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RESPONSE TO AN INTERVIEW

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In his March 1997 *Human Ethology Bulletin* interview of Richard Dawkins, Frans Roes asked some very incisive questions concerning slavemaking in ants. In response, Dawkins, as expected, gave well-articulated answers which, however, would have been more compelling if he had also alluded to empirical data to back them. After a question on manipulative signaling, Roes probes into less scientific issues with queries about religion, social manners, politics and morality, areas in which Dawkins has many firm opinions.

A general epistemological issue raised by Dawkins' answers to the latter questions concerns the possible disjunction between such labels as natural and unnatural, genes and memes, body and mind. I assume most, if not all evolutionists, maintain there is no disjunction. The natural and unnatural, genes and memes, and even the body and mind are at bottom ultimate products of evolution, seamlessly connected in one way or other in very complicated but causally necessary ways to be sure. Why? Because all things have material origins and therefore can never be causally dissociated from them --even memes and the mind. Disjunctions in the biological world just don't exist.

Now, if there indeed is no disjunction, if there is a perfectly smooth continuum between what is natural and what is unnatural (both have evolved), how can Dawkins claim he wants to do such a ".. very un-Darwinian" thing as contribute to a socialist world? How can he abandon ruthless capitalism that favors only self and kin and opt for socialism that supports both nonkin as well kin? How can he even conceive of such a thing with a mind shaped by evolution to serve his genes only? No gene recipe for chocolate eclairs will culminate in potato pancakes.

Of course, some processes of natural selection result in maladaptive phenotypes, but the conscious decision-making of an ardent evolutionist well-saturated with Darwinian memes can certainly alter such phenotypes... unless one learns early that an excellent strategy in certain circles is to openly speak socialist but covertly act capitalist which is surely not the case with Dawkins.

Entertaining such dichotomies as body and mind and gene and meme must be seen by evolutionary biologists as a Cartesian error of enormous magnitude. Material monists are obligated to have no part of such a heresy. I feel fortunate I do not feel this obligation. Being trained in world literature, comparative/experimental psychology, human development and cognition, as well as ethology, has cured me from early tendencies to be a monistic reductionist. It has also stifled tendencies to extrapolate or generalize freely across content areas and species. Actually, the cure makes me feel I am a better scientist since every time I look at phenomena I don't feel obligated to defend a theory. Also, such freedom allows me to disagree with Darwin's claim that "He who understands baboon would do more toward metaphysics than Locke."

(Darwin's M notebook). I wish a primatologist would take Darwin up on this--in cooperation, of course, with members of the metaphysical society.

One does not have to be a supernaturalist to raise the issue of disjunction. Alfred Russel Wallace posed the disjunction question concerning human intelligence when he noted that some human cognitive traits may have escaped natural selection in Pleistocene environments because they appear to have no utility value contributing to fitness. I am aware of speculations about how music may attract members of the opposite sex or appease enemies and how smart people may compete better in acquiring resources. But where's the empirical proof to support such speculations?

While not as socially favored as Darwin, Wallace certainly was no less insightful and no less experienced in the ways of nature. If I remember correctly, Wallace spent much more research time than Darwin with a wide range of cultures. Such experience must count for some insights on cognition as an adaptation that Darwin may have missed, or not missed but felt he had to ignore to keep dangerous disjunctive gaps out of the theory.

In my opinion, Dawkins has embarked, in several respects, on a potentially unfruitful course as exemplified in his interview. Several of his books (e.g. *The Selfish Gene, The Blind Watchmaker, River Out of Eden*) consist of exciting, often brilliant speculations about the implications of Darwinian theory for every living thing. However, frequently much of what Dawkins says has become repetitive (there is no evidence of intelligent design in nature) and dogmatic sounding (so give up the idea of a designer).

With all respect to his brilliant achievements, Dawkins, in my estimation, would do best by elaborating on science's uniquely exquisite, labor-intensive, objective, self-corrective methods that, when used collectively, separate science from other human cognitive activities. With such elaboration, he could more persuasively show how vital science is for understanding the natural order.

From the Editor

After 17 years, the journal *Ethology and Sociobiology* ceased publication with the end of the 1996 volume. It may be appropriate to suggest that we reflect on what the founding of the journal meant to those of us conducting research in these disciplines in the early days.

Ethology and Sociobiology was one of the first journals, perhaps the first, to specialize in publishing research on humans from an evolutionary perspective. Before its advent, researchers found it very difficult to publish work that alluded to evolutionary theory. Some journals, notably in developmental psychology, published work employing observational methods. However, reference to the theory behind using these methods was often taboo. Even today, it goes without saying, opposition to evolutionary theory remains entrenched in the social sciences.

E & S provided not just a forum but a widely respected one. It was carefully and critically edited, and its articles included some of the most widely cited in the behavioral sciences. Many academic libraries subscribed to it. Because of its success, it eventually expanded from quarterly to bimonthly publication. Occasionally, an issue was rather thin due to the flagging performance of us contributors, but Michael McGuire persevered and without lowering editorial standards. He served as editor-in-chief for the entire life of the journal, providing essential continuity to the field of human ethology and sociobiology.

E & S was tolerant of varied theoretical and methodological approaches, but rigorous in its standards. It fostered respect for our nascent discipline, and did not bend to the political winds to do so. Polemics and other ideologically-tinged pieces were not welcome, but solid data and well-grounded theoretical essays could always find a home. Michael maintained good relations with the various societies of ethologists and sociobiologists in Europe as well as North America, and the journal received submissions from numerous countries.

Due in no small measure to *Ethology and Sociobiology,* biological approaches to the study of human behavior have gained in respectability. The solid work of behavioral geneticists, endocrinologists, neuroscientists, primatologists, ethologists, and sociobiologists has drawn the attention of much of the educated public, to some extent bypassing our resistant colleagues in academia. As never before, ethology and sociobiology constitute major approaches to the study of human behavior. For example, an unusually large number of papers on humans are to be presented at this year's International Ethological Congress in Vienna, and the American Psychological Association convention in Chicago will feature a symposium by human ethologists on observational methods; both meetings are in August.

Now that ethological and sociobiological research is finally being published by many mainstream social science journals, we may lapse into forgetting the pioneering role of *E & S*. But the toehold we have finally gained is due in large measure to the institutional sustenance and moral support provided by that journal.

It is time then to express our thanks to Michael McGuire and his associate editors, Nick Blurton Jones, Bill McGrew, and Peter K. Smith, as well as to the consulting editors, reviewers, and contributors. We know what you have done for us and our field, and we shall remember.

...

At the same time, we wish the new journal, *Evolution and Human Behavior,* success. Its editors, Martin Daly and Margo Wilson, contributed as authors and reviewers to many of the articles that were published in *E & S*, and I can personally attest to their editorial care and helpfulness. Like Michael, they are well versed in all the major disciplines that constitute the evolutionary approach to the study of behavior. They seem to have assembled a fine editorial staff, which includes some of the former editors of *E & S* and quite a few ISHE members, including our president, Charles Crawford.

Heimat-Attachment and Return to the Native Place: Experiences with the Behavioral Biology of Migrants

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"Heimat" means more than just homeland and/or native place: nobody can choose his native land, but one can choose another home or even a spiritual "Heimat". According to Ina-Maria Greverus, "Heimat" is a socio-culturally structured space of reference and satisfaction. Few languages possess a special noun for this concept (among those that do are German, Hebrew, Arabic, Chinese, Japanese and Serbo-Croatian); in most others it is circumscribed, as in English. The "Heimat" term thus is culture-specific, while house, home and the connotations warmth, belongingness, shelteredness are universal.

Basically, however, "Heimat" is usually referred to as the place where one grew up and to which one is emotionally attached. This attachment is similar to habitat imprinting in animals, arising by way of continuous, emotionally loaded association learning. Later in life, people can acquire emotional bonds to other places; however, these secondary Heimat-attachments are conditioned by success experiences - personal, social and/or economic ones - while for the primary attachment, warmth, continuity and stability of home and environment are enough.

It is generally accepted that habitat imprinting serves as a guiding orientation for habitat selection and that the behavioral hallmark of attachment is seeking proximity to its object. Indeed, emigrants who return home often state that they did so out of homesickness. But homesickness is just an appetitive feeling and as such neither the cause nor the reason for a return. Furthermore, return from emigration is not a cyclic movement in the rhythm of the seasons, as of migrating birds or the gnus of the Serengheti; such migrations are found among humans only in nomadic pastoralists and seasonal laborers.

By contrast, return from emigration is sporadic and an exclusively human possibility. A decision to return home depends on the particular constellation of each individual case, on the aspirations, frustrations and preferences of the persons concerned.

There are some general rules about emigration. One is that elderly refugees who have never got over the language barrier of their host country tend to return as soon as possible. But statistics on emigration do not reveal why, under similar political and socioeconomic circumstances, some people go home and others stay.

In order to learn more about these issues, in 1989-90 I interviewed an odd agglomerate of returnees in Berlin; however, from the stories they told me, no generalizations were deducible. But then, by lucky coincidence, there appeared dozens of biographies of emigrants from Nazi times who had become dispersed all over the globe. Now I could analyze a large amount of biographical data on people who had permanently remained in their new country and on returnees. I did not need a computer to see the resultant picture: Stable couples who had built themselves a solid base of existence and had had children, who had grown up in the new country and who had continued to live there usually did not return to their country of origin.

I also got data on a group of 660 Berliner bachelors who in 1952 had been hired by Australian firms and had sailed there as immigrants. About 10% of them returned, almost all during the first 5 years. All the others found steady jobs, established businesses, married (many to Australian women), had children and were completely Australised. Only if both partners stemmed from the German circle of culture could their origin still be smelled in the kitchen. In all, this presented the same picture as did the refugees from the 30s.

What I needed was a control group with a different ethnic background that, if possible, had emigrated under other circumstances. Suiting my needs were the 170,000 Turks in Berlin, who originally had come as guest laborers. Many had come, made money, and gone back. Published research on this population also revealed a "high potential readiness" to return. They themselves regarded their stay as temporary. They invested in Turkey, tried to send their children there to high school or college and, above all, perennially talked about when and how they would return.

Reading these questionnaire results, I could not suppress a cynical smile: This I had heard before, from all the native-born Israelis living abroad. So I took a second look. The research had been ordered by the German government to test the impact of the law encouraging the return of guest laborers. Their number indeed went up, but so did the Turkish population of Germany. Those who had steady jobs brought their families over, imported brides, and produced offspring. Many of them subsequently established businesses.

The first guest laborers today have grandchildren born and raised in Germany. And as for these youngsters, in 1985 70% of them said that they would go back to Turkey whenever their parents would return. But only 5 years later, 75% stated that they aspired to German citizenship. To be on the safe side, I also phoned the Turkish-born social worker who 8 years before had conducted the interviews. I asked her: who had gone back? Her answer was: "No one, but they still talk about it every evening."

All this means that - independently of ethnic affiliation, reason for emigration, legal status, and the self-definition of the migrants - once they are allowed to stay and are granted some modicum of freedom, the dynamics of life take their course. Readiness for migration goes down as biological goal attainment goes up. And what are biological goals if not securing livelihood and offspring? Everyone who did not return had achieved these goals; so why should they move again? So we have here a case where a purely culturally and socio-economically conditioned behavior in the end corresponds to a law of nature.

And where does all this leave our "Heimat" attachment? We found it in the interviews and the answers to the questionnaires. The native home, its landscape and way of life are engrams, permanently imprinted mental pictures. But an imprinted engram is not necessarily followed by imprinted behavior. It is preferred, but if it cannot be followed...one goes on living as well as one can, and the engram remains in the head (and the heart). And here is the great fallacy of sociological inquiries into intentions and attitudes: they reflect the imprinting engrams of those asked, but have no predictive value as to actual behavior.

The "Heimat" attachments of migrants have other manifestations. Roberta Feldman of Chicago found that, when moving, city people prefer other cities, and suburbanites other suburbs. Rural and small-town people state that they would prefer their accustomed type of settlement, but circumstances often force them to move to cities in spite of their preferences. Feldman, as a psychologist, says that people develop settlement identities; in my opinion this is just another name for our habitat attachment, especially since according to a new finding of hers, in the majority of cases the preferred settlement type is also the one of childhood and youth. This study is important from yet another aspect of behavioral biology: it shows that people, like animals, recognize their habitat by its typical features and generalize them.

Another manifestation is the exclusively human possibility to reconstruct the old homeland in a new country. Examples of this are the German villages in Jerusalem, Haifa, Galilee, Missouri, and Columbus, Ohio. Many of them were planned by German architects, complete with gabled roofs and wrought iron fences. Germans have long since left these places, but their "Germanness" remained incarnated in the houses left standing.

There is also an opposite process with the same result: New immigrants tend to concentrate in low-grade urban areas, but then their physiognomies, the sounds of their language, the lettering on the signboards, the garments, shop windows, colors and smells all give the quarter an ethno-specific atmosphere. This in turn attracts newcomers of the same origin, who now can feel at home there - and get help and advice from their compatriots. Thus the Chinatowns and Little Italys came into being, and more recently Little Istanbul in Berlin-Kreuzberg.

Those German Jews who emigrated in the mid-30s and could take their belongings with them furnished their new homes with them; cultivated German cooking, German music and literature; and organized German-speaking social and cultural activities. In short, all that was good and important to them they took with them, whether to the U.S., South America, or (later) Israel.

The most prominent case of this phenomenon is Nahariyah, near the Lebanese border of Israel: Its houses were ordinary Mediterranean ones, but the place, with its delicatessens and sausage shops, German-language newspapers and German-speaking public, had so German an atmosphere that, when cut off during the war of independence in 1947, the saying went: "Komme was wolle, Naharia bleibt deutsch." Well, Nahariyah did not remain German. Just as in the German colonies in the U.S. a century before, the founding population was soon inundated by immigrants (from Eastern Europe and North Africa). All that is left from Nahariyah's quasi-German past is the main avenue along both sides of a rivulet - the only one of its kind in the country - and many German inscriptions on tombstones.

Today, with the global homogenization of architecture and town planning, of clothing, food and lifestyles, in this world of jet planes, the Internet and McDonalds, all this could hardly happen again. Nowadays almost all emigrants can (and do) visit their countries of origin, and watch what's going on there on TV. Even refugees from civil wars, as in Bosnia, can phone their loved ones trapped there. All this, for the refugees of Nazi times, was unthinkable, and more so for the millions who sailed to the New World in centuries past. Modern technology thus has defused much of the homesickness of emigrants and refugees.

So perhaps the stories I collected and told about are nothing more than a chapter of the history of human behavior. But even so, habitat attachment still supplies people emotional anchorage in their residential environment and also fulfills its primary function: by guiding them to select similar settlement types when they move, Heimat enables even highly mobile populations to maintain some stability in their residential habitat.

European and African Reproductive Success Differentials: Could We Have Predicted the Gap?

By Wade C. Mackey
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A basic tenet of Darwinian evolution is that some members of a population propagate more offspring than alternative members of that same population. If so, then, in the event that death rates remain the same, whatever inheritable traits that the more profligate members possess will increase their representation in the next generation. Humans do not transcend any of these tenets. To imagine otherwise is to incubate mischief. This (re)statement of such verities is not intended to be a novel insight. However, occasionally a reminder may trigger an inspiration. And the basics are often worth re-visiting.

A question comes to mind: Is human reproductive success the same across populations? From U.N. data bases, rates of natural increase (birth rates minus death rates) were surveyed across the large land masses of Africa, the Americas, Asia, Europe, and Oceania for 1990-1995 (United Nations 1995). The levels of natural increase per year were *not* equal across these areas:

Area	Rate
Africa	2.8%
Latin America	1.8
Asia	1.6
Oceania	1.5
North America	1.0
Europe	0.2
Mean (unweighted)	1.5
s.d.	0.9

The highest rate of natural increase (Africa) was 14 times the increase of the lowest (Europe). A 14-fold advantage is impressive. Thus, whatever general mosaic of factors has created the African advantage will be more represented in subsequent generations than the general European mosaic.

A survey of the extremes in the rates of fertility, as measured by lifetime births per woman, also illustrates a lack of demographic homogeneity. The countries with the ten highest fertility rates averaged 7.37 children per woman. Seven of the 10 are from Sub-Saharan Africa, and the remaining three are from Moslem countries in the Middle East:

Highest Fertility	Rate
Rwanda	8.29
Kenya	8.12
Côte d' Ivoire	7.41
Zambia	7.20
Oman	7.17
Saudi Arabia	7.17
Jordan	7.17
Niger	7.10
Tanzania	7.10
Nigeria	7.00
Mean	7.37
s.d.	0.45

The countries with the 10 lowest fertility rates averaged 1.51 children per woman (see below). All ten are from Europe (Smith-Morris 1990). The figure of 1.51 children is significantly lower than the highest ten's average of 7.37 (t [18] = 38.3; $p < .001$; 2-tailed).

Lowest Fertility	Rate
West Germany	1.38
Denmark	1.45
Netherlands	1.45
Luxembourg	1.45
Italy	1.45
Austria	1.50
Switzerland	1.55
Belgium	1.55
Finland	1.65
Sweden	1.65
Mean	1.51
s.d.	0.09

Next, let's use a more finely grained filter and divide Africa into two segments: Sub-Saharan Africa and North Africa. Added to the countries of northern Africa (Morocco, Egypt, Algeria, Tunisia, Libya) are the Middle East countries of Yemen, Saudi Arabia, Iran, and Iraq to form a Moslem swathe (n= 9). For both the Moslem swath and Sub-Saharan

Africa, the natural increase was approximately 2.9%. This rate was 81% higher than the world's average of 1.6%. This confirms the point that different sub-groups are more evolutionarily successful than others. The two areas represent examples of current evolutionary "successes," especially when compared with Europe.

Is there an arrow in our paradigmatic quiver which would have predicted the 14-fold differential? I cannot think how ethology/sociobiology would predict such a huge gap in reproductive success. Of course, my deficiency may be mine alone. No precedent would be set by such a unique deficit. On the other hand, there may be a shared lacuna.

If it can be agreed that differential rates of natural increase (i.e., evolution) *are* occurring, then the differentials really do need to be addressed. The "bio" part of humanity seems an unpromising candidate to explain the differentials. With the exception of twinning (Taffel 1995, Derom et al. 1995), the fertility of women (i.e., ability to conceive, gestate, and give birth) seems more homogeneous across cultures than otherwise. Moreover, ability to lactate seems roughly similar across the planet. In addition, mating seems highly equitable. With the contemporary U.S. as an example, virtually all females who manage to survive to puberty will mate at least once, given the large percentages of married women (about 95.5% will have married by age 50-54) and single mothers, to say nothing of other categories of non-virgins (U.S. Bureau of the Census, 1995). Accordingly, the large differences in natural increases across the land masses seem more a result of "cultural" differences than of physiological differences or mating opportunities.

If cultural differences were a reasonable candidate to explain the differentials in natural increases among the groups being compared, do we have a methodological armamentarium that can predict (still "predict", not "explain) "successful" populations versus "unsuccessful" ones? If we do not currently have such an arsenal, we probably need one.

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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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Why Don't We Drop the Darwinian Nametag?

By Bruce G. Charlton

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Isn't it about time that evolutionists grew up and stopped referring to themselves and their theories as 'Darwinian'?

Of course, the modern evolutionary approach derives from Darwin. And Darwin was a magnificent scientist and a fascinating human being and I love reading about him. Indeed many of my favourite books on natural selection and human affairs build themselves around the biography of Darwin (Helena Cronin's *The Ant and the Peacock*, Robert Wright's *The Moral Animal*, Daniel Dennet's *Darwin's Dangerous Idea* for example - all exceptionally fine books.

But sooner or later, this has got to stop. The preoccupation with Darwin, the sheer number of books on him, the way evolutionary theory is always introduced by means of an exposition of his ideas - all this is getting obsessional and pathological.

History and biography are one thing, and science is another. Both are wonderful, but distinct. Today's scientific theories of natural selection must and should stand or fall on the basis of current evidence and arguments; and what Darwin said and thought must take a very secondary place.

Otherwise, as all too commonly happens, debate that purports to be about natural selection ends up being a kind of historical, biographical - even theological - exegesis about what Darwin 'really' said, or meant, or thought, or would think were he alive today.

Rival sects emerge. Both claim Darwin as the one true prophet. One sect asserts that Darwin was primarily interested in complex adaptations, the other that he was primarily interested in explaining the origin of species. Each claims the coveted title Darwinian, each tries to be more-Darwinian-than-thou.

Some scholars assert Darwin's originality, bravery, rigour; others present him as a revisionist, intellectual thief, coward. Revolutionary or reactionary; political radical or conservative; patriarch or liberator? Antievolutionists think that by critiquing Darwin, or by contrasting his views with modern views, they have engaged in a scientific debate.

Give it a rest! 'Darwinism' is too reminiscent of the excesses of Marxism and Freudianism - obsessed with authority, lineage and the cult of a great man. All 'isms' are intellectually second-rate.

For instance, as a worker in the field of 'Darwinian medicine' I am dismayed at being saddled with the name. Surely it would be better to drop the 'Darwinian' as soon as possible in favour of 'evolutionary' or something else neutral.

Anyway, Darwinian medicine - insofar as it is valid, isn't 'Darwinian' any more than physics is 'Einsteinian' (or 'Bohrian, or Feynmanian). Many people have contributed to modern evolutionary theory. We learn current science from current debate and current papers, not from ancient authority. The concepts involved are essentially modern (whether or not they may be present in embryo in the Darwinian corpus is, for this purpose, irrelevant).

Natural selection, like any other theoretical concept (such as 'atom' or 'gene') has a definition that evolves as the science progresses. Democritus invented the 'atom' -

but we don't call atomic theory Democratic. Words used to label scientific concepts should not prejudge the debate, nor should they introduce misleading considerations. It doesn't matter (biologically speaking) what natural selection meant to Darwin; what matters is what it means now, to us, and how we use it.

'Darwinian' should become a taboo word among evolutionary biologists - we should leave it to biographers, historians and sociologists who are engaged in tracing intellectual lineages.

Presumably we are interested in evolution by natural selection, in adaptation, in speciation. Then those are the terms that should be used. It is profoundly unhealthy for evolutionary biology to be tied to the ghost of a dead man.

SOCIETY NEWS

ISHE Tax-Exempt in U.S.

Through the patience and tenacity of Bill Charlesworth, ISHE has gained tax-exempt status in the U.S. This means that membership dues and any contributions are tax-deductible.

Peter LaFreniere to Replace Linda Mealey as Chief Book Review Editor of Bulletin

Linda Mealey is withdrawing from her duties as Chief Book Review Editor. She will follow through on reviews that she has arranged, but henceforth all reviews will be arranged by **Peter LaFreniere**. Linda is stepping down because she wishes to prepare for her term as President in three years. She has done a herculean job for the *Bulletin*, arranging more reviews than ever before and editing most of them before passing them to me. Peter will be an able successor; we are delighted to have his services. He has been acting as French Book Review Editor, and will continue to do so along with his new duties. Please contact him if you wish to consider reviewing a book (see Editorial box).

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TREASURER'S REPORT

CREDITS		DEBITS	
Source	Amount	Source	Amount
Balance per June 1996	$16,982.79	President's expenses	$ 86.48
Members' dues	5,670.00	Editor's expenses	912.00
Interest on certificate of deposit	177.00	Treasurer's expenses	465.30
		Lapsed member invoices and postage	34.70
		Advertisement in *Skeptic* magazine	150.00
		40 copies of Vienna congress proceedings	1,800.00
		Bank fees for credit card processing	232,64
		Note: Debits do not include payment for printing and mailing of four *Bulletins* (estimated cost $2880.00)	
Total Income	$22,829.79	Total Debits	$3,681.12

Balance $19, 148.67

$17,000 of balance is invested in a certificate of deposit due Feb. 1998
Cash on hand $2,148.67

Submitted by Barbara F. Fuller

BOOK REVIEWS

Separate Worlds of Siblings: The Impact of Nonshared Environment on Development

Edited by E. Mavis Hetherington, David Reiss and Robert Plomin. Lawrence Erlbaum Associates, 365 Broadway, Hillsdale, NJ 07642 USA, 1994, $24.50 (hdbk.).

Reviewed by Nancy L. Segal, California State University, Department of Psychology, Fullerton, CA 92834, USA.

Recently, many behavioral geneticists have been directing needed attention to the ways in which environmental factors contribute to human behavioral variation. A significant recent contribution has been the demonstration that nonshared environmental events may influence the development of many traits (e.g., cognitive abilities, personality, and temperamental characteristics) more than do shared environmental events. This finding has assisted our understanding of why children raised in the same family may be so different. It has, in addition, led to reappraisal of current theories, assessment procedures and explanations of individual differences. The purpose of *Separate Worlds of Siblings* is to describe the work and methods on which this new conceptualization of the environment is based, to encourage new ideas about interactions between genes and environments, and to stimulate additional research in this area.

Chapter 1 (Plomin, Chipeur and Neiderhiser) offers an informative review of twin and adoption methodology, and defines key concepts. A comprehensive overview of behavioral-genetic evidence documenting the importance of nonshared environmental factors is presented. A compelling case for the influence of nonshared environment is made across a number of behavioral domains including general intelligence, special cognitive abilities, personality, and psychopathology. (Some additional recent studies that might have been cited, especially in the area of psychopathology, include Gottesman & Bertelsen, 1989 and Kendler et al., 1992a,b.) While the section on physical disorders makes specific reference only to obesity, there is a reference to a more general review (Dunn & Plomin, 1990), and later in Chapter 8 comes a discussion of effects of nonshared environment on coronary-prone behavior.

Chapter 2 (Rovine) discusses the use of sibling difference scores for estimating the nonshared environment. It is asserted that behavioral geneticists typically decompose phenotypic variance into genetic and environmental components, but do not consider relationships between sibling discrepancies and behavioral measures. Comparisons among three types of models (regression model, difference score model and contingency factor model) are made, and graphs are included to clarify the different data structures. Overall, this chapter defines a rich domain inhabited by behavioral-genetic researchers and measurement experts. Most importantly, this material encourages serious thinking and rethinking about the nature of data, the appropriate questions to address, and the model that would most effectively depict the expectations of the investigator.

Chapter 3 (Reiss, Plomin, Hetherington, Howe, Rovine, Tryon, and Hagen) describes a comprehensive ongoing research program directed toward answering three central questions: (1) What are the differences in the social environments of adolescents? (2) Are these differences a product of environmental processes or, alternatively, associated with sibling differences that reflect genetically-based tendencies? (3) What environmental factors are associated with later developmental outcomes? To its credit, the project includes large numbers of genetically informative kinships (identical and fraternal twins, siblings, half-siblings and unrelated siblings) and stipulates strict selection criteria: Children were between 10 and 18 years of age at the onset of study, unrelated siblings were no more than 4 years apart in age, marriages must have endured for five years (to be considered stable and to ensure comparability across groups), and the children must have lived together in the same home for half the time. It is conceivable that various factors in previous residences (e.g., parent-child relationship, marital conflict) could be associated with some of the developmental outcomes being measured. Analyses along these lines, if available, should be informative.

The inclusion of sibling groups that vary in genetic relatedness enables assessment of the contribution of genetic and environmental factors to variables of interest, as well as examination of the nature of these genetic and environmental effects. Many interesting and provocative findings in the domains of psychopathology and parenting are presented. There is, for example, little correlation between siblings' perceptions of how they are treated by their parents--a finding that is consistent with the effects of nonshared environments. Correlations are higher for parents' perceptions of how they treat their children. Thirdly, parents' perceptions of children's similarity varied with thelatter's genetic relatedness. These results suggest genetic effects on both behavior and environmental measures. It was also intriguing to learn of the small correlations (zero in one case) between children's absolute and relative difference scores for reports of parental aggression, indicating that these measures of sibling similarity are differentially informative. Examination of sibling social closeness as a function of genetic relatedness (a topic of concern to developmental psychologists, especially those with an interest in evolutionary psychology) would have been of interest. This aspect of development will, I hope, be considered as the study progresses.

Chapter 4 (Dunn and McGuire) focuses on the developmental significance of parent-child and child-child relationships in the lives of young children from Cambridge and Colorado. Data were gathered by means of interviews and observations. The most interesting aspects of the presentation are the findings concerning parental treatment of different-age siblings as they pass through a given age. It was found that parents tend to treat children in similar ways when they turn the same age, identifying developmental stage as a key factor in child treatment. Thus, mothers do treat children differently as a function of their different age level. This difference may be perceived by the children as constituting unequal treatment, despite the fact that equal treatment was dispensed as age-appropriate. This intriguing idea is clearly worthy of follow-up. Additional attention to microenvironmental events that make siblings differ is also worth pursuing.

Chapter 5 (Brody and Stoneman) considers differential parental treatment as a specific source of nonshared sibling experience and the effects of this treatment on sibling relationships. Following a review of relevant studies, they caution that despite relationships between parental treatment and sibling relations, causality is difficult to discern.

Findings from a study including both mothers and fathers is of interest, given the rarity of fathers as research participants in developmental studies. Parents were observed with the two siblings as they engaged in specific activities. It was found that while fathers interact with children less frequently on a daily basis than do mothers, fathers clearly have an impact on children's behavior. Paternal differential responsive and controlling behaviors were associated with higher rates of negative behavior from younger and older siblings, respectively. The families were also observed during discussion of problems that each sibling experienced with the other. Negative sibling behavior at the time was negatively correlated with paternal equality of treatment; unequal treatment from mothers was also associated with reports of conflict between siblings. The authors speculate that unequal treatement by fathers may be especially salient, as he spends less time with his children. Given the persistence of sibling relationships throughout the life span, ideas such as this are worth pursuing for both theoretical and practical considerations.

Chapter 6 (Tejerina-Allen, Wagner, and Cohen) presents information on differences in parental behavior as reported by mothers and by siblings, and how such information relates to suicidal tendencies and associated behaviors. A large community-based sample was available for analysis. It was found that (1) the non-shared environment did not account for more variance than the shared environment; (2) both shared and nonshared environmental factors were associated with suicidal behaviors; (3) differences in parenting were most significant when child oppositional behavior was the dependent variable; and (4) differences in harsh punishment were more likely to result in suicidal ideation by the more disciplined sibling than the less disciplined sibling. However, it was not the discipline per se that was responsible for these effects, but rather the siblings' perception of the difference in treatment. The possibility of individual differences in personality traits might be important to consider in this context and could be examined in the future. The authors acknowledge that little theory is available for generating specific hypotheses as to when shared-sibling parenting vs. within-family differences are important. This situation may explain the absence of expectations concerning direction of effects.

Chapter 7 (Rowe, Woulbroun, and Gulley) is an interesting and well-written chapter that raises challenging and provocative ideas about the nature of peer influence on sibling differences in personality. It is generally assumed that similarity between peers reflects mutual influence between interactants. This has been documented as affecting twins in that monozygotic (MZ) twins can show differences in behavior associated with differences in their peers; this effect must be environmental since MZ twins share all their genes. However, Rowe and colleagues' review literature suggesting that some degree of selection (not unlike assortative mating) occurs prior to the establishment of friendship relations and, to quote the authors, the more important influence is the "reinforcement of existing genotypes through the functional consequences of behavior for the individual" (p. 172). The relative degree of influence of peers versus assortment, in turn, appears to be trait-dependent. A pleasing aspect of this chapter is that it highlights important areas for future research and offers a fresh conceptualization of the developmental significance of friendships in the lives of children and adolesents.

Chapter 8 (Ewart) is a welcome addition to the behavioral-genetic literature on nonshared environment, as it explores a serious medical problem, coronary heart disease, that has significant behavioral underpinnings. While there are established links between hostility and risk, the author notes the lack of an effective conceptual framework for evaluating this information, and offers behavioral genetics as possibly providing the needed theoretical perspective. The main point is that coronary-prone behavior may be understood with reference to behaviors arising from genetically-based predispositions, in conjunction with individual experiences in the life histories of family members. Some nonshared experiences may be critical in appraising risk for coronary disorders. Specifically, there is a need to assess behavioral components relevant to emotional stress that might trigger coronary disease in a predisposed genotype. Results of a study that assessed the effects of interviews and tasks on children selected for high blood pressure are reported. Interestingly, the interview situation contributed more to ambulatory blood pressure than did conventional stress tasks, identifying the context and circumstance of stress as important variables.

It is argued that the application of a behavioral-genetic perspective would complement a social action model of coronary-prone behavior. Social interactions with others may prove a source of stress, so that examination of nonshared family factors (i.e, marital conflict, sibling and peer relations, and family structure) is advised. Longitudinal siblings studies designed with these themes in mind would be desirable.

Chapter 9 (Deal, Halverson, and Wampler) presents a conceptualization of sibling similarity as an individual differences variable. This interesting chapter casts sibling similarity in a new light by reminding us that sibling pairs vary in degree of similarity, a finding possibly obscured by correlations representing group level data. (This point is also addressed in Chapters 1 and 2.) Following a discussion of problems surrounding correlation coefficients and difference scores, the use of the "true dyadic score" is proposed - this is a correlation computed between test items for each sibling in a pair; this correlation is then entered into a data set as a measure of similarity.

This approach is illustrated with data gathered to assess competing hypotheses concerning treatment of siblings: (1) a cognitive hypothesis in which parents who perceive similarities in children treat them alike, and (2) a "prototype" hypothesis which posits that if a rearing practice works with one child it is used with the other child. A series of parent questionnaires on attitudes and perceptions was collected, and parents and children were observed across three settings. True siblings proved more alike than pairs of unrelated siblings, but the difference was not large - the authors explain that this might be associated with the constrained laboratory setting. Most interesting was that the prototype hypothesis was supported by the data, while the cognitive hypothesis was not. In conclusion, we are treated to a novel and interesting way of processing sibling data that will, I hope, generate additional analyses.

This volume includes several new and compelling contributions to the literature on nonshared environment. It was somewhat disappointing that the voices of the editors, all prominent researchers in the field, were generally silent: Aside from a brief introduction, no commentaries or concluding statements were provided. Additional efforts to draw meaningful links among the various contributions, many of which touch upon similar themes, would also have been helpful. This book is recommended for researchers and students with some acquaintance with current issues in behavioral-genetic research.

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Male Violence

Edited by John Archer. Routledge, 29 W. 35th St., New York, NY 10001 USA, 1994, $19.95 (ppr.).

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Violence is, universally, an integral part of the masculine mystique. As Paul Gilbert remarks in the concluding chapter, male violence may outrank disease and famine as the major cause of human suffering. Male violence is not a typical product of our (Western patriarchical) civilization, nor our (capitalist) mode of production; nor is it a male conspiracy in order to suppress, terrorize and exploit women. Barry McCarthy notes that in 'traditional' cultures too there is an almost universal, intimate bond between warrior values and conventional notions of masculinity.

Because, as evolutionary biology predicts, in sexually reproducing species one sex (mostly the males) competes for the ultimately limiting reproductive resource (mostly the females), armaments, vigor, strength, and fighting capabilities are in many species confined to, or more conspicuous in, the males. Agonistic behavior and its morphological paraphernalia are almost universally sexually dimorphic, and can be understood as reflecting the different optimum reproductive strategies of the sexes. This is, ultimately, the evolutionary rationale of all sexual dimorphism; not only in human societies are violence and aggression 'gendered' phenomena. These and similar observations have led Archer to take as the starting point of the book not the generality of aggression in the human species, but the predominantly male nature of most acts of violence.

The first section is devoted to "Aggression in Childhood." In his chapter, Michael Boulton outlines the difficulties in distinguishing between rough-and-tumble play and 'real' aggression. In both cases there are profound sex differences, which have commonly been attributed to differences in prenatal hormones, especially testosterone. Rough-and-tumble episodes and 'horseplay' may constitute one way in which older boys work out physical dominance relations.

Adopting a perspective derived from studies of dominance in other social animals, particularly primates, Glenn Weisfeld argues that boys compete so as to form dominance orders or hierarchies. Weisfeld discusses the ways in which boys' dominance relations are similar to those of other primates, and their importance for providing access to resources (and hence 'fitness' defined as reproductive success). Weisfeld also discusses the stability of the hierarchy over time, and the correlates of high dominance rank with other (personality) attributes. Finally, he outlines the association of aggressiveness and dominance position with social problems such as delinquency.

In chapter 4, Yvette Ahmad and Peter K. Smith describe their research on bullying, which was built on earlier research by Dan Olweus in Norway. For male victims, it was usually other boys who were the bullies. For girls it depended on age: at 8 and 11 years, they were more likely to be bullied by boys, whereas at 13 and 15 years of age other girls were the more frequent bullies. Girls were more likely to use and experience indirect forms of aggression such as spreading rumours.

Part II is concerned with violence toward other men. Arnold Goldstein discusses the male gang, concentrating on studies carried out in North America. Gang members are mainly males, 12 to 21 years of age, from poorer areas, with African Americans and Hispanics highly represented. The gang provides an alternative way of obtaining resources and social status for young males from poor and educationally disadvantaged groups.

McCarthy adopts a historical and cross-cultural viewpoint in considering the values behind men who adopt the warrior role. He shows that the warrior ethos (notably courage, endurance, strength and skill, and 'honor') is closely linked with concepts of masculinity. Ethnographers' reports suggest that participation in successful warfare by young men is a key to status and prestige within the group, including access to privileges and perquisites, and especially access to (nubile) women. In politically and socio-economically elaborate societies, a distinct warrior caste or military elite develops, characterized by strong in-group sentiments combined with a dehumanizing ideology toward out-groups.

In the next chapter, Archer considers violent disputes between pairs or small groups of men. The most severe violence occurs between young men. The typical precipitating event involves violation of perceived social rules reflecting on status and self-esteem. Alcohol and the availability of weapons play roles in the escalation of fighting.

Part III is concerned with male violence towards women and children. Robin Goodwin describes dating violence, or 'relationship aggression'. He includes both physical and sexual aggression in his discussion of the importance of cultural background for understanding the significance of these acts.

Neil Frude views marital violence in its cultural and societal context. Social class, characteristics of the relationship, and personal attributes of the individuals all form a background to the violent incident itself, which is commonly sparked off by quarrels about sex or money. Frude's interactional view does not imply that both protagonists are equally responsible; it is usually husbands who are violent.

Paul Pollard examines the characteristics of males who commit sexual violence. Rape and other forms of sexual assault are far more common than is generally supposed, and the typical rapist is an acquaintance who does not have a criminal record and is generally not reported to the police. Although rape proclivity seems to be a continuous attribute within the male population, the 'macho male' whose sense of self worth is bolstered by the pursuit of dominance over and exploitation of women is particularly likely to translate his misogyny into sexual violence.

In Chapter 11, Bernice Andrews describes her research on both physical and sexual violence towards children. Although previous research on physical abuse has tended to focus on the mother, she found that men were implicated much more frequently than women. Maternal psychiatric condition (particularly depression) and poor mothering increase the chance of both forms of abuse.

Kevin Browne reviews research on sexual abuse of children. The most common age of offenders is 35 to 40, but there is no clear profile of the typical abuser. Perpetrators are usually known to the victim, but not members of the immediate family. There is evidence for intergenerational transmission: one generation's victim may become the next generation's offender.

The final part of the book is concerned with explanations of male violence from a number of different perspectives. Angela Turner considers the genetic and hormonal evidence. She concludes that there is at most a small genetic component underlying delinquency, aggression and violence, but a greater one for the personality traits underlying these, such as sensation-seeking and impulsiveness. The evidence also supports a moderate association between testosterone and aggression. While there appears to be some neuroendocrinological basis to greater male violence, this potential can be reinforced or diminished depending on socialization.

Martin Daly and Margo Wilson present a Darwinian perspective on male violence. The evolutionary view explains why males and females have different reproductive strategies, resulting in conflicts of interest between males and females. Most male violence - against men, women, and children - can be understood in terms of these principles.

John Hoffmann, Timothy Ireland and Cathy Widom critically examine traditional socialization explanations of aggressive behavior, with roots mainly in psychoanalytic and social learning theory (especially Bandura's version). Although this theory provides a basis for understanding the transmission of aggression by family, peer group and the mass media, there remains the need to consider the sex of both the perpetrator and the victim in this, originally 'gender-free', theoretical perspective.

In a chapter on power explanations, Archer considers the feminist argument that husbands' violence towards wives forms part of a wider historical system, patriarchy. This explanation can be compared with an interpersonal status explanation for inter-male violence. The two explanations are linked by a common set of masculine values which endorse the use of violence to attain status in the eyes of other men, and to keep women subservient. Archer argues that these values arose from the conflict of interest between the optimum reproductive strategies of males and females, and from inter-male competition arising from sexual selection.

Anne Campbell and Steven Muncer argue that men and women think about aggression and violence differently. Men consider violence in instrumental terms, connected with obtaining tangible or abstract benefits, whereas for women it represents a discharge of emotion, a sign of not coping. The authors argue that these different meanings lead to mutual misunderstanding which may aggravate marital conflict.

Lastly, Paul Gilbert provides an integration of explanations for male violence, ranging from sources as diverse as psychoanalysis to evolutionary biology. He views aggression as a strategy for a variety of ends, such as coercion of others, self-representation, and achieving status. The tactics of intimidating others and gaining their admiration often merge in a single act of violence. Gilbert then examines the cultural context (capitalism and the major religions) which promotes ruthless and competitive masculine values, and devalues feminine attributes such as empathy, affiliation and compassion. These values, he argues, help to perpetuate male domination and violence.

This book is a good place to start for those who wish to gain familiarity with evolutionary thinking about human social behavior. It is also recommended to 'mainstream' social and behavioral scientists. Note: For this review, I have borrowed liberally from John Archer's eloquent introductory chapter.

Grooming, Gossip and the Evolution of Language

By Robin Dunbar. Faber & Faber, 50 Cross St., Winchester, MA 01890 USA, 1996, £15.99 (hdbk.).

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

This book is founded on the premise that extremely complex scientific problems of human evolution can have amazingly simple answers. The old problem: "Where does human language come from?" has a simple answer: "From grooming." The unexpected spin-off: we now understand "why humans have such large brains, and why we spend two thirds of our time gossiping about one another."

These quotations from the press release match similar assertions in the book itself. After a few pages it becomes obvious that Robin Dunbar intends neither to summarize and defend his "vocal grooming" hypothesis, nor to attempt a scholarly review of the evolution of human language. He simply presents his current scientific ideas in an easygoing, gossipy prose - printed vocal grooming offered to a large readership of laypersons. He does this very well and with so much confidence and persuasive power that some of his groomees may well take his words as facts of life and accept his train of thoughts as representative of modern scientific reasoning.

In a nutshell, Dunbar's argument runs like this: We know for a fact that in primates grooming is a matter of not just bodily hygiene but social hygiene as well. So it is not surprising to find that the amount of time spent grooming is correlated with group size: the bigger the species-specific group size, the more grooming is observed. It is not that primates in larger groups have more parasites in their fur; they use grooming as a social favor, as the grease of social life.

But there is another curious connection: The larger the group size of a species, the larger are their brains. That is quite reasonable when we consider that it takes brain power to keep track of all the faces of friends and foes, and who does what with whom. As we know now, the whole gamut of Machiavellian intelligence is no longer a sign of human superiority over the brutes, but an old primate trait that has found in humanity only its ultimate expression.

How does all this apply to the evolution of language and gossip? We know from the fossil record that over the past 3 million years human brain size has multiplied. Since we now know that in (selected?) primate species brain size and group size are correlated, we can predict the group size of our ancestors. So we can easily see from data on cranial size that Australopithecines must have lived in groups of around 60, *Homo erectus* made it to around 100, and modern humans can handle around 150 of their own kind.

And now comes the amazing leap of faith: Modern humans, blessed or cursed with their big brains, not only are stuck with those 150 other members of their group; each one of them has to be groomed in order to ensure social hygiene and harmony! How much time would we need to groom 150 fellow group members? The answer is very simple: we just convert "brain size" to "grooming time." In so doing, we see where the real problem in hominid evolution lay. Australopithecines were still well off in this respect; they groomed less than do gelada baboons today. But *Homo erectus* was in trouble: with grooming times around 30%, the time budget got rather tight, and a fortiori for modern humans, getting up beyond 40%. There is simply not enough time left for other important activities if social relations are managed the old fashioned, grooming way.

Therefore, verbal language had to evolve in order to insure survival of the human race. But since, as primates, we were used to spending a lot of time grooming anyway, we employ our verbal language primarily in endless gossip - "vocal grooming." Instead of simply telling each other briefly, once a day how we feel ("How are you?" - "Fine!"), we belabor trivial matters interminably.

All this is presented on some 200 pages within a wider context of more or less related observations concerning evolution, primates, grooming, language, and development. However, Dunbar neglects to mention other scholars who have preceded him with specific hypotheses in this field. These include Malinowski (1923) and his concept of *"phatic communication" (the* special case of human communication *"in which ties of union are created by a mere exchange of words");* Morris (1967), who first described *"grooming talking"* as *"the meaningless, polite chatter of social occasions, the nice weather we are having or have you read any good book lately form of talking,"* and my own work on "tonic communication" and bonding (Schleidt 1973).

But what I find most disappointing in this book - especially if it is intended as popular science - is its lack of balance in dealing with scientific evidence and reasoning. We hear many arguments for, and rarely any against, the hypothesis under consideration. Moreover, the book misleads lay readers by its simplistic chains of reasoning about complex relations. For example, often causation is implied when only a correlation has been found.

For the scientifically inclined reader this book can serve at best as a teaser, a spur to look up the critical details in Dunbar's more recent papers (1992, 1993 and Dunbar & Spoors 1995). The interrelations among group size, grooming time, and brain size in primates have been presented much more carefully in Dunbar's lead article (1993) in *Behavioral and Brain Sciences*, and what can be learned from them becomes much clearer in the multifaceted discussion that follows. This article shows not only that there is indeed a very interesting nest of problems, but also how far we are from understanding the question "Where does human language come from?"

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On Aggression

By Konrad Lorenz. Re-issued with a new introduction by Eric Salzen. Routledge, 29 W. 35th St., New York, NY 10001 USA, 1996, $15.95 (ppr.).

Reviewed by John Archer, Department of Psychology, University of Central Lancashire, Preston PR1 2HE, UK.

Konrad's Lorenz's contributions to the formation of ethology are widely recognized - his observations, his application of the comparative method to the evolution of displays, empirical studies of imprinting, and the application of the concept of releasers to humans. But his ideas about aggression, outlined in an engaging and readable way in this well-known book, were at best controversial when it was first published in German in 1963, and from the view of hindsight are misguided on several counts. The errors in Lorenz's views on aggression were well addressed by several critics in the years following the publication of his book. In my opinion, it contributes little to the modern study of aggression, and does not deserve to be re-issued and re-packaged thirty years later. This is clearly not a view shared by the publishers, nor by Eric Salzen, whose new introduction seeks to persuade a modern readership that *On Aggression* is still worth reading.

Before considering the background to the re-issue of *On Aggression* and the merits of the new introduction, I shall make a few comments about the contents of the book itself. On the positive side, it did seek to place the study of aggression in the natural world where it mostly belongs, in contrast to the view widely found in the human sciences, that aggression is an abnormal aspect of human behavior. This argument has recently been updated by Daly & Wilson (1994), who assessed violence on the basis of criteria derived from Darwinian medicine, to show that it generally does not fit any of the criteria for pathology (for a clear statement of a different position, see Raine, 1993).

On the other hand, the considerable misconceptions in Lorenz's writings on aggression are painfully obvious from the vantage point of modern evolutionary theory. His evolutionary arguments are all based on group selection, and are therefore almost invariably invalid. The reason for the evolution of "limited-war" strategies is not to avoid killing too many of one's own species or to promote harmony in the group. It is to avoid dangerous counter-attacks, as was shown formally in the game theory models of Maynard Smith (1982), but had also been pointed out by Geist (1966). The inadequacy of the policing-function view of dominance can also be found in two books on animal behavior and ecology which should have been available to Lorenz (Lack, 1954; Tinbergen, 1953).

More of the initial criticism of *On Aggression* centered on his general model of motivation, which he applied inappropriately to aggression (Berkowitz, 1967; Hinde, 1967; Johnson, 1972; Toates & Archer, 1978). Essentially, he did not recognize the distinction between appetites and aversions made by Craig (1928), and he viewed aggression as something that built up in the absence of performance, like hunger or a sexual urge. The whole weight of empirical studies of animal aggression (Archer, 1988), as well as those from social psychology (Berkowitz, 1993), shows that this is an untenable position. Lorenz's extension of his position on the motivational basis of aggression to human warfare was to prove particularly unpalatable for later commentators (see below).

There is, of course, much else in the book in addition to discussions informed by group selection and the hydraulic model of motivation. There is consideration of the causation of displays, partly anecdotal and partly theoretically driven, so that it is sometimes difficult to determine what is being claimed. There is also the argument that intense feelings of love are derived from the need to placate intense feelings of hostility, so that love only occurs when there is prior hostility between members of a species. Again, this interesting argument is misconceived. Looking at the phenomenon of love (or attachment) from the vantage-point of modern Darwinism indicates that it arises from the requirement (for the purpose of fitness) to maintain stable relationships.

So, the reader may be thinking, Lorenz's *explanations* are misconceived. But surely, as the cover of Routledge's new edition claims, his *observations* are sound. I am not sure that I can even agree with this. Take for example the statement that animals do not kill many of their own species, carrying with it the implication that it is human destructiveness that has to be explained. It is, like many of Lorenz's other observations, not based on careful quantitative analysis, and has more recently been countered by Williams (1988), who asserts that for most mammalian species, the murder rate is higher than in large American cities (Williams, 1988).

These and other examples (the existence of so-called vacuum activities and the appeasement gestures of wolves) are controversial because of the method on which they were based. Lorenz relied on verbal description, and he rejected experiments, quantification and statistical analysis (Bateson, 1989). In terms of methodology, he has much in common with those politically-motivated social psychologists who seek to replace positivist science with qualitative methodology. Like their empirical efforts, Lorenz's observations lack validity and reliability checks, enabling subjective bias to flourish, and they suffer from selectivity and argument by example (Morgan, 1996).

It is true, as the blurb on the back of the book says, that the main contemporary interest in *On Aggression* lies in the controversial reaction to aspects of what Lorenz was supposed to have said in the book. This reaction was the Seville Statement on Violence, made by twenty leading aggression researchers at a Colloquium on Brain and Aggression in Seville in 1986. There were five propositions about war, aggression and violence, all couched in negative and dogmatic terms, each starting with "It is scientifically incorrect to say...". What follows is usually a position held to originate from Lorenz's writings, although it can be argued, as Salzen does in his introduction, that Lorenz held none of these views. The importance of the statement lies in its endorsement by several learned societies including the American Psychological Association, which printed and reprinted it in *American Psychologist* in 1990 and 1994.

Personally, I do not like the Seville Statement. Its dogmatic tone, and the way it concentrates on the negative, makes it sound naive, and diverts attention away from what we do know about aggression. More importantly, it enables those who essentially support a Lorenzian position on aggression to go on the offensive. They now have a series of statements to criticize, and this tends to obscure the deficits in their own position which were exposed long ago. The authors of the statement have also unwittingly provided a reason for reprinting Lorenz's original book, when it deserves only to be a historical footnote in the history of aggression research.

Finally, how effective is Salzen's introduction to the new edition? It is very good in setting the context of the subsequent debate over Lorenz's ideas. It also gives a good appraisal of Lorenz's claims from a sympathetic stance without seeking to distort or defend those aspects that have stood the test of time least well. It is a very good defense of what in my view remains indefensible. Salzen rightly says that it was the motivational basis of aggression - the claim about its spontaneity, together with the link made between individual and group aggression - that was criticized when the book first came out. Despite these initial criticisms, Lorenz's views on aggression were retained by ethologists who maintained his general view of the subject ("classical" ethologists: see Archer, 1992), notably by Eibl-Eibesfeldt. In view of what was seen to be their influence in the wider community, the Seville Statement was drawn up, which as I have indicated only served to resurrect the debate.

Salzen tackles the issue of Lorenz's ideas on motivation, referring to them as "the ethological analysis of aggression." This is an unfortunate phrase because it is exactly how many psychologists do see Lorenz's views, as the only ethological analysis of animal aggression. It obscures a wealth of detailed empirical research and carefully constructed theory on animal aggression, as well as syntheses by myself and others (Archer, 1976, 1988; Huntingford & Turner, 1987; Archer & Huntingford, 1994). Salzen does admit that

"Perhaps Lorenz was mistaken in postulating an inbuilt specific urge for aggression" (p. xv). In my view, such an admission removes the foundation of much of Lorenz's theorizing about aggression.

But not for Salzen. He builds a revisionist position, saying that maybe there is no inbuilt aggressive urge, but that spontaneous aggression comes from another motive, for example for status and power. He is clearly referring to young male violence here, as he goes on to make a link with testosterone on the one hand and petty crime on the other. While it is true that *violent* (not petty) crime has a number of biological markers (Raine, 1993), some of which are undoubtedly linked in a causal way, the picture is much more complex than Salzen admits.

Also, the supposed causal link between testosterone and human aggression has not withstood careful empirical scrutiny (Archer, 1991, 1994; Halpern et al., 1994). Lorenz would have been vindicated if a chemical could be found that builds up in the absence of an aggressive outburst and then declines after such an outburst. If anything, testosterone levels *increase* after successful aggression or competition (Archer, 1994).

Salzen also admits to certain other errors in Lorenz's work; for example, it would have been useful to point out modern Darwinian thinking, and how this is necessary for clear thinking about functional questions. This is especially needed when looking back in time, as Lorenz was by no means alone among the biologists when he espoused group selection.

Salzen does his best to salvage Lorenz's contentious writings on war, where he applied the same argument as he did to intra-group aggression. Salzen is able to point to more recent research indicating the precursors of warfare in other species, notably in chimpanzees. Salzen admits that Lorenz did not distinguish between inter- and intra-group aggression as clearly as he should have. Nevertheless, there may be closer links between inter-group conflicts in animals and the simpler forms of human warfare than critics were initially prepared to acknowledge.

Of course, Salzen has the task of ultimately endorsing the reprinting and repackaging of Lorenz's book. In doing so he says that the importance of the book is to draw attention to the elemental basis of aggression and its driving forces. However, he also says that there is now little support for the type of aggressive drive that Lorenz suggested, which would seem to imply that we could more profitably look at other sources to learn about the motivational bases of aggressive behavior, rather than returning to Lorenz's fundamentally flawed view of the subject.

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Lying and Deception in Everyday Life

Edited by Michael Lewis and Carolyn Saarni. The Guilford Press, 72 Spring Street, New York, NY 10012, USA. 1993, $27.95 (hdbk.).

Reviewed by Jay R. Feierman, Department of Psychiatry, University of New Mexico, Albuquerque, NM 87113, USA. jrfeier@ix.netcom.com

In the Preface to this book the authors state, "We think this focus on emotion sets *Lying and Deception in Everyday Life* apart from other books on deception." However, the editors should have told this to the contributors, since "emotion" is neither defined by the editors nor emphasized in many of the book's chapters. The title words, "in Everyday Life," also sets the tone of this book. The authors add in the Preface, "...it appears that we all use and need deception in order to cope with social life, both within ourselves and in our relationships with others."

The book contains no previously-unpublished data. Nine of the book's 10 chapters have a psychological perspective. One chapter is developmental (Michael Lewis), one is descriptively empirical (Paul Ekman), and the rest are mainly second order (i.e., self-report) empiricism. The one non-psychological chapter (Robert C. Solomon), is philosophical.

Solomon's chapter contains some interesting thoughts on deception by Nietzsche, Kant, and Sartre. Both Nietzsche and Jung thought that humans need cultural myths, which Solomon re-defines as "collective self-deception," and then adds that it is "big lies" that hold most religions and cultures together. Solomon also reviews the 7-13th century Stoics, who argued that our emotions are distorted perceptions or judgments, that *all* emotions are, in this sense, self deceptive, with love being an example of the most pervasive self-deception. Outside of science, one is left with the bothersome question, what use is truth and why should natural selection favor it?

Many of the chapters (e.g., Sarni and Lewis) could benefit from cross fertilization from biology. Human ethologists should appreciate the statement, "...in simple societies when deception occurs, it is typically around aggression or dominance, access to food or goods, access to desirable mates..." How is this different from more complex societies? The authors also propose that a culture's etiquette can be considered a deception-enabling vehicle in that prescribed and predetermined behavior allows us to act in ways that are not congruent with the way we think or feel. As an example, the [universal] human process of bargaining "is a series of deceptions, ritualized into a technique in which goods are bought and sold."

Robert W. Mitchell's chapter,

"Animals as Liars: The Human Face of Nonhuman Duplicity," was painfully long and as confusing as its subtitle. Lewis's phylogenetically-myopic but otherwise interesting chapter on "The Development of Deception," following Bok, divides deception into three, ostensibly functional, categories: (1) lying to save the feelings of another, (2) lying to avoid punishment, and (3) lying to the self (which is not really a functional category). "One might argue that the function of deception in protecting the feelings of others is evolutionarily appropriate if one of the tasks of our evolutionary history was to develop and maintain complex social interactions."

In reporting his own previously published research on not quite 3-year-old children, Lewis says, "...through the use of an elaborate coding system that measured both facial as well as bodily postures, we found that we could not distinguish between those children who did not lie and those that did." No sex differences were found, although in the experiment to test lying, "girls resisted temptation better than boys,"[1] which gave the girls less about which to lie. In the same experiments, children who told the truth and did not lie when questioned about their peeking had the lowest IQ! Older children, ages 3-6, were also able to deceive adults without being detected, based on adults making detailed analysis of facial and bodily behavior coded by experimenters trained in facial coding procedures.

Lewis used the same experimental paradigm to study deception in Japanese children in Tokyo, ages 4-7. As with the American children, Japanese children were more likely to deceive as they got older. However, Japanese children were better able to resist temptation and had less need to deceive. In addition, compared to the American children, Japanese children showed less facial behavior, less smiling, lip biting, frowning, and nervous touching.

Saarni and Maria von Salisch's chapter, "The Socialization of Emotional Dissemblance," assumes through the word, "socialization," that children learn deception similarly to how they learn other arbitrary behaviors (e.g., when, where, and how to cover or not cover your head or when to take your shoes off) that are culturally transmitted accross the generations. The authors also discuss cultural-specific "display rules" and how, through cultural transmission, one learns to display affect through Ekman's proposed mechanisms of minimization, maximization, masking, and substitution.

The Chapter "Sex Differences in Lying: How Women and Men Deal with the Dilemma of Deceit," by Bella M. DePaulo, Jennifer A. Epstein, and Melissa M. Wyer, is weak. It is also phylogenetically myopic and, therefore, misses the big picture, i.e., the biology of sex differences as a basis for sex differences in deceit. In addition, almost all of the reviewed data are based on what people say they feel, think, and do, rather than on what they actually do, which is a weakness of most of the chapters in this book.

Sandra T. Sigmon and C. R. Snyder's chapter on "Looking at Oneself in a Rose-Colored Mirror" suggests an interesting metaphor for their thesis that "having a [self-deceptively] positive biased perception of reality characterizes most healthy individuals." However, the authors missed the opportunity to relate this norm to self-perception in non-healthy individuals who demonstrate the extremes of mood thought and behavior.

Roy F. Baumeister presents a different slant on self-deception in his chapter "Lying to Yourself: The Enigma of Self-Deception," in that his main thesis is that "...lying to others is often a vital part of lying to oneself. In other words, the self is more readily fooled if others are fooled too." His thesis that one fools others to better fool oneself is seemingly oblivious (i.e., in the citations) of the equally persuasive literature on the opposite argument, namely that one deceives oneself to be better able to deceive others (Lockard & Paulhus, 1988).

Baumeister cites studies that show that depressed people tend to see events more accurately than non-depressed people, and then argues that seeing the world (including oneself) in a favorably distorted fashion is an integral part of healthy adjustment. He cites other studies supporting the self-fulfilling prophecy that believing something is true helps make it come true and that motivation improves performance.

Paul Ekman and Mark G. Frank's chapter, "Lies that Fail," is vintage Ekman that should be familiar to all human ethologists. It is a summary of findings in Ekman's book (1992) on the detection of deception. Ekman's book reports on behavior per se, and is an excellent source on the detection of deception at half the price.

P. Randall Kropp and Richard Rogers' chapter, "Understanding Malingering: Motivation, Method, and Detection," is weak and disappointing and is beyond "Everyday Life." It reviews the mainly clinical literature on the dwindling differences between Malingering (for financial benefit) and Factitious Disorder (for psychological benefit), e.g., Munchausen's Syndrome, and their relationship to Personality Disorders. Were the authors to have taken a broader-than-clinical perspective, they would have seen the rich biological literature on feigned incapacity and could have framed the human issues more broadly.

Should persons interested in human ethology buy this book? Maybe. I have excerpted in this review almost everything in the book that I thought would be of interest to human ethologists. A much larger portion of the book, which I did not review, reports at best on second order data in which persons self-report how they think, feel, and did or would behave. I strongly suspect that the original collectors of these data, as well as the contributors to this book, were, at least some of the time, deceived by the very process they were attempting to study.

[1]So much for the truthfulness of the Adam and Eve myth!

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Membership Renewals

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Race in the Making: Cognition, Culture, and the Child's Construction of Human Kinds

By Lawrence A. Hirschfeld. The MIT Press, 55 Hayward St., Cambridge, MA 02142, 1996, $35 (hdbk.).

By Robert Kurzban, Department of Psychology, University of California Santa Barbara, Santa Barbara CA 93106, USA

The walls between social psychology and anthropology have been crumbling as workers in both fields realize that they are largely studying the same subject matter: the generation of culture by human minds. Hirschfeld's *Race in the Making* makes an important contribution to the elimination of those barriers between the disciplines. His breadth of knowledge allows him to evaluate the claims made by scientists in both fields in their respective studies of the role of race in human affairs. It is perhaps a harbinger of things to come that his analysis leads him not to a compromise between views, but rather to abandon both.

Hirschfeld begins by addressing the claim of many social psychologists that racial categorization is merely an instance of a more general human capacity for categorization. They reason that since distinguishing one thing (or kind of thing) from another is a principal part of cognition, racial categorization is an incidental result of the perceptual and conceptual machinery that picks out different kinds of things in the world. On this account, social categories, including racial categories, are discovered by attending to perceptible differences among groups of people. Racial categorization is thus a natural outcome of cognitive mechanisms which are particularly good at picking out patterns in the world. Advocates of this perspective usually hold that discrimination and prejudice emerge from an additional general tendency to favor members of one's own group or kind over members of other groups, regardless of the type of group in question.

Hirschfeld contrasts this account with the way in which non-psychologists, generally anthropologists and historians, have understood and explained race. In these fields, the brain has in large measure been removed from consideration. Instead, race is seen as a cultural tool of recent history, used to develop, solidify, and justify power relations among different groups of people (often Europeans among their colonial subjects).

Hirschfeld offers a reconsideration of how to think about race in particular and social categories in general. Underlying his approach is a belief that the human mind consists of a set of mechanisms each with its own particular function, a perspective known as "domain-specificity." Accordingly, he posits the existence of a set of cognitive procedures that specifically look for human kinds, in particular the human kinds that are constructed by the members of the culture in which each particular mind finds itself. In own words: "Human kinds are natural categories of the mind, in the sense that the mind is prepared to find them with little or no external encouragement...The notion of race is the outcome, the consequence, of this preparedness..." (p. 188).

One way to understand this view is by analogy to language. By now, most linguists agree that humans have a discrete system for learning language, the so-called Language Acquisition Device (LAD). The LAD is a collection of mechanisms that searches for linguistic entities such as phonemes, words, and a grammar constructed by local minds. In much the same way that the LAD expects to find some language in its cultural setting, Hirschfeld's system, which I will call the Social Kind Acquisition Device (SKAD), expects to find culturally relevant kinds of peoples.

In addition, the LAD takes continuous variables, such as the stream of phonemes in speech, and turns these into discrete categories (words). In similar fashion, the SKAD constructs categorical judgments from the continuous human variability it encounters, learning the locally determined human type boundaries. Lastly, just as the LAD evolved before English but nevertheless can learn it, we can be fairly certain that the SKAD evolved before humans were travelling large enough distances such that they were encountering the kind of physical variety that modern humans do today. Again, for better or worse, the SKAD is nonetheless able to acquire these "racial" distinctions.

To explore these ideas, Hirschfeld designed and ran some ingenious studies aimed at investigating the nature of children's development of their own concepts of human groupings in general, and race in particular. In his first experiments, he investigated the extent to which children believe one's race can change over one's lifetime, or even through generations. The idea behind the studies was to investigate if there is something special about children's conception of race as a marker of social kinds relative to other observable characteristics of a person. Children were shown an adult along with two children who were similar to the adult on one dimension (same race, occupation, or body build) and different from the adult on a different dimension. The children were asked which picture represented the adult when he/she was a child (or which one was the adult's child). Children chose the same-race child significantly more often than the child with the same body type or occupation as the one that represented the target both as an adult and as the target adult's child. This constitutes evidence that even 3-year olds seem to believe that one's race is constant over the course of a lifetime and heritable in ways that other physical traits are not.

Hirschfeld also examined the degree to which children perceive race to be a "biological" rather than "cultural" phenomenon. When children were given a scenario in which White parents mistakenly brought home and raised a child born of Black parents (or the reverse), children believed the child's skin color would resemble the natural rather than "adoptive" parents. Taken together with the previous study, these results suggest that a theory-like understanding of race develops early in life, and that children understand that race is an intrinsic, enduring property of a person, rather than just a simple perceptual attribute.

In a second series of experiments, Hirschfeld challenged the notion widely held in psychology that race is acquired visually. This view follows from the belief that racial categories exist "out there" in the world for children to discover, an idea that Hirschfeld shows is fraught with difficulties.

To investigate the role of visual perception, children were read stories in which the principal character interacts with four other characters. These people are all given descriptors on several dimensions, such as "the tall Asian grocer." The child's task was to recall the story as it was told to them. From their retelling, it was calculated how frequently the child used each type of possible descriptor (sex, occupation, race, behavior, or non-racial physical feature, such as 'tall').

There were two conditions, one in which children were read the stories aloud, and one in which children were given a picture book with the child narrating by using the pictures as a guide. If race were acquired and attended to by virtue of its physical correlates, the children should be more likely to recall characters' race in the study in which they are seeing the characters' pictures. In comparison, Hirschfeld's model predicts the reverse by virtue of his claim that detecting human kinds is driven by discursive rather than visual information. Indeed, this pattern is exactly what he found. Children recalled characters' race less frequently in the visual presentation than the verbal one.

Despite the convincing nature of the empirical work reported, there seems to be a small inconsistency in Hirschfeld's discussion of domain specificity, the engine driving much of the theoretical content. In line with modern views on knowledge acquisition, Hirschfeld argues that "experience is simply inadequate to explain how children come to share the concepts of their elders." Domain specific "theories" or "constraints" allow the child to structure the world around them. These cognitive structures define classes of entities to be found in the world (animals, inanimate objects, social kinds, etc.), and make ontological commitments about their nature. For instance, entities parsed as people are attributed beliefs and desires, whereas entities parsed as inanimate objects, like rocks, are not. The causal understanding of the behavior of people is governed by a kind of intuitive belief-desire psychology but (generally) not by the principles of physics, whereas rock behavior is understood by reference to physical causal principles such as gravity, but not beliefs or desires.

The difficulty arises in his description of domain specificity, where he argues that "a domain-specific competence functions as a stable response to a set of recurring and complex problems faced by the organism." It is, however, unclear whether he is talking about recurring in the sense of over the course of the organism's lifetime (ontogenetically) or over the evolutionary natural history of the organism (phylogenetically).

At times he seems to espouse the phylogenetic view, which gives a guide to defining domains, i.e., adaptive problems for which evolution can manufacture solutions. In contrast, his view that "domains" such as playing chess are the sort of repeatedly encountered problem for which a domain-specific competence can emerge seems to imply an ontogenetic interpretation. Clearly, chess-playing skills improve with experience, but the problem is that this kind of domain skill acquisition by experience seems to run into exactly the difficulty that domain-specific theories or constraints are supposed to solve, the insufficiency of experience alone to account for knowledge acquisition. It seems that Hirschfeld must either choose the phylogenetic interpretation or demonstrate how the ontogenetic interpretation avoids this pitfall.

Investigating the evolved psychology of how humans interpret human kinds is in its infancy, and investigators will undoubtedly continue to make progress in discovering the nature of the cognitive architecture that governs this ubiquitous mental activity. Hirschfeld's book will likely be the point of departure for future research into how humans spontaneously think about race - a topic that will remain central to social psychologists, sociologists, anthropologists, and anyone else interested in the ways in which human minds generate social behavior.

Human Sperm Competition: Copulation, masturbation and infidelity

By Robin Baker and Mark A. Bellis. Chapman & Hall, 115 Fifth Ave., New York, NY 10003 USA, 1995, $78.95 (hdbk.).

Reviewed by Esther Fallon, Psychology Department, University of Queensland, Brisbane 4072, Australia.

The authors of *Human Sperm Competition* are Robin Baker, a reader in Zoology at the University of Manchester, and Mark Bellis, Senior Scientist for HIV/AIDS monitoring and research at the Public Health Laboratory, Liverpool. Their approach to this book is from the perspective of evolutionary and behavioural ecology. They have also gone some way to integrate psychology, arguing that human behaviour will not be fully understood until an evolutionary perspective is applied to the psyche as well as physiology. Baker and Bellis do not just recycle past findings and reinterpret them in the evolutionary context of human sperm competition theory; their book contains almost as much of their own research, most of it using large human data bases specifically designed to test their theories.

Baker and Bellis define human sperm competition as the competition between sperm from different males for the prize of the egg(s) produced by a single female. When the authors began their research on sperm compeition in 1988, they found that academically "the area was virgin territory." They have since collated a mass of experimental research on nonhuman animals (beginning with studies on insect sperm competition in the 1970s) and humans (for example, the Kinsey reports). The collected body of research derives from every branch of the biological sciences and considers both the proximate and ultimate explanations of sexuality.

Because the authors are investigating relatively uncharted territory, they claim to expect controversy. In fact, the subtitle of the book suggests they may seek it. Alexander Harcourt (1995) wrote that "it is a pity that the authors [of *Human Sperm Competition*] often give the impression of being deliberately provocative, because I suspect that clinicians as well as their zoologist colleagues would have paid more attention to more gently presented findings and argumentation" (p. 129). Baker and Bellis expct that many people may consder the study of human sperm competition an unsavoury topic for academic consideration, because data on intimate human behaviour must be collected. They also challenge traditional biologial and current medical doctrine, which describe all characteristics of human sexuality as straightforward solutions to the requirements of fertilization. the authors argue instead that human sexuality in all its anatomical, physiological and behavioural detail is more likely the evolutionary product of sperm competition.

Baker and Bellis present their "kamikazee sperm hypothesis" as a major illustration of this argument. They believe that the majority of sperm are designed not for the pursuit of fertilization but for blocking or actively searching for and destroying sperm from other males in the female reproductive tract, that is, for literal sperm competition. Another argument they present in detail is the idea that evolved traits related to sperm competition are not limited to males. The authors suggest that females can and do promote sperm competition betwen the sperm of males via the evolution of behavioural, anatomical and physiological mechanisms, such as sperm storage organs, polyandry, and 'family planning' strategies to create a façade of monandry.

While the arguments about female sexual strategies are plausible and seem sound in the overall context of the book, the authors appear at times to think that scientific writing is exempt from the constraints of political correctness, and therefore sometimes, accuracy. They often use imprecise language which does not make clear the unconscious nature of sexual strategies. The result is text that, in places, smacks of misogyny: while males are portrayed as struggling (manfully) to out-compete other males, "females quietly and secretly manipulate and deceive them."

Another weakness of the book is the large number of assumptions, generalizations (from animal to human, from female to male) and speculations the authors make. Often they point out that they suspect that a certain process is occurring, but they cannot verify it for lack of research. Further, while the authors describe traditional theories of sexuality and other criticisms of these theories, they are forced to concede the lack of knowledge in the area.

They maintain that this dearth is simply because the idea of sperm competition in shaping human sexuality is such a novel concept, and that their main purpose for writing the book was to stimulate research. New approaches, they claim, are needed in this area for three main reasons: (1) scientific, to increase understanding of the reproductive behaviours of humans and other animals; (2) medical, to revolutionize the approach to infertility and artifical insemination; and (3) social, so that each sex may better understand the behaviour and motivations of the other. Throughout *Human Sperm Competition* they argue for additional research and suggest ways in which it may be carried out.

Baker and Bellis promote integrated interdisciplinary research to achieve a holistic understanding of human sperm competition. By no means are they purely naturists; they do not maintain that genes alone determine behavioural and other mechanisms of sexuality. In fact their book does not present the commonly argued dichotomous view of the nature/nurture issue. Instead they argue for an interactive approach between inherited attributes and the equally significant environmental influences in shaping those attributes.

Of particular importance is the environmental climate that they claim is produced between and within sexes of the same species, specifically in the development of balanced polymorphic strategies. To support this claim, Baker and Bellis pay particular attention to anthropologial and sociological data, examining differences between cultures as well as within them.

For this reason, the book should appeal to readers in a wide range of disciplines, from biologists, psychologists and medics with an interest in human sexuality, to evolutionary psychologists, anthropologists and behavioural ecologists. Through their adoption of a textbook style in *Human Sperm Competition*, Baker and Bellis facilitate comprehension by breaking the text into specialized chapters, and then more manageable sections and subsections. This results in a piece of work that is not only comprehensive and detailed, but also logically structured and able to be read easily from cover to cover. It is therefore likely to also be enjoyed and utilized by academicians, students and lay biologists and psychologists.

Despite the previously mentioned minor reservations, I find that Baker and Bellis make a convincing argument for the existence and significance of human sperm competition. This is no mean feat. Human sexuality is a diverse and complicated phenomenon; each element must work, not only by itself, but also in compex and subtle interaction with every other element. The result is an intricate tapestry in which each thread is reliant on the others both in its evolutionary development and in its current role promoting reproductive success of the individual. Baker and Bellis deftly cary out the difficult task of dissecting the various elements without losing sight of the delicate interplay among them. They are, ultimately, persuasive in arguing that sperm competition may have had a special role in developing each elemental thread as well as the patterns of the weave.

It is unlikely that *Human Sperm Competition* will make any significant changes in the practical world of science and psychology in the short term. However, the evidence and hypotheses Baker and Bellis have advanced are almost certain to have an important impact on research in this general area, particularly in relation to artificial insemination techniques. And while the book is likely to have many critics who find some arguments tenuous, *Human Sperm Competition* is as intellectually riveting as a good thriller.

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ANNOUNCEMENTS

Change of Current Literature Editors

After about 13 years of faithful and conscientious service, Bob Adams is withdrawing from the job of Current Literature editor of the *Bulletin*. Bob previously served as editor of the *Newsletter*. Under Bob's tenure, the Current Literature section received many favorable comments from members. There seems to be no comparable periodic compilation in our field.

Bob will be succeeded by Johan van der Dennen of the University of Groningen. We are very pleased to have so distinguished a scholar take over this important task. Johan is editor of the *European Sociobiological Society Newsletter*, and is Secretariat of that fine organization. I take it as a sign of good terms between our two groups that he has joined us.

Hereafter, then, please send notices of publications for listing in Current Literature to Johan (see Editorial Box). Be sure they have not yet appeared in that section. Include the full address of the first author.

Gesellschaft für Primatololgie

The fifth Congress of the German Gesellschaft für Primatologie will take place in Berlin 1-5 October 1997. For more information contact Prof. Dr. Dietmar Todt, Institut für Verhaltensbiologie, Haderslebener Str. 9, 12163 Berlin, Germany, e-mail todt@zedat.fu-berlin.de.

International Society for Research on Aggression

The 13th meeting of this group will be held in New Jersey in the summer of 1998, at Ramapo Collelge. For information please contact Prof. Roger N. Johnson, e-mail rjohnson@ramapo.edu.

International Ethological Congress

The 25th biennial IEC meeting will take place in Vienna 20-27 August 1997. For information please contact XXV IEC, Wiener Medizinische Akademie, Alser Strasse 4, 1090 Wien, Austria, tel. 43-1-405-1383-23, fax 43-1-405-1383-23, e-mail medacad@via.at, Internet URL
http://evolution.humb.univie.ac.at/events/iec.html.

Journal of Comparative Psychology

This American Psychological Association quarterly journal publishes empirical and theoretical research on various species, including humans. Areas such as behavior genetics, evolutionary psychology, behavioral rhythms, communication, comparative cognition, behavioral biology of conservation and animal welfare, development, endocrine-behavior interactions, methodology, phylogenetic comparisons, social behavior, and social cognition are covered. Editor is Charles T. Snowdon. Rates are $28 for U.S. APA members, $55 for U.S. others; $38 for international surface mail to members, $70 for others. For information: tel. 1-800-374-2721, fax 1-202-336-5568, e-mail www.apa.org/journals/com.html.

New Journal

European Psychologist covers research and development from the home of 48% of the world's psychologists. This English-language quarterly provides a platform for communication and cooperation among psychologists throughout Europe and the world. Devoted to psychology in its full breadth, the journal provides articles, reviews, and reports that address the international psychological community. Editor is Kurt Pawlik. Rates are $39 for American Psychological Association members regardless of location, and $49 for others. See previous item for contact numbers.

Biology and Politics Web Site

Research Committee #12, Biology and Politics, of the International Political Science Association has a new site on the World Wide Web:
http://ourworld.compuserve.com/homepages/steven_peterson_6.

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June 1997

Compiled by Bob Adams

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