

ASCAP NEWSLETTER

Across-Species Comparisons And Psychiatry Newsletter

Volume 2, No. 12, 15 December 1989

"Even when we are engaged in lively conversation and seem to be engrossed in purely verbal communication we remain ardent bodywatchers. .. Clever as we are at bodywatching, we do make errors." Morris¹

(c/o Russell Gardner, 1.200 Graves Building (D29), University of Texas Medical Branch, Galveston, TX 77550)

Note 1990 Subscription policy!
For ASCAP Vol 3 (Jan through Dec, 1990) we will need \$18 (US dollars) for the 12 issues. See subscription form at end.

For the philosophy guiding this newsletter, see footnote on p.10²
Newsletter aims: 1. A free exchange of letters, notes, articles, essays or ideas in whatever brief format.
2. Elaboration of others' ideas.
3. Keeping up with productions, events, and other news.
4. Proposals for new initiatives, joint research endeavors, etc.

Features: ASCAP began December 1987 as a Christmas greeting to many friends. Last year a graceful description of evolutionary biology seemed a good Christmas token. This year another token comes from Christopher Wills: The Wisdom of the Genes. NY: Basic Books, 1989 (a new book sent to us by Hagop Akiskol).

Mostly, however, this issue features replies to Kalman Glantz's (KG's) essay in the Oct ASCAP on the Aug meeting of the Human Behavior and Evolution Society (HBES) from James Fetzer, Paul Turke and Michael Ruse. Dan Wilson adds his HBES reflections.

Notes: Next issue, our first for the paid subscribers, features contributions from John Price and Michael Chance (both a letter - with a challenge to ASCAP- and a reaction to

KG). Feb will feature SW Itzkoff's response to KG (see letter below).

In January's issue also I hope to suggest some instruments potentially usable in a multinational, cross-cultural study that we will be talking about more extensively in March in Boston. Speaking of which, if you can come to this meeting, mark your calendar: Sunday morning, March 25, 1989. We expect this to be precursor to a more extended meeting of an interested group in New York in May. Meanwhile, send preliminary thoughts!

Quotes from The Wisdom of the Body by Christopher Wills.

Each species, no matter how simple or complex, has a history of three and a half billion years, extending to the first organisms that could truly be called alive. And each has a history so fraught with perils that the odds against survival are almost vanishingly small. Indeed the overwhelming majority of species have become extinct, through sheer chance, the inability to adapt, or both. The ones that are left are, for the most part, a superbly fit set of survivors...The history of each of us is a history of millions of ..hairsbreadth escapes, and each of these has had a part in shaping our genes and making them better at their job.

..The amount of information carried by..forty-six chromosomes is immense. The DNA is made up of subunits called nucleotides, and there are about six billion of these in the complete human chromosome set, an average of 130 million for each chromosome. All of this DNA would extend about three yards were it to be stretched out fully. It is in fact squeezed into the nucleus of the cell..about two millionths of the volume of a pinhead.

The nucleotides are like bits of information in a very long computer program, chopped into sub-

programs in the form of genes. But the program is far larger and the density of information far greater than in even our best computers. This becomes apparent when we compare the nucleus of a cell to a computer chip, our most sophisticated electronic device. If we ignore their clumsy electrical connectors to the outside world and the thick substrate on which they rest, the best current computer memory chips hold a million bits of information in a volume one millimeter on a side and ten-millionths of a millimeter thick. This may seem very small, but it is still twenty thousand times the volume of a human nucleus. If the nucleus were similarly inefficient, it could hold only fifty bits of information. But in fact, it holds 240 million times as much as that.

A string of one or two thousand of these nucleotides contains enough information to specify the synthesis of an average protein. If all genes made proteins, there would be enough genes on each chromosome to specify a hundred thousand or so, yielding a total of about two million.

But this would be far more proteins than actually exist in any organism. Even we, the most complicated organisms we know of, are apparently made up of no more than about sixty thousand different..proteins.

Since we produce a relatively small number of proteins compared with the number we could make, it is apparent that genes for proteins account for only a small percentage of the DNA. Much of the rest seems to be involved in other functions, such as regulating the levels of proteins that are made and determining the course of development of individual cells or groups of cells in the body. And **indeed**, a lot of **the DNA seems to have no** obvious function at all. All these stretches of DNA .. can be considered genes in the broadest sense, since careful copies are made by the cell and passed on to the next generation.

..As we explore the architecture of our genes, we may find clues to where the well-worn developmental paths lie, [and] how old they are., cross-species and cross-group comparisons are some of the most interesting evolutionary studies that will be embarked on in the near future. The kinds of information that can be obtained by comparing developmental genes will be very different from that obtained by comparing protein sequences. This is because developmental genes are much more than just protein-coding regions of the DNA. . .

Letters: Apr 4, 89

[In response to the ASCAP Readership Survey]: I like it [ASCAP]. I enclose the announcement of a Language Origins Society(LOS) meeting at Austin TX. And that of the European Sociobiological Society at Brussels.

One - trivial - remark: readability of the Newsletter would increase if you would use instead of a dotted line a straight double line and/or an open line between the various items.

Jan Wind, Human Genetics, Free University Amsterdam, Netherlands

Thanks for the not "trivial" but important suggestion (implemented this issue) from one who has written many newsletters. A real improvement.

Many apologies for delay in publishing your letter. It got lost and the meetings you mentioned have passed, so for that reason are not here announced. We'll be careful not let such happen again. A summary of LOS will appear in a future ASCAP.

24 October 1939

Thank you for your copy of ASCAP Newsletter, Vol. 2, No. 10, dated 15 October, 1989. I am indeed grateful for the opportunity to respond to the featured essay, "Conference Report", by Kalman Glantz. A copy of my reply is enclosed.

I would be greatly indebted to you if you could also publish my response in the December issue of your newsletter. You may also want to consider the benefits that might accrue from sending a copy of that special issue to each of those who received this one.

With appreciation.

James H Fetzer, U Minn, Duluth

Your reply follows below. Distribution will be same as October's.

11/15/89

Enclosed is my response to Glantz's comments in the October ASCAP Newsletter.

Thanks for the opportunity...

Paul Turke, U Mich, Ann Arbor

Your reply also follows below.

November 7, 1989

Many thanks for sending me the latest issue of the ASCAP Newsletter. I have read it through with some interest and particularly the summary of the human behavior and evolution meeting. Given the fact that I am referred to so favourably, as you can imagine, I don't have much to say by way of criticism! However a couple of points do come up that I might comment on.

First, I note the bemusement your colleague felt at the paper by James Fetzer (p7) where he, as a philosopher, took on the work of Leda Cosmides. Perhaps it won't surprise you, given the fact that I am a philosopher, that I thought that this was by far the most stimulating paper I heard at the whole conference! It seemed to me that Fetzer was doing a real service in challenging the very strong claims that people like Cosmides are making about the nature of rationality. He may, of course, be wrong but as I read him he was trying to spell out exactly what we understand by logic and showing that her empirical results really do not touch many of the basic claims. A lot of this came out more in discussion but part of the problem here was that we simply did not have a long enough discussion. Anyway, whilst I recognize that maybe philosophers do tend to be a lot more confrontational than others, I would like to say how much I enjoyed Fetzer's talk. Indeed, to the extent, that speaking now as editor of Biology & Philosophy. I have asked him to submit something on

the topic for possible publication in the journal (and in true confrontation style, I shall invite Cosmides to make comments on it!). If something comes of this I will let you and your readers know.

The other point is the poignant note at the end about Zhang Boshu, page 9. I, too, was worried that Zhang Boshu did not turn up, particularly given his interest in Marxism [and] human sociobiology. Once again I have had a hand here since I published a paper by him on the topic in Biology & Philosophy. I had not heard from him during the upheavals in China this last summer and as you can imagine I was somewhat loath to write to him because I feared that this might well make a bad situation worse. However I have since heard from him and whilst he does not talk about the troubles in China, he gave me no reason for concern about his own personal well-being. Indeed, he has been working on a book on sociobiology and Marxism and he gave me reason to think that he was continuing to work on this. He is not a student but a young scholar in the Department of Philosophy at Peking University and may well be out of all direct harms way. But if I hear any further I will let you know and conversely if any of your readers have an opportunity to get in contact with him (his address is Department of Philosophy, Chinese Academy of Social Science, Graduate School, Beijing, The People's Republic of China), I would in return appreciate any pertinent information.

Thanks again for sending me your newsletter.

Best wishes,
Michael Ruse, U. Guelph, Ontario

We appreciate your anathetic comments about Dr. Zhang Boshu as well as those about Dr. Fetzer.

30 November 1989

...enclosed is a reaction to Kalman Glantz: October 1989 piece. My article is entitled "The Sociobiological Dilemma." Perhaps it will stimulate further discussion on this crucial issue.
Best wishes.
Seymour W Itzkoff, Smith College

Your piece will appear in the February 1990 issue. In it you refer to your argument being further developed in The Evolution of Human Intelligence, a series of four books published by Peter Lang International Publishers, NY, 1983-1989. May I ask if you would provide ASCAP readers with the specific names and references. Informally, by the way, I hear that EO Wilson thinks highly of these books. Thank you in advance.

Quoted Abstract: Sapolsky RM: Hypercorticalism among socially subordinate wild baboons originates at the CNS level. Arch Gen Psychiat 1989;46(Nov) -.1047-1051.

Recent studies suggest that the hypercorticalism and dexamethasone resistance of depression arise, at least in part, at the level of the brain, ie, Cortisol - releasing factor (CRF) and/or other corticotropin-secreting hormones are hypersecreted. This article suggests a similar cause of the hypercorticalism of social subordination. Two troops of wild olive baboons, living freely in the Serengeti Ecosystem of East Africa, have been under long-term study. Consistently, in stable dominance hierarchies, subordinate males are hypercortisolemic relative to dominant animals. Furthermore, hypercortisolemic males are dexamethasone resistant. There are no rank-related differences in Cortisol clearance or adrenal sensitivity to corticotropin, suggesting a pituitary and/or neural locus of the hypercorticalism. Subordinate males were shown to secrete less corticotropin in response to a CRF-challenge than did dominant males. Following the logic used in similar studies with depressives, if subordinate males were hypercortisolemic despite decreased pituitary sensitivity to CRF, then this

implies that the hyperactivity of the adrenocortical axis is driven at the level of the brain. Furthermore, subordinate males were hyporesponsive to CRF after administration of metyrapone, which blocks Cortisol secretion and disinhibits the pituitary from feedback inhibition. Thus, the pituitary appears to have lost sensitivity to CRF itself in these low-ranking males. These observations are interpreted in light of behavioral data suggesting that these subordinate males are under sustained social stress.

* Featured Reply #1 to KG *
Kalman Glantz vs. Logical Reasoning
by James H. Fetzer

The peculiar essay in the 15 October 1989 issue of the ASCAP Newsletter entitled "Conference Report" by Kalman Glantz provides an exceptionally unbalanced and injudicious depiction of several of the sessions and events that transpired during the Human Behavior and Evolution Society meeting at Northwestern during August. What he had to say about the session in which my paper, "Logical Reasoning and Domain Specificity", was presented, for example, represented a complete distortion of the content of the paper, the mode of its delivery, and the exchange that followed. The editor of this newsletter has generously afforded an opportunity to respond, for which I am grateful.

Although it would be impossible to discern from reading the Glantz's report of this presentation, my paper offered an evaluation of the theoretical adequacy of a research program within evolutionary psychology. According to this research program, humans do not possess general-purpose reasoning modules (such as some philosophers and psychologists at times seem to have supposed) but instead possess evolved special-purpose (or "domain specific") reasoning modules. Some of the most important evidence for this position in evolutionary

psychology, moreover, has been advanced by Leda Cosmides in her doctoral dissertation of 1985 at Harvard, which I discussed in my lecture.

The purpose of my presentation was to explain why the views that Leda has developed do not support the conclusions that she wants to sustain. The reasons for this are many and varied, extending to her reliance upon use of the Wason Selection Task in a manner that cannot be theoretically justified. The argumentative fallacies that are committed during the course of the research--which include begging the question, ignoring obvious alternatives, special pleading, and attacking straw men--could sustain an introductory course on informal reasoning for a semester. I shall not attempt to review it here, but my critique can be found in the March 1990 issue of SYNTHESE. An expanded version will subsequently appear in Biology and Philosophy.)

The arguments that she presents are valid, but they are not sound. This means that, if you were to accept her premises, then her conclusions would follow. The problem is that her premises, in general, cannot be justified, in turn. This means that she has not provided appropriate grounds for believing that her premises are true. It does not mean that her conclusion is false. Indeed, I believe that there is as much prima facie reason to think that there are domain-specific reasoning modules as there is to suppose that there are domain-specific sensory modules. I admit that this hypothesis is plausible.

There is an enormous difference, however, between having a plausible hypothesis--such as that there are evolved domain-specific reasoning modules--and providing enough theoretical and empirical evidence to justify its acceptance. This difference is called "science". If the community of evolutionary psychologists wants to band together to protect "one of their own" from the

inadequacies and shortcomings of her research, then that, no doubt, is a possible response. Indeed, it is the attitude displayed by Glantz. But if this is our situation, then it would be more honest and less misleading to regard evolutionary psychology as a social fraternity than as an intellectual society.

As someone who has spent nearly twenty years in the classroom offering instruction in logic and logical theory, I believe that I am well-positioned to evaluate whether or not someone from another discipline is making informed and intelligent use of results from my field. I feel exactly the same way that most of you would feel if you were to discover that I was abusing work from your field in my courses and research in philosophy. Surely I have as much right to attempt to set things right as you would have under similar circumstances. By the same token, however, I do not just ask you to take my word for it. My arguments will be available for your public criticism.

The capacity for criticism is the hallmark of a serious discipline. I would be distressed if I believed that this person is representative of this field as a whole. In fact, Glantz himself provides excellent grounds for dismissing his views as mistaken and misconceived. He describes my reference to "modus tolens" (sic), for example, as "medieval-sounding apparatus", as though this were not one of the most elementary principles of logic. It is impossible to discuss the Wason Selection Task without reference to this rule, moreover, since its purpose is to test for our capacity to exercise it. No one who possesses so little knowledge deserves to be taken seriously on these subjects.

*** Featured Reply #2 to KG ***

Response to Glantz on HBES

Paul W. Turke

At the risk of confirming Kalman Glantz's³ characterization of me as pugnacious, I admit to disagreeing with much of his interpretation of my presentation at the Aug, 1989, HBES conference. In my view, Glantz has so exaggerated the positions taken by behavioral ecologists (BEC's) that the debate he describes doesn't really exist.

First, there is his statement (p5) that evolutionary psychologists (EP's) "simply point out that you can't assume that people are acting adaptively." How can Glantz think that this is a debatable issue after he has just reported on the efforts of BEC's (Pesce, Voland & Engel, Johnson, Judge & Hrdy, et al.) to demonstrate empirically that particular behaviors in contemporary societies are adaptive? By definition, one is not assuming that which one is attempting to demonstrate.

If anything (and this was one of the points of my presentation), this criticism should be applied in the other direction. That is, some EP's (Barkow, Cosmides, Symons, & Tooby) lapse into assuming that there is no basis for expecting the human psyche to produce adaptive outcomes unless the environmental context is that of !Kung hunter-gatherers. This really is an assumption on the part of certain EP's because they argue that studies which assess current adaptiveness are irrelevant. eg, Symons.

Notwithstanding claims by Symons and others, a number of theoretical arguments provide a strong basis for hypothesizing that the human psyche can be expected to function adaptively in environments that are very different, in some respects, from the environment of the !Kung. For instance, even if the Pleistocene was uniformly inhabited by people who subsisted in ways resembling the !Kung (which it was not--some Pleistocene inhabitants may not even have

been primarily hunter-gatherers), subsistence issues particular to the Pleistocene may not have been the primary selective pressures that molded the evolution of the human psyche. A reasonable alternative hypothesis is that social competition, which in many instances was only very indirectly related to the particulars of Pleistocene subsistence, primarily molded the evolution of the human psyche (eg, Alexander). In turn, it may be that the human psyche continues to be used for the primary purpose for which it was designed: to outwit other humans in the quest for status and resources. Thus, it is not all that obvious that the important aspects of the environment to which humans are adapted really have changed all that radically in the last ten thousand years. (Incidentally, since the tendency to produce fewer but better nurtured offspring has evolved again and again in a diversity of organisms, the observation that contemporary humans rarely produce more than a few surviving offspring -- in any society -- is not sufficient, in of itself, to refute the prediction that current behavior is adaptive.)

The foregoing discussion hints at one reason why it is important to test for current adaptiveness (see Betzig for some additional reasons).⁶ Specifically, as I proposed at the HBES conference, studies which indicate that people behave adaptively in at least some contemporary environments cast doubt on many nonevolutionary constructions of human nature (psychology), and can be used to distinguish alternative evolutionary constructions that are at odds over many issues pertaining to ontogeny and evolutionary background (see Turke).⁷ I, for instance, was concerned to show that evidence of adaptive behavior in a range of contem-

porary environments favors hypotheses suggesting that the human psyche is more general-purpose than some EP's would have us believe. The foregoing of course demonstrates that those of us who study adaptive significance are not somehow against studying the psyche, nor are we "phenotypically agnostic" (to quote one of Symons' exaggerations) .

Another telling example of exaggeration is apparent in Glantz's statement, "If the brain is set up as a general purpose processor, it can work just as well in all kinds of environments, so it can be expected to produce adaptive behavior in all kinds of environments" (emphasis in original,p5). Similarly, he states, "You need the general purpose hypothesis in order to assert that behavior is always adaptive, even outside the natural environment."³

In fact, however, no one with even a hint of evolutionary sophistication would ever imagine the psyche to be as infinitely flexible as the above quotations imply. In my presentation I reviewed, among other things, the hypothesis that consciousness is a relatively(but not infinitely) general-purpose mechanism which, through the production of social scenarios, allows individuals to deal adaptively with a range of novel socio-cultural phenomena introduced by other intelligent competitors (see Alexander). Furthermore, I never denied the existence or importance of special-purpose psychological mechanisms.

Glantz's agenda aside, it is appropriate to emphasize that if the human psyche is somewhat more general-purpose than some EP's imagine - - that is, if the relevant selective pressures of the Pleistocene were the often novel socio-cultural constructions of competing conspecifics -- one would predict that humans should be found behaving adaptively in at least some contemporary environments. By assuming this prediction to be either

generally false or irrelevant (e.g. Symons)⁵ some EP's assume away a plausible alternative hypothesis about the nature of the psyche, and at the same time reduce their own argument to tautology: the psyche must be designed to overcome specific problems that are, in some crucial respect, unique to the Pleistocene because behavior is maladaptive outside the Pleistocene, and humans must behave maladaptively outside the Pleistocene because their psyches are designed to overcome specific problems that are, in some crucial respect, unique to the Pleistocene.

The final point I'd like to make is that Glantz errs in describing my presentation as a "sucker punch." He states: "The surprising thing is that no time had been set aside for a reply. None of the evolutionary psychologists on the program had been scheduled to address the overall theoretical issues. It was as if the boxer from one ring suddenly ran over to the other one, got in a sucker punch, and skipped back to his own ring"(p5).

This is a narrow review. Glantz apparently is unaware that over the past few years a number of EP's have lectured at Michigan and elsewhere, under formats nearly identical to that at the HBES conference, on the supposed faults of BEC (sometimes termed Darwinian Anthropology). Glantz may be ignorant of this history of interaction, but: J Barkow doesn't have such an excuse. The larger issue, though, is so what if people present critical papers. It happens all the time. If Glantz and Barkow really think that critical papers should be confined only to round table formats, I hope they're outraged by the fact that a couple of the papers in M Ruse's session criticized R Alexander's recent book, The Biology of Moral Systems. Somehow, I suspect they're not.

*** Featured Report ***

Notes and Reflections: Evanston. 8/89

Daniel R Wilson

HBES Evanston was a good meeting. Having just been in Edinburgh, I was relieved to feel a coevolution of the HBES in conjunction with, not divergent from, the International Society of Human Ethology (ISHE). Some European colleagues sensed a schism, but meeting jointly soon should help. In the two meetings I was struck by subtle distinctions between ethology and evolutionism. The two have much in common, but evolutionism seems more inclusive even as ISHE is the broader organization.

However, if anyone should argue that all behavior is adapted/evolved at all times in all places, I would gladly provide a tour of my inpatient ward or of Times Square. Medicine has learned much of what it knows about normal physiology by scrutinizing the natural experiments of pathology. Working in this grand tradition, we see that disease results from either a misfit organism (and behavior) or altered environment. ("Fit" after all is a clothing metaphor!) Since no gene, individual or species ever completely fits an environment of evolution, nothing is truly normal or fit to the present. Evolution operates across time, not in the present. Pathology may consist of incomplete fitness to an archetypic environment or, at least, by fast changes in previously stable environs.

The difference is NOT important from the point of view of the clinician, except for rehabilitation strategy. Misfit is misfit. Pain arises from misfitness. To insist that a specific environment is the *sine qua non* of evolutionary psychology is not in the spirit of evolution itself and misses the fact that our genes code for a good deal of neuroplasticity. We also have a huge, poorly understood associational cortex which surely blurs rigid distinc-

tions between general vs specific brain operations.

Domain vs general mentalities are not a fresh concern unique to evolutionary psychology. Behavioral neurology--wellspring of this work--has struggled with this false dichotomy for a century. Disputes between "locationists" and "generalists" make Fetzer's tones toward Cosmides diplomatic. Neurologists now clearly localize "domain-specific" perceptual functions in the brain: consider hemi-quadranto-achomatopsia or the loss of color component only of vision in one corner of the eye field.

Incidentally, Cosimides gave a report on a clever experiment that showed men better at detecting bluff than women. How fascinating to do the same study on clinical populations, ie, paranoid males, histrionic females (or vice versa), but Leda Cosmides told me later that she has little access to patients.

The "Roundtable" was less a discussion than a series of reviews and aphorisms from "Fathers of the Field". Hamilton speculated on the biologic consequences of a socio-cultural dependence on Cesarean sections. He wonders about the prompt decoupling of fetal head size to pelvic aperture. He was hooted a bit by those who noted the obvious point that few women actually get these operations, but no one factored in the socio-political power of those who do. He also asked, more or less, why there is so little interest in HUMAN sex dimorphism of form and function. He especially referred to humanoid paleoanthropology but discussion led to modern, clinically-relevant queries. He noted that Australopithecines were quite dimorphic whereas Homo sapiens demonstrate surprisingly convergent trends for the sexes. This trend of dependence on female economic production via gathering forced the the social consequences of a subtle unisexation, or

"feminization" of male gender roles. This is an area I have snooped around from a clinical perspective before: put briefly, male bipolars and female hysterics may have particular difficulty with "mutual gender regulation" (Kalman Glantz's phrase).

Wilson also talked about future evolutionary studies: (1) what is the "unit" of culture? and, (2) what is the biochemical modulation of culture as it is linked to genes? He mused about "semantic memory"--serial, discrete, dedicated (for domain-specific learning). Culture, he reminds us; was and is linked to and shaped by evolution. Culture also shaped and may still be shaping genetical evolution (cf: discussions on Darwinian Anthropology). He seemed to endorse the re-evaluation some of us are undertaking of the link between ontogenic and phylogenetic schemae, ie, Freud, Ericson, Piaget, Kohlberg and other exponents of developmental stages that probably required (for their interiorization in the genome) stable phylopenies of phenotypic selection. In a side chat with him he modestly allowed as to how he is working on "biodiversity." Otherwise said, he is at the forefront to save the Earth from emphysema by saving its lungs in the Amazon rainforest.

Eibl-Eibesfeldt continued the theme of phylogenetic adaptations. He noted, as have P Gilbert and J Price, how social evolution displaces direct natural selection with something of an artificial selection. He also overtly mobilized an idea percolating at the conference that GROUP (not just kinship) selection might somehow be a feature of biocultural coevolution of evolved modes of symbolic identification; the superorganic may transcend organic identification. No one dusted off Wynne-Edwards, but his shadow was seen.

Richard Dawkins, the most callow of the assembled Knights, was energetic and spiffy. He declared that what is good is not, ipso facto, evolved.

Evolution in the dawkinsian scenario requires an exact replicating entity with increased frequency over generations before it is worth invoking "adaptive/evolved" ideas. Organisms and groups are not replicators but rather vehicles thereof. All humans, he believes, are preadapted in a latent or facultative way to various environs. There has been insufficient time for the various "tribes" of our human band to have evolved even subspecies level fitnesses. This is clarified in his writings on the extended phenotype. He urged us to no longer cite him as coiner of "memes" so as to speed its entry into the Oxford English Dictionary!

George Williams offered accessible "hard science" on evolutionary (nee' population) genetics. He was kind enough also to forgive :3J Gould's polemic trespasses. Without a dismissive tone he noted Gould's prime contribution has been to interest kids in biology via his column in Natural History. I was thrilled to hear him (along with a few others like Hamilton) annoint my beloved epidemiology as a major avenue toward understanding behavioral genetics and culture (I had coupled evolutionary genetics and psychiatric epidemiology last year in Ann Arbor). He made a point which, to a physician at least, is wise. He spoke of "cultural epidemics". (Culture, for all its vaunted plasticity and seductions, is not at all as closely linked to concerns of health and fitness as is/was biology. This states directly my own argument - rooted in Freud, Aristotle and even the Bible itself - that culture causes disease. Not all, but some diseases are induced by cultural wrenching of abiding adaptations.) He also reminded us that DNA is not some transubstantiation of God into matter. The gene is not molecular, it is semiotic - the coded message counts not the coding medium.

1. Morris D: Body Watching New York: Crown Publishing, 1985, page 7.
 2. ASCAP philosophy and goat. High scientific importance rests on comparing animal behaviors across-species to understand better human behavior, knowing as we do so that evolutionary factors must be considered for understanding properly such behaviors. To accomplish these comparisons, very different new ways of viewing psychological and behavioral phenomena are required. This in turn explains why we need new words to define and illustrate new dimensions of comparisons across species. We expect that work in natural history biology combined with cellular-molecular biologic research will emerge as a comprehensive biologic basic science of psychiatry. Indeed, this must happen if we are to explain psychiatric illnesses as deviations from normal processes, something not possible now. Compare to pathogenesis in diseases of internal medicine.
Some neologisms that hopefully will help implement these goals are those of:
 - a) Michael R. A. Chance-: "hedonic" and "agonic" refer- to the tore of groupings of conspecifics (members of a same species) i.e., relaxed and fun-loving versus tense and competitive.
 - b) J.S. Price: "anathetic" and "catathetic" describe conspecific messages. Catathetic messages "put-down" and anathetic "build-up" the resource holding potential (R) of target individuals.
 - c) Russell Gardner, Jr.: "psalic" is a 2 way acronym: Propensity States Antedating Language In Communica-tion and Programmed Spacings And Linkages In Conspecifics. This describes communicational states conjec-turely seen with psychiatric disorder and normality (human and non-human), ie, alpha psalic seen in manics, high profile leaders and dominant non-human animals. Eight psalics are named alpha (A), alpha-reciprocal (AR), in-group omega (IGO), out-group omega (OGO), spacing (Sp), sexual (S), nurturant (N), and nurturant-recipient (NR).

These new or renewed terms are initiated or elaborated in Chance, MRA (Ed) Social Fabrics of the Mind. Hove and NJ: Lawrence Erlbaum Associates, 1988.

 - d. Paul Gilbert: Social Attention Holding Power/Potential (SAHP) focuses upon the non-aggressive facets of leadership when this is deployed in the hedonic mode. See ASCAP v.2, #1 and his new book: Human Nature and Suffering. Hove and NJ: Lawrence Erlbaum, 1989.
3. Glantz K: Featured essay: conference report. Across-Species Comparisons and Psychiatry Newsletter 1989;2(10):3-9.
4. Symons D: A critique of Darwinian Anthropology. Ethology and Sociobiology 1989;10:131-144.
5. Alexander RD: Evolution of the human psyche. Paul Mellars and Chris Stringer, eds.. The Human Revolution. Univ.Edinburgh Press, 1989.
6. Betzig LL: Rethinking human ethology. Ethology and Sociobiology 1989;10:315-324.
7. Turke PW: Which humans behave adaptively, and why does it matter? Under Review, 1989.

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