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THE STORKS OF SALAMANCA

Upon arrival in this Spanish medieval university town the first thing I noticed were the large graceful storks circling overhead and nesting in the ancient towers, and I knew that this was a special place. Human ethologists were flying in too from all over the world for the 15th Biennial Conference of the International Society for Human Ethology. Our Spanish hosts, Francisco and Sally Abati, did a marvelous job welcoming their guests, and organizing the conference, banquets and several excursions. Plenary speakers were José Miguel Fernández Dols, Jaak Panksepp, and Carol Worthman who each addressed the theme "ethology of emotion". For more info on the conference see Society News and Photo Gallery.



HOMO SAPIENS IS BIOCULTURAL OR IS NOT: SOME RUBICONS ARE WIDER THAN OTHERS

by Wade Mackey

Rule #1: All politics are local.

- Rep. Tip O'Neill

Rule #2: All long-term politics are reproductive strategies.

Rules #3: All effective long term politics camouflage Rule #2.

- Ipsoc Macquire

If students of human ethology see humans as biocultural beings whose contemporary exemplars are products of generations of highly filtered ancestors, then both the "bio-" part and the "-cultural" part would be given focused attention. If both facets are not simultaneously given proper deference, then a distorted perspective is almost assuredly guaranteed. To resurrect an analogy, the knowledge of the area of a rectangle is difficult to determine if only the height is given, but the width is kept secreted. Let me suggest that current human ethologists are comfortable with rigorously analyzing "height", but are decorously and assiduously keen on avoiding "width".

To wit: A small to medium cottage industry has emerged and solidified which profiles the "biological " package that humans have inherited from Lucy and her descendants. Studies on behavioral strategies which lent an evolutionary advantage to some of our ancestors, but precluded others from having descendants, have graced journals and libraries shelves on several continents for several decades. Yet obvious cultural analogues with the same result have been systematically "no-shows" in those same journals and those same shelves for those same decades. With only one caveat, the fact that cultural evolution is occurring with some robustness is remarkably easy to demonstrate.

The caveat

It is a logical category that agencies of the United Nations, the staff of the U.S. Bureau of the Census, and the staff of the Yearbook of American and Canadian Churches are all engaged in a conspiracy of distorting the demographics relative to their respective charge. While keeping the caveat in mind, let's assume, just for the moment, that no such conspiracy exists, and that the numbers these organizations present are clean enough to be diagnostic. Diagnoses are available from a global perspective and from a perspective specific to the U.S. A global perspective The United Nations inter allia1 clearly illustrate that some countries/areas, e.g. Germany /Europe, are reproducing below replacement value (2100 children [minimum] per 1000 women); whereas other countries/areas, e.g. Egypt/Islamic north Africa & the Middle East are reproducing well above replacement value. It seems intuitive that, across generations, any biocultural formula which procreates in excess of replacement value will supplant or displace any biocultural formula which fails to replace itself.

Virtually all of Europe is below replacement value. In contrast, all of the Moslem swathe, running eastward from Mauritania to Pakistan, is well above replacement value. Not only is this swathe of countries attaining a greater proportion of the world's population per year, it is also sending to Europe large numbers of immigrants who bring with them their biocultural packages which generated their initial existence as well as the maintenance of that existence to adulthood. The ratio of the immigrants' assimilation into the "European" worldview versus cultural diffusion is not known, but certainly the level of cultural diffusion is not zero. As long as the reproductive gap between Europe and the Moslem swathe continues, then the Moslem swathe's biocultural formula is poised to supplant/displace the Europeans' biocultural formula: i.e. cultural evolution will occur.

A U.S. perspective

Within the hurly-burly, polyglot mega-tribe of the U.S., two patterns are clear. First, the U.S. has been below replacement value since 1972. Second, rates of natural increase are not

evenly distributed among the disparate groups. "Religion" serves as a good vehicle to illustrate the variable rates. The Shakers operated at the zero mark and are simply an historical footnote. From the 1960s to the 1990s, the numbers of Unitarian-Universalists shrank by 15%. During the same time frame, the ranks of Episcopalians decreased by a quarter.

On the other side of the coin and for the same time interval, the numbers of Mennonites and Latter Day Saints more than doubled. The Old Order Amish trebled and the Hutterites quintupled. Quintupling will beat stasis any day of the week, i.e. cultural evolution will occur. It would seem a tough case to make that the fertility differences - which are large - reflect differences in the biological potential to conceive and to birth children. The feedback loop that is more likely to be a reality is the one between cultural traditions and a psychology consonant with the expectations of those traditions.²

The key factor in both the global and the U.S. samples seems to be how a "culture" treats the relationship between the genders. A group/tribe/culture/clan can socialize its members to expect and to actualize gender egalitarianism or to expect and to actualize gender complementarity. Empirically, a bias toward gender egalitarianism, viz. Europe, Unitarians, Episcopalians, tamps down birth rates. A bias toward gender complementarity, viz. the Moslem swathe, Mennonites, Latter Day Saints, Amish, Hutterites, is associated with elevated birth rates. As mentioned earlier, the lack of focus by behavioral scientists on the "-cultural" facet of Homo's biocultural character and subsequent evolution is fairly obvious. What is less obvious is "Why?" "Why" is there a lack of interest. Our discipline, compared to other behavioral sciences, is much better positioned to understand the dynamics involved and then to explain those dynamics to the "huddled masses". An emphasis on the "bio-" part of the human condition, at the expense of the "-cultural" part, may not well serve the interests of our discipline nor those of the "huddled masses"

.FOOTNOTES

1 For the data, see Sivard, R. (1995) *Women...A world survey*. Washington, D. C.: World Priorities; UNESCO (1994) *Statistical*

Yearbook. NY: United Nations; United Nations (1990) *Demographic Yearbook*. New York: United Nations; United Nations (1992) *Demographic Yearbook*. New York: United Nations. United Nations. (1995) *Demographic Yearbook*. New York: United Nations. U.S. Bureau of the Census (1972 - 1997) *Statistical abstract of the United States (92nd -117th ed.)* Washington, D. C.: U. S. Government Printing Office; *Yearbook of American and Canadian Churches (1926 - 1998)* Nashville, TN: Abingdon Press, 1926 - 1998.

2 Using Occam's razor with some skill, Judith K. Brown ([1970] *A note on the division of labor*. *American Anthropologist* 72: 1073-1078) gives to us a clean theoretical perspective on the issue. Both J. Barkow ([1980] *Biological evolution of culturally patterned behavior*. In J. Lockard (ed.) *The evolution of human social behavior* [pp. 277-290]. NY: Elsevier); and W. H. Durham ([1990] *Advances in evolutionary culture theory*. *Annual Review of Anthropology* 19: 187-210) give a textured context to the argument.

ISHE FINANCIAL STATEMENT:

JAN - JULY 2000

BALANCE JAN 14, 2000	\$19481.52
<u>CREDITS</u>	
MEMBER'S DUES	\$2195.00
<u>DEBITS</u>	
BANK CHARGES	\$115.30
BULLETIN PRINTING	\$2435.56
BULLETIN POSTAGE (DEC 99/MAR 2000)	\$878.53
EDITOR'S EXPENSES	\$678.00
AIRFARE FOR SPEAKER (DR. PANKSEPP)	\$1022.52
DEBIT TOTAL	\$5129.91
BALANCE JULY 31, 2000	\$16546.61

Society News

Montreal was chosen as the site for the next ISHE conference of 2002 primarily because of accessibility. It was felt that too few members and students would be able to go to Australia. Potential sites for 2004 include: Moscow, Dublin, New York, Netherlands, and Oxford. Several themes for 2002 were mentioned: observation and measurement, primatology, conflict and conflict resolution, sex and gender - but nothing was decided upon yet. We will keep the format of inviting 3 or 4 plenary speakers to address the chosen theme. People expressed an interest in earlier publicity, advance press contacts, funding to enable us to offer special rates to students and retired faculty, and assistance to scholars from poorer nations to attend. Depending on attendance, an extra day may be necessary, especially to provide a free afternoon during the conference.

Call for Nominations

At the officer's meeting Karl Grammer informed us that he will be resigning as secretary so that he can devote more time to the ever-expanding ISHE web site. The officers have suggested that we create a new position for Karl as ISHE webmaster. If any member would like to contribute his or her thoughts on this matter please contact Linda Mealey.

Nominations for secretary are to be submitted to the current Secretary, Karl Grammer (at karl.grammer@univie.ac.at or see Officer's box for address), who will ensure that the nominee is willing to stand for office. Ballots for the election will be included in the next Bulletin. The duties of the secretary include chairing and reporting on Society meetings and soliciting nominees for offices. The officers make most of the decision's for the Society under our by-laws.

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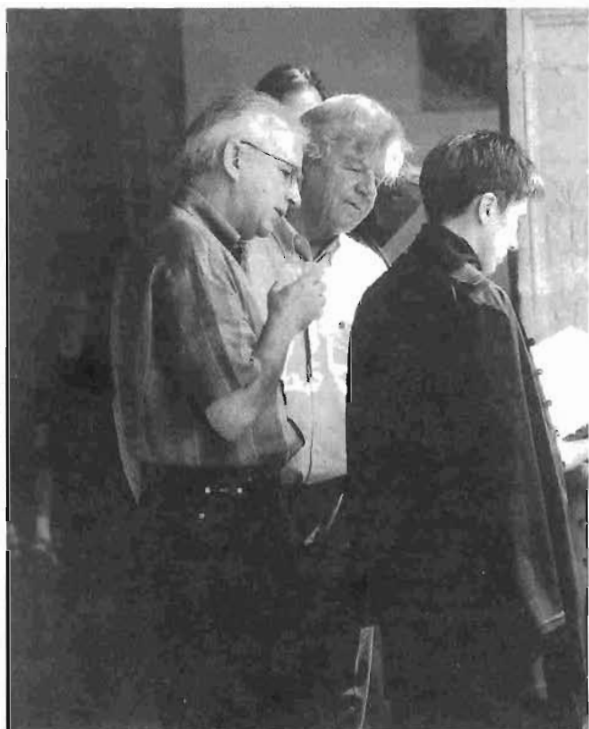
The Human Expression of Emotions: Facts, Challenges and Controversies.

José Miguel Fernández Dols
 Universidad Autónoma de Madrid

One of the most serious problems for contemporary psychology is its lack of descriptive background. For more than a century, psychologists have been trying to develop a coherent body of theory by resorting to an incoherent set of popular beliefs, anecdotal evidence, specific experiments, and questionnaires of all kinds. The outcome of this state of affairs is a series of assumptions based on researchers' and people's own versions of their behavior or, in the best of cases, the description of a few subjects in quite specific and not necessarily representative experimental versions of particular phenomena.

The psychology of emotion, and the psychology of the expression of emotion, is not immune from these problems. Darwin (1872) used anecdotal evidence and some questionnaire-based data to establish the universality of some emotions and their corresponding "expressions". Since then, research on facial expression of emotions has adopted different and even contradictory perspectives, but has always been based on a kind of empirical evidence that does not involve observational data, i.e., data on the actual behavior of people who are feeling intense, significant emotions.

Current psychology takes it for granted that people express a few basic emotions through a few prototypical expressions. Paradoxically, however, most of the empirical foundations of this assumption consist of data obtained from questionnaires or are not directly related to actual expressions of emotion. The relatively small amount of observational evidence on people who are feeling emotions has thrown up quite unexpected and somewhat disturbing data. My collaborators and I have been working on people's expressions in natural or quasi-natural encounters: bull-fighters, gold medalists, scared children, or students watching horror movies. We have found an interesting array of facial displays that do not fit current assumptions about facial expression and pose interesting challenges for future research and for the very concept of "expression of emotion".





Clockwise starting at top left:
(a) Detail from the inner courtyard at the Palacio Fonseca; (b) ISHE President Linda Mealey and plenary speaker, Jaak Panksepp, enjoying a banquet dance; (c) post-talk post-hockery with Jaak, Astrid, and Bill; (d) Street scene in Salamanca; (e) Viennese students during a mid-morning break (bleary-eyed after latenight extra-curricular activities); (f) Spanish minstrels at the banquet dinner; (g) Our wonderful hostess, Sally Abati; (h) A Viennese study session.
(Photos by Peter LaFreniere)

ISHE 2000 Plenary Abstract

Affective Neuroscience and Socioemotional Systems of the Brain: The Psychobiology of Sadness, Play, Laughter and Joy--Implications for Understanding Psychiatric Disorders.

Jaak Panksepp, Department of Psychology, Bowling Green State University, Bowling Green, OH 43403 USA.



The paradigmatic bases of the emerging field of Affective Neuroscience will be discussed. After a general summary of the basic emotional systems of the mammalian brain, most of the presentation will synoptically summarize our knowledge of how socio-emotional processes are elaborated at basic subcortical levels that all mammalian species appear to share.

Mammalian brains contain a variety of inter-related systems for controlling social behaviors and regulating social affect. All socio-emotional systems find their evolutionary roots among the circuits which control male and female sexuality. For instance, maternal CARE systems are organized around the same neurochemistries that control sexual urges--including opioids, oxytocin, vasopressin, and prolactin, among others. Those systems also control early social-attachments, partially through the modulation

of pain related brain systems that mediate separation-distress (PANIC).

The neuroanatomies and neurochemistries of the separation response have recently been clarified, and include activational neurochemistries such as glutamate and CRF, and inhibitory chemistries such as endogenous opioids, oxytocin and prolactin. It is through the neurodynamics of such emotional systems that social attachments, and the possibility of higher social emotions such as love, are established within the mammalian brain.

A related birthright of human and other mammalian brains is the impulse to engage in basic forms of play. PLAY systems are beginning to be systematically analyzed, providing new conceptions of the biological sources of joy. The neural systems for rough-and-tumble play are subcortically organized, with concentrations of key circuits in dorsomedial diencephalic and mesencephalic brain areas. Endogenous opioids which promote social bonding, can arouse playful tendencies in animals, while a large number of chemistries, including psychostimulants, such as those used to treat ADHD (Attention Deficit, Hyperactivity Syndrome) children, suppress such urges.

Evidence for the relationship between ADHD and play systems will be critically addressed. Congruences include various neuroanatomical (frontal lobe) and neurochemical (biogenic amine) relationships. The possibility that ADHD symptoms can be alleviated through the institution of early play interventions and other socio-environmental programs (i.e., the construction of more productive learning environments) will be discussed.

The premise that play systems of the brain are a fundamental source of joy has recently been supported by the discovery of a primitive form of laughter in rodents. A 50-KHz vocalization pattern is exhibited by young rats during play as well as during tickling. This response is especially closely related to playful impulses, providing a novel way to analyze systematically the unconditional and conditional sources of joy within the mammalian brain. This discovery has also yielded novel new approaches for studying the motivations underlying addictive behaviors.

BOOK REVIEWS

Where Biology Meets Psychology

Edited By Valerie Gray. Publisher, publisher's mail or email address, date. ISBN 0262581744 [Pbk, \$25,] and/or [Hdbk, \$60], #pp. 384

Reviewed by Stuart Silvers, Dept. of Philosophy, Clemson University full mail address. E-mail: sstuart@clemson.edu

Where Biology Meets Psychology, edited by Valerie Gray, is a collection of sophisticated philosophical essays on the concept and role of (Darwinian) evolution in contemporary cognitive psychology. The contributors to this volume include authors of important books in the philosophy of biology and philosophy of cognitive science and building upon these works conveys a distinct sense of "uncharted terrain" to the issues they explore here. The essays emanate from a 1997 conference of the International Society for the History, Philosophy, and Social Studies of Biology that was designed to bring together professionals for serious interdisciplinary exchange. The result is a successful anthology that I believe is a valuable resource, particularly for graduate study at the intersection of philosophy of biology and philosophy of psychology.

The book is divided into six sections, each dealing with its own circumscribed set of issues, and each presenting competing and conflicting theses. The sections are, Function and Teleology, Evolutionary Psychology, Innateness, Philosophy of Mind, Philosophy of Science, and Parallels Between Philosophy of Biology and Philosophy of Psychology. There are overlapping concerns with natural laws, explanation, representation, adaptation, and cognition. Because of its size and richness I will not be able to discuss all the papers in the collection. I've chosen the ones I found most provocative.

The first section deals with one of the central methodological issues in biology, adaptive explanation, to elucidate the notion of *function*. Karen Neander's specific concern is with coherence of *teleosemantic* theories that

ascribe a natural, teleological function to the human (and higher-order species) brain's capacity to represent its environment. Representational capacity confers an adaptive advantage upon organisms having it and thus enhances their fitness. Representations have content or meaning (which is what makes something a representation) and teleosemantic theories explain content in terms of causal relationships between an environmental occurrence and the organism's representation of it. *Ceteris paribus*, the natural function of representation R with content F is to *indicate* (or to *mean*) the presence of F in the environment if and only if F is there. This "standard" causal-teleological answer faces two debilitating technical problems: *atomism* (everything we think is individually causally generated) and *nativism* (concepts are acquired without learning). More generally, the questions are. "How can we represent non-existent things (unicorns)?" and "What selectional advantage accrues to this ability and our inability to detect some actually existent things (quasars)?" Neander outlines a molecular variant of teleosemantics where complex concepts are constructed from semantic simples. It's the work of scientists, not philosophers to discover what the semantic simples are for creatures with representational capacity.

Editor Hardcastle's paper distinguishes three models of *function*: ecological (backward looking), propensity (forward looking), and causal (atemporal). Each is subject to a crippling objection that they sanction explanations that are circular and/or vacuous. She recommends a pox upon all such philosophical houses and urges that we look instead to the diversity of functional explanation that scientists actually use in theory construction, whether in biology, anthropology, psychology, psychiatry, neuroscience, etc. Her point is that we eschew dictating from without (as it were) what an organism's functional requirement might be and adopt instead a notion of function as an explanatory heuristic. The explanations help us understand the organism's behavior and we derive the specific kind of function from it, and not *vice versa*. Her point is surely well taken but might be even stronger were she to acknowledge that scientists' use of technical language reflects a variety of extra-scientific influences, requiring

ultimately a kind of reflective equilibrium among linguistic resources.

Evolutionary psychology (or Psychological Darwinism) is one very hot topic these days. The section on evolutionary psychology has four papers. Grantham and Nichols' paper, "Evolutionary Psychology: Ultimate Explanations and Panglossian Predictions" is a critique of the predictive portion of the well-known and controversial Cosmides and Tooby hypothesis "that the human mind is a set of cognitive mechanisms that are adaptations to the environment of the Pleistocene" (p. 47). Grantham and Nichols argue that the Cosmides-Tooby project of studying the adaptive problems our ancestors faced can provide plausible ultra-adaptationist (Panglossian) evolutionary explanations of the development of our cognitive mechanisms as optimal solutions to them. However, they argue there is a significant asymmetry vis-à-vis the predictive potential of the theory.

Paul Davies' "The Conflict of Evolutionary Psychology" continues his sustained critique of evolutionary psychology. His main point is that the methodology of evolutionary psychology is barren: it tells us nothing about adaptive functions that we don't (and must) already know from the research methodology of contemporary cognitive psychology. Focusing on the evolutionary speculations of Cummins (1996), Davies argues that she conflates the methods of evolutionary history with those of cognitive psychology; Thus what appears to be a fruitful evolutionary hypothesis about adaptive psychological functions turn out to be plausible only because non-evolutionary cognitive psychology reveals that the functions in question are adaptive.

Lawrence Shapiro's "Presence of Mind" analyzes what he calls *best inference to mind* (a kind of ontological inference from theory to entity) by examining when it's appropriate to postulate psychological mechanisms to account for behavior-in-an-environment. In particular, he discusses the *environmental complexity argument* but concludes that because evolution can provide organisms with matching cue detectors to accommodate complex environments, a *kind of complexity* going beyond mere physiological detection is required for strategies dealing with

one's environment. He argues that it's environmental *heterogeneity* that evolves minds; the heterogeneity of cues provides information about the adaptive significance of the environment. When cues vary greatly, selection favors organisms with psychological (abstractive) means of identifying relevant conditions; that is, organisms represent to themselves the cues they detect as *evidence* of something else out there.

In "DeFreuding Evolutionary Psychology," David Buller argues that evolutionary psychology commits the sociobiological fallacy of confusing the origins of mind with the nature of mind. This same fallacy is present in natural law theories of ethics that confuse the origin of sexual function (reproduction) with the nature of sexual function, (intimacy, pleasure, etc.)

The Innateness section contrasts Ariew's narrow canalization view of innate capacity with Wimsatt's broad generative entrenchment theory of evolution. The latter offers a précis of his well-known integrated thesis of biological, cultural, and scientific evolution, the depth and richness of which I cannot hope to capture here.

In the section on Philosophy of Mind, Kim Sterelny considers the recent artificial life (AL) theory of intelligent action without internal representation and finds it unconvincing and inadequate as a theory of behavior. It does escape the dreaded frame problem but at the expense of an otherwise implausible theory. The notion of intelligence without internal representation reminds one of J. J. Gibson's direct (unmediated) realism project. The AL view consists in four claims: (1) that behavior is partitioned into modular, task-oriented skills; (2) that the behavioral repertoire of complex creatures is built up by adding behavior modules; (3) that classical AI underestimates environmental information; and (4) that there is behavior coordination via built-in motivational structures using environmental information. The hypothesis of local motivational cues is implausible because environmental complexity and complexity of behavioral control require an explanation of the relationship between the two. Representationalism not only explains this relationship, it offers a rich explanatory resource for what Sterelny is right to assert is essential to any concept of intelligent behavior,

an evolutionary theory of desires, the evolutionary explanation of preference ordering.

Finally, Wilson's "Individuals in Biology and Psychology" portrays a deeply significant parallel between biology and psychology. He gives a comparative analysis of the concept of the individual in the research programs (or metaphysics) in biology and cognitive science. He starts with an examination of Dawkins' (1982) idea of "extended phenotype," which Dawkins claims is a logical extension of his selfish gene theory. In biology, the issue is the proper characterization of the unit of selection: genes are nature's best (or only) replicators and the question is whether the phenotypic host is (narrowly) the individual organism or the wider relational context of organism and environment. The core of Wilson's critique of the extended phenotype thesis is that because Dawkins acknowledges no principled constraints on the relation between organism and environment, typically phenotypic effects required for variation *dissipate* into the world at large. Philosophers of science would say that Dawkins' science of extended genetics fails to pick out, and thus fails to project, any *natural kinds* for an evolutionary theory of heredity. There is then in Dawkins' extended genetics no distinction between and (even systematically) accidental correlations in phenotypic effects. Wilson then sketches an alternative view of individuals that, he argues, steers between (now amorphous) extended genetics and *smallism*, a kind of methodological individualism that fails for never addressing the question of what counts as an individual. Drawing on the narrow versus wide supervenience debate about the bearer of mental states in the philosophy of mind and cognitive science, Wilson argues that the same problems and their resolution apply *mutatis mutandis* to genetics and the theory of phenotypes. His alternative involves the identification of those various but *systematic* relationships between genes, organisms, and environments that constitute natural phenotypic kinds over which evolutionary theories of heredity causally generalize.

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- Evolutionary Aspects of Nutrition
and Health: Diet, Exercise,
Genetics and Chronic Disease***
- Edited by A. P. Simopoulos. (World Review of Nutrition and Dietetics: Vol. 84). Basel: S. Karger AG, 1999. hdbk, xiv + 145p., 17 figures & 21 tables, \$172.25. ISBN 3-8055-6827-4.
- Reviewed by Thomas R. Alley, Department of Psychology, Clemson University, Clemson, SC 29634-1355, USA.
- The five papers in this volume are proffered as representing "the state of the art in the evolutionary aspects of diet, physical activity, genetic variation and dietary response" (p. ix). This book does provide excellent reviews of several key issues, and the recurrent discussion of prehistoric hominid diets and the hunter-gather model will certainly be of interest to most ethologists.
- The book begins with a brief Preface in which the editor provides an overview of the book's contents. The first chapter, by James Neel, uses an evolutionary perspective to advocate a "lifestyle approach" to diet-related health problems. The basic framework, which is used throughout this volume, sees health problems stemming from the slow pace of human evolution

compared to the rapid changes in the human nutritional environment. Next, in the book's longest and perhaps best chapter, Loren Cordain focuses on one of these changes: the "double-edged sword" of our heavy reliance on cereal grains (estimated at 56% of human food energy). Due to agricultural development, in the evolutionarily brief period of about 10,000 years we have gone from "non-cereal-eating hunter-gathers" to being primarily dependent upon just eight grains. Consequently, there is "considerable genetic discordance" between our use of these grains as food staples and the foods to which we are genetically adapted, resulting in a variety of nutritional and physiological problems when grains are consumed in excessive quantity.

The middle chapter discusses the "carnivore connection" hypothesis that an insulin resistant genotype evolved because it was advantageous to our ancestors who lived in a high meat, low carbohydrate ('carnivorous') nutritional environment. Next, J. D. Chen reviews "Evolutionary aspects of exercise", focusing on respiratory and cardiovascular benefits, obesity, and non-insulin-dependent diabetes (NIDDM). Finally, the editor himself reviews "Genetic variation and nutrition" in a chapter that advocates the development of individualized dietary recommendations that consider energy expenditure and genetics predisposition.

Given the fundamental importance of food selection and eating behaviors, it is clear that ethologists and psychologists have not paid sufficient attention to these topics. Perhaps greater familiarity with the findings, issues and questions presented in this volume would lead to a reduction of this research gap. For instance, the split between hunter-gather and modern Western diet and lifestyle, a topic discussed in several chapters, raises interesting questions about how humans have strayed so far from the food-related behaviors of our environment of evolutionary adaptedness. Unfortunately, at more than \$1 (USD) per page, I cannot recommend that individuals or libraries with limited budgets purchase this volume unless a there is a clear need for the specific contents.

Ethnic Conflicts Explained by Ethnic Nepotism

By Tatu Vanhanen. Research in Biopolitics, Vol. 7, Stamford CT: JAI Press, 100 Prospect Street, Stamford CT 06901-1640, USA, 1999, pp. xix + 370 [hdbk, ISBN: 0-7623-0583-5].

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

Ever since biosociologist Van den Berghe (1981) presented his evolutionary analysis of the 'ethnic phenomenon', the 'primordialists' (who had the nerve to suggest that ethnicity might be more than an arbitrary sociocultural construction) have been drawing heavy flak from the social scientists, especially sociologists and historians, either due to total ignorance or total abhorrence of the evolutionary approach. Primordialism (*versus* social constructivism or instrumentalism: a 'silly' controversy based on an untenable antinomy, as Van den Berghe never tired of pointing out) was ridiculed, maligned, and rejected, almost as a matter of course, in most textbooks on ethnocentrism and nationalism. (Although, as Vanhanen rightly observes, cultural and primordial elements are mixed in most definitions of ethnicity). Recently, however, the tide seems to be turning, and Vanhanen's macroquantitative research on ethnic conflicts, a life-work spanning several decades, is an important contributing factor.

Conflicts are common in all countries of the world where people are divided into separate groups on the basis of racial, ethnic, national, linguistic, tribal, religious, caste, or other differences. The central argument of Vanhanen's study is that a significant part of the universality of ethnic conflicts can be explained by our evolved predisposition to ethnic nepotism, which is regarded as an extended form of kin nepotism. Evolutionary theories of inclusive fitness and kin selection (as first formulated by Hamilton) explain the evolutionary origin and universality of nepotism. Ethnic groups can be perceived as extended kin groups or 'superfamilies'. Thus, members of an ethnic group tend to favor other members over nonmembers because they are more

closely related to their group members. This disposition to favor kin over non-kin, and close kin over distant kin, becomes important in social life and politics when people and groups of people have to compete for scarce resources.

People belonging to the same ethnic group tend to support each other in conflict situations. Van den Berghe ([1981]1987) noted that "the degree of cooperation between organisms can be expected to be a direct function of the proportion of the genes they share; conversely, the degree of conflict between them is an inverse function of the proportion of shared genes" (p. 7). Our tendency to favor kin over nonkin has extended to include large linguistic, national, racial, religious, and other ethnic groups. The term 'ethnic nepotism' can be used, according to Vanhanen, to cover this kind of nepotism at the level of extended kin groups. It does not matter, from the perspective of ethnic nepotism, what kind of kin groups are in question. The crucial characteristic of an ethnic group is that its members are genetically more closely related to each other than to the members of other groups. Therefore, in this study 'ethnic groups' refers, not only to racial, tribal, and national groups, but also to language groups, castes, and old religious communities. A problem with this definition is that people are related to each other at many levels, from the level of the nucleus family to the species level. Ethnic groups are therefore never absolutely distinct and exclusive. Any level can provide a basis for ethnic nepotism. In other words, the boundaries of ethnic groups depend on the situation and are always to some extent socially constructed. Furthermore, Vanhanen assumes that our behavioral predisposition to ethnic nepotism, because of its evolutionary roots, is an integral part of human nature shared by all human populations.

Two basic hypotheses on the political consequences of ethnic nepotism are presented: (1) significant ethnic divisions tend to lead to ethnic interest conflicts in all societies, and (2) the more a society is ethnically divided, the more political and other interest conflicts tend to become canalized along ethnic lines. These two hypotheses are tested using empirical evidence covering 148 contemporary states during the period 1990-1996. 'Ethnic divisions' and 'ethnic conflicts' are operationalized into empirical variables, and the data are analyzed by

correlation and regression techniques. An important part of the book (pp. 79-203) is dedicated to profiles of ethnic cleavages and ethnic conflicts in single countries, in which the conflicts are examined in greater detail together with the institutional and other situational factors that could explain large deviations from the overall pattern.

Vanhanen finds that the degree of ethnic conflict is indeed strongly related to the degree of ethnic divisions. Moreover, ethnic divisions seem to have produced ethnic conflicts in practically all countries of the world. Wealthy and highly developed countries seem to be nearly as vulnerable to ethnic conflicts as poor and traditional societies. According to Vanhanen, this is because all human populations share the same adaptive predisposition to ethnic nepotism. Therefore people easily learn ethnic attitudes and adopt psychological mechanisms associated with prejudice, scapegoating, and discrimination.

The results indicate that, as hypothesized, ethnic conflicts have emerged in practically all ethnically divided societies, which implies that the question is of a universal human phenomenon. Everywhere in ethnically divided societies people belonging to the same ethnic group tended to align with their relatives in social and political interest conflicts. They are following the imperatives of ethnic nepotism, their unconscious impulse to support their relatives in conflicts because it has furthered their own inclusive fitness. Therefore, ethnic conflicts in various forms are very common in ethnically divided societies. ... Furthermore, if the ultimate cause of ethnic conflicts is in human nature, in our evolved behavioral predispositions or epigenetic rules, we have to accept the conclusion that ethnic conflicts are extremely persistent, if not absolutely inevitable, in ethnically divided societies (p. 231).

Some other findings of this ambitious macroquantitative research project are (1) that external interventions tend to intensify ethnic conflicts (although they do not cause them); (2) that the struggle for territorial rights tends to intensify ethnic conflicts; and (3) conspicuous inequalities between ethnic groups tend to intensify ethnic conflicts.

Earlier studies by Rummel (in the 1960s) and Haas (1974) (reviewed in Van der Dennen, 1981), which are not cited by Vanhanen, found that the heterogeneity in composition of a population is consistently associated with the frequency of wars, military actions and foreign conflict casualties. Countries with many different ethnic groups, language communities, nationality groups and religious and racial groups enter wars more often than homogeneous polities. Rummel (1997) recently found that two simple measures, the number of ethnic groups and the number of religious groups a state has, are related to its collective (internal *and* external) violence: the more groups the more violence. A modest number of other quantitative studies found that ethnic diversity is also a major predictor of several indices of (violent) crime.

Not mentioned by Vanhanen either is my ongoing research on ethnocentrism-cum-xenophobia in pre-industrial societies (and many primate and social carnivore species), where it seems to be universally present too (reported in Van der Dennen [1995]). Further support for the primordial nature of ethnocentrism and ethnic nepotism is presented in Reynolds, Falger & Vine (1987), Shaw & Wong (1989), Flohr (1994), and the recent volume edited by Thienpont & Cliquet (1999). Concern for and commitment to the own group ('community concern' and a 'sense of belonging' or group identification based on personal recognition) even seems to be characteristic of our 'phylogenetic nephews', the common chimpanzees (*Pan troglodytes*), according to De Waal (1996), but not surprisingly this is also the only species with extremely violent and lethal intergroup raiding in its behavioral repertoire.

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Species of Mind: The Philosophy and Biology of Cognitive Ethology

By Colin Allen and Marc Bekoff. MIT Press paperback <<http://mitpress.mit.edu>>, 1999, 220 pp., \$18.50, ISBN 0262-51108-8. (reprint of 1997 hdbk., ISBN 0262-01163-8).

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This volume deals with what may be the last frontier of ethology: cognitive processes - those intangible mental representations that often have complex and unpredictable effects upon behavior. The authors' main goal is to provide

the conceptual framework for "a viable empirical research program" to study cognition ethologically. Their rationale for their framework makes clear why what they offer is not just a clone of evolutionary psychology. Their ultimate goal is part of a larger attempt to naturalize the mind. That is, to define the mind as no less a natural object than a stone or a tree and to study them as such.

Historically, this attempt has run into at least two major problems. The most intractable problem has been finding a way to deal with two ontologically very different phenomena, the observable physical world and the subjective psychological world, without (and this is the hard part) misrepresenting the distinctive properties of each. As has been repeatedly demonstrated, no one has yet satisfactorily solved this problem -- either cognitive processes have evaporated in the face of observable neural electrochemical activity, or the latter has been judged as an irrelevant or uninformative material representation of the former. The second problem arises when consciousness is chosen (as is commonly done) as the target of study. For many reasons that can't be gone into here this choice almost invariably ends in failure. For example, as Allen and Bekoff point out, Donald Griffin's pioneering attempt to launch a new discipline of cognitive ethology in the 1970's and '80's ultimately shipwrecked itself by insisting to focus on consciousness.

Well aware of such problems, Allen and Bekoff take a more promising approach. I say this for three reasons. First, they make an impressive effort to grapple with the tough conceptual, definitional problems that plague cognition and its relationship to behavior. (Allen is a philosopher.) They then insist that these problems get resolved in such a way to ensure that empirical research is possible. (Bekoff is an ethologist.) A rare and productive combination to say the least.

Second, the authors rely on ethology's interdisciplinary approach to deal with problems raised by their conceptual analysis. Philosophy, psychology, anthropology, animal biology and ecology are called upon, when relevant, to address ethology's four questions of phylogeny, causality, function, and ontogenesis. And their first step to answering these questions is extensive field observation.

Third, unlike some current proponents of evolutionary cognition, Allen and Bekoff make no extravagant claims about their ambitious approach. Such modesty is welcome in face of many current explanations of human behavior provided by writers convinced that Darwin had it all right. He may be right, but until there are more descriptive empirical studies on the actual factors that shape so-called "evolved behavior" there is reason to be skeptical of their claims. For example, those studying evolutionary adaptations often neglect to obtain empirical evidence for the environment that purportedly shaped such adaptations. They rely instead on assumptions about the environmental contexts and circumstances in which the behavior occurred. A case in point: It is often assumed that savannas today are like savannas a million years ago and that is why a certain behavior has been selected for. Such an assumption may be right or wrong but giving a causal explanation without actually observing on an individual basis the environmental circumstances under which the behavior occurs and contributes to fitness is not good science. Observational studies obviously can not be carried out on events that happened millions of years ago. But observations can be made on events and the environmental circumstances associated with them if we have good reason to believe that they have not changed much over the lifetime of a species.

Let me explain. Allen and Bekoff choose two contexts that appear quite stable over species' time: social play and antipredatory behavior. Choosing these two contexts not only also addresses two important philosophical and cognitive issues, intentionality and representation, but two important biological issues, survival and fitness, as well. And to put empirical icing on their cake, social play and antipredatory behavior and their associated stimuli do not need to be assumed historical truths. They are available for observation here and now.

Social play is an excellent choice because conspecific behavior as an environmental pressure (unlike physical factors such as terrain) most likely does not change its selective value over phylogenetic time. Also, social play is a great source of "subtle behavior patterns" such as "facial expressions, eye movements, and body postures." As the authors point out, identifying such patterns helps fill the void left by the

"disturbing lack of detailed answers" of much existing empirical research (p. 112).

Antipredatory behavior has the same theoretical value and research accessibility as conspecific social behavior. As the authors note, "adaptive responses to predation" are found virtually in all living beings, plants and animals. Vigilance and predatory classification (by the animals themselves) are processes that directly implicate the ability of individual species' members to cognitively represent their environment. Both abilities as well as "estimation of number, social cooperation, and communication [as cognitive responses to predators] are all likely to have had their evolution affected by predation" (p. 138).

So much for brilliant armchair strategy. Now for the hard part: field observation. They do not back off. As they stress, "a return to basics is needed, for many studies have been conducted using simplistic and misleading presuppositions" (xiii). What is necessary is a big and broad database before coming to any conclusions about adaptations. This sounds like good, old-time scientific ethology.

The beauty of their whole approach is that it is consciously structured to thwart premature conclusions about behavior. Good science proceeds first with ample and comprehensive description before jumping in with explanations. Some researchers apparently do not understand that their explanations will most likely be incomplete if they limit their choices of data to favor only their theoretical presuppositions. That is why this book is so important. Even though Allen and Bekoff have faith in evolutionary theory this does not limit their choice of data. In fact, they are open to phenomena they may not currently be aware exist. Their message is: Let's look at the broad picture; let's consult folk psychology as well as everything we know scientifically about cognition and its relationship with behavior in general; let's look at different species in the wild; in short, let's be open to whatever is out there. Now this is a viable research program.

Essay/Review

Is MacDonald a Scholar?

Culture of Critique

*An Evolutionary Analysis of Jewish Involvement
in Twentieth-Century Intellectual and Political
Movements*

By Kevin MacDonald, Department of Psychology, California State University at Long Beach, Long Beach, CA 90840-0901 USA. Westport, Connecticut: Praeger, 1998.

Reviewed by Frank Salter, Max Planck Institute, Andechs, Germany

Most readers of this *Bulletin* will be aware of the controversy that embroiled ISHE member Kevin MacDonald at the recent annual meeting of our kindred organization, the Human Behavior and Evolution Society (HBES). At a special session MacDonald was charged with anti-Semitism and his scientific standing questioned. Any review must now be counted as contributing to that controversy since it bears on MacDonald's status as a scholar and evolutionary psychologist. With this in mind I decided to combine the book review with a description of the recent controversy concerning *The Culture of Critique* among human evolutionists. I shall be arguing that much of the criticism of MacDonald is founded on ignorance of his scholarship and a confounding of political and scientific issues.

Charges of anti-Semitism, political motivation, and shoddy scholarship are clearly plausible to many colleagues. The broad political Left, which constitutes the academic establishment since at least the 1960s, views interest in evolutionary accounts of human nature, and even claiming that such a thing exists, as tantamount to fascism (Singer 1998). This prejudice was directed at the pioneers of the evolutionary approach both in the U.S. and overseas, such as the late Bill Hamilton, Irenäus Eibl-Eibesfeldt, Edward O. Wilson, Richard Dawkins, Napoleon Chagnon, and many others.

The new leaders of America's evolutionary scene have been at pains to dispell this image. The name of a leading journal, *Ethology & Sociobiology*, was changed to an anodyne substitute, and an emphasis was maintained on cross-cultural universals at the expense of human biodiversity. Individual and group differences apart from age and sex are still largely ignored, with race and ethnicity conspicuous by their near absence from America's leading evolutionary academic journals—*Evolution and Human Behavior* (the HBES home journal), *Human Nature*, and *Politics and the Life Sciences*. Given such a defensive posture it is little wonder that a long, cold inspection of Judaism should raise a storm. What is one to make of a scholar who: (1) like so many anti-Semites takes pains to show the great overrepresentation of Jews in radical political movements such as post-WWI bolshevism in Russia and Central Europe, the Communist Party of America, and the New Left of the 1960s and 1970s (including the claim that in 1928 Jews were 1000% overrepresented among socialist Reichstag deputies); (2) who revives the old Nazi canard about Freud by alleging that he was a Jewish activist nurturing hatred of "Aryan" Europe, leading an essentially Jewish cabal of psychoanalysts intent on subverting Christian sexual standards, (3) who portrays Jensen's hereditarian theory of IQ as mainstream; (4) who maintains that on average Jews constitute a quarter of America's elites and draws attention to 58% representations in the senior ranks of Hollywood (which it "dominates"), 50% of network television producers, and 40% of elite university law faculties; (5) who maintains that since the mid 1960s the media elite has pursued a leftist agenda that includes enforcing racial integration; (6) who goes so far as to question the appropriateness of large Jewish overrepresentation in a democratic elite? (7) who suggests that European-Jewish intellectual prominence is genetically based and the result of eugenic processes within traditional Jewish communities; (8) who argues that Jewish intellectuals such as Franz Boas, Felix Frankfurter, Harold Laski, Max Lerner, Morris Cohen, and Robert Merton, accelerated the deChristianization of America's public life by selectively promoting as cultural heroes Gentiles who advanced their goals, such as Margaret Mead, John Dewey, and Justice Oliver Wendell Holmes; (9) who agrees with T. S. Eliot's most

famous anti-Semitic statement, that any large number of free-thinking Jews is undesirable if one wants to maintain or develop a society in which a Christian, ethnically homogeneous tradition can flourish.

Surely it is reasonable to be outraged at such a person being associated with a respectable academic association? Well, not if that person is Stanley Rothman, Mary Huiggins Gamble Professor of Government at Smith College, New York, who makes the first six of these points and is a member the Association for Politics and the Life Sciences (1974; 1978; Rothman & Lichter 1996/1982; Rothman & Snyderman 1988; Lerner et al. 1996; Lichter et al. 1986); or Prof. Arno Motulsky, Professor emeritus at the University of Washington, Seattle, who makes the seventh point (1995); or David Hollinger, Professor of History at UC Berkeley who makes points eight and nine and whose 1996 book was favourably reviewed in the Jewish press; but *certainly* if that person cites Rothman's, Motulsky's, and Hollinger's sources and becomes the centre of attention.

The fact is that most of the above descriptions (but not the speculations) are uncontroversial in the specialist historical and sociological fields on which MacDonald draws. As we shall see in the review, these and most other assertions that have elicited the wrath of some colleagues are not only true but truisms, to those acquainted with the diverse literatures involved. Apart from the political sensitivity of the subject, much of the problem facing MacDonald is that his knowledge is often too far ahead of his detractors to allow easy communication; there are not enough shared premises for constructive dialog. Unfortunately the knowledge gap is closing slowly because some of his most hostile critics, including colleagues who make serious ad hominem accusations, have not bothered to read MacDonald's books. If this sounds incredible, please read on.

The controversy

1. MacDonald agrees to testify as an expert witness for historian David Irving, the plaintiff in a defamation lawsuit against historian Deborah Lipstadt. The latter accuses Irving of denying the Holocaust. MacDonald neither denies nor minimizes the Holocaust but expresses concern about freedom of speech. His testimony, concerning certain Jewish organization's

techniques for silencing critics, is published as a court record available at MacDonald's webpage along with his correspondence with Irving before the trial (<http://www.csulb.edu/~kmacd>). After much controversy (reported below), Irving loses his case, and is found to be a Holocaust denier.

2. Journalist Judith Shulevitz writes a critical article in her *Culturbox* segment of *Slate*, an online magazine (24 Jan. 2000), criticizing MacDonald for giving evidence in the Irving-Lipstadt trial. Ad hominem are preceded by a confused summary of MacDonald's three books. Shock is expressed at MacDonald's statements on Freud, Jewish eugenics, and many more. Shulevitz makes several disparaging remarks about MacDonald's alleged prejudices, such as that his ideas about Jews "represent the broadest, ugliest, and most vicious anti-Semitism passing for scholarship in this country today." This is the beginning of an attack on MacDonald's academic standing. "A man in his 50s, MacDonald is still an associate professor of psychology at a third-rate school . . ." She expresses surprise that MacDonald has been allowed to hold his office of secretary-archivist in HBES and to be active within the organization. Why have evolutionary psychologists not "policed" their discipline? All of the leading HBES members interviewed by Shulevitz claim not to have read his books on Judaism, but "they expressed extreme shock and said he contradicted the basic principles of contemporary evolutionary psychology" based on Shulevitz's verbal summary of MacDonald's ideas. MacDonald replies in *Slate's* letters column (25 Jan. 2000) by describing Shulevitz's article as "yellow journalism." "Some of her statements are simply overly general, others simply false, while others are incomplete or take my thoughts entirely out of context." Regarding the personal attacks, he writes: "Actually I have been a full professor for about five years now. (I got a late start because of my involvement in 60's radicalism.) I like to think of [California State University Long Beach] as a second rate institution. It's not quite UC-Berkeley, but it's pretty good. Whatever Shulevitz may think, there are many fine professors and students here."

3. Answering Shulevitz's call for HBES members to take a stand on MacDonald, and on the basis of her summary of MacDonald's book, John Tooby,

HBES president, criticizes an aspect of MacDonald's thesis (Jewish genetic segregation), as well as an idea that is not part of MacDonald's theory (genetic group selection). Tooby agrees to a *Slate* discussion with Shulevitz, with MacDonald relegated to observer status and limited to defending himself in the letters section. In this discussion, Tooby claims that MacDonald is a "fringe" academic because of the low number of citations for his Judaism trilogy (not mentioning the substantial citation rate for MacDonald's other publications), that he does not qualify as an evolutionary psychologist because his ideas conflict with certain precepts set forth in Tooby's own writings, that his claim to be an evolutionary psychologist is quackery, and that his writings constitute a "crime" (*Slate* 3 Feb. 2000). In his last *Slate* posting (15 Feb 2000), Tooby refers to "the netherworld of marginal scholarship (of which MacDonald is a typical example)." In a subsequent article in the tabloid *Newtimes* L.A.(T. Ortega, "In the hotseat", 24 May 2000) Tooby compares MacDonald to the death-camp doctor Josef Mengele.

In MacDonald's 3 Feb. 2000 *Slate* response (see <http://www.csulb.edu/~kmacd/Tooby.htm>) he suggests that Tooby has not read his extensive review of population-genetic literature indicating that there are genetic frequency differences between Jews and Gentiles and that these differences have been maintained by endogamous Jewish marriage practices. There are, MacDonald notes, profound scientific differences between himself and Tooby: "While Tooby and [coauthor] Cosmides focus exclusively on domain-specific psychological adaptations designed to solve recurrent problems in our evolutionary past, I emphasize in addition the importance of domain-general mechanisms, especially the g-factor of IQ tests, that facilitate the achievement of biological goals in complex, non-recurrent environments. . . . My views have much more in common with those of David S. Wilson . . . and the cultural selection models of Robert Boyd and Peter Richerson."

Another HBES member who takes up Shulevitz's call is Steven Pinker (*Slate* 27 Jan. 2000), who states that MacDonald would never have been able to present papers at HBES conferences if the latter were peer reviewed, that HBES's official journal has never published an article by MacDonald, that MacDonald's ideas are "preposterous" and do not warrant the

attention of peers, that MacDonald posits genetic group selection for humans, and that his theories are consistently "value-laden". Pinker adds one criticism of merit, that MacDonald should have studied at least one control group to allow comparison with Judaism. In his books MacDonald does in fact compare Judaism with ancient Sparta, Roman society, and Medieval Catholicism, but by undertaking a new project on "diaspora peoples" MacDonald implicitly concedes that more work is needed in this direction. Pinker admits that because he has not read MacDonald's books it is possible he is being unfair, while indicating that Shulevitz's summary has saved him the trouble of such reading.

On a personal note, it is overdue that John Tooby and Steven Pinker apply their professional skills seriously to critique MacDonald's work in the appropriate scientific forums. This now seems obligatory as a matter of professional duty given the severity of their attack on a colleague who has refrained from ad hominem throughout this sorry event. Still, it is now too late to reverse the harm done to both MacDonald's and probably HBES's reputation by what can only be judged reckless, unscholarly, and plain uncivil slurs. For these they should apologize.

4. In response to Shulevitz, David S. Wilson (*State* 25 Jan. 2000) supports MacDonald based on a reading of his first volume, noting that he is engaged in developing a general theory of groups taking Judaism as an example. In what must be the understatement of the new millennium, Wilson attributes unscientific motives to MacDonald's HBES critics: "[I]t is shameful how quickly those who are sensitive to being demonized are willing to demonize others. Even evolutionary psychologists, who have experienced their share of persecution in academic circles, seem more concerned to protect their own reputations than to defend the work of their colleague."

5. At the June 2000 HBES, a session organized by D Kriegman discusses MacDonald's theory of Judaism, with MacDonald responding. Scientific questions are raised by Kriegman and John Tooby, but political concerns take centre stage, and no point of scholarship is raised in the discussion period. Richard Wrangham states that MacDonald's books are approved by neo-Nazi

organizations, and invites him to disown this connection, an invitation MacDonald implicitly refuses in his insistence on keeping to scientific issues. Fists are shaken at MacDonald from the floor. MacDonald had his supporters. At one point during proceedings, James Fetzner objects with a call for academic free speech and receiving loud applause.

Clearly this reaction to *Culture of Critique* by a journalist and some HBES colleagues constitutes an attempt to dismiss the author's standing as an evolutionary psychologist. It is one thing to question a scientist's political judgment, another to downgrade his status as a scientist and scholar. In the following synopsis of *The Culture of Critique* I sample each chapter's main sources. Are they credible? Are MacDonald's empirical claims well documented? As will become apparent, the sources for many of the claims for which MacDonald has been criticized are mainstream. It follows that if MacDonald (but not his sources) is to be condemned, logic requires that critics pick on aspects of his analysis that are distinctive to him. Following the synopsis I identify some of these distinctive aspects.

The book

The Culture of Critique is the third and final volume in MacDonald's trilogy on Judaism and anti-Semitism. His central thesis, stated in the first volume (*A People that Shall Dwell Alone*, 1994) is that Judaism is a group evolutionary strategy. This type of strategy is an experiment in living, one that can work or fail, that can raise or lower group members' reproductive fitness. An adaptive group evolutionary strategy protects inclusive fitness by achieving subsidiary goals such as resource acquisition, group defence and conquest. Group strategies are usually traditions, but can be invented using domain-general intelligence. They culturally manipulate evolved domain-specific psychological predispositions, such as dominance and ethnocentrism. The second volume (*Separation and its Discontents*, 1998a) applies the same approach to anti-Semitism in Medieval Europe, especially Spain, Poland, and Nazi Germany, positing a dialectic between Jewish and Gentile group evolutionary strategies.

The third volume brings the analysis up to the present, looking beyond traditional Judaism to examine the ethnic strategies of

secular, assimilating Jewish intellectuals. Common to such strategies has been intellectual criticism of Gentile society, religion, and institutions, which MacDonald maintains have been aimed at neutralizing actual and potential threats to Jewish security and status.

Chapter 1. "Jews and the radical critique of gentile culture: Introduction and theory." This is a brief review of historical sources on the radicalism of assimilated Jews, beginning in the Middle Ages, and sets out MacDonald's theoretical frame based on his first two volumes.

Chapter 2. "Boasian school of anthropology and the decline of Darwinism in the social sciences." It is argued that cultural anthropology in the United States was founded by a largely Jewish circle of academics led by Franz Boas, who had a strong ethnic identification, promoted universalist ideology, and opposed Darwinian thinking. MacDonald relies on such scholars as Frank (1997), Degler (1991), Hollinger (1996), Stocking (1968), and White (1966), all mainstream sources.

Chapter 3. "Jews and the left." MacDonald argues that radical ideology has been attractive to Jewish intellectuals because universalism blurs ethnic distinctions, defusing anti-Semitism and ameliorating marginality. The marginality thesis is not original, advanced by R. Michels before WWI and by C. Liebman (1979; quoted in Rothman & Lichter 1996/1982, 110—11, 118—19). Sources for Jewish overrepresentation on the Left include Rothman and Lichter and S. J. Gould, who thinks that most American Marxists are Jewish (Ruse *ibid.*).

Chapter 4. "Jewish involvement in the psychoanalytic movement." MacDonald portrays the early psychoanalytic movement as resembling the Boasian school in being a predominantly Jewish group idolizing an authoritarian leader. The robust Jewish identity of Freud and of the psychoanalytic vanguard, and Freud's racial chauvinism and hostility towards what he described as "Christian-Aryan" society are claims drawn by MacDonald from mainstream sources (see Rothman 1974; 1978; Yerushalmi 1991).

Chapter 5. "The Frankfurt School of Social Research and the pathologization of Gentile

group allegiances." MacDonald draws on a vast literature examining the ideas and social relations of the group of largely Jewish intellectuals gathered around Max Horkheimer and Theodore H. Adorno which, before and after WWII fused Marxism and psychoanalysis to produce a radical theory of psychosocial development and prejudice. Many leading members possessed a strong Jewish identity (Marcus & Tar 1986).

Chapter 6. "The Jewish criticism of Gentile culture: A reprise." Here MacDonald draws together the lines of analysis developed in the previous case studies, finding unifying threads of collectivism and valuing of consensus over individualistic disputation. He raises theoretical questions about the interface between evolved psychology and cultural messages: "What evolved features of the human mind make people likely to adopt memes that are inimical to their own interests?" (241).

Chapter 7. "Jewish involvement in shaping U.S. immigration policy." MacDonald documents Jewish leadership of the effort to eliminate ethnic criteria for U.S. immigration. "Jewish activism on immigration is merely one strand of a multi-pronged movement directed at preventing the development of a mass movement of anti-Semitism in Western societies" (245). MacDonald reviews Congressional debates from the early 20th century and the (largely Jewish) scholarship on the Jewish defence agencies to conclude that Jews took a leading role in delaying the 1924 quota system and finally having it repealed in 1965. This assessment might be wrong, but can MacDonald be condemned for accepting what analysts report, and, in the case of some Jewish analysts, report with pride? (eg. Cohen 1972, 49; Goldberg 1996, 127; Johnson 1988, *; Neuringer 1971, 392—3; Raab 1993).

Chapter 8. "Conclusion: Whither Judaism and the West?" Here MacDonald applies the theories developed in his three volumes to speculate about the stability of multi-ethnicity in Western societies, discuss the rapid demographic decline of European-derived peoples in the United States, and evaluate the risk of communal conflict in the United States, including anti-Semitism.

Conclusion: What is distinctive about MacDonald's theory?

As I hope has been made clear, MacDonald presents his readers with a broad and detailed scholarship that can usually be challenged only through matching his assiduous attention to many specialist literatures. I have made no attempt here to critique his theories beyond noting their mainstream documentation, but some of his most visible opponents have done even less, while adding personal and very public attacks to their criticisms. Unfortunately for those who rebel at his empirical claims, these are mostly not MacDonald's assertions but the expert opinions of leaders in various scholarly and scientific fields. Certainly, whether his theories are ultimately viable or not, MacDonald is a scholar of considerable analytical power and scope.

Several major aspects are distinctive to MacDonald's analysis. His is the first significant historical-sociological application of Boyd and Richerson's (1985) theory of cultural group strategy, which he elaborates into evolutionary group strategy theory (1st volume). He offers an evolutionary interpretation of Social Identity Theory (2nd volume). I suspect both are destined to become influential. But for me what is most impressive, and this is the achievement of *Culture of Critique*, MacDonald has shown theoretical and methodological pathways linking the micro-level analysis of human behaviour with the macro-level dynamics of contemporary culture. He has done so on a narrow front, in a monumental case study of social relations affecting one people's struggle to survive and prosper, but that is a big start.

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Ever Since Adam and Eve: The Evolution of Human Sexuality

By Malcolm Potts & Roger Short, Cambridge University Press, 1999, 350pp, ISBN 0521470420, US \$74.95 (hdbk) [ISBN 0521644046, US \$29.95 (pbk)]

Reviewed by Linda Mealey, Psychology Department, College of St. Benedict, St. Joseph, MN 56374 [Lmealey@csbsju.edu]

There are a large number of books on evolution and sex out on the market right now; perhaps too many. But this one is worth your attention. *Ever Since Adam and Eve (ESA&E)* is the most interesting and successful "coffee table" book I have ever seen. This expensive case bound volume is printed on quality, heavyweight paper, is richly illustrated (in gorgeous color), and is full of boxed "asides" relating fascinating historical facts and anecdotes about sex (as the title says) "ever since Adam and Eve".

Although organized into 13 chapters, the material is presented such that one can pick up the book and start reading virtually anywhere. Indeed, I started out by picking through the pictures and their captions, never intending to actually read through the entire thing. I was, however, so enticed by the compelling presentation and by the compendium of little-

known facts and legends that I eventually read it straight through.

I am amazed (and a bit envious) that the authors (long-time friends and academics with training in human and veterinary medicine, respectively) have produced a book that is so accessible to so many. For those who know nothing about sexuality or evolution, complex and up-to-date scenarios are summarized in a narrative style that makes for easy and fascinating reading. Readers who are familiar with most of the scientific facts and theories, will find that the cultural, religious and historical aspects will maintain their interest. Furthermore, unlike many easily accessible books, the text of *ESA&E* never drops to a level of oversimplification that is frustrating to the expert or misleading to the lay reader.

In addition to its strict adherence to principles of academic credibility, *ESA&E* differs from most "coffee table" books in that it comes with an index and a (limited) chapter-by-chapter bibliography. The text itself is not cluttered with footnotes or references, but both scientific and historical sources are listed at the back (an average of about 30 references per 25-page chapter). Although I doubt the book was ever intended as such, these features will allow it to be successfully used as a text for an introductory-level, multi-disciplinary class on human sexuality.

The only audience who will not like this book is that consisting of religious conservatives who dislike the discussion of evolution, sexuality or other religions. The authors allow their anti-religious sentiments to show through -- more strongly in each successive chapter. Always, these comments are in context and, I think, appropriate (e.g., chastising the Catholic church's position that a condom should not be used even in the context of a marital relationship in which one partner is infected with HIV and the other is not). In the penultimate chapter the authors present the position that the earth is over-populated by humans, that there will be (and already are) Malthusian consequences, and that both Church and State are at fault for impeding the spread of modern family planning information and technology. Although a few may feel uncomfortable with this position, rather than detracting from the book, I find the

presentation of this compelling and well-documented argument to be one of the many reasons I would like to see this book in every high school, college, and public library.

A final reason I would like to see this book widely distributed is that it presents concepts of evolution in such a straight-forward and nondogmatic way that even those who might be predisposed against "this view of life" (but may be attracted to the book because of its quasi-religious title or lavish artwork) will find it hard not to consume such a sweet offering, and they will undoubtedly be educated in the process. For that reason, even if you don't have a personal use for it, do yourself (and all of us) a favor and order it for your local library.

The Evolution of the Snark

Comparative Psychology: A Handbook

Edited by Gary Greenberg and Maury M. Haraway. Garland Publishing, Inc. New York and London, 1998, 914 pp. [Hdbk, \$135, ISBN 0-8153-1281-4].

Reviewed by Harold Gouzoules, Department of Psychology, Emory University, Atlanta, GA. 30322. Email: psyhg@emory.edu

Fifty years ago, Frank Beach's classic paper "The snark was a boojum" bemoaned the moribund state of comparative psychology that had resulted from excessive concentration on a small number of species, primarily the white rat, and a preoccupation with studies of learning (Beach 1950). Beach's allusion to the Lewis Carroll poem, "The Hunting of the Snark", had comparative psychologists pursuing their snark, animal behavior, which turned out to be that dangerous species (the boojum), encounters with which were not typically survived. Twenty years later, the future looked equally foreboding for comparative psychology, as Hodos and Campbell (1969) and Lockard (1971) documented continued problems with the field as it struggled to embrace evolutionary perspectives while, at the same time, maintain its intellectual integrity and independence from ethology. The predicted demise of comparative psychology

was, it seems, premature, if the vigor and vitality of the field at the end of the century can be assessed from this massive handbook, with its nearly 1000 pages and contributions from over 100 authors. How comparative psychology finally overcame the threats to its survival is, appropriately, an evolutionary tale that becomes clear through the chapters of this volume: the field gradually diversified to the point where, as Douglas Candland notes in the book's forward, there are now only fuzzy boundaries among the various subdisciplines of animal behavior research. The territory covered in Comparative Psychology is therefore very extensive.

This handbook is arranged in eight sections that cover: the history and philosophical foundations of the field; pivotal concepts, issues and theoretical developments; methodology; physiological correlates of behavior; portraits of the behavior of a variety of species and other taxonomic groups; the historically and still key issues of learning and development; a selection of behaviors that illustrate ecological and evolutionary relevance; and a final one on cognitive processes. The section 'Groups and Species' stands out as something of a hodgepodge in that it includes narrow issues in applied animal behavior (e.g., 'Alleviating fear in poultry' and 'Dogs in service to humans'), coverage of individual species (e.g., 'Mountain sheep' and 'Orangutans'), and broader reviews of higher taxa (e.g., 'Old World monkeys' and 'Snakes'), but other sections of the book are more coherent and focused. Overall, the depth of the chapters is about what one expects in a handbook: authors have selectively foraged through literature that, in some areas of animal behavior, is vast, and present digested information efficiently and concisely. Chapter length varies for the different topics, ranging from fewer than two (e.g., 'Epigenesis') to more than a dozen pages (e.g., 'Chimpanzee behavior: A comparative cognitive perspective'), usually depending on how narrow or broad is the focus. There are curious exceptions, however: the chapter on 'Laboratory simulations of foraging' is twice the length of the one on 'Foraging'. Surprisingly, some important areas of animal behavior that, arguably, deserve separate chapters are considered only tangentially in Comparative Psychology. For example, communication receives attention in chapters on

'Language in animals', which focuses mainly on attempts to teach human language to animals, 'Bird song development', and 'Gibbons: the singing apes', but no chapter is devoted to general theoretical, methodological or empirical considerations in this large and significant area of animal behavior.

Overall, I found the production values of the book good, though considering its length, tables, figures and, especially, photographs are used rather sparingly, and a few that are included do not contribute much (e.g., a photo of Sue Savage-Rumbaugh and two infants chimpanzees sitting in a car). This is not really much of a shortcoming for professional animal behaviorists seeking a quick summary of a particular area, but might be more of a drawback for students or the interested lay person. The book has separate indexes for species, authors, and subjects, and references are located conveniently at the end of each chapter. It is definitely a handy reference that I frequently will use in teaching; its rather hefty price might prevent it from becoming common in personal libraries, however.

One lingering question for me was whether or not the title of this book was, in fact, appropriate. Does comparative psychology really exist as a separate discipline anymore or, as Candland suggests, have traditional boundaries in the study of animal behavior disappeared? Today, comparative psychology might be best characterized by what it is *not*, rather than by any unique set of interests or methods. Evolutionary approaches, for example, while certainly represented in this handbook, do not dominate its pages, but otherwise the focus is clearly animal behavior in its modern, pluralistic sense. I suspect that academic genealogies tracing back to the times when ideological battle lines were drawn between the likes of Lehrman and Lorenz likely account for the continued use of labels like these that, today, have little significance. In any case, Comparative Psychology is a valuable handbook for any student of animal behavior, regardless of their intellectual ancestry.

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ANNOUNCEMENT

On behalf of the International Council of Ethologists and the Ethologische Gesellschaft e.V. we cordially invite you to attend the 27th International Ethological Conference to be held on August 22-29, 2001, in Tuebingen, Germany.

Tuebingen is one of the prettiest cities in southwestern Germany, in the state of Baden-Wuerttemberg. This community of about 85.000 is located 40 km south of Stuttgart, the state capital. It has been a college town since 1477, when the University of Tuebingen was founded, and has produced such luminaries as the astronomer Johannes Kepler and the philosopher Georg Hegel.

It is our intention to bring together the various branches of ethology and related disciplines to enhance communication between conference participants. Therefore, the conference is open to all ethologists and scientists working in related fields. Main topics will be represented by six plenary sessions (twelve plenary speakers) and by corresponding sessions with contributed spoken and poster papers. Of course there will be ample room for other topics. Symposia and roundtable discussions will supplement the program.

For further information please visit our Website: <http://homepages.uni-tuebingen.de/ethology01> or: ethology01@uni-tuebingen.de

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New Books – September 2000

- Aitchison, Jean (2000). The seeds of speech: Language origin and evolution. Cambridge University Press. 304p., ISBN 0-521-785715. \$13.95 (pbk).
- Berkman, L.F., & Kawachi, I. (eds.) (2000). Social epidemiology. Oxford University Press. 416p., ISBN 0195083318 (hdbk).
- Cartwright, J. (2000). Evolution and human behavior. MIT Press. 400p., ISBN 0-262-53170-4. \$22.95 (pbk).
- Cziko, Gary (2000). The things we do: Using the lessons of Bernard and Darwin to understand the what, how, and why of our behavior. MIT Press. xi + 290p., ISBN 0-262-03277-5 (hdbk). \$37.95
- DeLoache, J. S., & Gottlieb, A. (2000). A world of babies: Imagined child care guidelines for seven societies. Cambridge University Press. xii + 316p., ISBN 0-521-662648 (hdbk). \$49.95
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