ASCAP NEWSLETTER

Across-species **Comparisons And Psychopathology Newsletter**
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"Psychiatrists and psychologists whose interests extend beyond the "what" (diagnosis) and "what's to be done?" (therapy) of mental disorders, to their "why" (etiology) and "how" (pathogenesis) have increasingly turned to evolutionary biology for explanations." Roger K Pitman

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.

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Features:

Announcement: Seymour Itzkoff has published a new book entitled: The Road To Equality: Evolution and Social Reality. Praeger Trade, 1992.

Hans Eysenck states about it "In order to appreciate [Itzkoff's] work readers should first of all throw all their prejudices overboard and try to look at the facts without wanting to punish the messenger because they do

not like the message."

Arthur Jensen "A very broad-based and philosophical perspective informed by up-to-date research in mental testing, human genetics, and sociobiology in the light of modern thought about human evolution."

Announcement: Lionel Tiger published "Living with abortion," in the 1992 (Sep/Oct) The Sciences, pp.12-15.

Letters; *30.12.92*

I've just returned from reading a paper on Socio-Mental Bimodality, Agonic and Hedonic at the session on Complex Social Behaviour in Human and Non-Human Primates at the International Theoretical Archeology Conference in Southampton, UK, and met Chris Knight, Ian Watts and Camilla Power of the Radical Anthropology Group (RAG) located at University College & East London College, London University and also Jeffrey Steele and Steven Sherman of Southampton University who are also members of the RAG. I mention all these people because they have come up with a theory of the origins of human society and the origins of language, *two aspects of the same event in the middle to late Pleistocene, when Homo sapiens males were forced by their women folks to go hunting for big game and then to bring the meat to the home base. I leave it to those who want to follow this up to find out the astonishing means they employed to do this and thereby created a conceptual culture. Suffice it to say that it was related to the phases of the moon with which the women's reproductive cycle were together in phase. An altogether remarkable insight largely confirmed by the interpretation of rock paintings up to 100,000 years old and by its ability to provide an origin for many rituals of present day hunter-gatherers. That I should attend a conference which, from the past, gave me so much insight into the present is till a source of wonder for me.*

The relevant literature is The Origins of Human Society and The Origins of Language both by Chris Knight obtainable from Camilla Power, Radical Anthropology Group Publications, 42 Gardiner House, Surrey Lane, London SW113TD UK.

Readers might also like to know that an updated set of essays on the two modes edited by me is included in the current number of WORLD FUTURES 1992,-35 (1-3) entitled Socio-Mental Bimodalitv.

Michael Chance, Birmingham, UK

Letters (continued): *1/20/93*

Paul Gilbert suggested that I write you to inquire about the ASCAP Newsletter and the group of scholars interested in the interface between evolutionary theory and psychopathol-ogy. This focus is very congruent with my interests and I would like to learn more about the newsletter and organization.

Kent G Bailey, Virginia Commonwealth University, Richmond, VA

Letters (continued): *January 27, 1993 I continue to enjoy the ASCAP Newsletter which I find continually stimulates my own thinking about evolutionary aspects of psychopathol-ogy. ...*

It is certainly a shame that the APA did not include your proposed symposium in the May program. However, the program committee of the APA has always been quite unpredictable. I think the best way to look at it is that you have an idea whose time is still coming even though it has not come as yet (among our fellow psychiatrists). Aaron Tim Beck, U Penn, Philadelphia

Letters (cont): *February 7, 1993 May I express my most appreciative thanks for sharing with me your notes and the articles on evolutionary psychopathology. Your words of encouragement strengthen me as I try to find professional place in academic of the U.S. Boris Dashevsky, Cincinnati, OH, USA*

Report From The London Zoo

by John S Price

A joint meeting of the Primate Society of Great Britain and the Association for the Study of Animal Behaviour, held at the London Zoo in Dec 1992, was entitled "The Ecology of Social Systems" and contained many examples of alternative strategies, both for reproduction and for predator avoidance. A number of the talks concerned species which adopt a lek system of mating, which is widespread among birds (especially grouse), fish (especially cichlids [pronounced siklids]) and mammals (especially ungulates, and also the hammer-headed bat) *[Ed: note abstract below on insect leks as well!].* A lek is "a communal area in which two or more males of a species perform courtship displays" (Encyclopedia Britannica). The area is "traditional" in that the same area is used year after year, and it may or may not be divided into small territories which are defended by individual males and are used only for mating; they contain no resources such as food or shelter; the females approach the lek as they come into oestrus, inspect a variable proportion of the males and/or territories, choose usually just one for mating, and leave shortly after mating has taken place. The lek system seems to evolve when food is widely dispersed and it is not possible for males to defend a territory in which one or more females can raise their young. *Papers About Leks* Tim Clutton-Brock of Cambridge talked on "The Evolution of Mammalian Leks" and described his work with the Uganda kob. Much interest attaches to whether the females choose between the males or between the territories. The female kobs are not more attracted to males with horns than to hornless stags, or even to males at all, but seem to be attracted to particular territories, especially those that are central in the lek. They much prefer a territory which already contains oestrus females, Additional work (heavy work) by Tony Deutch from East Anglia U showed that the topsoil of the territory is attractive, as moving the topsoil from the most popular territory to another site in the lek transformed the new site into a popular territory. Since the lek uses the same place year after year it is likely that some territories become heavily impregnated with urinary pheromones, so that popular territories become more and more popular as years go by.

Andrew Rossiter from Guelph U in Canada described the reproductive behaviour of cichlid fishes in Lake Tanganyika. The males form a lek and each male who has won a territory in the lek builds and then defends a nest composed of gravel and sand. The males weigh about 100 grams, and the next materials weight up to 10 kg, so the nest represents a big investment in resources for the owner (the nest may, of course, be built by several successive owners). Several territory-owners maintained their territories for the full seven months of the study period; each was visited by about 20 females per day.

The females live in a shoal near the lek, and when each comes into reproductive condition she approaches the lek and enters one of the territories. After a courtship dance, the female hovers over the nest and lays her eggs, the male at this time hovering a few feet above the female.

When the egg have been laid, the female sucks them into her mouth, and male and female then change places. The male ejects his sperm in the place where the female laid her eggs, and then the two change places again. The female then takes the sperm into her mouth, where it fertilises the eggs, and she then swims back to the shoal, continuing to brood the eggs in her mouth.

If a male is removed from his territory, he is replaced by a "floater" or non-territorial male who has been hovering about the lek. Rossiter recognises two types of floaters. Type 1 floaters are local fish who have spent a long time around the lek and *know the borders of all the territories.* This is evident from the fact that when they take over a territory they spend very little time in border disputes with their neighbours, unlike the type 2 floaters who swim in from other leks and spend so much time learning the boundaries of their new territory in border disputes that their mating performance is severely curtailed. This is the first demonstration that floaters are using their time constructively in learning the geography of the lek rather than just hanging about waiting for "dead men's shoes."

In addition to the territorial males and the two types of floater, there are three other types of male, performing alternative reproductive strategies, and all of whom are morphologically distinct.

There are "sneakers" who dash into the territory as the male and female are changing places after the male has ejected his sperm and before the female has taken it into her mouth. He ejects his own sperm in large quantities over the relatively small amount of territory-holder's sperm, and then dashes off again. These sneakers are recognized by their high gonad/soma index, as the enormous amounts of testicular tissue give them a round shape: they are specialised just for dashing in and ejecting sperm, and they do not have to deploy the musculoskeletal resources needed for nest-building and territorial defense.

Then there are "pirates" who take over a nest by force for a few days, ejecting the territory owner who may either hang around the lek or swim off elsewhere. The pirates are large, outside the range of variation of the other males.

Finally, there are the female mimics, who fool not only the territorial males but also the human observers. Their strategy is to impersonate the female form, and to shadow a female as she enters a territory; in this way, they are tolerated by the territorial male, and, intervening during the second exchange of places by the courting couple, they consume the sperm of the territorial male and replace it with their own.

Comment About Leks Although fish are a long way from man in phylogenetic terms, Andrew Rossiter's analysis of male reproductive strategies in this cichlid illustrates the whole concept of alternative strategies in a way that might be impossible from mammalian behaviour. The cause of the variation into four separate morphological forms is not known, but must be either genetic or dependent on environmental factors during development. They are true alternative strategies, in that only one is possible for any individual at any one time, and an intermediate strategy would almost certainly result in lower payoff than any of the four strategies described. The choice between territory owner and floater is made during negotiation with other males (ritual agonistic behaviour), while that between floater 1 and floater 2 is not known. The two floaters are alternative strategies

in that it seems unlikely that one male fish could/'the geography of two separate leks in mind, so the advantage of knowing one lek well balances the advantage of keeping many leks under surveillance. The two floater strategies are examples of alternative losing strategies. In terms of the psychopathology of the lek, we could say that the type 1 floater is manifesting an in-group omega psalic, whereas the type 2 floater is manifesting an out-group omega psalic.

Although leks do not occur in man, it is instructive to consider what is our nearest equivalent. The village hop (dance) is an aggregation of nubile creatures in which mate choice is made, but there are many differences from a lek: the sex roles are less differentiated, mating does not usually occur, and there is an expectation of continued association of couples after the event. Perhaps the nearest equivalent, if we reverse the role of the the sexes, is the brothel or red light district. Here a group of potential sexual partners aggregate and display to visiting members of the opposite sex, who then choose a partner, mate, and leave without any expectation that the mating has implications for parental care; we could even say that the red light districts in which the women display themselves in the windows of their properties are equivalent to those leks in which territories are defended, whereas the brothel is equivalent to a lek in which the whole arena is common to all participants.

Clearly any similarity between the lek and the brothel is due to convergent evolution. Nevertheless, squeezing the last juice of our across species comparison, can we learn any- thing about lek/jDy using the brothel as a referential model in the sense of Tooby and DeVore; or anything about brothels by using the lek as a model? I think the answer must be no, even if we increase the similarity by considering the pimp/prostitute sub-system rather than the females on their own, and thus allow fighting for territory into the "red light" scene. I think the reason the model is so lacking in heuristic value is the fact that, in spite of a formal similarity in organisation, the engine driving the two systems is different. The fuel of the engines is the thing of value transferred during the lek/brothel transaction. In the lek this is sperm, and it is of value because its reception by the female gives Darwinian fitness to the male. In the brothel, although sperm is transferred (or used to be before the introduction of safe sex) the sperm is of no value, and its transfer is incidental to the transaction, whereas the real driving force is the transfer of money. I suggest that the conceptual difference between money and sperm is the factor which renders an apparently promising referential model useless.

Papers Not About Leks Alan Dixson from Franceville reported on social asymmetry in a group of mandrills, living in a closed off area of forest in West Africa. Of the six males, three consorted closely with the females, and these were fat, with large testes, bright red noses, and high serum testosterone; they had a clear rank order and most of the matings were performed by the highest ranking animal; DNA fingerprinting showed that the third ranking male sired no offsprings at all. The other three adult males adopted a peripheral position in the group, were markedly thin and "out of condition", had small testes, dull red noses and low serum testosterone. During the study period the role of the alpha male was usurped by the former #2, whereupon the former alpha became peripheral, lost "condition" and finally became

solitary. These observations are yet more evidence for the profound effect of ranking stress on both physiology and behaviour.

There is no space to describe the paper by John H Crook on fraternal polyandry in Tibetan Buddhist villages, but his book "Tibetan Buddhist Villages" is shortly to be published in Delhi.

Recommendation for reconciliation between attachment and social rank theories in evolutionary psychiatry.

by Leon Sloman

Many of us who share a common interest in an evolutionary approach to psychopathology seem to travel in different paths. The challenge we face is to determine how we can integrate our different conceptualizations and to also clarify any fundamental differences that exist between our points of view. In order to achieve this, we should take more advantage of existing forums like ASCAP and annual meetings of our organizations.

There is one area where I would very much welcome feedback from ASCAP readers. Attachment theory and social rank hierarchy theory (which deals with competition and aggression) are often viewed as competing models. Yet a fundamental issue, both in individual growth and in interpersonal relationships, is the reconciling and integration of aggression and caring. This is exemplified in Melanie Klein's notion of the depressive position and Freud's Oedipus Complex. Therefore rather than viewing attachment theory and social hierarchy theory as competing models, we should be trying to integrate these two perspectives into one single model. For example, Kohut the pioneer of self psychology, pointed out that a poor "sense of self" which presumably results from an insecure attachment may prevent an individual from coping appropriately with winning or losing. As this is an area which I am planning to study, I would very much appreciate hearing from ASCAP readers about any data they know about or any comments they have on the interrelationship between attachment and social hierarchy theories.

John Price makes a distinction between mechanisms that occur at different brain levels. For example, he distinguishes between "voluntary yielding" and the involuntary "depressive yielding" which he also labels the "basic yielding strategy". According to John, (and I believe that Russell supports this view), the individual who is in an agonistic confrontation may decide very quickly that his adversary is too strong or powerful and then exhibit "voluntary yielding", which leads him to back off or submit. If the "voluntary yielding" fails to occur and the contests proceeds, the unfavorable outcome may cause him to become increasingly discouraged and feel progressively more inadequate, with the result that he is unable to continue the struggle. This is an example of "depressive yielding".

The problem that I have with John's distinction is that I doubt whether we often make practical decisions based on "pure reason" without some influence from underlying biological mechanisms. For example, John's "voluntary yielding" could well be the result of a biologically programmed response to the awareness that one's opponent is more powerful than one's self, namely a mild and short-lived yielding reaction. I would argue that the difference between John's "voluntary yielding" and 'ciepressive yielding is largely a difference in intensity. I therefore prefer not to use the term "voluntary". I believe that the "yielding reaction" is elicited quite frequently during the course of normal social interaction and often lasts perhaps a number of seconds or minutes.

Paul Gilbert discussed the function of depression in ASCAP Newsletter last year. He states "depression results from the activation of some internal control mechanism(s) that evolved to inhibit animals in low rank/status or losing positions". I label that internal control mechanism the "yielding reaction". Depression may result from an escalation of aggression that elicits a more powerful yielding reaction. I feel it is helpful to distinguish between the "yielding reaction" and the act of submission or "giving way." One function of the "yielding reaction" is to elicit the "giving way" response, which will bring the encounter to an end. In the case of our depressed patients, this "giving way" has failed to occur. Therefore, although the yielding reaction may be containing the aggression, it has not achieved the purpose of ending the conflict, which needs to happen before reconciliation may occur.

Beck's model of cognitive therapy represents a monumental contribution. He has more recently presented a very elegant formulation of the ethologi-cal underpinnings for his theoretical model. It seems to me that clinical observations can be helpful in providing a better understanding of biological mechanisms. However, the clinical model can also come to be a procrus-tean bed if one feels forced to adapt the ethological model to fit in with one's therapeutic model. I can see a possible advantage in first developing a model of the biological mechanisms and then examining the clinical implications. Beck has said that "depression works in a complicated way to force conservation of energy by reducing demands or goals and limiting activity." Although I do not have any trouble with this state-ment, I feel that it would be helpful to be able to spell out in more detail the precise nature of the biological mechanisms that contribute to depression. For example, Beck has said that "self criticism is probably redundant in depression since the patient already perceives self as un-loveable or helpless". I would argue that self criticism can at times be adaptive by causing the individual who is losing to accept that he is not going to win and causing him to "give way". I feel one reason that depression is complicated is that at least two mechanisms contribute to depression; an escalation of the yielding response and a failure to give way. In order to terminate the yielding response, one needs to fight on and achieve success or victory or to give way.

I prefer to talk about the function of the yielding reaction rather than the function of depression. I see depression as resulting from an es-calation of the yielding reaction, but as the person becomes more depressed, the adaptive mechanism comes to have a maladaptive effect, which may overshadow adaption.

Nesse has written on "What is Mood For?" and argues convincingly that the function of low mood is to steer the individual from less to more productive activities. I agree with what Nesse says but feel that he has overlooked the role of mood variation in controlling or steering aggression. High mood enables the individual to express the aggression required to face new challenges while low mood in my view serves to bind aggression. Nesse's arguments are very persuasive, but his failure to consider aggression in my mind greatly reduces the clinical useful-ness of his model in doing psychotherapy.

The Problems of Extrapolating from Creole to DNA to Protolanquage; A Reply to Derek Bickerton.

By Bernard H Bichakjian In the December issue of ASCAP

Derek Bickerton (DB) responded to my earlier discussion of his account of language origin with veiled vulgarity ("waste in...), threat ("close to actionable liable"), and name-calling ("gradualism is a religion").[10] I shall not reply in kind — that is not my style, nor am I in the same predicament: there is sufficient evidence to support my arguments.

I shall begin with a word of gradualism. To my knowledge this issue is still unsettled among biologists, and I would be highly surprised if a debate between two linguists would yield the clinching argument. I leave it to biologists to prove whether Nature makes jumps or not. I shall address the two issues more directly related to languages:

1. Is there a basic grammar coded in our genes?

2. Have languages evolved since the beginning of speech?

Innatists, essentially Chomsky and DB, though their basic grammars are not identical, answer the first question with a resounding "yes," and are compelled to say "no" to the second, since subsequent evolution would in their eyes take at least another genetic jump. The grammars of English, Basque, and Guarani, for instance, are all said to be variations of the same theme.

Let us examine the evidence. Chomsky introduced his innatist theory some thirty-six years ago, and since then no shred of biological evidence, neither molecular nor even clinical, has been found that would support the genetic coding of a basic grammar; instead, there emerged among independent biologists a consensus, both on genetic and neurological grounds, against the plausibility of such innate grammars and of their sal-tationist developments.

From the linguistic vantage point, one can reconcile the innatist scheme with the typological data only when innate grammatical structures are reduced to propositional logic, which is the direction taken by Chomsky, but which leads inexorably to the negation of an innate *grammar*, or by claiming, which is what DB does, that everyone is born with an English like grammar (nominative, subject-verb-object, sentence-embedding, etc) and "what the child then has to do is modify [it]... until it matches the grammar of the community in which he is raised." The unlikely genetic endowment is now coupled with a language acquisition scheme that runs contrary to the current understanding of developmental neurology.

Let us now turn to language evolution (question 2), where the empirical data are available. DB writes: "there is no degree of disparity at all in the degree of development of *languages* — all, so far as anyone can determine, are equally complex" (pl3). With this type of reasoning one can also argue that there is no biological evolution, because, as Lewontin pointed out, "we cannot all agree that a dog is more complex than a fish, although fishlike forms preceded doglike forms by 500 million years and were their ancestors" The task of the linguist is not to gape at the "complexity" of, say, French and Latin, or English and Indo-European and conclude that we have "complex" languages at both ends of the line of descent, but to examine the modern features and compare them to their antecedents. Such analytical work would bring into light the developmental course of linguistic features, and the subsequent assessment of their neurological underpin-ning and their linguistic functionality would suggest a likely explanation of why languages have evolved along the observed course. It is through such careful observation, insightful thinking, and great courage that biological evolution was recognized and, at least partially, explained. Today, the linguistic data

deserve no less, but DB apparently prefers the quicker course of extrapolating from the creole languages he knows to a form of DNA that is neither substantiated nor considered to be plausible, and thence to an equally implausible form of protolan-guage. Unfortunately, the empirical data belie the results DB's double extrapolation. Instead, they reveal a steady evolutionary course from the dawn of history to this day, and one may surmised that this process has been active not just during our observation period but for as long as speech as been used.

I can only conclude that no evidence has been found for genetically coded grammars, and that a language acquisition pattern that would modify would-be innate neural pathways into environmentally-suitable ones is not in line with current thinking among developmental neurologist. On the other had, the empirical data (historical and reconstructed) do reveal the steady development of linguistic features and suggest that the observed evolutionary movement is not a culture trait of the languages of the last few millennia,but an inherent characteristic of human speech.

Adding consumer's information for the linguistically naive on Chomsky-evolution issues is the following excerpt from John Casti's Paradigms Lost. Casti is a bright generalist who takes up a number of current controversies (eg, sociobiology and artificial intelligence) including that of Chomsky's innate language. He lines up arguments like a courtroom; summary arguments and verdict follow: To be absolutely clear on the point to be settled, let's review the bidding. Chomsky's argument is that all normal children receive as part of their genetic birthright a unique language-acquisition device, or language organ. This organ contains a hard-wired universal grammar, which children use to learn their native language quickly and efficiently. The two key points of contention are whether the language acquisition device is (1) innate, ie, inherited, not learned, and (2) unique, ie, specifically designed for language and not just part of a general problem solving apparatus. ... I've included Fodor along with Chomsky [in arguing this] even though Fodor is primarily a philosopher of mind and not a linguist... to dispel the view that Chomsky is the only one who holds to the Prosecution's case. In fact, a large number of linguists support Chomsky's case, but they do so in ways that are so similar... that there is no reason to distinguish among them in abroad treatment of this kind....

On the matter of language acquisition, there's no doubt for me as to where to place my money: firmly with the Prosecution and its claims for innateness and uniqueness ___

I find it hard to countenance any of the claims by Piaget, Sampson, et al. that the human language facility is just part of the general problem-solving and learning machinery of the brain.... there's too much empirical evidence.... why should language acquisition skills disappear for most of us in late childhood if the acquisition device is part of general learning...? If I can learn to dance the tango... at the age of forty, why can't I learn to speak Russian or French with equal ease...?

Cavalli-Sforza LL, Menozzi P, Piazza A: Demic expansions and human evolution. Science 1993;259:639-646.

Geographical expansions are caused by successful innovations, biological or cultural, that favor local growth and movement. They have had a powerful effect in determining the present patterns of human genetic geography. Modern human populations expanded rapidly across the Earth in the last 100,000 years. At the end of the Paleolithic (10,000 years ago) only a few islands and other areas were unoccupied. The number of inhabitants was then one thousand times smaller than it is now. Population densities were low throughout the Paleolithic, and random genetic drift was therefore especially effective. Major genetic differences between living human groups must have evolved at that time. Population growths that began afterward, especially, with the spread of agriculture, progressively reduced the drift in population and the resulting genetic

differentiation. Genetic traces of the expansions that these growths determined are still recognizable.

Abstract; Kabzdela KS, Richard AF, Pereira ME: Social relations in semi-free-ranging Sifakas *(Propithicus ver-reauxi coquerll)* and the question of female dominance. Amer J Primatoloqy 1992;28:139-145.

Female social dominance over males is thought to characterize most of the prosimian primates of Madagascar. It has been reported in Propithecus ver-reauxi coquireli in the wild but intersexual relations were not fully characterized. In this paper we examine female-male spatial and agonistic relations in semi-free-rang ing P.v.coquireli inorder to evaluate intersexual social dominance and related behavioral asymmetries. Two hundred hours of focal sampling were conducted on two pairs of P.v.coquireli at the Duke University Primate Center, Durham, NC. Behavioral categories including approach, departure, follow, pass, replace, and aggression were scored and recorded. Our results show strong assymmetry of aggressive encounters, suggesting female dominance over males. No submissive signals were observed. The analysis of spatial relations showed that males were more active than females in maintaining proximity and, on average, male spatial movements could be predicted by female location and activities more often than vice versa. These results indicate a spatially central role for the female of each pair.

Abstract: Srere HK, Wang LCH, Martin SL: Central role for differential gene expression in mammalian hibernation. Proc Natl Acad Sci USA 1992; 89:7119-7123.

Mammalian hibernators experience dramatic reductions in body temperature, metabolic rate, respiratory rate, and heart rate during hibernation. These changes are precisely controlled and reversible with only internally driven mechanisms, suggesting specific biochemical regulation. We present a model that integrates our observations of differential liver gene expression during preparation for, and maintenance of, the hibernating state, with the known phylogenetic interspersion of hibernating species in several major mammalian lineages. This model predicts a major role for the differential expression of existing mammalian genes in the biochemical regulation of hibernation.

Abstract: Droney DC: Sexual selection in a lekking *Hawaiian Drosophila:* the roles of male competition and female choice in male mating success. Anim Behav 1992;44:1007-1020.

The importance of sexual selection in Drosophila grimshawi, a lek-forming Hawaiian Drosophila, was anlysed in the laboratory by studying details of male and female mating behaviour at spontaneously-forming leks in large and in small observation chambers. Sexual selection was found to be intense, as is often hypothesized in Hawaiian Drosophila. However, little evidence was found for a significant role of active female choice in male mating success although females can reject attempted copulations. Male body size was significantly related to mating success, but mostly because large males spent more time sexually active at lek sites, which resulted in increased mating success, and not because large males physically dominated smaller ones. Variation in larval environment significantly affected male size, and thus, environmental factors may have a large, but indirect, influence on male mating success.

Abstract: Golani I: A mobility gradient in the organization of vertebrate movement: The perception of movement through symbolic language. Behav and Brain Sci 1992; 15:249-308.

Ordinary language can prevent us from seeing the organization of whole-animal movement. This may be why the search for behavioral homologies has not been as fruitful as the founders of ethology had hoped. The Eshkol-Wachman (EU) movement notational system can reveal shared movement patterns that are undetectable in the kinds of informal verbal descriptions of the same behaviors that are in current use. Rules of organization that are common to locomotor development, agonistic and exploratory behavior, scent marking, play, and dopaminergic drug-induced stereotypies in a variety of vertebrates suggest that behavior progresses along a "mobility gradient" from immobility to increasing complexity and unpredictability. A progression in the opposite direction, with decreasing spatial com-

plexity and increased stereotypy, occurs under the influence of the nonselective dopaminergic drugs apomorphine and amphetamine and partly also the selective dopamine agonist quinpirole. The behaviors associated with the mobility gradient appear to be mediated by a family of basal ganglia-thalamocortical circuits and their descending output stations. Because the small number of rules underlying the mobility gradient account for a large number of behaviors, they may be related to the specific functional demands on these neurological systems. The EW system and the mobility gradient model should prove useful to ethologists and neurobiologists.

Evolution of flying is not usually an interest in psychiatry, but to the extent that ASCAP readers have an interest in evolution, the big news in the Feb 5 Science concerns whether *Archaeopteryx* was principally a land-dweller or a branch/air-dweller: Abstract: Feduccia A: Evidence from claw geometry indicating arboreal habits of *Archaeopteryx.* Science 1993;259:790-793.

The Late Jurassic Archaeopteryx has been thought to have been a feathered predator adapted to running that represented a terestrial stage in the evolution of true birds from coelurasaurian dinosaurs. Examinations of claw structure, however, shows that (i) modern ground- and tree-dwelling birds can be distinguished on the basis of claw curvature, in that greater claw arcs characterize tree-dwellers and trunk-climbers, and (ii) the claws of the pes (hind foot) and manus (front hand) of Archaeopteryx exhibit degrees of curvature typical of perching and trunk-climbing birds, respectively. On this basis, Archaeopteryx appears to have been a perching bird, not a cursorial predator.

Abstract: Petty F, Kramer G, Wilson L: Prevention of learned helplessness: in vivo correlation with cortical serotonin. Pharmacol Biochem & Behav 1992;43:361-367.

Learned helplessness (LH) is prevented by pretreatment with acute benzodiazepines (BDZs), sub-chronic tricyclic antidepressants (TCAs), or escapable stress (ES). We have investigated the role of serotonin (5-HT) in LH prevention by these three prevention paradigms, using microanalysis to measure in vivo 5-HT in frontal cortex (FC) after LH testing. Rats receiving pretreatment before inescapable stress with any of the three methods of prevention--BDZx, TCAs, or ES--showed escape behavior in the shuttle-box test for LH comparable to naive unstressed controls. K^+-stimulated 5-HT release in all three groups receiving pretreatment was also similar to naive unstressed controls. Rats receiving saline before inescapable stress showed significantly more LH behavior in the shuttle-box task and had significantly lower 5-HT release as well. This suggests that LH correlates with a significant decrease in intracellular releasable 5-HT in FC, and that three different techniques for LH prevention, acute BDZs, subchronic TCAs, and ES all similarly prevent this 5-HT depletion.

On learned helplessness, ranking theory, Paul Gilbert's questions, in-qroup psalic, & human-nonhuman continuity, by RG

Does learned helplessness reflect psychopathology or sociophysiology? The above abstract illustrates the power of the animal model of learned helplessness (LH). Biomedical researchers Fred Petty from Southwestern Med School in Dallas and Fritz Henn from SUNY Stony Brook are amongst the Iowans-moved-away who are continuing to investigate this behavioral phenomenon first seen in dogs by psychologist Seligman.

The above research illustrates how this concretely defined behavioral state has become an enduring useful animal model for medication-response depression. It can be explored with pharmacologic and molecular probes; conclusions are nearer about potential brain areas involved (frontal cortex) and metabolites involved (serotonin, K^+). This is one of the exciting areas of an emerging science dealing with abnormal behavior and its neuronal counterparts.

What,however, does it have to do with the *normal* behavior and its

neuronal counterparts? Obviously much. Rats and humans seem to have some mechanisms in common if the analogy of LH and depression continues to hold. But rats do not generally have a problem with clinical depression (at least that we know about and deal with) , so that one assumes that we are dealing with evolutionarily ancient mechanisms that are there in the rat and—human also of course— for adaptive purposes. This implies a role for LH in normal individuals. Can LH be brought into sociophysio-logy? Moreover, can sociophysiologi-cal ideas help the further patho-physiological questioning that might be investigated as a result of this thinking? Knowledge about normal circulation was helpful in learning more of circulatory pathology.

Paul Gilbert in his last book on depression has dealt with this. He says (pl29) (his italics): *""[P]at-tern generation" as a property of innate potential* is a crucial factor for understanding psychopathology. Under certain kinds of stress, ele-ments of the psychic system may become organised in such a way that we say the person has become ill, or dysfunctional."

About LH, he says (my italics): "The first response to uncontrollable trauma is often (though not always) to become fearful and to struggle to find an escape. If this is not successful the system *changes* to a state of helpless passivity." I emphasize "changes" not "learns," though later, when discussing the theory more extensively, (pl75-180), Paul represents Seligman's meaning by quoting him exactly: the animals "learnt that their responses were ineffective in controlling the shock." The phenomenon is not disputed. But to highlight the learning instead of the fact that a powerful, inborn com-municational propensity state was triggered, is like noting blood is shed if one is cut. Of course, but that fact seems to omit the important component of the biological program.

Paul of course does not neglect this: he wonders if LH is a basic plan: "the effects of helplessness have been observed in many species suggesting that it is a highly conserved psychobiological response pattern in evolutionary terms...the effects of loss of control on avoidance learning, explorative behaviour, vegetative and appetitive processes are so consistent as to suggest that this is a basic plan and not a random effect of psychobiological organisation producing arbitrary effects."

This last clause pinpoints why I've been vaguely distressed for a long time about the LH term. Somehow the fact that the animal *learned* and that what was learned was *helplessness* is the least of it. After all, the depressed person is only partly "helpless," (John Price and Leon Sloman have discussed how manipulation of others through not only helplessness but ineptitude can be understood with a basic plan concept presumed to underlie depression --see Leon's essay above about attachment and ranking issues--implied by "helplessness" and agonistic issues —implied by "no fight!" signals).

Paul compares "ranking theory" with LH. He points out that there is no major conflict between the two, al-though ranking theory is more explicit about a number of predictions. Both point out that controllability of outcomes is crucial although the LH model ignores many of the "[s]ocial dimensions of control." "Internal self-attacking" is more understandable from ranking theory.

Paul Gilbert's investigational issues: Investigatory work needs doing with the central idea that LH and ranking theory make different pre-dictions. The following extrapolates issues Paul raises on pl79-180:

(1) Will LH from non-social stimuli be less difficult to extinguish than

that from social events, ie, will it disappear more quickly? Ranking theory would predict a positive answer, that social slights and put-downs (catathetic signals to use John Price's term), will cause a more long lasting depression.

Moreover, will human depression from LH-like stimuli:

(2) reflect more of a *lack* of coherence in depressive response pat terns (eg, shame, anger control, assertiveness, envy, self-attacking, and "the social complexities of the depressed person") than ranking theory would predict? and

(3) reflect no difference in the kind of hostility expressed regard less of whether it is expressed up hierarchy *vs* down hierarchy?

(4) Finallly, using human reactions to stress as seen in post-traumatic stress disorder, the LH model pre dicts no difference in depression ac cording to social rank, but ranking theory would: if there were inferior self-perceptions beforehand, the per son should feel more depressed.

Communicational propensity states: Let me make a plug for my old term, In-group Omega Psalic, as a more precise description for what has been labeled as LH.[18] Psalic stands for Propensity State Antedating Language In Communication & Programmed Spac-ings And Linkages In Conspecifics. In other words, an animal (nonhuman or human) exhibiting what we call LH is in fact deploying an evolutionarily ancient program that makes it more likely to deploy certain communications and not others. That it has *learned* to do so in a particular circumstance is less important than the fact it is something easily triggered in the wide range of creatures that Paul mentions. One doesn't need *much* by way of learning apparatus to have the learning occur: its automaticity resembles the learning that occurs when a hot stove is touched: immediate, powerful, long-lasting.

Still, the circumstances of such learning are a critical topic of future research. As Paul states, we've much to do to clarify the model from the social interactive approach. Moreover, until we do, the socially and conceptually impoverished LH model will continue to hold sway.

The modifier of psalic (which is a noun), *In-group omega,* refers to the communicational or relating problems of the lowest ranker in a group: if one wishes to continue in the group, or can't get away, then one is well advised to hunker down and not move: "Show no energy. Signal 'no competition' to those victorious." Using the term of John & Leon, one visualizes a "yielding person."

Compare it to the state of being in love. Very retarded, nonverbal people can become erotically hooked to another person: they too can achieve a "sexual psalic," a programmed, communicational propensity state that had already been in our genes long before our ancestors had language. It creates a desire or hunger for closeness to a fellow human being.

Yielding strategies and psallcs emphasize different things. However, whether one speaks of voluntary or involuntary yielding (which Leon emphasizes in his piece above), the concept of yielding strategy (YS) is one that he propounds (and I do too in papers co-authered with him); I wish to explain some things by using an inclusive concept that explains more.

Strategies are black-box or artificial intelligence concepts, similar to the idea of "Darwinian algorithm" (DA); they imply (with no argument from me) that the animal calculates. Psalic is a more elaborate concept which encompasses that the brain calculates but goes beyond it:

(1) There is an emphasis on *conspecifics*, members of one's own species; yielding doesn't work with predators — nor with outgroups for that matter, (as Paul discusses on p

281) but the out-group omega psalic (mentioned by John Price in the zoo-report above) handles the latter as a separte communicational propensity.

(2) *Communication* is emphasized (or relating to use Birtchnell's term).

(3) That an organismic *state* exists is labeled by psalic. A state is implied with LH, but what is labeled is an implied relationship (ie, that the individual is helpless with respect to others). The neuronal device may be for the purpose of relating but is located in the individual. State emphasizes that. Activities of cells and neurons occur in each of the individuals, not in the interaction between them.

(4) The exact communication is not predicted by the psalic in operation. The would-be lover in sexual psalic may use poetry, flowers or candy. But the *propensity* to try accomplishing a particular message-effect is implied by the psalic label. (The potential effect on the receiver of the LH message, helplessness, as mentioned above, is only one of a constellation of intended messages that depressed people or normal individuals who are low ranking deploy.)

(5) Psalic implies the ancientness of the communicational state, *antedating* language for instance. Like bones in a particular position, the ancient genes have continued to provide the living organism with a repertoire of communicational states. The current exhibition of them may vary with the animal and its repertoire of signal possibilities, but useful evolved adaptations stay around.

(6) In-group omega psalic is one of a number in parallel with sexual, alpha, alpha-reciprocal, spacing-avoidant, nurturant, nurturance-recipient, and out-group omega psalics. This puts what is labeled as LH in a context with other communicational states on a similar level of abstraction. Each has a pathological and a normal counterpart in humans and parallels in animals. Each requires investigation.

Human-nonhuman continuity. John Casti takes a point from paleontologist SJ Gould as follows: "genes have given up their sovereignty over the major human behavior patterns as a result of *Homo sapiens*'s most distinguishing feature —an extraordinarily large brain."[19]

On the other hand, another Gould (ethologist JL) is characterized by JS Kennedy in his new book on The New Anthropomorphism as flatly dismissing "the idea of any difference of any kind between human and sub-human behaviour."[20]

What the "extraordinarily large brain" seems to accomplish differently is not the abandonment of old strategies that worked reasonably well, but variations of them. The difference seems to be refinements in how that is accomplished (language, planning, more subtle and extensive gradations of human social rank, for instance). Behaviors signaling low rank were not abandoned, but continued from species with smaller brains to those with enhanced frontal lobes. Evolution is after all Francois Jacob's famous tinkerer?[21] When old devices have worked, natural selection hasn't invented new algorithms, just tinkered with the old.

But back to my original debating issue, the naming of learned helplessness. The emphasis of the name highlights the human-nonhuman discontinuity, not the continuity. We need to work to emphasize the latter. The human beings-first chauvinists won't let us forget the distance and discontinuity between animal and human!

In summary, serotonin, potassium channels and the frontal lobes are there in rats *and* humans for accomplishing adaptive purposes. We are grateful to Petty and colleagues not only for their findings as these relate to depression but also as they relate to normal sociophysiology.

1. Pitman RK, Orr SP: Psychophysiology of emotional memory networks in post-traumatic stress disorder. Writ ten as a chapter for proceedings of the Fifth Conference on the Neurobiology of Learning and Memory, UC Ir vine, October 22-24, 1992.

2. c/o R Gardner, 1.200 Graves Building (D29), University of Texas Medical Branch, Galveston, TX 77550 FAX: 409-772-4288. 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."

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4. Clutton-Brock TH: Mammalian mating systems. Proc Royal Soc of London BPU. I989;236:339-372.

5. (see ASCAP Newsletter l990;3:#2[Feb]7-l0).

6. Tooby J, DeVore I: The reconstruction of hominid behavioral evolution through strategic modelling. In The Evolution of Primate Behavior: Primate Models. Albany: State University of New York Press, 1987.

7. Dixson AF: Observations on the evolution of the genitalia and copulatory behaviour in male primates. Journal of Zoology. 1987:213:423-443.

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11. Rutherford W (Ed): Language Universals p. 149).

12. Lewontin R: Behavioral and Brain Sciences 1990:740.

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14. Seligman MEP: Helplessness: On Depression. Development, and Death. San Francisco: WH Freeman, 1975.

15. A current study on brain volume in depressed people showed 7% less frontal lobe mass in patients; however, the controls were more highly educated and 35% of the patients had had ECT! Once again imaging studies draw conclusions about pathology not normality using inadequate methods. (Coffey CE, Wilkenson WE,

Weiner RD, Parashos IA, Djang WT, Webb MC, Fiegiel GS, Spritzer CE: Quantitative cerebral anatomy in depression: a controlled magnetic resonance imaging study. Arch Gen Psychiat 1993;50:7-16.)

16. Gilbert P: Depression: The Evolution of Powerlessness. NY: The Guilford Press, 1992.

17. John Price has made the point that the depressed person is self-abusive when talking to those higher in the hierarchy than he and angry towards those lower.

18. Gardner R: Psychiatric syndromes as infrastructure for intraspecific communication. Chapter in Michael R.A. Chance (Ed.), Social Fabrics of the Mind. Hove, East Sussex, England: Lawrence Erlbaum Associates, Publ., 1988, pp 197-226.

19. Casti, p.206.

20. Kennedy JS: The New Anthropomorphism. Cambridge: Cambridge University Press, 1992, p.17.

21. Jacob F: The Possible and the Actual. NY: Pantheon Books, 1982, pp34-44.