staff box).

Submit items for Current Literature to Bob Adams (see box). Please be sure that the item has not yet appeared.

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