For evolutionary psychiatrists, psychologists and others interested in sociophysiological Integration

ASCAP

Volume 9, No 8, (Cumulative #105) August 1996

"You seem to be troubled by all this evolution stuff. I don't see anything new in it. You always said God made man out of mud and this new theory says He made him out of prepared mud."

Young Robert Frost trying to calm his religious mother's unease about Darwin's discoveries.[1]

Across Species Comparison and Psychopathology (ASCAP) Newsletter Aims

- ♦**A** free exchange of letters, notes, articles, essays or ideas in brief format.
- ♦Elaboration of others' ideas.
- ♦Keeping up with productions, events, and other news.
- ♦Proposals for new initiatives, joint research endeavors, etc.

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ASCAP Society Mission Statement

The ASCAP Society represents a group of people who view forms of psychopathology in the context of evolutionary biology and who wish to mobilize the resources of various disciplines and individuals potentially involved so as to enhance the further investigation and study of the conceptual and research questions involved. This scientific society is concerned with the basic plans of behavior that have evolved over millions of years and that have resulted in psychopatho-logically related states. We are interested in the integration of various methods of study ranging from cellular processes to individuals in groups. The ASCAP Newsletter is a function of the ASCAP Society.

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activation of medial temporal structures during episodic memory retrieval; trade-offs between life-history traits and a secondary sexual character in male collared flycatchers; a functional neuroanatomy of hallucination in schizophrenia; heritability of a sexually selected character expressed in both sexes; central command neurons of the sympathetic nervous system: basis of the fight-or-flight response.

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Concerning paleobiology, sociophysiology, interpersonal and group relations, and psychopathology

ADDRESSED TO & FROM ...

TEACHING MODULES: A SUGGESTION FOR THE ASCAP NEWSLETTER

I think that it would be great to have a numbered "Teaching Module" included with each issue of The *ASCAP* Newsletter.

Since the ASCAP Society is a teaching society in many ways, such Teaching Modules would be consistent with our overall mission. Each module will advertise the ideas of an expert in a particular domain of evolutionary psychopathology, while providing a single source for citation and learning for the reader. Such modules might even be ordered in lots at a nominal cost for classroom use.

I would suggest a single author for each module, with instructions to be brief, pithy, but as inclusive as possible within a 4-6 page double-spaced format.

The author would introduce the topic in the first paragraph, which might include a sentence or two of historical material. The main body would include a clear exposition of the main concepts, principles, and findings associated with the particular domain of theory/research. Technical terms would be used sparingly, and specialized jargon and esoterica would be avoided. In the final paragraph, the author would briefly summarize major points, draw prudent conclusions, and say a few words about future directions. Up to 5 definitive and highly selected references would be encouraged.

Each Teaching Module would closely resemble an entry in a high-quality encyclopedia on a particular topic. A numbered Module might look like this: "Teaching Module 1: History and Current Status of ISS Theory", by John Price, or Leon Sloman, or Russ Gardner. In my undergraduate Abnormal Psychology class, I routinely include something on ISS theory and depression, and a Teaching Module on the topic would make an excellent course handout. Other possible Modules might include Dan Wilson on Darwinian Epidemiology, Randy Neeseon Darwinian Medicine, Russ Gardner on leadership and mania, and so on. Self-review of books in progress or recently published books would qualify as Teaching Modules as well.

The success of Teaching Modules will rest entirely on the willingness of ASCAPers to submit modules for publication in The *ASCAP* Newsletter. Please address any comments you have on this matter to Russ or to me. We hope to hear from you.

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From E-Mail

Respect and Goffman

Charles Crawford wrote:

Has anyone read Irving Goffman on the subject of "face" and how it might relate to respect? I haven't, but I would like to hear from those who have.

Frank Salter responded:

In my recent book, *Emotions in Command: A Naturalistic Study of Institutional Dominance,* (Oxford, 1995) I review Goffman's face concept and trace its development in socio-linguistics and micro-sociology in general. As one of his admirers admits, Goffman tended to borrow ideas without acknowledgment. But his footnotes make it clear that he was aware of the developments taking place in the 1950s and 60s in animal behaviour, especially primate ethology. He liked Lorenz's concept of releasing stimuli, for example.

I agree with Charles' suspicion that the face concept has biological relevance. It is one of the few sociological and cultural anthropological concepts directly translatable into the biological parlance of status, rank, and dominance, perhaps because of Goffman's ethological contacts, perhaps because he pioneered observation as a research tool in

sociology, perhaps because he was partly trained by a great (Australian) anthropologist, or perhaps because the face concept has a common sense meaning in all human societies (e.g. the elaborate etiquette associated with it in Japanese tradition; the saying "to lose face" precedes Goffman).

In *Emotions in Command,* I also review the face concept as applied or implied in studies of rank and dominance in hunter-gatherer (h-g) communities. There, the bully has a rough time. Social etiquette among the Australian Aborigines, for example, or the !Kung Bushmen, has evolved to maintain group cohesion given the contingencies of voluntary association and assertive egalitarianism. This interested me, because I was looking for precursors to the command (or authoritative directive), a ubiquitous aspect of institutional life. Blunt commands are one component of bullying. This behaviour is considered highly aggressive in h-g societies, and sufficient cause for ridiculing and ostracizing the command-giver. My interpretation is essentially the same as Charles, that we are evolved to resist subordination, because that state has usually been associated (between adults) in loss or delay of access to resources, including mates.

But we are status opportunists, and the need for respect is open ended in the manner described by Hobbes. H-g equality is a self-organizing and dynamic, predicated on the individualized small group. The balancing mechanism breaks down as population growth and storable production surpluses lead to anonymity and loss of interdependence. But the lust for status, once balanced against that in others, remains. Hence the market for cultural devices for manipulating behaviour, for engineering asymmetry in social control, a market monopolized in pre-modern times by the aristocratic and religious establishments.

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Income & Offspring

First, Paul Okami wrote:

"In their 1983 2nd edition of Evolution, Sex & Behavior, Wilson & Daly published a graph of U. S. wives' fertility, across age groups, by husband's income. Findings were unremarkable with the exception of the childlessness category, which was negatively related (for all age groups) with husband's income - that is, the greater the income the less likely that the couple was completely childless. Does anyone know if this relation still holds?"

Mike Waller quoted him and wrote:

Paul, can you clarify what you mean by "findings were unremarkable..."? Do we assume that the less well off the subjects were, the more children they had?

This would certainly be unremarkable in terms of the conventional middle class perception that the poor "breed like rabbits"; but it does present a bit of a facer for the basic Darwinian model.

In respect of the latter, the statistic you are querying seems to me to be distinctly supportive. If the better off are consistently more likely to have children whilst on average their families are smaller, does this not point to the outworking of a classic big K strategy?

Some years ago I read in an The *ASCAP* Newsletter a very elegant put down of a piece of research which had revealed that although 17 century German farm labourers had larger families than those of the farm owners, a few generations later the farmers had significantly more descendants extant

The ASCAP reviewer dismissed it as yet another example of a social anthropologist rediscovering "Darwinian elites".

As an evolutionary strategy, small families might be less effective in societies with a welfare safety net. But taken in the round, I should have thought that bestowing inherited wealth on the few rather than the many, was a pretty effective way of maintaining social advantage.

Mike Waller
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Paul Okami subsequently said:

"The assumption - that the less well off the subjects were, the more children they had -would be unfortunate, Mike, since this is 'not' the portrait that emerged from the data cited by Wilson & Daly.

For example, for wives underage 29, the wealthy were quite a bit more likely than the poor to have had three or more children (although the relationship was not strictly linear across all income categories), whereas for wives between the ages of 30 and 49 there was less discernable patterning, with wives whose husbands made less then three or four thousand dollars being just as likely as those whose husbands made over $25,000 to produce 3 or more children, but somewhat more likely than some - but not all - of those who were middle class (but not in the "over $25,000 bracket) to have had 3 or more children. (Remember, by the way, these are 1970 dollars).

Results for "two children" and "one child" also showed variable or contradictory patterns. Only "childlessness" showed a *clear, linear pattern across all age categories."*

Mike Waller then wrote:

Regrettably, Paul, I have no easy access to statistics that would help you update this data, but when you locate them, would you let us all know what you find? As my original misunderstanding makes clear, even if these figures are not perfect in terms of Darwinian theory, they can be read as broadly supportive.

Yet for almost the entire period since Darwin wrote we have been routinely scared to death by doom-mongers warning us that the "haves" are being continually out-bred by the "have-nots".

The key questions therefore seem to be: were the 1960 - 1970's an unusual period wherein the wives of the wealth routinely did their dynastic duty whilst easily available contraception (plus rising material aspirations) were applying a new downwards pressure to the birth rate of those less well off?

Or have the doom-mongers (amongst whose number I count myself) always had it wrong?

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New Bulletin Board System at HBES Website

A software system called "WebBoard" has been added to the HBES website:

http://psych.lmu.edu/hbes.htm

There are several "conferences" in which users may post messages, reply to others' messages, etc. I've initially set up the system with the following conferences:

Book Reviews Bulletin Board for Teachers Employment / Jobs Offered Graduate Programs Official HBES Announcements (moderated)

If you have ideas for additional conferences, please let me know.

If you have anything to post, please do so! The usefulness of this system will depend on input from HBES members.

P.S. Within the near future, I plan to add WebBoard to the non-HBES electronic journal, Advances in Evolutionary Psychology. This will allow readers to post their comments on target articles.

Advances in Evolutionary Psychology
(Electronic Journal)
(under construction):

http://psych.lmu.edu/aep.htm

Michael Mills
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New Book on Adaptive Individuals:

There's a fair amount that may be of interest to ASCAP and HBES denizens in the following new book, in particular some of the classic and new papers on the Baldwin effect and other interactions between learning and

evolution. In addition, the paper by Johnston on costs and benefits of learning (and my commentary on it) might be relevant for adding some functional considerations to the evolution of brain size....

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Adaptive Individuals in Evolving Populations: Models and Algorithms

Edited by: Richard K. Belew and Melanie Mitchell

Proceedings, Volume XXVI, Santa Fe Institute Studies in the Sciences of Complexity

About the book:

The theory of evolution has been most successful explaining the emergence of new species in terms of their morphological traits. Ethologists teach that behaviors, too, qualify as first-class pheno-typic features, but evolutionary accounts of behaviors have been much less satisfactory. In part this is because maturational "programs" transforming genotype to phenotype are "open" to environmental influences affected by behaviors. Further, many organisms are able to continue to modify their behavior, i.e., learn, even after fully mature. This creates an even more complex relationship between the geno-typic features underlying the mechanisms of maturation and learning and the adapted behaviors ultimately selected.

A meeting held at the Santa Fe Institute during the summer of 1993, brought together a small group of biologists, psychologists, and computer scientists with shared interests in questions such as these. This volume consists of approximately two dozen papers that explore interacting adaptive systems from a range of interdisciplinary perspectives. About half the articles are classic, seminal references on the subject, ranging from biologists like Lamarck and Waddington to psychologists like Piaget and Skinner. The other papers represent new work by the workshop participants. The role played by mathematical and computational tools, both as models of natural phenomena and as algorithms useful in their own right, is particularly emphasized in these new papers. In all cases the chapters have been augmented by specially written prefaces. In the case of the reprinted classics, the prefaces help to put the older papers in a modern context. For the new papers, the prefaces have been written by colleagues from a discipline other than the paper's authors, and highlight, for example, what a computer scientist can learn from a biologist's model, or vice versa. Through these cross-disciplinary "dialogues" and a glossary collecting multidisci-plinary connotations of pivotal terms, the process of interdisciplinary investigation itself becomes a central theme.

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For more information on this book, visit the web page:

http://www.santafe.edu/sfi/publications/Bookinfo/aiineptofc.html

Please E-mail any contributions to ascap@beach.utmb.edu, or mail hard copy and 3.5" HD diskette to: Russell Gardner, Jr., c/o Frank Carrel, Department of Psychiatry & Behavioral Sciences, University of Texas Medical Branch, Galveston TX 77555-0428, USA. WordPerfect, Microsoft Word or ASCII format preferred. Diskettes will be returned to you. Thank you.

ARTICLE:

by Tim Miller, Kent Bailey, and Russell Gardner

Miller-Bailey Debate

Russell Gardner introducing:

I am very pleased to see the following, as the gentle debate exemplifies idea-exploration in hedonic space, in this case on E-Mail in cyberspace in recent months and we are benefited by the protagonists' willingness to let us see what they had to say. The core issue, as I see it entails the translation of concepts in evolutionary biology, and clinical and practical reality. Ultimate or evolutionary explanations must be realized in genomic, neural, and behavorial forms. In this debate, we see two honest psychologists tussle.

Tim at the end felt, in the words of Robert Frost's poem in his collected works, *"They would not find me changed from him they knew /Only more sure of what I thought was true."*[1] Kent listened and appreciated a number of Tim's points, but I don't sense a weakening of his stance either. The arguments involve the Bailey position which fundamentally considers the data from a clinical prespective underlain by neuroanatomy. He understands basic plan thinking as well as the need to do things not necessarily guided moment-to-moment by research data, but the Miller point has great power that research and research data must certainly be one's goals. So enjoy a good read. Let us see your own thoughts about the ideas involved.

Tim Miller to Kent Bailey about 1996 EEA formulations in *The ASCAP Newsletter.*[2]

My first comment will be brief. I just don't understand one of your fundamental premises. Perhaps we disagree, perhaps you've not made yourself clear as you would have liked, or I may be just dense. You repeat the same premise in a number of places. It regards your distinction between "biological success" and "cultural success." As I understand evolutionary psychology, neither people nor other animals specifically seek reproductive success. Rather they, seek its prerequisites. If they do so, reproductive success takes care of itself.

For example, a male grizzly bear would seek a rich territory with a good den site to drive off encroaching male bear competitors, the richest diet possible, as much copulation as possible with as many female bears in estrus as possible, with the healthiest and most vigorous females getting the highest priority, to kill cubs, especially male cubs, sired by other male bears, to drive competing male bears away from as many female bears as possible, particularly when the females are in estrus.

We could go into more detail, but that's the general idea, as you well know.

If a male bear does this, he will almost certainly produce many viable offspring, probably many more than the average male bear. However, he doesn't count offspring. He just does what comes naturally

It seems to me that humans do about the same thing. They compete for status. They compete for access to food, shelter, and so on. They compete for access to the most desirable mates. They copulate most frequently with the most desirable mates, and actually appear to be more fertile with more desirable mates. If humans do all of these things, babies inevitably come along, unless there are problems with infertility. When babies come along mothers maternal grandmothers, and maternal uncles usually bond to the babies, fathers often do, and paternal relatives sometimes do, depending upon confidence in paternity. When adults bond to babies, once again they do what comes naturally, which is to provision protect, educate and socialize them, very eagerly and attentively.

When someone does all of these things successfully, it's likely that he or she will be reproductively successful (relative either to the average human, or to the average human in his or her habitat or social group), barring unforeseeable catastrophes.

I don't think there's any reason to believe that humans are consciously calculating the likelihood of reproductive success when they make decisions about whether to copulate or not, or who to copulate with, or whether to get up in the middle of the night to nurse the baby. Neither do I see any reason to believe that modern people who fail to reproduce any offspring at all feel particularly stressed by their "biological failure." To the contrary, childless American couples report somewhat higher levels of well-being and marital satisfaction than comparable American couples with one or more children. In the same way, among Americans, both well-being and marital satisfaction are negatively correlated (slightly) with the number of children produced.

On the other hand, modern people who fail to achieve the usual human prerequisites to reproductive success tend to be stressed and unhappy, to have poor emotional and physical health and to die prematurely from most causes. I'm talking about, for example, people who are on the very bottom of the status, income, social skill, health or IQ hierarchies; men who have enjoyed little success attracting desirable, sexually receptive women; women who have had very little success getting their sexual partners to invest resources in them and their children. Ironically, in contemporary society, such people might, in some cases, produce quite a few children. (A number of people more or less fitting this description live in my downscale neighborhood.) This is because, in modern times, characteristic human prerequisites to reproductive success are much less related to actual reproductive success than they were in ancestral times.

Given the above, I don't understand how you can make a distinction between biological success and social success, unless you simply count the number of offspring. This seems evolutionary irrelevant. All you really would be counting, it seems to me, would be birth control failures, impulsivity, tendency to copulate while intoxicated, and attitudes about family planning.

Bailey's reply to the 1st installment:

1. Your point about "reproductive drive" is well-taken. I anticipated some need to clarify on that. Of course, this raises the traditional proximate/ultimate issues. We are built (or have "basic plans to use Russ's terminology) to recognize, seek out, and consume stimuli that are "good" for us at the proximate level of functioning; "good", species-appropriate functioning overtime will, probabilistically, lead to "good" or ultimate reproductive outcomes, as compared to a proximately deficient animal or person. Appetitive behavior, goal-seeking behavior, and reinforcing processes (e.g.., experiencing pleasure following the emission of species-appropriate behavior- Herrnstein, 1976). Motivation (proximate) is a big part of the picture, and I simplify (oversimplify) the process by referring to the "drive" for biological success. There may not be such a thing technically, but without "it" the species would be extinct in short order!! By the way, I liked the bear imagery!

2. I agree that humans do not consciously calculate reproductive probabilities, etc., as you point out. Michael McGuire has spoken of an internal sociotiologic "calculator" in the past, that helps us make "adaptive" decisions" along the way, but conscious ness and the neocortex are probably only indirectly involved. For me, "truth" is in the limbic and reptilian systems, not the conscious neocortex. For me, the neocortex is a great post-hoc rationalization machine that fabricates socially appropriate explanations for what is really instinctive" activity. But when do humans ever truly understand the "drives" that impel, whether they be evolutionary, economic, or theistic?

3. In sum, I think we are proximally motivated ("driven") to do good species things that will (ulti mately) lead to positive reproductive outcomes. Something analogous happens culturally, I think, out the proximal hardware used is "borrowed" from the

evolved mechanisms that evolved to subserve reproductive success. They are co-opted as Gould would say. Thus, the wonderful feeling we get when our opus comes out, draws from pre-existing dominance and status designed in the EEA for entirely different accomplishments. Getting our book published is, thus, analogous to winning the most beautiful bride or prevailing over the other male competition. Don't think we have had time to evolve specifically "cultural" motivational and reinforcement mechanisms per se.

4. All of this said, each of us, I believe, is sublimi-nally aware of whether we are "succeeding" biologically and culturally, and, as I argue, this subliminal recognition has considerable implications for psychopathology when cast in the fourfold model. Recently Gloria Steinem, in her fifties, "suddenly" noticed that she had no offspring and something was "missing" in her life. At one level, she had always been aware of such issues; they only be came conscious much later. Intuitively, I think, most of us judge ourselves intuitively as biological/cultural successes and failures, but most people could not articulate these deep feelings. I believe they are there, and are involved in much of what we call psychopathology.

Miller's second installment:

Items one through three in your last reply seem reasonable to me.

Our possible disagreements surface in item four.

You write: *All of this said, each of us, I believe, is subliminally aware of whether we are "succeeding" biologically and culturally, and, as I argue, this subliminal recognition has considerable implications for psychopathology when cast in the fourfold model.*

The gist of my original message, which I may have failed to make sufficiently clear, is that cultural success and biological success were synonymous in the EEA. The distinction is a purely cognitive one (we might just as well call it a neocortical distinction), not an evolved, instinctive, or limbic distinction.

I agree that it is indeed possible in the modern world to achieve social success without biological success, or vice versa. It is also possible to achieve both, or neither. But because this rarely occurred under ancestral circumstances, we have not likely evolved to be stressed about social success in the absence of reproductive success. Conversely, I don't see how we could have evolved to celebrate biological success in the absence of social success.

(Under such circumstances in the EEA, our ultimate reproductive success would still be much in doubt because we would expect a high rate of mortality or reproductive failure among our children and grandchildren if we were socially unsuccessful.)

You write: *Recently Gloria Steinem, in her fifties, "suddenly" noticed that she had no offspring and something was "missing" in her life. At one level, she had always been aware of such issues; they only be came conscious much later. Intuitively, I think, most of us judge ourselves intuitively as biological/cultural successes and failures, but most people could not articulate these deep feelings. I believe they are there, and are involved in much of what we call psychopathology.*

In this passage you seem to imply there is some connection between failure to reproduce and psychopathology. Where are your data? The evidence I am aware of indicates pretty clearly that there is no relationship between number of viable offspring, ("biological success," as you call it) and psychopathology, mental health, well-being or any other direct or indirect measures of psychopathology.

My main point is, if the distinction between biological and social success is a false one, then whether the fourfold model? Perhaps you can clarify your distinction between biological success (or failure) and social success (or failure), and justify your position that there is some instinctive mechanism for separately evaluating these two variables. Perhaps you can cite

data indicating a relationship between "biological success" and psychopathology.

I agree that there may be some proximate mechanisms that evolved to help us avoid unproductive pair bonds, or to adapt optimally to our own infertility. For example, childless couples may tend to lose interest in each other after a certain number of years, and seek out other partners, in response to an evolved tendency to search for other mates if one's present mate shows signs of possible infertility. Helen Fisher's "four year itch" is relevant to this question, even among couples that have produced a child.

The tendency to lose interest in a possibly infertile mate might be stronger in women than in men, because women are certain of maternity. Men who have been able to sleep around a little (or more than a little) can enjoy the fallback position of imagining that they may have fathered unidentified children, or that they will have such an opportunity in the unforeseen future. Similarly, women (and men, to a lesser extent) who reach the end of their fertile years without having borne children may become unusually interested in their nieces and nephews. I have the impression that people in this situation also have a tendency to form parent-like bonds with their pets.

Finally, there is some reason to believe that people who run out of opportunities to contribute to their ultimate reproductive success, for any of a number of reasons, have higher suicide rates and poorer mental and physical health than others. (Such people are likely to feel that they "have nothing left to live for") However, such a set of circumstances might occur whether or not the person in question has living children, and is just as likely to be associated with "social failure" as "biological failure." Such an effect would be hopelessly confounded with social isolation and pre-existing poor mental or physical health, unemployment, poverty, and so on.

In any case, I would be surprised to see data indicating that childlessness is associated with psychopathology in healthy, employed, socially active people with good social support and family ties.

In other words, I would be surprised to see data indicating that childlessness ("biological failure") is associated with psychopathology in "socially successful" people. If such data do exist, I would be very interested to see them.

Bailey's reply:

1. Your statement that biological success and cultural success were synonymous in EEA was helpful. That was true perhaps, but I suspect there was still some culture and neoculture that was targeted back on itself and not procreation per se, I suspect. Thus, one might be "successful" within that cultural residue, but still be do poorly reproductively.

2. Anyway, mismatch theory emphasizes the progressive separation of biology and culture, especially starting 40,000 or so years ago when culture was just tooling up and biology was stabiliz ing. Since that time, I find it quite likely that culture (and especially "neoculture") began to develop and create its own goals (ideas, innova tions, still additional culture, etc.) in a way sepa rate and distinct from the ongoing Darwinian symphony of death and reproduction.

3. I am not sure whether we agree on this point or not, but I see (as maybe you do) culture as being mediated more by the neocortex and biological success more by the limbic systems. It seems that culture is based on ideas (neocortex) whereas biology (and biological success) are based on limbic emotion and motivation. Of course, the neocortex might be "aware" of limbic things going on, but the thrust of human behavior is more at the feeling level than the thought level it seems to mo. I continue to suggest that the neocortex often serves as an ex post facto explainer of spontaneous "instinctive" things.

4. It remains to be seen how well yoked reproduc tive success and social were in the EEA. Biologi cally, only reproductive success was ultimately important, and it may have made little difference as to whether one felt socially successful about his offspring. It was the offspring that counted ulti mately. However, nature uses many proximate mechanisms at many levels to ultimately target toward reproductive success.In fact, good health, reproductive effort, good looks, are all biological, but all are also proximate mechanisms that increase the probabilities of the only ultimate success (more offspring, at least within the family). These proxi mate mechanisms work fairly independently I would think (including a "motivated drive" to be biologically successful). Ultimately, the test is whether these mechanisms - singly or in various combinations - can lead to higher levels of family reproduction for person X as opposed to person Y.

In the EEA, a person who was less "driven" to achieve species goals (in the form of individual or family reproductive output) would, I think, indeed be at a disadvantage relative to a more vigorous cohort.

5. As to the issue of data on success-failure vis-a-vis psychopathology- this is certainly a challenge I face and one that all evolutionary psychologists have to face (with the possible exception of David Buss). If we get too tough about data, then evolutionary psychopathology is due for a short run. My goal at this point is to conceptualize as best I can and then collect data consistent with the model. Who do we know (except possibly Buss) who has comprehensively supported his/her model with hard data? Even his extensive data are based on sex differences., Buss tells us little about psychopathology. I make no pretense about provid ing definitive data at this point.

6. It seems to me that social science in America has always tied itself to a vague notion of "cultural success" (if everything is "cultural", then certainly "success" would be as well"); if we follow cultural determinism, then our goals would be culturally determined as well. Ergo, the cultural success piece.

7. It seems that Darwinian biological science (and determinism) has erred in the other direction, by putting everything in the reproductive basket. All things must lead to reproduction, ultimately, in the Darwinian model. This approach is important, but the culture piece must fit in somewhere if we are to understand the modern human being. I "solve" the dilemma (to my satisfaction), by trying to incorpo rate both into the fourfold model. I suspect that most people are rarely conscious of the biological and cultural imperative operating at any given time- but if the notion of cultural determinism has any merit whatsoever, how could it not be the case that "cultural success" would be at the center of things? Likewise, if Darwin was right, how could it not be the case that reproduction (and its proximate motivational mechanisms) is at the heart of human motivation?

8. The thrust of your recent critique centers primarily on the data issue. As far as I am con cerned, "data" are lacking in every single nook and cranny of social science. What single fact do we have in social science, beyond lawful physiological phenomena such as relations between levels of illumination and pupillary constriction-dilation? On the other side of the coin, what data do we have that truly refutes an existing theory? The only example I can think of is perhaps Derek Freeman's refutation of Margaret Mead's early work in Samoa. Now in psychology fads do change, but rarely because of the force of hard data. After 80 years of research in psychotherapy, for example, aside from a few hard-core behaviorists, I never see "data" driving the process of therapy. People do therapy a certain way, and then seek out a few studies to rationale their approach.

9. I would be interested to know what evolutionary approaches to psychopathology there are that are rigorous and firmly data-based? There are some good starts (e.g., as shown in Mealy's BBS paper on sociopathy), but they are rudimentary and few and far between.

10. When the smoke clears, you are of course right that data will ultimately separate the sheep

from the goats theoretically. Your critique reminds me that the fourfold model must resonate with the facts if it is to be worth anything. I tend to get caught up in the idea, and sometimes forget that.

Miller's response, June 18,1996

I'll reply item-by-item to your last message:

In item 1, you write: *but there was still some culture and neoculture that was targeted back on itself and not procreation per se, I suspect. Thus, one might be "successful" within that cultural residue, but you might be doing poorly reproductively.*

How could that have occurred? If it had, those people would not have perpetuated their genotype. Even if it had occurred, how can we know what the consequences were-emotional, psychological, social, and others?

In regard to item 2: There are (and have been) many different cultures. We can't suppose that they all hold the same views re biological success or define cultural success in similar ways. We can't generalize about them.

Additionally, cultures are not necessarily homogeneous, e.g., some individuals within our own culture might regard failure to reproduce as a catastrophe, while other might regard it as a blessing. Our culture actually does not take a clear stand on this point. So where does this leave us?

Regarding Item 3: You seem to suggest that the limbic system cheerleads for reproductive success, whereas the neocortex cheerleads for cultural success, and that mismatch occurs when the two are rooting for opposing teams. I can't support this view. Reproductive success is facilitated by the neocortex. Success is impossible without its contribution. It helps us do what is necessary to succeed. That is the only reason it could have evolved. The limbic system tells us what to want. One of the things it tells us what to want is cultural success, because, under ancestral conditions, cultural success has normally led to reproductive success.

Furthermore, although cultural requirements sometimes contradict or ignore instinctive drives to some degree under some circumstances, there is no culture I can think of where cultural success invariably contradicts reproductive success. To the contrary, in most cultures, even in the modem world, cultural success is a moderately good predictor of reproductive success. Of course, there are occasional exceptions, such as celibate priests.

In regard to item 4: Societies in which cultural success sometimes superseded reproductive success would have been swallowed up by their more fertile neighbors. They would not have existed for long. Consequently, it is quite safe to assume that in the EEA, Cultural Success and Biological Success were essentially synonymous.

In Item 5 you write: As to *the issue of data on success-failure vis-a-vis psychopathology- this is certainly a challenge I face and one that all evolutionary psychologists have to face.*

This is only a problem if we presume that psycho -pathology means something in the natural world. I have never seen a remotely adequate definition of the term.

You continue, *If we get too tough about data, then evolutionary psychopathology is due for a short run.*

I couldn't disagree more profoundly. If we *fail* to get tough about data, evolutionary psychology is in for a short run. Evolutionary psychopathology car only be a subset of evolutionary psychology.

You continue, *My goal at this point is to conceptualize as best I can and then collect data consistent with the model. Who do we know (except possibly Buss) who has comprehensively supported his/her model with hard data?*

Every few months, we see more good data in *Ethology and Sociobiology,* in *Human Nature,* sometimes even in *Journal of Personality and Social Psychology,* not to mention many other worthy books and journals.

It's no good to justify interesting speculation by calling it a model. A model is supposed to organize and explain a mass of data that was previously snarled. It will not be contradicted by existing, reliable data.

A "good" model will predict relationships between existing data that had not previously been noticed. It will also make testable predictions. As far as I can tell, your model meets none of these criteria.

I don't understand your item 6. In item 7, you write, *if the notion of cultural determinism has any merit whatsoever, how could it not be the case that "cultural success" would be at the center of things?*

Culture defines the arena in which humans compete for reproductive success. For example, different cultures might define skilled dancing in different ways. Dancing that appears skilled to one culture might appear artless to another. But, in each culture, dancing is an opportunity to demonstrate good health, vigor, coordination, unimpaired brains, youthful, fertile bodies, and so on. Successful demonstrations are conducive to reproductive success (in the absence of birth control).

Beyond that, I don't think I understand your comments in item 8, so will not venture to reply to it.

In regard to item 9,1 can't agree that a paucity of data is good reason for data-less speculation. Yes, psychology is a mess. That's no reason to make it a bigger mess.

In regard to item 10, the possible error to which you admit is one that has been committed repeatedly throughout the history of psychology. The great hope offered by evolutionary psychology is that future psychologists will commit that error less often. *c8*

As promised, The ASCAP Home Page is here!!!

The WWW Address for the The ASCAP Home Page is:

http://psy.utmb.edu/ascap

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For comments, suggestions, or questions to the WEBMASTER of The ASCAP Home Page, please send E-Mail or Snail Mail to:

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ARTICLE:

*by **Jerome** Barkow **and Ferdinand Knobloch***

Natural, Sex, and System Selection (from E-Mail)

Dear Dr. Knobloch,

I have your ASCAP articles in front of me.[1] You discuss leadership and the involuntary male altruists involved when a leader has multiple wives - that is the framework of evolution, of course.

Then you talk about the leadership's role in brokering values from different spheres of exchange in order to decide what a "fair" balance is. (An anthropologist would argue that it may not be the leaders who do this but a category of individuals sometimes referred to as "cultural brokers," but following that line would lead to digression.)

Then you begin to talk about what kinds of individuals the leadership would favor but it is not clear to me that favor has anything to do with inclusive fitness, a link that needs to be specified.

You go on to talk about flexibility of followers. There are two literatures you might refer to at this point. One has to do with the ethological literature on social hierarchy, pointing out the strong selection pressures to accept subordinate statuses (much better than injury or death, from a fitness perspective); the other is the "culture as an information pool and an arena for conflict among contending, self-interested groups".

The most important problem is the empirical one -how to demonstrate that leaders do enforce the values you say they do, and how to demonstrate that their followers enhance their genetic fitness by accepting the influence of the followers.

One empirical approach might be that of Betzig's despotism work - she shows that the men at the top have immense reproductive success, you would have to show that the middlemen/loyal followers have greater fitness than the rebels.

When you speak of the group as a whole "performing meta-selection," you need to show what that has to do with gene frequencies for it to be clear that you are still in an evolutionary frame. As written in ASCAP, it seems to me that you do not address this problem sufficiently.

Going on to the second article, I am not familiar with your 1979 book but the framework is at least somewhat reminiscent to me (remembering that I am hardly an expert in psychotherapy) of Berne and his well-publicized transactional analysis. You, however, are arguing that a social exchange paradigm orders our fantasy (schemata) life such that we end up -thanks to self-deception, a.k.a., the usual Freudian mechanisms of defense - winners. This strikes me as an interesting way to use evolutionary psychology as part of an approach to therapy - you might want to compare it with efforts by those with similar goals (Nesse, Glantz, etc.).

From my outsider's perspective, models and theories intended for clinical use should be evaluated primarily in terms of whether they advance therapy, rather than, say, the extent they fit into the (academic) cognitive sciences or personality theory. In general, though, I appreciate and specifically agree with what I see as two of your major points.

The first is that, intra-psychically, there is no distinction between kin and non-kin -I imagine that our mechanisms evolved in an environment in which the people around us would inevitably have been kin, so there was no selection pressure to make any distinction.

The second is that, intra-psychically, there are only small groups - it doesn't matter if the reality is that we are dealing with nation-states or with groups of evil spirits, our psychology evolved in a small-group context and that is how we conceptualize and react

emotionally, in terms of small groups. You note that D. S. Wilson agrees with you. Perhaps.

Under your framework, one could argue that something like cultural group selection is taking place. Personally, I doubt it very much - human societies do not usually exist long enough, or lineages remain in power long enough - for cultural group selection , in my opinion.

Rather, what is probably happening is selection for the ability to switch strategies - sometimes we are loyal followers, other times we are leaders, often taking both roles within a lifetime. This finding would be consistent with the primate literature, which shows individuals changing rank and even alliances, at times (see de Waal's work in particular).

Jerome H. Barkow j.h.barkow@dal.ca

Dear Dr. Barkow:

In answer to the second part of your response to the second article, *Mental Life as a Small Group Process.* I was happy to see that you understood immediately what most psychotherapists don't, since they are immersed in the old paradigm involving the fallacy of intrapsychic-interpersonal dichotomy (although you too use that jargon).

I talk about the new paradigm as that of H. S. Sullivan, though he was not clear enough, so his students did not fully understand, as I have shown and will shortly indicate here.

I anticipated your understanding after reading your chapter in your book with Cosmides and Tooby, and mentioned it on my poster at HBES.

Yes, we both agree — a belief in which I felt very lonely so far - what you express AS "THERE ARE ONLY SMALL GROUPS" and "NO DISTINCTION BETWEEN KIN AND NON-KIN" (the second of which I see as part of system selection).

Dr. Barkow said:

"*... the framework is at least somewhat reminiscent of ... Berne and his well-publicized transactional analysis... you might want to compare it with efforts by those with similar goals (Nesse, Glantz, etc.*...)."

Ferdinand Knobloch responds:

I have not found anybody in that group who would fully overcome the old paradigm characterized by the fallacy of intrapsychic-interpersonal dichotomy. To get an idea what I am talking about, the following example stems by chance, from Birtchnell:

"Although Bowlby has tried to focus attention on observed behavior, the essence of attachment, as it is in all form of close involvement, is what psychoanalysts have called the internalization of the object."

"This is a difficult concept. It means that somewhere within the psyche is a representation (internal relating) in the same way as she/he relates to the actual person in the real world (external relating). The individual is related to by this representation."

In the Group schema model, there is nothing "difficult" that at some stage the child's relationship to mother develops to that degree that her image is stabilized and the child can relate to that image for satisfaction. There is nothing "internal" about the image of mother: because our brain as a computer is user-friendly and copies reality: we imagine our mother outside and not in our head.

Despite our language habits (innocent for ordinary life, but disastrous for high priests of psychotherapy), this can be understood by people not spoiled by long training and psychoanalytic "internal demonology".

Many psychotherapists and particularly psychoanalysts cannot. (To be fair, I met in Vancouver one philosopher of science who could not understand it either. It is very deeply ingrained in language habits and in our visual model of outer and inner world.)

You mention Glantz. Glantz and Pearce's eclectic point of view is apparent, e.g., from this quotation:[2] *"We certainly don't mean to imply that one should not look to childhood or consider concepts of intrapsychic structure."* (Points where I strongly disagree with the authors are not closely relevant to this topic.) In my conceptual analysis I was helped by studying three philosophers who in Prague continued in the tradition of Ernst Mach and A. Einstein, Ph. Frank, Carnap, and H. Feigl. The fallacy is mixing the phenomenal and physical space.

Whereas the image of the mother is in physical space no doubt a process inside the body, in the head, the imagined mother is located in our external phenomenal space, though we have at the same time specifically, a message inside that it is our image and not real person.

Dr. Barkow said:

"From my outsider's perspective, models and theories intended for clinical use should be evaluated primarily in terms of whether they advance therapy, rather than, say, the extent they fit into the (academic) cognitive sciences or personality theory."

Ferdinand Knobloch responds:

I agree! When in charge of psychiatry of the university polyclinic in Prague and of psychiatric outpatient services (outpatient psychiatry was regarded as of less value by psychiatrists), I built in 1952-1968 a system of integrated psychotherapy with two therapeutic communities. I copied the two therapeutic communities at the University of British Columbia, Canada in 1972-1990.

Integrated psychotherapy is not only most efficient (thanks to the patients who are the main healers), but also least expensive. If there would be interest, I would be willing to lead a workshop of self-experiencing one's own group schemas during some conference. When I led such workshops at different places - Czech Republic, Germany, Chicago, and New York, including Harlem Hospital, Japan, Mexico, and Cuba, and seeing similarities in group schemas of about 30,000 people, I often thought about the words of H. S. Sullivan: *"We all are much more simply human than otherwise..."*

Ferdinand Knobloch
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More response from Ferdinand Knobloch:

I have been very grateful for your thoughts. And I am painfully aware that I do not address the problem with data.

If you could give me a detailed picture how australo-pitheci or at least homini 100,000 years ago lived, I could perhaps be more specific on positive side. (Did the outcast who stole a leg of a mammoth or slept with the wife of the chief have higher inclusive fitness than the ordinary guy?)

So at this initial stage we have to reverse the question: Do you know or can you imagine a society in the hunters/gatherers phase (which perhaps matters most, 99 %) when groups COULD AFFORD to ignore the items enumerated and survive the next raids? And I repeat those which I enumerated ana which the leadership (pars pro toto) tries to achieve

Organize an Efficient Group
Compete Successfully with Other Groups
Supervise Social Exchange
Prescribe Equivalences of Social Exchange
Advertise Rewards for Certain Kinds of Altruism
Particularly for the Good of the Group It
Increases its Own Fitness

If anecdotes mean anything, David had growing popularity through three millennia, because he stopped the panic among the warriors of his tribe by killing one enemy, whether by bravery or technical innovation. He became king, his inclusive fitness

was enormous, and if one Evangelist can be trusted, even God chose his son to be born in David's lineage.

Natural selection is not a System selection, but exemplifies Intraspecies selection such as sex selection. (By the way, systems interfere with sex selection: the choices of partners are by far not determined only by competition of men and choice of women, but also by the systemic pressure of society. Those in power and rich ones, who cannot "afford" to have many children (because tending to K-strategy), they aim at quality not only in their children, but also in their spouses. The conflict between sex and system selection has filled literature for ages! Equal marriages also contribute to the stability of the system.)

Morals need hierarchy. If K. Lorenz and de Waal are right, dogs have conscience, but cats don't. The "latent structure" of GROUP SCHEMA, is explained in the second article of the June issue of *The ASCAP Newsletter.*[1]

By the way, it is interesting how even such empiricist as de Waal takes intrapsychic assumptions from individualistic psychology, for granted as when talking about the conscience in chimpanzees which he warns may not be "real" conscience, because it is fear of punishment and says: *"Guilt is based on self-reproach and is therefore independent of the knowledge of others."* How can he be is so sure?

Though Dr. Barkow will likely agree with the several first items, he may have doubts about altruism. I agree that it is complicated, because hand in hand with selection for altruism goes selection for deceptive altruism and self-deception (rationalization, etc.). Yet I think that something positive has been left in us. Is it not true, as A. Seley says so well, that nothing so powerfully contributes to human happiness and even health as one's habitual tendency to generate satisfaction and gratitude in others?

I also think that the doubts of the economist R. H. Frank about the inadequacy of reciprocal altruism are worth considering. Of course, the flexibility which Dr. Barkow stresses is there - which I stressed about the accepting of rules. Even a weak authority in Milgram's experiments can impair sympathy/ empathy. This my second question to Dr. Barkow: do you have better explanation for the facts of human concern, altruism - do you think the explanation by reciprocal altruism is adequate?

The development of altruism beyond reciprocity has been so complicated, that my guess would be that it went only so far and further.

Dr. Barkow says:

"Under your framework, one could argue that something like cultural group selection is taking place. Personally, I doubt it very much, human societies do not usually exist long enough..."

Ferdinand Knobloch responds:

I am not talking about cultural selection -I assume it has been going on throughout human evolution and much longer and exists in rudimentary form in other primates. De Waal suggests for instance, that: *"Familiarity with the social code is so great among chimpanzees that individuals may act as informants about transgressions of others... ."*

Yes, altruism is complicated, but even under a mild pressure of system selection, a quality may show if it persists long enough. And that has been going on I believe, with altruism, but only to certain degree further development stopped, in balance with other strategies.

Ferdinand Knobloch
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E-MAIL ADDRESS CORRECTION

Carolyn Reichelt wrote to say that her correct E-Mail address is not what was in The *ASCAP* Newsletter July issue, but reichelt@mem.po.com.

ARTICLE:

by Russell Gardner
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Human Behavior and Evolution Society — 1996

This year the meeting of HBES took place in Evanston, III., at Northwestern University, a green and beautiful campus overlooking Lake Michigan, all the more beautiful because spring included seemingly incessant rain, which assured hydrated vegetation. Our conference allowed the sun to come out, I gather, and we not only had sun but Texas style temperatures!

But no complaint, this was a splendid setting for an excellent conference with an all-time record of over 300 registrants and a rousing, 'take it to them' talk by Edward O. Wilson who touted a word made familiar to *ASCAP* readers some time ago by John K. Pearce, consilience, 'to jump together'. Dr. Wilson is working on a book with that as its topic. He expects a convergence of science — especially biology — and the humanities will set the stage for academia of the next century. This will happen, he suggests, because there will be a consilience of disciplines. Things will make new sense in the light of biology. He doesn't duck from the enemies, arguing that sociobiology remains the word that should be used, not pallid substitutes such as evolutionary psychology. Sociobiology has won, he declared, buttressed (we learned from Napoleon Chagnon's introduction) by being one of *Time Magazine's* 25 most influential people. And the word will go out; in addition to your reporter, Constance Holden from *Science* was there are taking many notes, not only on E.O. Wilson but many others. My personal highlights, recorded here, are biased by four simultaneous sessions usually going in parallel and the fact of a needed early departure; I missed the last day and a half.

Aaron Tim Beck, who organized a paper session on depression, told me of a pre-conference at Northwestern University with the two award winners of the Aaron T. Beck ASCAP essay contest, Souhir Ben Hamida, this year's winner, and Nicholas Allen, last year's winner. Both also presented at HBES and acquitted themselves well, thereby amply and additionally justifying the judgements of our committee, which was headed on both occasions by Mark Erickson. Mark was also present at the meeting; he intends to make use of comments by both winners for the next round of the competition. I talked to a number of young people who seemed interested in submitting their work for the next go-around — and also in joining the ASCAP Society.

By the way, David Paxson, a recent new member of the society, mentioned that the key feature that caused him to subscribe was the concept of sociophysiology. From his work first in business and now in his job in Montgomery County, Maryland, doing rehabilitation with prisoners he had personally concluded there *has* to be a physiology for the behaviors with which he and his clients struggle, that are all, ultimately, social in nature.

For me one of the most interesting, ASCAP-relevant findings of the conference was that of psychiatrist Asa Nilsonne from the Karolinska Institute in Stockholm. She found that having a depressive or anxiety disorder definitely has positive reproductive consequences: she studied nearly 10,000 twin pairs in the Swedish Twin Registry. Twins hospitalized for these disorders had more children than those never hospitalized for this (1.34 vs 1.19). Given the tremendous number of subjects this was overwhelmingly significant statistically, though as a weak effect, it probably doesn't affect any one family very much.

In that same session (the one chaired by Dr. Beck), Edward H. Hagen, reported on his theory that post-partum depression was implicitly a statement to her husband (or other family members) that there was risk she wouldn't care for the child; therefore,

someone else had better do that. He called our attention to a most interesting datum, from an article by M. W. O'Hara, that bears strongly on John Price's observation that depressed people are quiet towards up-hierarchy figures but often aggressive towards those down-hierarchy, such as the family dog or the children. The finding was this: when depression measured by the Beck depression inventory was compared in women and their husbands, the correlation in 51 marital pairs at 6 months after delivery, the correlation was poor and nonsignificant. But if the husband's depression at 6 months is compared to the wife's at 9 months, a correlation of .416 emerged ($p<.001$) accounting for 17.3% of the variance! Thinking of the issues of the July *ASCAP* issue, I wondered if this was *aggression directed downwards* causing the wives to be depressed as well? From conversation, Ed felt it to be more likely *neglect.* Ed followed up his discussion with a later E-Mail message about some of methodology involved.

Some excellent plenary sessions featured bird life. Stephen Emlen noted that birds which strongly aggregate may either split for other sites starting their own families or stay together (with close relatives helping with the new offspring). Which they choose indicates assessment of the environment: if there are new territories and ample food, then the newly adult ones quickly split; but if there are no new frontiers, or low quality ones only, they stay at home. Extrapolating to humans, the real reason for the breakup of the extended family is informational: our modern age of communication has meant that new opportunities seem more evident for the youngsters newly emerging.

Another bird researcher, feminist Patricia Gowaty, focused on the competition between the sexes and the need for females to be forceful. She has been especially struck by the work of Bill Rice which showed that drosophila males allowed to evolve free of female counter-measures (females were held constant) became ever more male-like in their characteristics, victimizing females in the process. ASCAPian Annette Hollander wondered at the resistance that women exhibit in *not defending* their gender interests more strenuously. She conjectures that amongst other factors inclusive fitness means that grandmothers should look out for their grandsons (as well as their granddaughters).

Karl Grammerfrom Vienna made a striking presentation on the battle of the sexes as really a war of signals, olfactory and visual being the ones he specifically studied in his Ludwig-Boltzmann-Institute for Human Ethology (Constance Holden from *Science,* featured this in News and Comments.) ASCAP owes him specific thanks as he has made sure we have a European Web page. But we owe him more thanks now because he is clarifying the powerful role of pheromones in human sexual interaction, especially given human concealment of ovulation (female crypsis), something that has many theories (he counted 32 published). Males seem to deploy philandering strategies so that they would desire behavioral cues of ovulation but none have been discovered, female crypsis being more or less complete. Moreover, women are not fertile until they had been exposed to male pheromones for a time. Male hormones usually "stink" to females, but not, Grammer and his coworkers have discovered, during mid-cycle! This means that human females have used the tendency for male philandering to their own advantage, by choosing the optimal male for the select time of copulation. Males select for attractiveness but copulines, female pheromones secreted mid-cycle, cause men to equilibrate attractiveness ratings. Not only that but in discotheques, an observational study found, non-pill taking women going there alone advertise their reproductive state at mid-cycle by showing more skin, tightness of clothes and skirt length.

I saw Klaus Atzwanger from the same institute who has just completed data collection on walking and depression (Attention Leon Sloman whose theories this reflects!). He didn't want me to disclose what are preliminary findings but will send us a summary after he presents the more worked up data at the Vienna meeting of the International Society for Human Ethology in August (it has arrived, - see *The ASCAP Newsletter,* September 1996 issue.

Carol Weisfeld (with Glenn Weisfeld and Norma Schell) from Detroit presented research on married couples. They concluded that for greatest happiness there should exist some asymmetry with the male *moderately* dominant: too much male dominance and the wife is unhappy; female dominance means the husband is unhappy. In their sample the women were dominant when older. Dominance was measured by visual contact when speaking (*vs.* visual contact when listening).

Informal breakfast presentations made up for my inability to hear but a fraction of the formal presentations. This included work by next years HBES organizer in Tucson, A. J. Figuerada. He has done considerable work on factors determining (or deterring) family violence. Work in Tucson that showed sex, money and paternity were factors in domestic violence there. Women's kin made a difference: Work near Madrid showed that extended kin networks seemed to deter violence not from support to woman but by threat to the potential perpetrator.

On another morning, I learned from Londoner anthropologists Chris Knight and Camilla Power about their work (with Ian Watt who couldn't come) on the beginnings of art as involved with the male-female competition (like Karl Grammer's analysis but investigated paleoanthropologically) on levels quite removed from consciousness. The key finding which is reported in the April, 1995, issue of the *Cambridge Archeological Journal* involved the discovery of hematite crayons that were 120 K years old. They suggested that these crayons were the first instruments that had been used for the creation of art. If these were used for paint, it was probably body paint.

Why??? The synchrony of female menstruation means that it would pay to advertise redness on the body, suggesting to the male admirers that copulation would be more likely fertile. Ceremonies still performed in the Kalahari and other parts of Africa seem to echo themes of female unity and matrilocal lineages.

Curiously, as I wrote the above in this report while sitting in a hotel lobby, I saw that three black women, warmly bonded with each other, were *all* wearing lovely red dresses; moreover, three more of various ages young and old were all in red also, passing in front of the hotel window. After noting this on the computer, I gathered courage to venture over to ask one of the first three about the concurrent red dresses. The coincidental use seemed to be unlikely to be the tradition of hematite-ochre body paint carried forward. She told me that her church believes in unity and so everyone dresses alike on certain days. She mentioned, when I asked more questions, that theme colors at other times include purple, black and white. What a coincidence that today, just hours after hearing Chris and Camilla about this theory, that *red* should happen to be the jointly agreed on color used by these ladies in Evanston, IL, in 1996, 120 K years after the hematite crayons.

Dr. Knight felt that the general notion of an era of evolutionary adaptedness (EEA) was surely incorrect, noting that EEA was not a static period over several million years and that one should not confuse the evolution of bipedality with the evolution of increased cranial size as sometimes happens. He emphasized rather that for a 2 million year period there was a plateau in brain size which ended about 2-300 K years ago when again there was another steep slope on this trend. Dr. Potter noted that this extremely expensive reproductive cost meant that women *needed* men for much more their sperm alone. How to extract increased energic investment entailed cooperative efforts on women's part: if those neither pregnant nor lactat-ing menstruated in synchrony, then the men would know that they must work hard over considerable periods for an occasional stab at getting their sperm connected with an egg.

They have other conjectures too: why is the average ovulatory cycle exactly that of the moon's? They suggest that there in contrast to the sun, there are no direct lunar effects upon the organism. But the evidence is great that human rituals have

been heavily affected by such events as the waxing and waning of the moon. Rituals celebrate change, with the hope perhaps that the change will be for the better (some of this may have involved conversation with Ellen Dissanayake at the same breakfast table. She is also interested in the origins of art, writing *Homo Estheticus,* for instance, and clearly had had previous conversations with the London investigators.)

Serotonin came into the spotlight with across-species comparisons between rats and humans. Brian Knutson from Bowling Green State University found that rats in the open field test are more fearful and that dominance asymmetries are magnified in the social play of juvenile rats (instead of only play the aggression resulted in more savagery).

He also spent time in San Francisco working with Victor Reuss and others in evaluating normal volunteers taking SSRIs. He said later that the Raleigh-McGuire work had inspired him with respect to which evaluative tests he used to assess the effects. They expressed less hostility and cooperated more (as on a test of solving a puzzle that had to be done together). He concluded that in both species serotoninergic drugs have the effect of smoothing relationships and reducing irritability and conflict.

Does brain volume relate to intelligence in the range of variability available for normals?

P. Thomas Schoeneman noted that before precise measures such as the MRI for determining brain volume, the correlations had been estimated to be about 0.2; with the advent of more precise measures, the relationship has been elevated to 0.4; so it is clear that there is a relationship. He did a study of 30 sister-pairs in which he found that several tests of cognitive function correlated with total brain volume, r = 0.46, with grey matter, r = 0.51, white matter, r = 0.14, and with brain stem, r = 0.46. All of this was between subject variance and that there was no within family variance. Further inspection of his data provided no indication that technical factors could explain more than 30% of the variance.

Sibling order continues to command respect as a significant variable. Frank Sulloway reviewed behavioral genetic research showing that siblings are as variable as the general population and that unrelated children raised together have no specially correlated behavioral traits. He noted that when organisms compete for scarce resources, behavioral diversity would be advantageous, and sees that in the ontogeny of human offspring, niche partitioning occurs similar to that of species.

Catherine Salmon from McMaster University predicted that middle siblings are different from first or last borns. Her prediction has them less family oriented, less likely to name a parent as the person to whom they are closest to, and less likely to name their mother as an individual to whom they feel closest. Also they would be less likely to compile a family history. I didn't catch all of her data-set, but her abstract asserts that she found all of her predictions confirmed and that this was not a function of large vs small sibships. I believe that most of her data came from undergraduates but creatively, she also looked to historical societies for information and found that 259 family genealogists were much less likely to be family historians.

Ulrich Mueller in conjunction with Allan Mazur examined the yearbooks and other evidences of the characteristics of the 1950 class of West Point graduates. Their pictures were rated for dominance by groups of people; also the careers of each person was tracked and rated for success. Also they determined sociability, intelligence and scholastic attainment and learned how many children resulted from family data. The man rated most dominant had more children and was more successful.

Mueller and Mazur interestingly found that people who were most dominant but less intelligent and sociable were less than usually successful. Try to push your way when you don't have the goods and you go further down than someone else. *c8*

ARTICLE:

by David Paxson & Mike Todd
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WHY WE NEED ALIENS (from E-Mail)

When I was at HBES in Chicago, I listened to Vince Sarich and J. Barkow discuss in their Plena-ries how easily people create In-Groups and Out-Groups.

Barkow talked of how political leaders use fear of the Out-Group to their own ends. They always categorize the Out-Group as "powerful, organized and dangerous"; and then set themselves up as the one who can save the In-Group from the Out-Group.

While Barkow's points were well taken, in my perception he seemed to be doing it a little himself. To me it seemed he was characterizing the current Hate Groups as powerful, organized and dangerous.

While I find such groups obnoxious, I tend to see them on the fringe of society. I also see them as weird, wacky and somewhat disorganized. But who knows, maybe they are dangerous and I need to join a group that will fight their assaults.

Anyway, this weekend I went to see the newly opened movie "Independence Day". I find it fasci-nating that people are waiting in lines around the block to see 1:00 a.m. sold out shows in my city (Washington D.C.). Of course I was willing to wait in line mostly to observe this phenomena. Great movie, good fun and be sure to see it.

The crowd at the film seemed to me to be especially drawn to the dynamic that all of nations had to join together to fight this alien attack. In other words, the aliens were the ultimate out-group. And believe me, as characterized in this film, the aliens are powerful, organized and dangerous.

There is even a scene in the movie where Israeli and Arab fighter pilots have hidden their planes together somewhere out in the desert.

Those of us who have studied Astronomy know that scientifically the chance of an alien visit to the Earth, with friendship or malice, is of a VERY low priority.

But please don't tell that to the folks in the street. People suspect that an external threat may be the only way to achieve any type of worldwide political peace that lasts.

Anyway, it is an interesting phenomena, and I could go on. But I would like to hear the opinions of other HBES and ASCAP folk on this. My hypothesis is that even if the Aliens don't exist, we will have to invent them.

P.S meant to say that an Alien visit is a VERY low "probability", but it probably holds for "priority" as well.

David Paxson

For what it's worth, David Paxson described a classic social psychological finding — a superordi-nate goal goes a long way towards breaking down ingroup/outgroup distinctions.

The seminal work in this area was done by Sherif and his colleagues in the mid-1950's and published in 1961. The "Robber's Cave experiment" is de-scribed in almost every introductory social psychol-ogy text ("psychological" social psychology, that is—I don't know if "sociological" social psychology texts cover this). The reference for the primary source is the following:

Sherif, M., Harvey, O. J., White, B.J., Hood, W E & Sherif, C. W.: *Intergroup Conflict and Coopera-tion: The Robber's Cave Experiment,* Norman, OK Institute of Group Relations, 1961.

The study actually makes for a great story, because it involves real people (adolescent boys, not college sophomores) in a fairly naturalistic setting (a summer camp) engaging in moderately heinous behavior, and because it would probably not even get a first look by a human subjects IRB today.

Mike Todd

After I sent my note about superordinate goals and the diminution of ingroup/outgroup distinctions, I thought of a big confound in my linking of the "Independence Day" phenomenon described by Paxson and the phenomenon observed in the Robber's Cave experiment.

In the Robber's Cave experiment (and in later applied work by Eliot Aronson) in-group/out-group distinctions were broken down in order to accomplish a common goal that little to do with people outside of the interaction among folks from the two groups under consideration.

In the Independence Day example, the various human groups not only have a superordinate goal (save the whole damn planet), but also their goal involves fending off "people" who are more "out-grouperish" to any one ethnic group than the ethnic groups are to each other. That is, in comparison to these weirdo aliens, Arabs probably seem positively "in-groupy" to Israelis, thus further bolstering their desire to cooperate.

I'm not sure what this has to do with a general discussion of evolved mechanisms of human cognition behavior, but I thought I should cover myself.

Mike Todd

WANT TO VISIT SOME RELEVANT INTERNET SITES?

Ludwig-Boltzmann-Institute **for Urban Ethology Home Page:**

http://evolution.humb.univie.ac.at/institutes/urbanethology.html

The Institute was founded in 1991 by the Ludwig-Boltzmann-Society and is directed by I. Eibl-Eibesfeldt and Karl Grammar. The Institute is currently hosted by the Institute of Human Biology at the University of Vienna. The research is dedicated to all kinds of mass-phenomena in big cities. In the year 2000 more than 80% of all people will live in urbanized areas - this is certainly not the environment of evolutionary adaptedness. But most of us do quite well in this relatively new environment, despite daily hassles, stress and anonymity. The question then becomes in how far evolutionary theories can predict human behavior in such an anonymous urban environment.

Karl Grammar's Home Page. He is the Director of the Ludwig-Boltzmann-Institute for Urban Ethology, & is an ASCAP member:

http://evolution.humb.univie.ac.at/institutes/humanbiology/grammer.html

In this century human livelihood migrated increasingly from the land to the cities. From 1950 to 1978 the urban world population doubled. If this trend continues, the number will even double until the year 2000; then the majority of the world population will live in cities. The urban environment has become the favored habitat for humans.

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ABSTRACTS & EXTRACTS...

Nyberg, L, McIntosh, A. R., Houle, S., Nilsson, L.-G., & Tulving, E.: Activation of medial temporal structures during episodic memory retrieval

Gustafsson, L, Qvarnstrom, A., & Sheldon, B. C: Trade-offs between life-history traits and a secondary sexual character in male collared flycatchers

Silbersweig, D. A., Stern, E., Frith, C, Cahill, C, Holmes, A., Grootoonk, S., Seaward, J., McKenna, P., Chua, S. E., Schnorr, L, Jones, T., & Frackowiak, R. S. J.: A functional neuroanatomy of hallucination in schizophrenia

Wilcockson, R. W., Green, C. S., Day, T. H.: Heritability of a sexually selected character expressed in both sexes

Jansen, A. S. P., Nguyen, X. V., Karpitsky, V., Mettenleiter, T. C, Loewy, A. D.: Central command neurons of the sympathetic nervous system: Basis of the fight-or-flight response

Nyberg, L, McIntosh, A. R., Houle, S., Nilsson, L.-G., & Tulving, E.: Activation of medial temporal structures during episodic memory retrieval. *Nature,* 1996;380 715-717

Medial temporal lobe structures have been implicated in human episodic memory. Patients with medial temporal lesions show memory deficits, and functional neuroimaging studies have revealed activation in this region during episodic encoding and retrieval when data are averaged over a sample of subjects. The relevance of such observations for memory performance has remained unclear, however. Here we have used positron emission tomography (PET) to examine cerebral blood flow related to verbal episodic retrieval. We observed strong positive correlations between retrieval and blood flow in left medial temporal structures in individual normal human subjects. In addition, multivariate analysis showed that regions in the left medial temporal lobe were dominant components of a pattern of brain regions that distinguished a high-retrieval condition from conditions of lower retrieval. These results suggest that medial temporal activity is related to retrieval success rather than retrieval attempt, possibly by reflecting reactivation of stored patterns.

Gustafsson, L, Qvarnstrom, A., & **Sheldon, B. C: Trade-offs between life-history traits and a secondary sexual character in male collared flycatchers. *Nature,* 1995;385:311-313**

It has often been suggested that sexual selection may have important consequences for life-history evolution and vice versa. We manipulated the parental effort of male collared flycatchers (Ficedula albicollis) by changing the number of offspring in their nests and found a trade-off between parental effort and the size of the male's forehead patch (a secondary sexual character) in the following year. We report here that, in addition to this intra-genera-tional trade-off: the size of the forehead patch in first-year males was negatively related to the change in brood size of the nest in which they were raised. This has consequences for reproductive success because males with large patches mate with more females and have higher lifetime reproductive success. To our knowledge, this is the first experimental demonstration that life-history traits and secondary sexual characters trade off against each other. Our results support the suggestion that the life-history consequences of sexual ornaments are important in their evolution.

Silbersweig, D. A., Stern, E., Frith, C, Cahill, C, Holmes, A., Grootoonk, S., Seaward, J., McKenna, P., Chua, S. E., Schnorr, L, Jones, T., & Frackowiak, R. S. J.: A functional neu-roanatomy of hallucination in schizophrenia. *Nature,* 1995;378:176-179.

Hallucinations, perceptions in the absence of external stimuli, are prominent among the core symptoms of schizophrenia. The neural correlates of these brief, involuntary experiences are not well understood, and have not been imaged selectively. We have used new positron emission tomography (PET) methods to study the brain state associated with the occurrence of hallucinations in six schizophrenic patients. Here we present a group study of 5 patients with classic auditory verbal hallucinations despite medication, demonstrating activations in subcortical nuclei (thalamic, striatal), limbic structures (especially hippocampus), and paralimbic regions (parahippocampal and cingulate gyri, as well as orbitofrontal cortex). We also present a case study of a unique, drug-naive patient with visual as well as auditory verbal hallucinations, demonstrating activations in visual and auditory/ linguistic association cortices as part of a distributed cortical-subcortical network. Activity in deep brain structures, identified with group analysis, may generate or modulate hallucinations, and the particular neocortical regions entrained in individual patients may affect their specific perceptual content. The interaction of these distributed neural systems provides a biological basis for the bizarre reports of schizophrenic patients.

Wilcockson, R. W., Green, C. S., Day, T. H.: Heritability of a sexually selected character expressed in both sexes. *Nature,* 1995; 374 158-**159.**

Sexual selection is thought to be responsible for the evolution of exaggerated male characters and of female mate preferences. Evolutionary mechanisms driven by an advantage to the progeny are only effective if the preferred character has a large genetic component of variance; in most systems in which sexual selection operates, little is known of the relevant genetics. We have measured parent-offspring correlations, and report here that the preferred character (adult size) in seaweed flies has large additive genetic variance in males, but not in females. Virtually all the variance in male size is attributable to a chromosomal inversion system and, consequently, because this system is also a major determinant of larval viability, male size could be used by females as a reliable indicator of offspring survival.

Jansen, A. S. P., Nguyen, X. V., Karpitsky, V., Mettenleiter, T. C, Loewy, A. D.: Central command neurons of the sympathetic nervous system: Basis of the fight-or-flight response. *Science,* 1995;270:644-646.

During stress, the activity of the sympathetic nervous system is changed in a global fashion, leading to an increase in cardiovascular function and a release of adrenal catecholamines. This response is thought to be regulated by a common set of brain neurons that provide a dual input to the sympathetic preganglionic neurons regulating cardiac and adrenal medullary functions. By using a double-virus transneuronal labeling technique, the existence of such a set of central autonomic neurons in the hypothalamus and brainstem was demonstrated. These neurons innervate both of the sympathetic outflow systems and likely function in circumstances where parallel sympathetic processing occurs, such as in the fight-or-flight response.

AS CITED BY ....

Cover page

[1] Meyers, Jeffrey, *Robert Frost: A Biography.* Boston, MA: Houghton-Mifflin, 1996.

Miller, Tim, Bailey, Kent, & Gardner, Russell: Miller-Bailey Debate ... page 6

Frost, Robert, *Into My Own,* lines 13 and 14. In Frost, Robert (Edited by Lathem, Edward C): *The Collected Poetry of Robert Frost. The Collected Poems, Complete and Unabridged.* New York, N.Y.: Henry Holt and Company, 1979, p. 5.

Interactive Musings on the EEA, Excerpts from E-mail with a last word by Tim Miller, *The ASCAP Newsletter,* 1996;9(6):19-27.

Barkow, Jerome & Knobloch, Ferdinand: Natural, Sex, and System Selection (from E-Mail)... page 13

[1] Knobloch, Ferdinand, Mental life as a small group process - Towards a theory of group schema, *The ASCAP Newsletter,* 1996;9(6):17-19.

[2] Glantz, Kalman & Pearce, John K., *Exiles From Eden: Psychotherapy From An Evolutionary Perspective.* New York: Norton, 1989.

WANT TO VISIT SOME MORE RELEVANT INTERNET SITES?

THE INTERNATIONAL SOCIETY FOR HUMAN ETHOLOGY HOME PAGE:

http://evolution.humb.univie.ac.at

The International Society for Human Ethology aims at promoting ethological perspectives in the scientific study of humans worldwide. It encourages empirical research in all fields of human behavior using the full range of methods developed in biology and the human behavioral sciences and operating within the conceptual framework provided by evolutionary theory. The Society aims at promoting the exchange of knowledge and opinions concerning human ethology with all the other empirical sciences of human behavior. It administers its funds to support this purpose. The Secretary for this Society is Karl Grammar.

INSTITUTE FOR HUMAN BIOLOGY AT THE UNIVERSITY OF VIENNA, AUSTRIA

http://evolution.humb.univie.ac.at/institutes/humanbiology/humanbiology.html

This page was compiled by Karl Grammer and Katrin Schafer.

In 1922, Rudolf Poch founded the INSTITUTE FOR ANTHROPOLOGY and ETHNOGRAPHY. Poch's famous expeditions to Australia, New Guinea and South Africa enriched both scientific disciplines enormously and in 1927, two new institutes followed: The ANTHROPOLOGICAL INSTITUTE and the INSTITUTE FOR ETHNOLOGY. The ANTHROPOLOGICAL INSTITUTE was renamed in 1972 by Emil Breitinger, the former Director to the INSTITUTE FOR HUMAN BIOLOGY. The institute belongs to the Faculty of Formal and Natural Sciences, University of Vienna.