ASCAP NEWSLETTER

Across-Species Comparisons And Psychopathology Newsletter
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"[T]here are few equivalences in the terms used to describe the cognitive capacities of animals and humans; and those that are used often fail to do justice to the complexities of human cognition."

Merlin Donald

The ASCAP Newsletter[2] is a function of the

International Association for the Study of Comparative Psychopathology (IASCAP)[3]

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.

IASCAP 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 that focusing on cellular processes to that focusing on individuals to that of individuals in groups.

Correspondence with **IASCAP** is c/o R Gardner, secretary, and editor of The ASCAP Newsletter 4.450 Graves Building (D28), UTMB, Galveston, TX 77555-0428, U.S.A. Phone: (409) 772-7029 FAX: (409) 772-6771

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Comment: The following abstract comes from Henry Nasrallah at Ohio State University. It mentions Christopher Frith whose new book on the neuro-psychology of schizophrenia is reviewed in Nature, where Frith noted a bizarre paradox in psychiatry: it is "widely believed that there is an organic basis to schizophrenia", but "a patient can only be diagnosed schizophrenic as long as the organic cause of the illness is unknown."

Abstract: Deakin JFW, Simpson MDC, Royston MC, Slater P, McKenna PJ, Brophy J: Social signaling and schizophrenia: the significance of neurochemical and morphological abnormalities of the basolateral circuit. Schizophrenia Research 1991;4:357.

... tR]adioligand binding to pre and post synaptic glutamatergic elements (D-[H]aspartate to uptake sites and H-TCP to receptors) was bilaterally in creased in orbital frontal cortex from the brains of schizophrenics compared with controls. Results from three independent studies report regionally specific neurochemical abnormalities in orbital frontal cortex in schizophrenia (increased kainate binding & glutamate content, & reduced G-protein). We suggested that schizophrenic orbital frontal cor tex receives an abnormal, exuberant, possible imma ture glutamate innervation from the contralateral hemisphere through the anterior commissure or cal-losum resulting in an increased number of glutamatergic synapses. In polar temporal cortex and amygdala presynaptic markers for glutamate and GABA...neurons showed showed evidence of bilateral losses...more marked on the left. In a new series of brains (13 schizophrenics vs 18 controls) we find morphometric evidence of bilateral frontal and predominantly left-sided temporal lobe atrophy___

Orbital, cingulate and polar temporal cortex and amygdala are part of the basolateral circuit and are connected by a major forebrain tract, the un-cinate fasciculus which merges with the contralateral tract through the anterior commissure. There is evidence that polar temporal cortex processes high order social cues such as facial expression and gestures. Orbital and cingulate frontal cortex may use this information (via the un-cinate fasciculus) to infer what is in other people's minds (Frith's second order processing). The evolution of right handedness may have resulted in left cerebral dominance for transmitting social signals through gesture and facial expression. These are detected in the left visual field and are thus processed by the right hemisphere. This information has to cross to the left hemisphere to initiate replying gestures and expressions. It is suggested that disturbances in connectivity within and between basolateral circuits mediates dyspraxia and dysgnosia for social communication which may underlie schizophrenic symptomatology, eg, paranoia as dysfunctional inferences about the intentions of others. A speculative possibility is that this circuitry is brought into play in late adolescence to mediate assortative mating and that this has a genetic basis which is disturbed in schizophrenia.

Abstract: McGuire MT, Marks I, Nesse RM, Troisi A: Evolutionary biology: a basic science for psychiatry? Acta Psychiatr Scand 1992;86:89-96.

Evolutionary biology has much to offer psychiatry. It distinguishes between ultimate and proximate explanations of behavior and addresses the functional significance of behavior. Sub-theories, frequently voiced misconceptions, specific applications, testable hypotheses and limitations of evolutionary theory are reviewed. An evolutionary perspective is likely to improve understanding of psychopathology, refocus some clinical research, influence treatment and help integrate seemingly unrelated findings and theoretical explanations.

Is the double term "evolutionary biology" redundant? DX Freedman noted this about "biological psychiatry."

Chaos Theory and Psychology

by John Pearce

This is how chaos theory works. Equations are "deterministic" by their nature; that is, they can always be solved for any values you care to specify. However, for some equations calculating solutions may not be easy. An equation that is said to be "chaotic" uses as data (within the equation) the results of previous solutions of the equation. Such an equation must be solved on a computer. The computer repeatedly solves the equation, plugs in the solution, and solves the equation again. As these solutions are generated they are plotted in a phasic space—a graphic space where the values change with each rotation of 360 degrees, forming a trace comparable to the trace of the earth as it rotates 360 degrees around the sun.

Nature is not tidy and predictable. Most natural systems are chaotic. Indeed, even things we regard as predictable, like the orbits of the planets in our solar system, turn out to be chaotic. Using current data, running orbits on super-computers for billions of cycles to simulate the passage of billions of years, the path of the earth around the sun turns out in the long run to be chaotic-not regular and predictable as it is in the short run.

The chaos in "chaos theory" is not chaos as the word is usually used; it does not refer to total unpredictability. It refers to semi-predictability; semi-predictability in the long run. "Chaos" in this technical sense is not randomness.

Equations that are chaotic turn out to be, in the long run, very sensitive to initial conditions. This was originally discovered when an MIT weather scientist was attempting to simulate weather on a computer. He noticed tiny differences in initial conditions produced huge differences in the results after the computer had run thousands of calculations. This sensitivity was discovered by accident; a computer run had to be stopped and restarted. When restarting the numerical data was changed a tiny bit something like rounding off at the seventh digit. That tiny change turned out to eventually make a big difference. (This is the source of the witty but misleading saying that the beat of a butterfly's wing in Peking can cause a hurricane in the Atlantic Ocean. It is misleading because only rarely does a butterfly have so awesome a role in weather; it just means that small differences can matter in the long run.)

The surprising discovery about chaotic equations, the reason we are interested, is that these apparently unpredictable equations turn out to have some average solutions. After thousands of rotations in the phase space, the solutions clustered around certain points on the phase graph. Those points were called "attractors" because they seemed to attract the solutions like a gravitational field attracts planets. The discovery was that the solutions to chaotic equations, while not predictable at any particular time, were somewhat predictable.

Human behavior is somewhat predictable too. Hence, a possible analogy. Attractors are analogous to typical human behavior, or, more precisely, to the action of human preferences on behavior. (An amusing word play: we are attracted to what we prefer.) It's a rough analogy, but has the virtue of combining unpredictability in detail with good average predictability. Another parallel to psychology: the experience of the system influences the behavior of the system. Small differences can have big consequences.

The chief flaw of chaos theory as an analogy for human behavior is this: 1. Chaotic systems are closed systems. Psychological systems are open systems. You can never specify all the elements that will influence people's behavior. 2. Chaotic equations are solved for many, many cycles within a phase space. Although many human experiences are repetitious, they are not that repetitious, and, often we find that crucial human experiences are singular. (The maxim is: there is no sciences of the unique. Certainly there is no predictive science of the unique.)

Although fatally flawed for psychology, chaos theory is a good match for many cyclical biological systems. For example, healthy heart beats are regularly irregular. If a heart beat stops being irregular it means that it is being driven by some single factor, which is usually organic pathology. So a heart monitor can be hooked up to a computer that charts the heart's behavior in a phasic space and locates the attrac-tors. Then when there is a change in the attractors, or the heart becomes regular, the clinician can expect trouble. This use of chaos theory could increase the sensitivity of cardiac monitoring.

Because it is a mathematical theory, chaos theory is impressive. Because it is a promising tool for dealing with many biological systems, we are even more impressed. Gracious, chaos theory is an attractor! But don't be fooled into thinking it offers much to psychology. I'm afraid it does not. Human systems are too open and important experiences too singular for chaos theory to be used in psychology.

Observations about ideas in the 2/93 ASCAP by Carolyn R Reichelt

With regard to Leon Sloman's inter-esting piece: as time goes on, I grow less and less enamored with the notion of voluntary yielding. I think there is only yielding based upon recognition of reality. As such, it can be a legitimate survival strategy, but I question whether it is ever truly voluntary and suggest that it becomes a problem for the animal when it seems to conflict with biological imperatives. Sometimes in all our debating we seem to lose sight of those ultimate biological concerns. At core, all animal behavior is motivated by adaptive self-interest and that implies an underlying selfishness (not meant pejoratively—only biologically). The subordinate macaques at the feeding sites at Cayo Santiago give way when dominant animals appear, but they appear cautious, furtive, and unwilling as they do so. When the riots in Los Angeles were analyzed, greed seemed to have as much or more to do with their continuation than some abstract concern about social justice (although the ideal of social justice in itself arises from adaptive needs). All the wolves feeding on a moose try to snatch as much meat as possible, even though the alphas are given the best spot at the bony table. Animals should be constantly motivated to do whatever is necessary to remain fit. Thus, when yielding occurs without depressive consequences, it should be prompted by real or perceived biological self-interest. If I am an animal without much neocortex, I will yield in a situation because I am genetically programmed to do so and I won't think about it because I can't. If I am a male robin, I will allow myself to be run out of another male's territory because the dominant has a redder breast than I have and can make himself appear bigger and tougher than I can. Besides I can find another territory in the yard two houses over and I presumably am incapable of dwelling on my frustration anyway. Biologically, the yielding seems to give me the best chance of reproducing rather than getting seriously pecked in a territorial battle.

But what if the yielding is forced in circumstances that appear to con-tradict my biological self-interest and I am able to recognize the con-tradiction, at least unconsciously? Suppose I am a human male and I take early retirement because it's my only realistic option as my company down-sizes (the other option the company offered was a lesser job a thousand miles away). Like the robin, I'm yielding because uprooting myself at age 53 appears a worse pecking than the early retirement. I no longer have access to my workplace territory and my family's standard of living will be significantly diminished. I CAN think about the losses, financially and in prestige. I yielded voluntarily in theory; I made the choice; I even told myself how much I'd enjoy the freedom. But the truth is that the yielding was not voluntary; I gave way, but I didn't want to. Biologically I am designed to keep on hunting and gathering and this retirement seemed to threaten that ability; it threatens my (unconscious) need to survive to reproduce in the future. It made me feel that I am worthless, a failure, helpless — and I became depressed.

Individuals may yield without actual overt force being applied, but that doesn't mean that it is voluntary. It may simply be a way of protecting themselves from something that may be worse. The contest may not proceed in external reality, but it certainly continues internally in the yielder's fantasy. Sloman says that the difference between voluntary and depressive yielding is one of intensity. I disagree; I believe the yielding is more apparent than real. The individual does what is required for self-protection, but covertly the battle goes on. Giving way DOES occur; yielding does not. Depressed patients are furiously angry—and terribly frightened; they've been forced to give way and they don't want to accept that; at some level somewhere within they feel that their survival is threatened by the real or imagined loss. The difference between the depressed yielder and the individual who doesn't become depressed in a similar circumstance probably lies in the degree of perceived threat to the individual's ultimate concerns.

I believe that at least some other animals may become depressed--the deposed alpha wolf or primate, for example, or the dog whose "beloved" human companion becomes ill or dies. Is it possible that depression occurs in those animals with sufficient cortex to imagine success or failure, to recall a vanished superiority, to feel the pain of changed circumstances, to recognize loss, to know themselves uncared for or unloved? Depressed patients ruminate and obsess over their "inferiority", imagined failings, and anything else that confirms and supports their "deserved" unhappiness. Such thinking only serves to perpetuate the depression; the cortex and limbic system reinforce one another in a terrible cycle of pain feeding negative thinking feeding pain. They "recognize" that they are helpless and unable to fight back; not only that, they begin to see themselves as globally responsible. "I just <u>knew</u> that plane Jill took was going to crash, but I let her go anyway".

If a depressed individual feels that he cannot defend himself and his genetic future adequately, than OF COURSE (in his fantasies), he will be deserving of punishment and pain. At its most basic, that is his reason for being; if he can't defend that, he cannot and SHOULD NOT survive. Beck's great gift to depressives— cognitive therapy—breaks into the cycle with reality imposed from without through education. The patient gradually gains perspective and can better judge when yielding is reasonable and not a threat to survival, a failure or disgrace. It can cool his rage at himself for his perceived weakness. It permits the patient to find other ways of obeying the promptings of his biological imperatives.

And this leads me to learned helplessness (LH) and animal models. I really like Russell's focus on LH in the 2/93 ASCAP, but I'm not a bit sure that I would describe the model as socially and conceptually impoverished. It has its uses, even though the name itself may be less than inspired. It seems to me that LH is rather a good model for what I suggest above; the animal is thwarted in his effort to protect himself (and his genetic future) from danger, find food, or whatever the experimental method may utilize. Because of some incomprehensible external power (God punishing me, says the depressed patient), the animal is not able to perform as his adaptive mechanisms tell him he must. His autonomy is taken away; he is forced into a subordinate position, dependent on the whims of an uncontrollable fate or superior being. LH is the model for the animal's paralysis when he can't deal with the sudden change in his ability to control his biological destiny. I doubt that rats feel anger in the human sense, but they certainly can be aggressive (always potentially related to anger in the human) and the experiment even blocks an outlet for aggression. IF a human were in that situation, he'd likely be pretty angry (prisoners of war, for example). He'd yield, because he must, but he might well see this with rage and—possibly—become depressed —at the time or later (posttraumatic stress). Russ is right: the rat or the POW are COMMUNICATING—as does the depressed patient, but I suggest that part of the communication is false. The giving up (or giving way) is the surface, but that is a lie; underneath is the truth; the yielder is enraged at being prevented from fulfilling his felt biological aims, but he must hide his anger to live to fight later. The passivity of LH may be a conservation of energetic resources; the anger or aggression gets turned upon the depressive himself... on his own weakness and inability to defend his biological self-interest.

Aren't we talking about facets of the same mechanism in LH and ranking? I disagree with Russ's assertion that "internal self-attacking" is more understandable from ranking theory; it fits just as well with LH, which reinforces my belief that LH is only a part of the hierarchical whole. If one is helpless and is furious about it, whom else will he attack, if not himself? You can't beat up on God! His own impotence, his helplessness, produces self-reproach and an effort to explain the helplessness by attacking himself as bad, stupid, weak, a failure, a coward, deserving of punishment, whatever. And feelings of helplessness in the real world can be a consequence of ranking: just ask that early retiree or the deposed alpha wolf. I'll bet In-group Omegas feel helpless a lot.

In short, the depressed, impotent, and subordinate (at least for now) individual is communicating only an apparent yielding; underneath, the war continues as rage at his own felt helplessness and ineffectualness. The anger may stem from the thwarting of a primitive drive toward dominance and the increased fitness rank carries. Yes, "Serotonin, potassium channels and the frontal lobes are there in rats and humans for accomplishing adaptive purposes". So let's put the depressive's feelings into that context as well.

More on 'Learned Helplessness and Ranking Theory' by Paul Gilbert

Russ has recently drawn attention to the relationship between learned helplessness theory and ranking theory. This seems to me a key area. I remember in NY in 1990, Kalman Glantz, myself & Russ deciding to work on this, but it kind'a fizzled out. Anyhow perhaps I could comment.

Learned helplessness was first observed as a phenomenon in 1948 by Mowrer & Viek in rats. During the 1960's Seligman and his colleagues began research on the incubation of fear in avoidance learning. As I recall the original hypothesis was to study the effects of increased fear on avoidance learning when the capability to escape was available. Thus animals were first exposed to uncontrollable stress (which was believed to put up their level of fear) and then released. It was found that, far from being facilitated in avoidance learning, a large percentage (but not all) animals showed inhibited avoidance learning. This became the paradigm for learned helplessness theory.

The explanation for the poor performance learning of animals who had been exposed to uncontrolled trauma was put down to learning that responses in the pretreatment phase (uncontrollable shock period) were ineffective to avoid the shock. As far as I am aware, Seligman did not believe that he was exploring innate psychobiological patterns. Possible links with Selye's GAS were around but to my knowledge came to nothing.

In the latter 60's and early 70's people like J Weiss from the Rockefeller Institute noted if LH animals were removed from the experimental situation and allowed to recover, the poor avoidance learning of these animals dissipated within 48 hours or so. Weiss and his colleagues argued that this dissipation effect was not at all like an avoidance learning paradigm. Normally avoidance learning is very resistant to extinction. They argued that therefore the LH phenomena was a direct biological effect due to uncontrollable trauma (They called their idea the motor activation deficit hypothesis, if I remember correctly). In any event, they explored the neurochemical changes observed in exposure to uncontrollable shock. In one early experiment they found that animals with yoked controls showed very different patterns of neurochemical change. Animals who are presented with shocks that they could learn to control showed heightened noradrenaline turnover whereas their yoked partners showed depletions. The difference between the two groups was not the degree of shock but the degree of control. Since that time considerable work has been done exploring the biological consequences of lack of control. A good introduction and overview is given by Anisman in 1978.

The issue about whether this phenomena is a learned phenomena or not is probably not an issue. Classical conditioning theory studies show neutral stimuli come to activate unconditioned responses. For example, it is well known that Pavlov showed that animals could learn to salivate to a bell if the bell was associated with food. Of course the actual salivation response itself is not learned (ie unconditioned); it is part of one's biological equipment. In a similar way the learned helplessness response appears to be some kind of innate potential, (unconditioned whole body response) or as I call it, capacity for certain types of pattern generation. It is no difference in principle to the capacity for salivation. What the learned helplessness research demonstrated, was the contingencies by which various biological changes, particularly neurochemical depletions can be activated. In fact J Grey, in his The Neuropsvcholoqy of Anxiety, has discussed this dilemma about whether this is a learned or biological response and he also believes that the argument is rather of little value. Indeed in any avoidance learning situation one is looking at the degree to which certain events activate (or inhibit, or habituate) the primitive fight/flight response. There has never been any suggestion that the fight/flight system is not part of an innate functional system. The coordination of all the physiology to make the fight/flight system work (heart rate up, energy metabolism etc) simply could not be learnt and psychologists know this perfectly well. Some evolutionary thinkers who exclude domain general systems in favour of domain specific systems only seem to me to be confused on this point and often fail to make the distinction between classical and operant learning (see Gilbert, 1992). The potential for classical conditioning (eg, to salivate to bells) is a very important evolved ability and does speak to general systemic properties. Learning involves under what conditions these innate potential are activated.

But I wander from my theme too much. What we have in learned helplessness however is more in terms of the activation of major changes of biological state. I have argued that this potential to changes state in this way is an innate potential, and that what the learned helplessness research is tapping into are the contingencies which activate this potential state. So it is both a learned and non-learned effect. The basic dimension is control. Lack of control over aversive situations produce certain biological changes. This seems to be an innate biological plan. Learned helplessness shows the types of exposure to uncontrollable stress which will activate it.

Russ suggests that rats do not generally have a problem with clinical depression. However, although I am not familiar enough with the literature I do remember reading that rats can show depressed-like states in over-crowded colonies where the subordinates tend to become very withdrawn and hide away. My other half, Jean, is studying cockroaches at present and they show the same behaviour. The subordinate males, put into a new territory with other males, run and hide! This may or may not be a good model for the forerunners of depression.' There has been some work exploring indirectly the effects of uncontrollable stress on dominant submissive behaviour in rats also. But in any case the underlying dimension is one of control. In social animals, social control is key for access to reproductive resources.

It is true that John Price argues that the depressed person is not helpless. However they experience themselves as helpless and what power they can exert seems to be somewhat negative. One should here distinguish between powerless to obtain positive reinforcers (benefits) and powerless to avoid negatives. There is a behavioural literature on this.

Perhaps a better word is thwarted. This brings me to the point that learned helplessness seems to be a state which encourages inhibited behaviour. Thus the two dimensions are control and inhibition. Those who experience control seem to become more activated; those who experience no control (trying and failing) become more inhibited. This is the basic plan from which the social behavioral repertoires then emerged. What is inhibited in depression is challenging behavior and general go-getting and explorative behaviour.

The point about the social dimension is that much of our evolution has involved evolving tactics of social manipulation. Thus most of our experiences of being thwarted arise from interactions from other individuals. This feeds in to our multiple capacities for conceptualization of interpersonal relations and opens up the area to discourses on shame, envy, assertiveness and so forth. Now as far as I know learned helplessness has been concerned primarily with the dimension of control and not with the implications of loss of control within a social context. It is the introduction of the social context which is crucial to reproductive strategies which complicates the experiences of depression. This leads to experiences of shame, inferiority, helplessness and so forth. Russ states that "exhibiting what we call LH is in fact deploying an evolutionary ancient program that makes it more likely to deploy certain communicational states and not others," is in accord with this view. What these communications do is to send no challenge signals to con-specifics. That is, the animal signals its state of inhibitedness that it will not challenge for mating partners and so forth, at least not in direct contest. This is in agreement with Russell's basic position.

What Russ has done with his extraordinarily powerful theory of psalics is to highlight the communicational dispositions that flow out of the dimension of controllability of outcomes. As he points out, the complexity allowed to humans via their increased cortical structures does not change the basic themes or goals that are sought.

My final point is that one should make a distinction between biosocial goals (what an individual is aiming to achieve), strategies (the way these are achieved via aggression, appeasement etc) and algorithms which are the reasoning models, (social comparison). If one doesn't do this then the data can become confusing. A psalic represents a particular pattern between a goal, a strategy and an algorithm. Thus in a state like depression we should be able to specify what goals seem to be thwarted, how the various strategies are under different states of excitation and inhibition (eg increased tearfulness, reduced assertiveness) and how the individual is evaluating him/herself in relationship to others (eg as inferior, down rank etc). All three components are important in understanding the operation of any particular psalic. To take a sexual psal-lic as a more obvious example, the goal is to form a sexual union with a partner, the strategies are to attract a partner, the algorithms are the reasoning processes one uses to be attractive (buying candy, buying flowers, being nice).

More on Frequency Dependent Selection
by Dan R Wilson

Those interested in Frequency Dependent Selection (FDS) may wish to know that there is a Royal Society book of papers of that very title published in 1988. I am trying to hunt down the proper reference. In any case, Nick Mascie-Taylor (Head of Biological Anthropology here at Cambridge), who is kindly teaching me a bit more about population genetics, avers that the idea of FDS is actually quite old. It was very much a part of the evolutionary synthesis of the 1930's. Moreover, the core ideas go back to the late 1800's. I am, therefore, sorry to have suggested otherwise in my last note to ASCAP.

In defense of my egregious error let me note that the authors of the evolutionary synthesis, particularly the mathematical biologists, are not easy to follow. Even so eminent an evolutionist as Ernst Mayr (1993), in his ninth decade of life and with the zip of the nineteen year old, notes the recondite density of the important mathematical work of Fisher, Hal-dane, Wright and others. If Ernst is stymied by all this, we lesser mortals have no cause for shame. I will stick to my treasured Dobzhansky who was so skilled at both mathematical biology and cleanly crafted prose.

On the whole, FDS is one of several ways in which population genomes maintain non-gaussian gene distributions. FDS arises, unlike the other means of transmitting discontinuous characters, almost exclusively as a function of the intrinsic species demography and less as a function of extrinsic agencies, although there are ecological feedback circuits, to be sure. So, FDS is akin to balanced polymorphism (classically exemplified by sickle cell), but not so clearly linked to an external vector (malaria). Rather, population density itself appears to trigger bi-modal responses in the genome.

John appears to be extending the idea of FDS in an ingenious direction. That is, he is keen on understanding how allelic behavioral traits in humans are partitioned in the genome via FDS. Let me digress a bit before returning to FDS itself. One wonders how many of our clinical concerns are due to the radically changed population density of the post-Pleistocene ascendance of mass society. Ernst Mayr cited the rise of mass society as perhaps the key problem in understanding the current human state. This was at the McLean Hospital Symposium integrating cognitive psychology and evolutionary biology (Ellis, 1992). It was a interesting forum which John Pearce reviewed for ASCAP last year: eclectic with the inimitable Albert Ellis giving his colorful rap on rational-emotive therapy—which he links to evolutionary foundations. Glantz & Pearce expanded on ideas familiar to those who have read their excellent, clinically relevant book *Exiles from Eden.* I had the great pleasure of moderating the sometimes incendiary dialog. Highlight of the day was the cameo appearance of Ernst Mayr who emphasized the phenotypic demands modern mass society places on the human genome.

A video of the symposium is available through the Office of Continuing Education, McLean Hospital, 115 Mill Street, Belmont, MA 02178. It is also worth CME credits!

The ASCAP audience would do well to know that Mayr considers evolutionary psychiatry as an enduring hobby. Like so many others, notably Darwin, Mayr first studied medicine before taking up interest in evolution. His medical roots are well evidenced in "Schizophrenia as a Morph", a Nature paper he wrote with Sir Julian Huxley (1964). The reasoning of this paper is quite like my own ideas regarding manic-depression. In further conversations with Ernst (regarding subsequent changes in nosology of the major psychoses) he now agrees that the ideas are relevant more to the genetics of affective disorders rather than the more etiologically heterogeneous schizophrenia.

Back to FDS. In connection to the work of John Price and other clinical readers of ASCAP, Mayr's general concern about mass society raises the possibility that some human phenomena are emergent with heretofore unat-tained population density, and not in a deeper sense evolved. They are really phenotypic reactions or "exaptational" phenomena, all of which are not stable genomic features. I suggest that much of the clinical element in some psychiatric syndromes are cases in point. As to my own interest in manic-depression, for example, I doubt that there was much prehistoric manic-depression, *per se.* Rather, there was expressed the gregariousness, social dominance drives, creativity and all the rest (Wilson, 1992). The pathology may be largely an artifact of mass society, ie, a disease of civilization. If so, evolutionary analyses will tell us little about the specifically pathological aspects of these genes. Instead, darwinian selection is currently at work on the fluctuant phenotypes of these alleles, a fact which makes it difficult to assess their current adaptive state, if any. On the other hand, there may be aspects of these genetical propensities which were truly sculpted by FDS in the environment of evolutionary adaptation and are thus still adaptive. Darwinian analysis can be quite revealing in this area.

Having mentioned Mayr, I must mention that beyond the joy of reading highly lucid evolutionary historiography written by a key participant in the events chronicled, there is in this recent paper a subtext relevant to current sociology of science in our academic niche. Mayr basically says that the evolutionary synthesis was achieved when the two schools of evolutionary biology began to understand one another. These schools were (and remain) the exact mathematical biologists on the one hand, and the naturalists on the other. Darwin hated mathematics—barely passed Cambridge on this account. Still, he intuited much of what was later expressed in the exact terms of mathematics, eg, Hamilton's kinship selection. Mayr and others note that there is regionalism involved in these respective approaches--the naturalists tended to be European and the exactists Russian or Anglo-American.

The dichotomy between exact and naturalistic biology persists. Inevitably, it has been bruited about in our various circles that there are lines of stress amongst the various groups of evolutionists and ethologists. Of course the similar squabbles of our own various Evolutionary Psychologists and Darwinian Anthropologists are well known. Likewise, the tiffs between this or that research group. But of all these dichotomies, I think the variations between continental Europe and America are the most interesting. This is especially because it seems to me to be a variation on an old theme in science history (Mayr, 1993; along with the excellent book by Bowler, 1992—reviewed next issue). Indeed, it seems to have much in common with Max Weber's analysis of the distinctions in cultural features in America and Europe at the turn of the century (Gerth, transl., 1970).

Sociology of biology

by Dan R Wilson

One benefit of my travels in Europe has been to better appreciate the subtle sociological distinctions between Old and New World biology. In the spirit of David Hull's studies of cladist and phyletist wars, it is useful to assess the themes of groups in science. The point of this brief sociological analysis is not so much to annoy as to evoke via a few catathetic signals, though I probably will succeed only in inviting the wrath of all without edifying any!

As a sociological example of scientific process, the Human Behavior and Evolution Society (HBES), a basically N American group, strives for exactitude whereas the International Society for Human Ethology (ISHE), a more continental group, is ideation-ally more flexible and less formal. Which is to be preferred is hard to say. The best achievements in the history of evolutionary science have happened when hardheaded biology is wedded to a love of life so typical of field biologists. Where this has happened in a single person (W Hamilton, EO Wilson, RD Alexander, etc), the results can be profound. I, for one and as a member of both organizations, would like to see a greater synthesis of the two themes and no more so than in the little world of our evolutionary and ethological psychology meetings.

As to the HBES, I fear there is a hardening of the agenda creeping in. It is increasingly less likely that a hypothetico-deductive (or, heaven forbid, a frankly speculative) paper will be accepted. There is too much genuflection to empirical data and design. Will it soon become a white lab coat affair? In short, HBES is being academically socialized in an all too brittle framework. One problem with this is that the wavefront of research becomes restricted and increasingly self-referential. The hearty give and take of the early meetings seems to be losing its base. The clinical dimension and other more anecdotal and speculative contributions are fading under severe "selection pressure" which tends to promote conventionally dry academicity of a purely inductive type. Yet both Sirs Karl Popper and Peter Medawar, among others, have long established that historical questions cannot be answered with empirical data. Still, the *inductive process* so entrenched in America especially has become 'politically scientifically correct' in application even to the study of that most historic process of all: evolution!

The ISHE on the other hand, though a less judgmental and more openly curious group, suffers from an incomplete devotion to darwinian thinking. It is more loosely knit, almost a tweedy affair. The ethological agenda of course greatly overlaps darwinism, but unlike darwinism it is by no means a self-correcting and comprehensive science. Still, there is a deep respect for life that pollinates their thinking and their meetings. More social effort is paid to establishing what Paul Gilbert usefully (though not in application to these groups) calls 'safety' for the purposes of exploration of new ideas. This, I find quite attractive, fun and not particularly subversive to the cause of science. Yet I can appreciate how some schooled in a more contentious style of academic discourse—perhaps typical of American graduate schools—do get aroused when ideas are not buttressed with overwhelmingly persuasive data.

But one does well to recall that in science history intellectual paradigms (to use Kuhn's hackneyed concept just this once and never again in ASCAP) buttressed by overwhelming data have crashed repeatedly. Indeed, one of Darwin's most monumental tasks was to turn the mass of naturalistic data about the intricacy of "Nature's Design" *against* the prevailing teleological interpretation by which such intricacy was empirical proof of a Creator. Paley's theological philosophy (complexity proves design intended by God) of nature reigned supreme in his day and he was required to master its tenets while at Cambridge (Desmond, 1992). In any case, one notes that those tweedy naturalists Lorenz and Tinbergen did work of Nobel quality without concerted selectionist interpretations.

Yes, other evolutionists and ethologists have also done work of Nobel quality, but still, for whatever reasons, the tweedies have actually won a prize and the rest of us have not (yet).

I think, if my archetypes are at all accurate, that the sharper critics ought to pull in their intellectual plumage and be more academically altruistic. Meetings, especially those of HBES, ought not to be the intellectual equivalent of rutting season replete with metaphorical headbanging and the other behavior which to this anthropologist and psychiatrist appear as rather unseemly, quasisexual sublimation. On the other hand, we do need to be on the alert for social deception in the intellectual realm as elsewhere — they who would be sneaky in the reproduction of their ideas are to be guarded against. I imagine that such might slip through a meeting of ISHE. On the whole, none of our meetings need be intellectually 'read in tooth and claw'.

To use Price's wonderful ethnological story of the chief who farted at the solemn occasion, our meetings perhaps should aspire to accept without rancor the odd fart (I strive to be a higher grade eructation) without devolving to become little more than occasions for congenial eructations. To this end, we should all note that the common interests of etho-evolutionists throughout the world far and away outsize any differences in style or substance. The overall darwinian agenda is too important to get mired in early fallings out among ourselves. We have found the enemy and (unlike the famous insight of Pogo) it ain't "Us".

If we need an outgroup intellectual omega we need look only so far as the unrepentant social contructivists who so dominate much academic life, particularly in the humanities, some social sciences are, sad to say, much administration. But that for another day. Meanwhile, let us tighten up the ethological and evolutionary team and get on. To that end, I am well pleased that there are tentative plans to have a concurrent meeting of ISHE and HBES in 1994 (most likely July or August). It is to be in Boston, although Bermuda is a more obvious spot to integrate the transcontinental attitudes noted above. If this comes to fruition it could well be a wonderfully stimulating affair with a wide range of people, ideas and approaches. I, for one, further believe that there is much to be gained in having a tribal convocation from time to time which draws together the various bands foraging in the intellectual ecology of dar-winism. Rather like the great gatherings of the Sioux Nation or the septs of the Highland clans, it might strengthen the totemic links between sibling groups rather than allowing intellectual rivalry.

Terminology and Jung in South Africa

by Leon Sloman

I have just returned to Toronto after a trip to England, Zimbabwe and South Africa. My first stop was in Derby, England, where I had a productive and enjoyable stay with Paul Gilbert and his family.

We have previously used the term "yielding reaction", which is triggered by losing an agonistic encounter and, in turn, triggers "giving way", which terminates the "yielding reaction". Paul is now expressing reservations about the terms "yielding reaction" and "giving way". I agree with him that the term "yielding reaction" could create confusion, because yielding seems similar to submission. I propose that we replace the term "yielding reaction" with either "inhibitory reaction" or "helpless-hopeless reaction" or strategy.

Paul proposes that instead of "giving way" we use the term "reconciliation", although I wonder whether one should rather say "internal reconciliation", because what one is referring to is a giving up of goals or aspirations. One might then distinguish between an act of submission, which is an interpersonal transaction, and "internal reconciliation", which is intrapersonal. I would appreciate any feedback on what would be considered to be the most appropriate terms.

Paul and I discussed the relationship that fear and anxiety have to the "inhibitory" or "yielding" reaction. If a prospective agonistic encounter engenders considerable fear or anxiety in one participant, one might anticipate a greater likelihood of that individual losing the encounter. Similarly, a history of losing encounters may make the individual more fearful in subsequent encounters. This may perhaps, in part, account for the frequent co-existence of depression and anxiety.

In London, I had lunch with Peter Rohde, Harley Street psychiatrist and friend of John Price. He had been impressed by John's early papers. We discussed how these ideas could be relevant in psychotherapy; I was pleased to find that there was much common ground between us.

In South Africa, I contacted Patrick Tummon, psychoanalyst at the Cape of Good Hope Centre of Jungian Studies, near Cape Town. I had met Patrick at the ISHE meeting in Amsterdam and was pleased to have the opportunity of interacting with a Jungian who has strong interest in ethology. I have been interested to explore to what extent ethology might provide links between different schools of psychotherapy. To my knowledge, cognitive therapists like Aaron Beck and Paul Gilbert have been particularly interested in ethology. However, Daniel Kriegman in Boston is a psychoanalyst who has a background in self-psychology and a strong interest in ethology.

Patrick was born in Ireland and trained in engineering and city planning. However, he had been fascinated with psychology and proceeded to obtain a psychoanalytic training at the CJ Jung Institute in Zurich. Since arrival in So Africa, Patrick has visited game reserves and has followed various bush trails that enabled him to make wild life observations. But he has missed having another therapist with whom he could discuss ethological observations.

Patrick found that Jung's theory of archetypes made the transition to evolutionary thinking quite easy. The ethologist focuses on inbuilt mechanisms that process certain aspects of the environment, and also trigger genetically programmed responses. The Jungian analyst describes these as archetypal responses. Could one therefore argue that Jungians have a more explicit recognition of the role of ethological responses than any other schools of psychotherapy?

Patrick struck me as a gifted therapist. He expressed strong interest in the relationship between myth, dreams, ethology and psychotherapy. Patrick was fascinated by bushman myths that express archetypal themes. The same themes were represented in his patients' dreams and he utilizes his knowledge of the patient and his understanding of the patients' dreams to make interventions that incorporate ethological (archetypal) mechanisms. I encouraged Patrick to write up the bushman myths and clinical material that he presented.

Patrick and I discussed the relationship between Bowlby's ideas on attachment and agonistic behaviors. He makes a practice of evaluating how his patients' attachment experiences, eg, not having a good enough mother, influences ways the patient handles agonistic interactions. He has a hierarchy of interpretations, and the specific intervention would depend on how far the patient has regressed back. For example, if the patient is very regressed, one needs to focus on attachment issues before dealing with the patient's agonistic responses. Patrick was able to demonstrate the dangers of being too reductionistic. For example, a patient with a strong need to be powerful and to control might have a submissive facade.

Patrick views folk tales as natural products of the mind. For this reason, cultural criteria must conform to innate mechanisms. When I was discussing with Patrick how positive assortative mating could have had a valuable adaptive function, I gave as an example the rich and powerful man who chooses a beautiful bride from the lower classes. He then pointed out how this was illustrated in folk tales by the story of Cinderella.

Now that I am back, I am enjoying looking back on these discussions and hope that it will be possible to continues these dialogs. Perhaps ASCAP will play some role in this.

A new ASCAP subscriber is David H Rosen, the Frank N McMillan Professor of Analytical Psychology, at Texas A & M University at College Station, TX. We hope that he and his students will contribute to these dialogs.

The following quote comes from Merlin Donald who speculates in his book on the bigger human brain. [lP257-8]

> Bruner classified narrative skill as a form of thinking, rather than an aspect of language. But it might be seem more simply as the natural product of language itself. Language, in a preliterate society lacking the apparatus of the modern information-state, is basically for telling stories. Language is used to exchange information about the daily activities of the members of the group, to recount past events, and to some extent to arrive at collective decisions. Narrative is so fundamental that it appears to have been fully developed, at least in

its pattern of daily use, in the Upper Paleolithic. A gathering of modern postindustrial Westerners around the family table, exchanging anecdotes and accounts of recent events, does not look much dif ferent from a similar gathering in a Stone age set ting. Talk flows freely, almost entirely in the nar rative mode. Stories are told and disputed... a col lective version of recent events is____ hammered out as the meal progresses. The narrative mode is basic, perhaps the basic product of language.

The supreme product of the narrative mode, in smaller preliterate societies, is the myth. The myth is the authoritative version, the debated, disputed, filtered product of generations of narrative interchange about reality. In conquering a rival society, the first act of the conquerers is to impose their myth on the conquered. And the strongest instinct of the conquered is to resist this pressure; the loss of one's myth involves a profoundly disorienting loss of identity. The myth stands at the top of a cognitive pyramid in such a society; it not only regulates behavior and enshrines knowledge, but it also constrains the perception of reality and channels the thought skills of its adherents.[1'P257-8]

Abstract; Wickings EJ, Dixson AF: Tes-ticular fuction, secondary sexual development, and social status in male mandrills (Mandrillus sphinx). Phvsiol & Behav 1992;52:909-916.

Positive correlations between dominance rank and plasma testosterone levels have been described for adult males of several primate species in captivity, but the relevance of such observations to free-ranging animals is unclear. The Centre International de Recherches Medicales in Gabon maintains a breeding group of 45 mandrills in a six hectare, naturally rainforested enclosure. The study describes correlations between dominance rank (in agonistic encounters), levels of plasma testosterone, testicular volume, body weight, and development of secondary sexual characteristics (red and blue sexual skin on the muzzle and rump areas) in male mandrills under semi free ranging conditions. Two morphological and social variants of adult male mandrill were identified. Large-rumped or fatted adult males (n=3) remained in the social group and exhibited maximal development of sexual skin coloration as well as large testicular size and highest plasma testosterone levels. By contrast, slimmer-rumped or nonfatted males (n=3) lived a peripheral or solitary existence and these exhibited less development of their sexual coloration and had smaller testes and lower plasma testosterone levels. Longitudinal studies of gonadal development in these six males revealed that testicular volumes and plasma testosterone levels increased most rapidly during pubertal development (4-5 years of age) in the three animals which proceeded to the fatted condition. These included the highest ranking, group-associated male which exhibited the most intense sexual coloration and had higher testosterone levels, although this was not correlated with testicular volume. This study shows that in the male mandrill social factors and reproductive development are interrelated. Physicial features characterising dominant, group-associated males are subject to more rapid pubertal development, and circulating testosterone levels are higher in such individuals both during puberty and in adulthood.

Abstract: Long M, Langley CH: Natural selection and the origin of *jingwei,* a chimeric processed functional gene in *Drosophlla.* Science 1993;260:91-95 The origin of new genes includes both the initial molecular events & subsequent population dynamics. A... Drosophila alcohol dehydrogenase (Adh) gene, previously thought to be a pseudogene, provided an opportunity to examine the two phases of the origin of a new gene. The sequence of the processed Adh messenger RNA became part of a new functional gene by capturing several upstream exons and introns of an unrelated gene. This novel chimeric gene, jingwei (law), differs from its parent Adh gene in both its pattern of expression & rate of molecular evolution. Natural selection participated in the origin and subsequent evolution of this gene. From the body of the article: It has been proposed that retrotransposition can be a major source of intron loss during evolution, as it was with jgw. The evolution of jgw also demonstrates that retrotransposition can be the source of new, intron-containing genes in eukaryotic evolution.... The analysis of the early molecular evolution of jgw was functional from the beginning and experienced strong adaptive evolution. Prevailing theories of the origin of new genes assume initial relaxation of selection. Our results provide a contrary interpretation in which natural selection is present throughout the origin of a new gene.

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2. c/o R Gardner, A.450 Graves Building (D28), University of Texas Medical Branch, Galveston, TX 77555-0428. FAX: 409-772-6771. For ASCAP Newsletter Volumes 3 (Jan through Dec, 1990), 4 (same months, 1991), and 5 (same months, 1992), please send $18 (or equivalent) for each 12 issue set. The first two volumes (1988 and 1989) of thirteen and twelve issues respectively are available on request without cost. For subscription to the 1993 set of 12 issues (Volume 6), the cost is $20/year. Make checks or money orders out to "Department of Psychiatry and Behavioral Sciences, UTMB." If the payment is not in dollars, please double the amount for the exchange costs. If you pay in check, please be sure that your return address is printed on the check.

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organization has no official budget.

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