THE ASCAP NEWSLETTER

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

Volume 7, No. 2, 15 Feb 1994 (Cumulative #75)

"Truth often lives on the outskirts of ancient controversies and moves to the center of town only when both thesis and antithesis are transformed." Gerald Edelman[1]

The ASCAP Newsletter is a function of the ASCAP Society[2]

Newsletter aims

1. A free exchange of letters, notes, articles, essays or ideas in 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

ASCAP Society Mission Statement:
The 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 psychopathologically related states. We are interested in the integration of various methods of study ranging from cellular processes to individuals to individuals in groups.

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May Meeting Update/Tentative Masthead change

Plans are ripening for the May 21,1994, meeting. We are grateful to Debra Snyderman for the work that she is doing. Regretfully, the art history may be too problematic for this meeting.

In discussions with President Paul Gilbert, we've realized that the January designation of it as a business meeting trivializes its importance. This is the first international meeting to coalesce the mission and other characteristics of the group. Accordingly, the schedule of the meeting will feature a keynote talk from President Gilbert himself. He will set our tone and agenda.

We should all ponder how under his leadership we should proceed further. How should work be done on organizational components. Should there be work groups? What? How?

We made some practical decisions about name so that Paul can meaningfully write to journals and newsletters to enhance involvement and membership. These decisons are based on input from members.

Perhaps the best designation of the society may simply be

ASCAP. Keith Dixon this issue points out that the added I in IASCAP was confusing. So for now we will forget acronymic meanings. The issue is not closed, however. Should there need be acronymic meanings, the issue can be brought up in May or debated here.

From John Birtchnell and Michael Chance, we have the plea to include not only evolutionary biology and psychopathology but normal interpersonal relations as well. Done. What do you think of the new byline that replaces evolutionary with paleo- (with a nod to Kent Bailey) and also includes sociophysiology, interpersonal and group relations, and psychopathology.

Paul is taking the initiative to write various journals and newsletters to advertise the group. Again if you plan to come to the May 21, 1994, meeting, 9 am to 3 pm, Room 139, Jefferson Alumni Hall, 1020 Locust Street, Philadelphia, Pennsylvania, U.S.A., let Erica Ainsbury (see below) know immediately so we can monitor participant number

Managing Editor

Amongst the other changes on the masthead of Volume 7 of The ASCAP Newsletter is the name of Erica Ainsbury as managing editor. We have welcomed her with great pleasure in the newsletter's central office. Fittingly, she comes originally from London, England, and studied in Manchester, just miles from Michael Chance's Birmingham from where not only did the Lunar Society of the Eighteenth Century stem, but also the pioneering ethology of our founding president.

Erica nicely fosters a hedonic mentality in the office and is also extraordinarily efficient and helpful, gradually coaxing stubborn computers into domestication. They aren't there yet, but we hope for a still better newsletter facelift and we anticipate with some eagerness, the connection of our office with the internet set up by John Pearce.

Note the questionnaire! Please read the following abstract of the research that John Price and I are conducting, answer the questionnaire, and send it back (by mid-March to have your assessment included in the tabulated results. We look forward to your opinion and hope to distribute the questionnaire to other groups of psychiatrists or trainees also. Please give us feedback!

Assessing the social competition model of mania & depression

Specific aims:

(1) What are the defects of the social competition hypothesis according to professionals and informed non-professionals?

(2) How does professional training and experience relate to the responses?

(3) How do age and sex relate to the responses?

Background and significance:

The social competition model of mania and depression states these mood changes evolved to subserve winning and losing in social competition. The clinical condition of mania would therefore be based on a basic plan for assuming a high social rank. Depression is based on a basic plan for falling in rank. The function of the mood and associated behaviors is to accommodate the individual to the requirements of the social role represented by assumption of or change in rank.

Methods:

The social competition model has been a controversial or ignored formulation so that we are undertaking a survey of several groups comprised of professional and informed non-professionals to determine their individual impressions and criticisms of the model. To gather this information systematically, the accompanying survey instrument has been devised, deploying ratings of fourteen statements along with additional comments and demographic informantion in addition. Respondents will be asked to identify each statement as representing a major and minor defect in the model, or not a defect.

Individuals will be anonymous unless they choose to designate name for which a space is left. The respondents will be consenting by sending the completed questionnaire.

The data will be compiled and analyzed and later published in the newsletter and possibly in other places as well.

The December 1993 Human Ethology Newsletter has arrived with an announcement of the 3-7 August 1994 meeting in Toronto, Ontario, Canada. Questions about arrangements should be directed to Irwin Silverman, chief organizer: phone 1-416-736-5822; FAX 416-736-5814; Psychology Dept, York U, 4700 Keele St, North York, Ontario, CANADA, M3J1P3 E-mail ISILV@VM1.YorkU.CA. Questions about program should be directed to Linda Mealey: phone 1-612-363-3155; Psychology Dept, College of St Benedict, Collegeville, MN 56321 USA; E-mail LMEALEY@csbsju.edu.

Michael Chance Column

December 24, 1993

You ask us to specify the focus of our endeavour (in ASCAP Nov 93). Well! I'm going to make a very radical, and at first sight way out, suggestion - namely that we become less focused on our belly button! ie, less psychiatrically oriented, and endeavour to relate our thinking to the world as a whole. This we can do by asking how are the social structures related to the necessary economic changes which should follow the break-up of the Soviet Union and the inherent weknesses of the remaining capitalist societies.

Fortunately the initial thinking on this subject has been undertaken by William Keegan in his excellent book The Spectre of Capitalism in which he starts out by writing "while Communism has failed Capitalism has not necessarily succeeded", and continues.[3] "The urgent problem facing the world now is how to manage Capitalism efficiently, humanely and safely". Maynard Keynes commented that capitalism in itself is in many ways "extremely objectionable." He was referring of course to its deliberate lack of care for those displaced into poverty at the base of an hierarchical system which allows the rich to get richer and the poor poorer. This is where we can contribute the insight that hierarchical systems have biological roots which can also give rise to contrasting supportive network type social structures on understanding of which is necessary to complete a fully developed understanding of possible future social developments. This has already been grasped and used to reconstruct industries by a few advanced Industrial managers, notably Hurst, Rush, White and Wedgwood-Oppenheim.

Few of us can be expected to argue that we are not living in a period of rapid and crucial change; crucial partly because looming larger each year is the ecological crisis of over exploitation and ruthless spoliation of the environment by capitalist expansionism. Hence one of the urgen problems for us is to learn how to de-escalate people's expectations either differentially thoughout the world in the industrialized and industrializing countries or just at home. Tall order some may say, but we may have reached market saturation even now and alleviation of suffering turn out to be a good market leader. In any event, becoming aware of reality is part of ethological method, and so far has paid off handsomely: so we are really etho-psychologists. There will be those who think these concerns cannot be the province of a professional body already devoted to psychiatry. To them I would suggest that there are only a few within any profession who are radical enough and able enough to forego the belief in the exceptional character of humanity and be prepared to see ourselves as part of the biosphere in an evolving universe, albeit with a potential capability to influence the direction of evolution by virtue of the unique systems forming properties of the neocortex. Only by recruiting these people can we expect to greatly enlarge our following and hence eventually our influence.

Attention to what is only considered relevant ot one's own interest inevitably becomes more and more restricted - so what I am suggesting is that by having a new common focus, each of us will benefit in the way we come to think of our own specialty as well as helping to clarify the body politic how the necessary care and humanity should be administered in the community.

January 3, 1994

Fasten this addendum to my letter defining a new focus for our work. In it, I mentioned the need to understand how to bring about de-escalation of people's expectations.

The day after the letter was posted, Bob Hawke (the former prime minister of Australia) in an interview on British Television (BBC2) stressed the importance of reducing people's expectations especially in periods of frustration after oppressive regimes have been replaced, as in South Africa. In order to make it possible to make it possible for the economy to meet the basic needs of the black populations living in shanty towns (eg, the provision of sanitation in new housing), these will have

to built largely by the people living in these towns and the state will have to plan the provision of the necessary materials. Also the layout of the housing is crucial to the reconstruction of a community life. Would the ASCAP membership feel able to draw up a proposal along these lines?

Michael R A Chance, Birmingham, UK

Letters

January 9, 1994

Many thanks for your letter of December 13th '93 and the volumes of the Newsletter for 1993. They make interesting reading and made a nice Xmas present when they arrived.

I have no ready answer to the problem of a Name for the Society. If I have understood the discussions so far, the aim is to sell the society's interests and images further afield and attract new members. In that case, the name should 1) convey the essential thrust of the society's interests, 2) allow outsiders to identify with the aims, ie, it shouldn't be cryptic, 3) if possible it should retain its flavour to the founding and present members, and 4) it should be snappy enough to be readily remembered (a good brand name, so to speak).

If one compares IASCAP with ASCAP one sees that the former title refers to the main focus of interest of the Society, ie, psychopathology, whereas the latter includes the major mode of enquiry, ie, across species comparisons. It seems to me unusual to have almost the same abbreviation for two different titles. What is common to both is ASCAP (or even ASCOP) so why not retain this as meaning the "Association for Comparative Psychopathology"? The newsletter then becomes the ASCAP Newsletter. The term "International" seems unnecessary.

To be honest, the phrase "across species comparisons" seems clumsy in a title and, I suspect, may even scare off otherwise interested and potentially valuable future members because they cannot (at first) identify themselves with it. In contrast, "psychopathology" is what the club is about and says something to many people. Those who see the club's interests as extending beyond just psychopathology have a point, but since psychopathology can only be considered in relation to what's non-pathological, the latter would not be lost by emphasizing "psychopathology." The special status that the club attaches to "across species comparisons" will surely become apparent from the Newsletter itself and the society's activities, and so doesn't need to be specified in the main title of the Society or its Newsletter. I feel that simply calling the club and the Newsletter ASCAP or ASCOP conveys the main objective, it might satisfy older members who wish to retain a name which has come to have special meaning, and in any case, ASCAP "rolls nicely off the tongue". I hope you don't mind a newcomer making some proposals, but it sounds like fun to discuss them.

Thank you once again for your letter and for accepting me as an ASCAP member. I am busy going through the last issues of the Newsletter and have found much to think about. I have already made copies for some of my colleagues here in Berne and they seem to have struck a responsive chord in most. I'm looking forward to an informative and exciting time. Incidentally, we have a large ethology group at the University here so that I will pass copies over to them as well.

Keith Dixon, Sandoz Res, Berne, SWITZERLAND

Letters (Continued)

January 12, 1994

I've been very much interested in the continuing correspondence between John Birtchnell and John price. I am convinced that John Birtchnell is on to something important in terms of his vertical and horizontal axes. Our clinical research indicates that some patients have a predominantly "individualistic" or competitive orientation and are thus very sensitive to issues of status, dominance, and superiority, others who would be arrayed along the horizontal axis are much more "sociophilic" and are sensitive to issues of closeness, intimacy and acceptance. The literature does seem to show a good deal of congruence between the personality type and the type of stressor that precipitates depression. Thus, the sociophilic (who would be on the "close" end of John's horizontal axis) who become depressed have had an interpersonal loss, rejection, or abandonment. The individualistic (those on the vertical axis) who become depressed will ordinarily report a precipitating episode of defeat, failure, or loss of status.

One of the problems in all of our extrapolations is that when we try to make ethological comparisons, we are prone to try to fit the clinical syndrome - such as depression - into the procrustean bed of our theory. Thus, many of the speculations that I have observed simply do not seem to fit the kind of clinical depression that we see in our offices or in the hospital. What I would like to see is a careful analysis of the phenomenology of some individual cases and then try to determine which theory, applying Occam's razor, has the greatst explanatory value. Unless we have some kind of criteria or standard, I am afraid we are not going to hear what is going on because of the grinding of the axes!

I wonder if there have been any new developments about the meeting of the evolutionary people at the time of the APA in May. I would very much like to attend the meeting and truly regret that since we will be in the process of moving to a new location in the suburbs, we won't be in the position to be the host.

Aaron Tim Beck, Center for Cognitive Therapy, U Penn, Philadelphia, USA

Letters (Continued)

January 14, 1994

Thank you for the kind and gracious words in the ASCAP Newsletter and the very extensive piece on my article (ASCAP Nov 1993). I am pleased that you enjoyed it and I do hope that it stimulates further discussions for your Association. The brief that I was given by the editors was to write an article that would shake some of the neurobiology comunity out of its lethargy. This article was read by Gerald Edelman, a Nobel Laureate, and he was so enamoured of it that he invited me to be a Visiting Fellow in his Neurosciences Institute in La Jolla. I have only now got back from California after a four month visit. Be all that as it may, I would be very happy to discuss "basic plans" with you. I would need to do this before I actually wrote anything for your readership, since I am unclear yet at what level your "basic plan" concept is being implemented most usefully....

I send you one other article that I have written on evolution and palaeontology which I wrote on the retirement of a colleague. I am unsure how you will react, but the neo-Darwinists have gone almost apoplectic. I make no apologies for what I have written (as unpopular as it may seem)....

I would be most grateful if you could send me the original publications and definition of "basic plan" so that I can evaluate that rather carefully....

George L Gabor Miklos, Integrative Biology Laboratory, The Australian National University, Canberra,AUSTRALIA

Miklos GLG: Emergence of organizational complexities during metazoan evolution: perspectives from molecular biology, paleaeontology and neo-Darwinism. Mem Ass Australas Paleontols 1993; 15:7-41.

Summary: The popular theory of evolution is the modern synthesis (neo-Darwinism), based on changes in populations underpinned by the mathematics of allelic variation and driven by natural selection. It accounts more for adaptive changes in the colouration of moths, than in explaining why there are moths at all. This theory does not predict why there were only 50 or so modal body plans, nor does it provide a basis for rapid, large scale innovations. It lacks significant connection with embryogenesis and hence there is no nexus to the form of evolution of form. It fails to address the question of why the anatomical gaps between phyla are no wider today than they were at their Cambrian appearance. It has no predictions about macromolecular and cellular evolution in the Archaean, about evolution via symbiogenesis, nor the manner in which cells and organisms alter and revise their genomic rules as they evolve. These gaps are being addressed by molecular biologists who are making spectacular inroads into molecular embryology, in terms of understanding the mechanisms underlying place-dependent molecular interactions at cell and substrate surfaces.

We need to compare genomes with their developmental end-points from different phyla to ascertain what is functionally common and what is different. At the primary level this reduces to solving macromolecular interactions involving multisubunit protein complexes. However, what needs to be uncovered more generally are the principles of organisation; namely, how lots of molecules and cells are put together in the same place.

I believe that the future lies in a synthesis of Molecular Embryology, Genetic Engineering and Palaeontology. The origin of body plans is the essence of metazoan evolution, not *the origin of the species. I present data and arguments as to why we need to start afresh, free of the literary pastiche of evolutionary 'just so' stories, purportedly buttressed by a backbone of mathematical solidity. Fortunately, evolutionary biology is becoming dominated by macromolecular and cellular interacitons, embryos and fossil morphospace, not by mathematical debates.*

I believe that the search for the Holy Grail (evolution of complex morphologies and nervous systems) has been conducted in the wrong place and at the wrong levels by evolutionary biologists. The important answers do not lie predominantly in descriptions of allelic variability in natural populations, nor in the processes of speciation. They are to be found in: 1, mechanochemical interactions of embryos, 2, molecular and cellular comparisons of modal body plans between phyla, and 3, rate of explosion of morphospace and neuronal space in the Cambrian.

There is much in the body of Dr Miklos's article; only his conclusions are additionally included here.

Conclusions: I have drawn attention to at least five levels at which evolutionary insight has been sought, and the degree of success or otherwise that has been achieved at each level.

The first level involved description of genomic hardware (genes and their control sequences) in different life forms. Here the facts from genomic dictionaries are pouring out in a torrent of ordered nucleotides.

The second level involves description of transient molecular interactions and molecular machines in diverse eukaryotes and a comparison with eubacterial and archaebacterial genomes. When completed, this will provide the major tapestry of life at the cellular level.

The third level is that of time and place dependent cellular interactions within organisms, with the most difficult challenge being that of nervous system constructions.

The fourth level of comparison between phyla to understand stairways of complexity that allowed different body plans from a common ancestor. Less urgently we need to determine the extent to which the modal body plan of a phylum can be modified in different lineages within that phylum.

The ultimate synthesis is that of functional genome capacity in its fullest sense, namely what are all the possible life forms that can be constructed given all components of the genetic hardware in a given genome. Inherent in this is an understanding of all possible modal nervous systems that were, and can be built given a particular genomic tool box and the underlying morphological constraints.

Finally, it is necessary to acknowledge that after over a century of the dominant paradigm, the evolution of major complexities in the history of life has had very little to do with the origin of species. The seamless moving footway of neo-Darwinism that was to have smoothly transported us from allelic variation in natural populations to understanding body plans in different phyla has led to a culde-sac. The origin of phyla is not speciation 'writ large'. To understand what fuelled origins of phyla, the complexities that emerged long ago from macromolecular and supracellular complexes and from symbiogenic events will need to be understood via molecular embryology, where the quintessence of evolutionary truth is to be found.

Response to psychological kinship theory of Bailey and Wood[5]

by Mark Erickson

Freud based his psychology on the assumption that our evolutionary ancestors commonly directed incestuous and intensely aggressive behavior toward immediate kin.[6] This view created the theoretical dilemma of how to explain the containment of incest and 'murderous' aggression within the same human family. To circumvent this problem Freud argued that childhood repression, via Oedipal dynamics, provided a proximate mechanism for the control of familial sexuality.

We now know the naturalistic assumptions which Freud made were incorrect. Incest is rare in nature and anthropological data demonstrate that humans innately avoid incest - given appropriate rearing conditions.[7] Although intensely aggressive and even murderous behavior occurs between unrelated conspecifics in many primate species, such behavior is virtually nonexistent between parents and offspring.[8] Further, we now know that

altruistic behavior, which Freud believed was also a manifestation of a form of repression or sublimation, is common in nature, is adaptive, and occurs primarily between close kin.

Although understandably not comprehended by Freud's psychology (inclusive fitness theory was decades off) biological kinship has clearly been a key parameter in the evolution of social behavior in nature and in our own species. It therefore seems likely that an innate psychological experience of kinship exists which manifests as adaptive kin-directed behaviors.[9]

Bailey and Wood in the Nov ASCAP have begun to explore the psychology of kinship and how "psychological kinship" may play a key role human experience and in psychotherapeutic relationships. They write, "at base, kinship is the product of a process of classification that typically occurs unconsciously .... through brain mechanisms particular to a given species." They then propose that humans possess brain mechanisms which, "can be modified and elaborated through learning and cultural influences."

If I understand this correctly, the focus of Bailey and Wood's research is the plasticity or malleability of evolved psychological kinship mechanisms in our own species. They ask to what extent can the psychological experience and behaviors associated with kinship be extended beyond the circle of biological kinship and what are the potential benefits? In a recent article, for example, they discuss the nature of psychological kinship within the therapeutic relationship. They raise the interesting question of whether the extension of the psychological qualities of kinship is a key factor in effective therapy and the helping professions in general.[10]

Their work, in certain respects, resembles that of the object relations theorists but is more directly related to biology. This makes it more amenable to a biologically integrated, or top-down and bottom-up, analysis. For example, ethologically significant, nonverbal cues from a therapist may influence a sense of psychological kinship. In a pilot study which I carried out, subjects examined a number of photos of people smiling. They were to choose the smiles that would be most appropriately directed towards a family member. Across subjects there was a regular agreement as to the quality of a smile and whether it was appropriate within a familial context. Perhaps there exists an extensive nonverbal 'language' of kinship which might also paly a role in therapeutic relationships. At a very different level of analysis it is possible oxytocin, a hormone which influences kin-directed bonding, has a molecular role in establishing a sense of psychological kinship and a beneficial psychotherapeutic relationship.[11]

Is Slavin and Kriegman's "Evolutionary Biological Resuscitation" of Psychoanalysis the Right Medicine?

by Mauricio Cortina

Daniel Kriegman contends that psychoanalysis is in dire need of an infusion of evolutionary thinking to keep the proverbial (psychoanalytic) baby from being thrown out with the dirty water (ASCAP Oct 1993). In their recent book The Adaptive Design of the Human Psyche, Slavin and Kriegman offer an evolutionary perspective that promises to transcend the "great debate in psychoanalysis" as to whether humans are primarily self serving, pleasure seeking creatures or whether humans are primarily relational creatures (Kriegman's emphasis).[12] Their book is one of the most ambitious efforts to ground psychoanalysis in evolutionary principles since Bowlby's seminal work.

Slavin and Kriegman argue that Bowlby, Lichtenberg, Kohut, Mitchell and other "relational" theorists ignore or underplay issues of aggression, conflict and deceit, which they believe are intrinsic human features. They propose to rescue an understanding of this darker side of human nature from Freud's outdated mechanistic and evolutionary metapsychology, offering us a new evolutionary synthesis. They see their work as an attempt to restore the notion of conflict as an intrinsic part of human nature.

It may be that as a group, relational theorists leave out something important in their views of human nature. However, there are significant differences between Fromm, Kohut, Fairbairn and Bowlby that make this generalization about conflict difficult to sustain without creating a straw man argument. For instance, Fromm did believe that human nature is inherently conflictual and he based this belief on an explicit evolutionary argument.[13] One may disagree with his line of reasoning, but he did not ignore conflict. More importantly, I do not think that Slavin and Kriegman have succeeded in their goal of providing an alternative theory of conflict rooted in evolutionary thinking. Due to limitations of space I will content myself with a few points.

The main limitation of the Slavin and Kriegman approach is that they have based their work on a particular evolutionary point of view made popular by Dawkins' book The Selfish Gene and the theory of genic selection.[14] According to Dawkins, organisms are merely "survival machines - robot vehicles blindly programed to preserve the selfish molecules known as genes."[15]

For the unsuspecting, it would seem that the "gene's eye view" that Slavin and Kriegman are introducing to psychoanalysis has found unanimous acceptance among evolutionists. This is not true. I find an alternative view articulated by Steven J Gould that conceives natural selection operating at multiple levels of dynamically regulated, genealogically derived hierarchies (genes, organisms, populations and species) more interesting and compelling (more of that later).[16] Slavin and Kriegman insist that their approach is not reductionistic. I think the "gene's eye view" they adopt is a radical form of evolutionary reductionism.

Slavin and Kriegman borrow extensively from Dawkins, Hamilton, Trivers and Williams and these authors' concepts of kin altruism, reciprocal altruism, inclusive fitness, and parent-offspring conflict. Before discussing specific uses Slavin and Kriegman make of this body of work it is important to understand the general theory on which it is based. The basic concept is that selective pressures operate only at a genetic level (genic selectionism). Here I rely heavily on Gould, Lewontin and Sober for their critical analysis of genic selectionism.[17]

The Fallacy of Misplaced Agency

In the classical view, organisms are the agents doing the "struggling" for reproductive success. In Williams and Dawkins' "gene's eye view", genes are the main protagonist in the struggle to leave more copies of themselves. The organism has become a "vehicle" doing the bidding for the genes. The "unit of selection" in genic selectionism are individual genes.

The premises for gene selectionism are captured by the following "genic syllogism."[18]

Genes cause phenotypes.
Phenotypes cause reproductive success.
Causality is transitive.

Therefore, genes cause reproductive success.

The problem with genic selectionism is not the claim that selection can occur at a gene level; it can and does, as the phenomena of "selfish DNA" demonstrates. The controversy is about the more radical claim that natural selection occurs only at the gene level. At the heart of the matter is a confusion about causality. An analogy used by Sober and Lewontin may illustrate this confusion.[19] In the phenomena of linkage two genes are tied together to the same chromosome. Although selective pressures may be favorable for only one of the linked genes, both genes will be selected and will increase in frequency in descendent offspring even though one of the linked genes may have a neutral effect. There is no selection for the gene with neutral effects, but only for the gene with a positive selective value. In the case of pleiotropy two phenotypic traits are caused by the same underlying gene complex. Selection leads to the proliferation of both, but one phenotypic trait is neutral and gets a free ride.

As Sober points out, there is a critical distinction to be made between selection of and selection for.[20] In this analogy there is a selection of the free riders but only a selection for the gene and phenotype with positive fitness values. The confusion over genie selectionism is based on a similar fallacy. It is the case that genes record the history of selective forces operating over time. This has been taken to mean that genes cause the selection process as Gould points out:

All evolution by selection, whatever its level of causation is recorded by change in frequencies of genes (the lowest level of the causal hierarchy). Since genesrecord all changes, some evolutionists have been fooled into assuming that genes therefore cause all changes. But scribes are not agents, and bookkeeping is not causality.[21]

A general property of hierarchical systems has been widely misinterpreted, according to Gould.If you disturb a hierarchy at any level, the units above the disturbance will remain unaffected while the units below will be reshuffled to accommodate the change. This makes the lowest level -- in this case genes ~ an attractive place for bookkeeping (this is a case of selection of). I would suggest calling this confusion over the causal role played by genes in the process of natural selection, the fallacy of misplaced agency (paraphrasing Whitehead's fallacy of misplaced concreteness).

Genes as Faithful Replicators

Another argument advanced by genic selectionists to support their claim that genes are the only legitimate unit of selection, is that genes are faithful "replicators" (from David Hull's distinction between replicators and interactors). Genes are faithful replicators capable of making exact copies of themselves without diluting the genome as organisms do (in sexual reproduction offspring contain only half the genes of each parent). Hence the argument that genes are better replicators than organisms. This argument is also debatable. Species can also be seen as faithful replicators. Species become extinct and also reproduce themselves by the process of speciation. Extinction and reproductive success are usual criteria suggesting a natural selective process. Species that are spun off from a common trunk resemble their parental populations far more than any other. In effect, speciation resembles asexual reproduction, so replication "returns at a higher level of the hierarchy ~ species and populations".[22] Any "units of selection" argument using "faithful replicators" criteria would have to concede speciation fits the bill.

Emergent Properties and Natural Selection

The phenomena of emergence offers another vision of life's hierarchies and is a strong argument for considering genic selectionism's broad claims reductive. The study of emergent properties has become one of the key concepts unifying efforts to develop scientific models capable of grappling with complexity.[23] If, as Gould believes, selection acts on properties emerging at different levels of life's hierarchies such as genes, phenotypes, populations or species, genes can not be the exclusive unit of selection. Waddington provides an example of an emergent property with his concept of epigenesis - that is, an association which arises from genetic interaction during development. According to Waddington transmission of genetic material has two "groupings". Genes are held together as neighbors by chromosomes in a more or less contingent fashion. A second grouping emerges epigenetically, with selective pressures favoring phenotypes that are most adaptive to any given environmental challenge. The epigenetic response can become "fixed" through the process of genetic assimilation. This is very different from genetic determinism, as genes and environment interact in nonadditive ways, selecting phenotypes with emergent properties that obey a Darwinian selectionprocess.[24]

Altruism, Selfishness and Deceit from a "Gene's Eye View'

While the general theory that underlies Slavin and Kriegman's work is seriously flawed, this does not mean their main thesis is wrong, namely that human nature is inherently conflictual and that the nature of the conflict involves two opposing motivations: (1) pro-social, "mutualistic" motives, and (2) "self-interested, self-promoting" motives.

There is no need to prove that humans are by nature social animals. Bowlby's work, has made the point eloquently albeit on a different basis than Kriegman and Slavin. That humans are also capable of wanton destruction, deceit and selfishness is a matter of historical record. The disagreement is on how to understand this conflict. Are selfishness and deception built into our evolutionary "design" as Slavin and Kriegman contend? Can parental caretaking and sacrifice for others be explained by the theory of kin altruism and reciprocal altruism?

Genic selectionism predicts that animals will be altruistic to the extent that their "selfless" behavior will benefit their descendant heritage. This view involves a redefinition of fitness to include kin (inclusive fitness), rather than a "skin-bound" view of fitness limited to the interests of a single organism (the classical view). From this point of view, parental care may reduce a parent's personal fitness but increase parental inclusive fitness.

Stripped of jargon, kin altruism basically amounts to a carrot and stick theory of motivation played out at a genetic level. The carrot is the gene's success in leaving more copies of itself in descendent offspring ("inclusive fitness"). The stick penalizes genes with extinction if they do not play this game well. As Slavin and Kriegman point out, this "gene's eye view" cuts both ways. There are benefits and costs for developing "altruistic" strategies.

Trivers' parent-offspring conflict theory is an example of this gene focused cost-benefit analysis. The basic premise is that the closer the kinship ties, the more likely "altruistic" behavior will predominate among kin since the degree of altruism is subordinated to costs and benefits imposed by selective pressures on a gene level. For instance, since parents share 50% of their genes with their children, there is a large overlap between the interests of parents and their offspring. Offspring also share 50% of genes with full siblings, but there is much less of a genetic incentive to act

altruistically. From a "gene's eye" perspective, a sibling is mostly a competitor. The cost-benefit ratio has to involve a greater pay back for the altruistic sibling to be worth the cost. Here is an example of Trivers' analysis quoted by Slavin and Kriegman.

Parents and offspring are expected to disagree over the behavioral tendencies of the offspring insofar as these tendencies effect related individuals...(For example, in interaction among siblings)...an individual is only expected to perform an altruistic act toward the full sibling whenever the benefit to the sibling is greater than twice the cost to the altruist.[25]

The "altruistic" sibling will receive a net benefit to his "inclusive fitness" by forgoing his tendency to act selfishly when there is a large benefit for the sibling who is the recipient and a minimal cost to the "altruist". A similar argument is made for parental investment in their children. Although generally there will be a significant overlap of interests, there is a limit to parental altruism. An example is the "weaning conflict." As offspring become more independent, the benefit of nursing for the offspring is low while the cost for the mother is high. Hence mothers will wean their offspring despite vigorous protest.

Since altruists can become prey of their selfish kin, Slavin and Kriegman believe, following Trivers, that the more selfish individuals must hide their selfishness, just as the altruists, in any given population need to become more sophisticated in detecting selfishness. As the more sophisticated individuals become selected, deception becomes part of a "deep structure of the psyche." Using the same argument, wouldn't individuals who become good at "catching" their selfish peers or kin also get selected? Good detection of character and truth telling should also become part of our deep psychological structures!

If our primordial motivational incentive is ultimately a cost-benefit calculus, we are indeed, as Kriegman and Slavin state, "semi-social", for as "members of a sexually reproducing species, nobody loves us as we love ourselves."[26] Let's agree with Slavin and Kriegman that this cost-benefit "gene's eye view" of motivation is generally correct. Then we would surely see manifestations of this evolutionary bias (distal causation) at a proximate level of causation and would predict that cost-benefit factors would be a dominant motivational theme throughout ontogenetic development.

I will give two examples from developmental psychology and one from organizational psychology to show that data from these different disciplines calls into question this cost-benefit model. Piaget's classical study of moral development suggests that a self-centered view of the world that only considers personal interest's ("what's in it for me?") is partly the result of children's cognitive egocentrism. Under normal conditions this cognitive limitation gives way to a growing capacity for reciprocity and egalitarian moral judgments as children are able to take the perspective of others in evaluating moral actions. From another tradition, attachment research shows that infants whose primary caretakers are sensitive to the infant's signals and affects, develop mental representations of reciprocal and need satisfying relationships. Conversely, infants whose primary caretakers are insensitive, intrusive, overstimulating or rejecting, develop mental representations of relationships where there are minimal expectations that legitimate needs will be met. Then defensive maneuvers develop to protect against rejection, intrusiveness or overstimulation which compromise children's early development and limit the capacity for reciprocity with peers and cooperation with adults.[27] As a toddler's passionate drive toward autonomy collides with parents' needs to set limits and inculcate social mores, conflict inevitably ensues. Not surprisingly, toddlers with a history of secure attachments, are best able to negotiate limits and begin to show a capacity to develop "goal-corrected partnerships" to use Bowlby's terminology, without compromising their autonomy. Insecure toddlers remain much more self-centered in their development. Autonomy is compromised as insecure toddlers become passive, excessively defiant or counterphobically ignore danger.[28]

In the arena of organizational psychology, extensive research and experience shows that cost-benefit and carrot-stick theories of motivation are only effective under limited conditions and have limited effects. In fact, programs based on these premises are counterproductive when used as a motivator in the workplace and in the classroom because they undermine an intrinsic desire to explore and learn. Programs based on rewards also erode cooperation and trust.[29]

This body of knowledge does not fit with Trivers' model. If a cost-benefit calculus were a dominant motivation, programs based on rewards should be fantastically, successful. They clearly are not. Trivers' model does not allow us to take into account other motivations such as the innate

"epistemic hunger" and a need to make sense of the world. Trivers also ignores maturational and qualitative factors in development. A metapsychology built on Trivers' model, can at best account for a limited sector of functioning among humans. At its worst it distorts and oversimplifies the complexity of human motivation.

An Alternative Theory of the Phyletic Origin of Kin Altruism

Scientific disputes over theory are seldom settled by facts alone. Theories gain ascendence in proportion to their perceived heuristic value. With this thought in mind, I will piggy-back on Kriegman's observation in his ASCAP article that humans are the most neotenic of all species and this remarkable characteristic is a central feature of hominid evolution. I agree, and would add that neoteny and the theory of r and K selection, taken together, offer an alternative theory to account for the emergence of extensive parental caregiving to offspring and the development of affectional attachments among kin ("kin altruism").

As is well known, the theory of r and K selection grew out of an effort to include population dynamics and life history strategies - such as timing of reproduction, fecundity and longevity -- as important adaptations in their own right, rather than just being consequences of the more classical focus on morphology and function.[30] Briefly, r strategies will predominate when populations can expand without developing negative feedback on their growth rates as a consequence of diminishing environmental resources.K selection will predominate when there js negative feedback on growth rates in species or populations competing for the same environmental resources. K selection will be favored when environments have reached their "carrying capacity" but are relatively stable and benign. "r" selected organisms take advantage of environmental niches that are virgin and abundant but unstable. Here is a list of some of the characteristics of r and k selected organisms.

r selection	K selection
rapid development	slow maturation
short life spans	long life spans
parental care limited	extensive
high fecundity rates	small broods

In r selected organisms large amounts of the organism's resources are dedicated to reproductive efforts with few resources going to the nurturing of offspring. K selected organism dedicate large amounts of resources to nurturing offspring and relatively few resources are dedicated to reproductive efforts. In sum, r selection favors rapid maturation and a large quantity of offspring. K selection favors slow maturation and quality of offspring.

Clearly, the rate of sexual and somatic maturation becomes an important linchpin in controlling life history strategies. Although the importance of these adaptations had been relatively neglected, in Ontogeny and Phylogeny, Gould links the classical study of the relationship between ontogeny and phylogeny, infused with misleading but suggestive morphological comparisons, to the field of population dynamics and life history strategies.[31] Gould believes that the parallels observed between phylogeny and ontogeny are best explained by regulatory mechanisms controlling the timing of development (heterochrony). Acceleration and retardation of development are key processes that occasionally can have significant macroevolutionary implications. In simplified terms, acceleration of development is associated with r strategies, precocious development and early sexual maturity (progenesis). Acceleration can also lead to "recapitulation" the basis of Haekel's famous biogenetic law. Retardation of development is associated with k strategies. In neoteny retarded development is accompanied by delays in sexual maturation and somatic differentiation with retention of juvenile morphologies.

The combination of k strategies with neoteny act in synergy, producing a series of cascading effects that are hallmarks of primate and human evolution. Retarded sexual maturation requires intensive parenting (or a high degree of parental investment, to use Trivers' terminology) to nurture offspring through a protracted juvenile stage of development. Intense parenting will increase affiliative needs throughout the life cycle, setting the stage for highly sociable species (incidentally, it is probably out of this matrix, and not just protection from predators, as Bowlby believed, that attachment bonds may have evolved).

By retaining juvenile forms, flexibility is directly selected as an adaptive strategy. Retention of juvenile forms confers a high degree of plasticity to development. Playful activity and an expanded potential for learning are some of the consequences. In neotenic species, rates of development are very retarded. Consequently, there is a prolongation, of embryonic growth rates to later stages of development. Particularly in humans, the prolongation of embryonic growth rates of the

brain is very marked, persisting into the first year of extra-uterine life. Undoubtedly, this process must have contributed to the evolution of higher cerebralization observed in hominid evolution. Prolongation of embryonic rates into infancy interact with retention of juvenile forms, enhancing the potential for learning from experience.

I hasten to add, the neotenic interpretation of human origins does not shed any light on issues such as understanding the origin of selfless behavior toward non-kin, our capacity for self-awareness and recognition (or denial) of our mortality. These developments may be, as Gould believes, side effects of having a large brain, rather then capabilities that were directly selected. But that is another story.

Social rank and brain serotonin in fish

by Russell Gardner

Introduction

In his letter this issue, Aaron Tim Beck emphasizes that we be data-oriented. I have encountered three papers that deal with social rank and brain serotonin levels in fish, completed by zoophysiologists who indicate no involvement with the involuntary subordination theory of depression.[32] Their research bears directly on it, however, so I replicate their abstracts below; envision them as the "methods and results" sections of a paper that might have tested the hypothesis expressed in a paper by John Price, Leon Sloman, Russell Gardner, and Peter Rohde accepted for publication by the Brit J Psvchiat in August, 1993. Entitled, "Social competition hypothesis of depression," it discusses a topic well known in these pages and indeed stemmed in part from earlier versions within them.

Relationship to the hypothesis hinges on two parts of their investigation: (1) the following statement is the first sentence of the first article: "When held in small groups, juvenile Artic char... develop an essentially linear dominance hierarchy, in which the dominant fish is the most aggressive and usually the largest individual." Do the less aggressive fish move less as is true of humans and chickens low in the social rank hierarchy?

(2) They measured brain serotonin. Since serotonin-reuptake inhibitors are potent antidepressants (presumably increasing brain serotonin), one would predict that the fish in an involuntarily subordinated state would move less and have lessened brain serotonin.

John Price was inspired many years ago by Thorlief Schjederuppe-Ebbe's findings on the peck order of chickens. The lowest ranker looked like a depressed person. If the resemblance was the result of action of homologous structures, then what people experience as depression is indeed a very ancient communicational phenomenon: reducing a winning opponent's savagery would happen if the losing animal took on the face of no activity and no threat. If fish in enclosed tanks exhibited phenomena parallel with those of people, this would represent an even more ancient version of involuntary subordination than chickens (the branching from common ancestors would have been even further back, in pre-tetrapod antiquity).

As summarized in the Dec ASCAP and in this issue, George L Gabor Miklos strongly emphasizes that body plans haven't changed since the Cambrian. Abstracts below in this issue by Hoy and Kavenau underline that the neural and behavioral components of body plans are extremely conservative.

First, with respect to the fish movements, we would expect less movement because people with serious depression move less. As Tyge Schelde has described so well in great detail, increased movement is one of the first signs of improvement as people recover from depression. Generalized movement precedes social movement; only belatedly is there a full conscious appreciation that the person feels better. As clinicians, we know we should not discharge the patient too soon; it is as if their large movement motor system is ahead of later derived phenomena, so that the person's increased energy may drive their suicidal ideation to a lethal extent when the impulse strikes them during this stage of illness.

Second, is the serotonin reduced as implied by the stunning success of the selective serotonin reuptake inhibitors as antidepressants (fluoxetine, sertraline, paroxetine)? The fact that more serotonin in the synaptic cleft improves depressed mood implies that in a fish model, serotonin should be lower when involuntarily subordinated.

Methods and Results

1. Winberg S, Nilsson GE: Time course of changes of brain serotonergic activity and brain tryptophan

levels of dominant and subordinate juvenile artic char. J Exp Biol 1993:179:181-195.

Abstract: Concentrations of serotonin (5-HT), 5-hydroxyindoleacetic acid (5-HIAA) and tryptophan (TRP, the amino acid precursor of 5-HT) were measured, and 5-HIAA/5-HT ratios, calculated, in the telencephalon, hypothalamus and brain stem of Artic char (Salvelinus alpinus) *with 1-21 days experience of a dominant or subordinate position in a pair. Brain 5-HIAA levels and 5-HIAA/5-HT ratios (an index of serotonergic activity) increased rapidly in all three areas of brain in subordinate fish and remained high for up to 21 days. The brain stem 5-HIAA concentration in dominant fish showed a temporary increase after 1 day of social interaction, but returned to the control level 2 days later. The social interactions did not affect 5-HT concentrations in any of the brain regions. An initial, but temporary, increase in brain TRP concentration was seen in both subordinate and dominant fish. After 1-3 days of social interaction, brain TRP levels declined. This decline was most pronounced in subordinate individuals which, after 7 and 21 days, had hypothalamic TRP concentrations significantly lower than those of controls. Moreover, TRP levels in the telencephalon after 21 days, and in the hypothalamus after 7 days, were significantly lower in subordinate individuals than in dominant fish. These results show that subordinate experience rapidly causes a sustained increase in brain 5-HT metabolism which does not correlate with changes in brain TRP levels. Thus, the increase in brain 5-HIAA concentration and in brain 5-HIAA/5-HT ratios probably reflect an increase in functional 5-HT release, a phenomenon that appears to have a wide distribution in the brain.*

2. Winberg S, Carter CG, McCarthy ID, Zhong-Yang H, Nilsson GE, Houlihan DF: Feeding rank and brain serotonergic activity in rainbow trout *Oncorhynchus mykiss.* J Exp Biol 1993; 179:197-211.

Abstract: Two methods for assessing the status of an individual rainbow trout Oncorhynchus mykiss *within a group hierarchy, radiographic determination of individual food intake and analysis of brain serotonergic activity, were compared. The results showed that individual food intake, measured as the average share of the group meal, and brain sertonergic activity, measured as brain levels of 5-hydroxyindolacetic acid (5-HIAA) or as 5-HIAA/5-HT (serotonin) ratios, were inversely correlated with each other, suggesting that both methods could be used as indicators of the position of the rainbow trout in a dominance hierarchy. In addition, specific growth rate correlated significantly with brain 5-HIAA/5-HT ratios.*

The results indicate that the increase in brain 5-HIAA/5-HT ratios in subordinate individuals is caused by an increased use (release) of 5-HT in these fish, and not by an increase in the level of tryptophan, and amino acid precursor of 5-HT. The relationships between social rank, food intake, growth, stress and brain serotonergic activity of fish in a social hierarchy are discussed.

3. Winberg S, Nilsson GE, Spruijt BM, Hoglund U: Spontaneous locomotor activity in Artic char measured by a computerized imaging technique: role of brain serotonergic activity. J Exp Biol 1993;179:213-232.

Abstract: Using a computerized video-image analysis system, spontaneous locomotor activity was measured in dominant and subordinate individuals of Artic charr (Salvelinus alpinus) *and in individuals treated with inhibitors of serotonin* (5-*HT) synthesis and reuptake. Artic char were put together in pairs. After 1 week, subordinate individuals were found to have elevated brain levels of 5-hydroxyindoleacetic acid, a major 5-HT metabolite, suggesting an increase in sertonergic activity. The subordinate individuals had significantly lower spontaneous locomotor activity than the dominant fish. Similarly, Artic char displayed a significantly reduced locomotor activity when their serotonergic activity was stimulated by the specific 5-HT inhibitor zimeldine. In contrast, fish treated with 5-HT synthesis inhibitor p-chlorophenylalanine showed a significant increase in locomotor activity. Dominant, subordinate and pharmacologically treated fish all had very similar activity rhythms for the 18 h test period. Thus, neither the previous social experience nor the pharmacological treatment seemed to affect the diurnal activity rhythm* per se. *Taken together, these results suggest that the brain serotonergic system inhibits locomotor activity and support the possibility that 5-HT is involved with the inhibition of locomotor activity displayed by subordinate fish.*

Discussion

The data are contrary to those expected from an involuntary subordination view of depression. The "involuntarily subordinate" fish moved less in the artificial containment of the tank, just as patients move less in clinically depressed states and chickens do when lowest in the peck order.

In contrast, serotonin levels and turnover were increased. Moreover, when treated by the antidepressant, zimelidine, the motor behavior decreased still more. When an inhibitor of serotonin synthesis was used, activity of subordinate fish increased.

These are interesting data. Of course, those who argue that we *cannot* freely extend our observations to animals may have some support from this. After all zimelidine did *not* increase movement in the fish whereas decreasing serotonin did, contrary to what would be expected in people. We look forward to more from the zoophysiologists on how the human and fish compare and contrast.

T Bullock on comparative studies: *The brain has diversified and advanced in evolution more than any other organ; the variety of nervous systems and behaviors among animal species is thus available for our exploitation. Comparative neuroscience is likely to reach insights so novel aas to constitute revolutions in understanding the structure, functions, ontogeny, and evolution of nervous systems. This promise requires pursuit on a wide front, in respect to disciplines and in respect to the species, stages, and states compared. It also requires deliberate concentration on the differences among animals, in addition to the prevailing concern for the basic and common.... Without due consideration of the neural and behavioral correlates of differences between higher taxa and between closely related families, species, sexes, and stages, we cannot expect to understand our nervous systems or ourselves.*[33]

Hoy RR: Evolutionary innovation in behavior and speciation: opportunities for behavioral neuroethology. Brain Behav Evol 1990;36:141-153

Abstract: *Populations of animals differentiate by speciation. How speciation takes place, for long a puzzle for evolutionary biologists, should be regarded as a research opportunity for neuroethologists and comparative behaviorists. It is now clear that behavior may play an important part in the process of speciation. The existence of sexually dimorphic anatomy and behavior in many animals has provided a rich subject for investigation by comparative neurobiologists and behaviorists, who emphasize analysis of proximate mechanisms that generate dimorphism in phenotype. However, sexual dimorphisms also figure prominently in the theory of sexual selection. Sexual selection is viewed as a primary behavioral mechanism in the process of speciation. Some examples of 'explosive' speciation are presented, and the putative role that sexual selection plays in these cases is reviewed. A consideration of any evolutionary selective process must include genetics, and I will briefly summarize suties that indicate that even complex behavioral systems such as sexual selection, which sociobiologists refer to as a 'strategy', can have a relatively simple genetic basis.*

From the Introduction: *The importance of relating phytogeny to behavior dates back to the very roots of ethology. The basic idea is that species-specific, stereotyped behavioral traits can be homologized, just as morphological ones can. This was recognized by Charles Otis Whiteman [1897], of the Marine Biological Lab in Woods Hole, at the end of the 19th century, who wrote the marching orders for the founding of ethology: 'Instincts and organs are to be studied from the common viewpoint of phyletic descent'.*

Kavanau JL: Conservative behavioural evolution, the neural substrate. Anim Behav 1990;39:758-767

Abstract: The evolution of neural circuitry differs from that of other structures in a significant respect. Natural selection cannot physically eliminate most neuronal pathways for obsolete functions, because the individual neurons of these pathways usually are multifunctional, also being components of pathways for adaptive functions. Selection for loss of obsolete neural functions probably operates largely through mechanisms that suppress or neutralize the corresponding neuronal pathways without disabling pathways for adaptive function. Accordingly, rather than becoming eliminated physically, obsolete neuronal pathways tend to become inhibited or have their outputs blocked; obsolete pathways not selected against merely cease to be accessed. These mechanisms of loss of neural function provide one of several intrinsically conservative bases for neural ontogeny and evolution. Since the central nervous system encodes and programmes motor outputs for behaviour, the obsolete systems that are retained include pathways for obsolete behaviour. In vertebrates, such behaviour can sometimes be elicited directly by appropriate stimuli, but when the neuronal pathways for the obsolete behaviour are in an inhibited or blocked condition, they must first be rendered accessible (which can

occur adventitiously, pathologically, experimentally or in hybrids). The vertebrate central nervous system also plays a large role in programming the ontogeny and maturation of gametes. Egg formation in many species appears to be highly conservative, extensively retracing ancestral stages. The conservativeness probably derives from influences both of being under neuronal control and of having an evolutionary history in which the provisioning of eggs was continually on the increase.

From the body of the article: *Today, [Haeckel's biogenetic] law stands in disrepute. Few biologists, however, would dispute the less inclusive characterization, 'ontogeny retraces some developmental stages of many ancestral features.*

Miller BL, Chang L, Mena I, Boone K, Lesser IM: Progressive right frontotemporal degeneration: clinical, neuropsychological and SPECT characteristics. Dementia 1993;4:204-213.

Abstract: The behavioral, neuropsychological and single photon emission computerized tomography characteristics of 5 patients with progressive degeneration of the right hemisphere are described. In all, the brain regions with greatest involvement were right-frontal and temporal. Psychosis, compulsions and behavioral disinhibition were the dominant, and often first, symptoms. Affect was flattened and the patients seemed distant and remote. Neuropsychological testing did not reveal a consistent pattern that helped localize the abnormality to the right frontotemporal region. These patients contrast dramatically to those with left frontotemporal degeneration in whom behavior and psychiatric status is often normal. This study suggests that the right hemisphere may be primary for the control of social conduct.

Ortiz de Montellano BR: Melanin, Afrocentricity, and pseudoscience. Yearbook of Physical Anthropology 1993;36:33-58

Abstract: A component of the Afrocentric movement has incorporated a theory that black people, including ancient Egyptians, have superior mental, physical and paranormal powers because they have more melanin both in their skin and in their brains. By extension it is also claimed that black people have more melatonin and beta-melanocyte stimulating hormone in their systems and that these compounds also contribute to the superiority of people of color over whites. In this paper, these claims are detailed and refuted. A review of the genetics and biochemistry of human pigmentation shows that all humans have similar amounts of neuromelanin (brain melanin), and that its concentration is absolutely independent of skin color; that adult humans do not synthetize beta-melanocyte stimulating hormone; and that human melatonin has no clearly demonstrable physiological function and no relationship to skin color. "Melanists" also distort human evolution by claiming that European whites are descendents of negroid albinos. The main problems posed by this ideological movement are that it will increase the already rampant scientific illiteracy in this country, it will contribute to further widening the gap between the races, and most imporantly, it is being introduced into the public school curriculum under the guise of multicultural education.

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Is Siberia as a site of human origins? *Archeologist Y Mochanov of the Russian Academy of Sciences has found stone tools that could be as much as 3.4 million years old - older than the artifacts used to establish eastern Africa as our ancestral stomping ground - in... Siberia.*

Siberia wasn't a Garden of Eden back then - climate experts say it was probably at least as cold then as now. Nevertheless, Mochanov has unearthed 4000 stone artifacts, most choppers and scrapers, in deposits dating from 1.8 to 3.4 million years old. The "Diring" site is 2500 miles west of Anchorage... in the Siberian province Yakutsk.

Mochanov's discovery - which implies that an unknown group of hominids arose in Siberia - is likely to cause quite a stir next year, says Robson Bonnichsen, director of Oregon State University's Center for the Study of the First Americans. Scientists are already lining up to examine the find. Still, even if the artifacts are only 100,000 years old, says Bonnichsen, the find "would not be accommodated by current theories of human evolution."

Mochanov...next month...presents his findings in lectures across the United States. He's bringing his artifacts for scrutiny.

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