ASCAP NEWSLETTER

Across-Species Comparisons And Psychopathology Newsletter
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"The communicator's contribution to the event is not adequately described just by saying that it performed a particular signal. Its behavior has other components, some of which must be evident to the recipient of the signal: it occurs at a particular place or particular time, for instance. The communicator may be seen to be standing or heard to be moving. It may be inescapably obvious that the communicator is fighting or eating... ' All acts, formalized or not, are informative." W. John Smith[1]

The ASCAP Newsletter[2]
is
a function of the

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

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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.

Challenge: How should sociophysiological science proceed? One way is to key on the medical advances that relate distinctive illness behaviors to new methods of examining the brain. Thus, the Archives of General Psychiatry, the premier research-focused journal of the U.S. often centers on the results of imaging research. The December issue begins with the following articles, involving the magic technologies of PET, SPECT and MRI, computed imagery methods for studying the brain:

(1) "Brain morphology and schizophrenia: a magnetic resonance imaging study of limbic, prefrontal cortex,

and caudate structures."[4]

(2) "Regional cerebral blood flow in monozygotic twins discordant and concordant for schizophrenia."[5]

(3) "Frontostriatal disorder of cerebral metabolism in never-medicated schizophrenics."[6]

(4) "Hypofrontality in neuroleptic-naive patients and in patients with chronic schizophrenia: assessment with xenon 133 single-photon emission computed tomography and the Tower of London."[7]

(5) "Negative symptoms and hypofrontality in chronic schizophrenia."[8]

(6) "Striatal metabolic rate and clinical response to neuroleptics in schizophrenia."[9]

These six lead articles present evidence that deficits in the prefrontal portion of the brain are awry in schizophrenia. Of course, these brain-parts are closely connected to the caudate and other parts of the striatum, subcortical nuclei with downstream connections from the cortex. One group found that 'negative symptoms' (disinclination to associate with other people, maintain body hygiene, etc) are related to the "right dorsolateral convexity," while another group focused on the "left mesial frontal cortex" and found abnormalities there.

As pointed out in an Aug UTMB lecture by Craig Risen, Emory U, Atlanta, controls are centrally important; this work overall encounters many other methodological issues as well. 'Solid' findings of today will be surely succeeded by those of tomorrow built upon this still early work.

But how extraordinarily exciting! Windows to the complex organ we know as the brain are now newly achieved. But they require equally sophisticated new ways of understanding the complex behavior of people and other animals. Such mechanisms will surely involve prefrontal cortex and striatum beneath it. These disease-focused efforts will blaze trails.

Readers of ASCAP know that great problems remain in deciphering what is meant by illness and how it relates to normal physiology, especially sociophysiology, as previously stressed in these pages.

I put a challenge to you to relate these findings to basic plans that underlie the illness. What *are* these negative symptoms really in the functional context of the human world? How do they relate to shyness? to a person's overanalysis or wrong analysis of fearsome stimuli? to inhibition of social stimuli (body hygiene) that has communicational value? to the distance dimensions of Birtchnell or to the strategies of Gilbert (see below)? What will we eventually be able to infer about the normal from the abnormalities now investigated?

There is practical advantage to brainstorming about these matters now: new imaging equipment is available or developing quickly in many centers (as at UTMB for instance); the technology is ever improving. I believe that fresh ideas will be tried by them and that the investigators in control of the machines are looking for creativity.

So think about it and let ASCAP know: what would you like to investigate if you had the apparatus to do it? How might we measure the communicational state of normals and patients to find congruencies as well as differences? How can we learn not only about the diseases but the substratum upon which the disease operates, a substratum of normal function which is still as *terra incognito* as Africa was to 16th and 17th century explorers.

Paul Gilbert called attention recently to R Post's discussion of recurrent manic and depressive episodes. Post discusses molecular components of stressful experience which seems to switch on basic plan(s) responsible for mania.

Post doesn't mention 'basic plan' concepts; rather, he focuses on the pathology, not on what is deviated from, on triggers not what is triggered. But should what is triggered be looked at too? Similarly for the negative symptoms of schizophrenia should we ask? Which are the components that stimulate the condition and which are basic plans that are there to be stimulated, probably for some evolutionarily reason that fostered gene perpetuation?

And *where* is that in the brain? We now can dare to ask such questions.

Letters: *29 November 1992*

Thank you for the recent ASCAP (15 Nov 92) . I am sorry to hear of the shortsightedness of the APA powers-that-be. We must keep trying! ...As *Ascap is now the official publication of IASCAP, may I suggest that "Proceedings" be substituted for "Newsletter"? Whether Newsletter or Proceedings, I enclose my subscription check! Finally, may I get the first two volumes of ASCAP for sake of completion.*

As for news, we are having a grand time here in 'ye olde Cambridge'. Through the kindness and hospitality of Professor Sir Hans Kornberg, F.R.S. (Master of Christ's College, eminent biologist and nice man), Sandy and I had the great pleasure of dining at High Table under the portrait of Darwin....

One benefit of such a sabbatical is the luxury of catching up a bit. I read ASCAP with great interest, as ever, but now with more time. Articles by Paul Gilbert as well as John Price caught my attention. I offer a few comments [to be included in a future issue].

Dan Wilson, Cambridge, ENGLAND

About the idea of calling these "proceedings" — don't proceedings usually reflect content of a scientific meeting? This can be an informal newsletter with give-and-take over time and space, and still represent the association. But perhaps additionally we should aim at publishing formal papers as another function of the organization. Readership, please let us hear other ideas about how this might transpire!

Abstract; Traupe H, van Gurp PJM, Happle R, Boezeman J, van de Kerkhof PCM: Psoriasis vulgaris, fetal growth, and genomic imprinting. Am J Med Gen 1992;42:649-654.

...[T]wo independent lines of evidence suggest... genomic imprinting of a major gene for psoriasis vulgaris. First, the birth weight of children from psoriatics is influenced by the sex of the psoriatic parent. Children from fathers with psoriasis are considerably (270 g) heavier than children from mothers with psoriasis ($P<.004$). Second, the disease manifestation (penetrance) depends in part on the sex of the psoriatic parent. Offspring from fathers with psoriasis and male "gene carriers" are significantly ($P<0.015$ and $p<0.007$) more often affected than offspring from mothers with psoriasis and female "gene carriers." Of 91 children grandchildren with psoriasis 59 (65%) have an affected grandfather and 32 from a psoriatic grandmother. This deviation from the expected distribution is significant ($P<0.04$). Genomic imprinting is...a special case of epigenetic modification. We propose that epigenetic modifications of a major predisposing gene in somatic tissues could cause differences in disease activity of psoriasis and could account for the often unpredictable clinical course the disease takes.

Abstract: Oksenberg D, Marsters SA, O'Dowd BF, Jin H, Havlik S, Peroutka SJ, Ashkenazi A: A single amino-acid difference confers major pharmacological variations between human and rodent 5-HT_{1B} receptors. Nature 1992; 360:161-163.

Neuropsychiatric disorders such as anxiety, depression, migraine, vasospasm & epilepsy may involve different subtypes of the 5-hydroxytryptamine

(5-HT) receptor. The 1B subtype, which has a unique pharmacology, was first identified in rodent brain. But a similar receptor could not be detected in human brain, suggesting the absence in man of a receptor with equivalent function. Recently a human receptor gene was isolated (designated 5-HT_{1B} receptor, 5-HT_{1DB} receptor, or S12 receptor) which shares 93% identity of the deduced protein sequence with rodent 5-HT_{1B} receptors. Although this receptor is identical to rodent 5-HT_{1B} receptors in binding to 5-HT, it differs profoundly in binding to many drugs. Here we show that replacement of a single amino acid in the human receptor (threonine at residue 355) corresponding asparagine found in rodent 5-HT_{1B} receptor. In addition, these findings show that minute sequence difference between homologues can cause large pharmacological variation. Thus drug-receptor interactions should not be extrapolated from animal to human species without verification.

Abstract of review article: Thompson JN, Burdon JJ: Gene-for-gene coevolution between plants and parasites. Nature 1992;360:121-126.

The concept of gene-for-gene [GFG] coevolution is a major model for research on the evolution of resistance against parasites in crop plants, reciprocal evolution between species in natural plant populations, and mathematical models of the dynamics of coevolution. Recent studies have begun to challenge the prevailing view that natural selection within local plant populations is the major evolutionary process driving this form of coevolution. The emerging pattern from these studies suggests that metapopulation structure, including the effects of gene flow and genetic drift, may be at least as important as local natural selection in determining the genetic dynamics and outcomes of these evolutionary arms races. [GFG coevolution is defined as "for each gene that conditions reaction in the host there is a corresponding gene that conditions pathogenicity in the parasite."]

Review of Bateson Festschrift

by JS Price

I've just been reading the Festschrift for Gregory Bateson[11] and was interested to read that ethology was a major influence on Bateson's research group in the decade starting 1952. In a chapter headed "One thing leads to another" (pp43-64) John H Weakland (Dept Psychiat, Stanford U) says of their approach to schizophrenia: "In getting into this, somehow or other, we were looking at all these things from a natural history, ecological approach. We used this viewpoint partly because both Bateson and I came out of anthropology, and partly because somehow we'd gotten into reading Lorenz and Tinbergen on animal ethology".

If we accept that family therapy developed out of Bateson's group, then its birth depended on two coincidences. One was that Chester Barnard, then head of the Rockefeller Foundation, had read Bateson's Naven[12] about his work with the Iatmul, in which he starts to wrestle with the ideas of symmetrical and complementary relationships, and so provided funds for what would otherwise have been such a vague project as to be unfundable (its title being "The Significance of the Paradoxes of Abstraction in Communication"); the other was that premises happened to be found for them in the VA Hospital in Menlo Park (near Palo Alto) where there was no shortage of schizophrenic patients — otherwise these two anthropologists might never have got involved with psychiatry and the double bind might never have been formulated. They were joined by two psychiatrists, Bill Fry who was just starting out, and Don Jackson, who had been practising in Palo Alto for some time; also by Jay Haley, with a background in communication theory.

Even then they did not limit themselves to the communication of psychiatric patients. Among other things, they studied a ventriloquist and his dummy, and the training of guide dogs, "interested in how the trainers would deliberately give two different and contradictory messages at the same time"; and they watched

otters and other animals in the zoo, noting particularly the otter's "play face" which gave a context for the interpretation of the following signals, and gave them an experience of "metacommunication".

So their theoretical approach was a mix of anthropology, cybernetics, communication theory and comparative ethology. From these origins they developed the "interactional approach" which emphasised the recursiveness of communication, and substituted the systemic approach for the linear epistemology of classical psychiatric theory and description. Psychopathology was seen as a function of the interaction rather than of the individual (whether "mentalistic" or physiological). From this seed, family therapy developed. And this influence of comparative ethology on the theoretical underpinnings of family therapy may be why the latter seems, of all approaches to psychiatry, the most in tune with our evolutionary perspective.

The title of the Festschrift is a quotation from Bateson: "(as each year passes) we shall know a little more by dint of rigor and imagination, the two great contraries of mental process, either of which by itself is lethal. Rigor alone is death, but imagination alone is insanity".

Here is another quote, from a chapter entitled "Forming warm ideas" (pp 65-81) by Arthur P Bochner: "The only common denominator among imaginative theorists seems to be artful dodging"!

Bochner (then an Associate Professor of Speech at Temple U) extracted from Bateson's work what he called:
Bateson's Rules of Thumb

1. Study life in its natural setting being careful not to destroy the historical and interactional integrity of the whole setting.

2. Think aesthetically. Visualise, analogise, compare. Look for patterns, configurations, figures in the rug.

3. Live with your data. Be a detective. Mull, contemplate,inspect. Think about, through, and beyond.

4. Don't be controlled by dogmatic formalisms about how to theorise and research. Avoid the dualisms announced and pronounced as maximums (sic) by particularising methodologists and theorists. (They'll fire their shots at you anyhow).

5. Be as precise as possible but don't close off possibilities. Look to the ever larger systems and configurations for your explanations. Keep explanations as close to your data and experience as possible.

6. Aim for catalytic conceptualisations; warm ideas are contagious ... by warm ideas I mean ideas that compel us to move closer to our subject matter; ideas from which we can cast new rays of insight, open up new lines of thought, extend our territory into new avenues of enquiry, and amplify our understanding beyond what we knew before.

And Bochner urges:

1. Scientific activity is recursive. To see phenomena a scientist must transform them; having transformed them, he or she is transformed by them.

2. Data cannot tell us what to ask of them, nor what they mean. This suggests that the meaning of data is never beyond challenge, never closed to other meanings, never capable of absolutely falsifying or verifying. What we do to and/or with data is an intellectual activity.

3. Ideas are as important as facts and nowhere is it evident that they are inducible from them (facts). We need imagination not rules; intuition not technique; warm ideas not cold facts. Particularly in the social sciences where the entire question of measurability of phenomena and lawfulness of behaviour is so troublesome, what we need is inventive people not conformists, fertile

thinking not rigid rules to follow.

Finally, a chapter by WB Pearce, LM Harris and VE Cronen (from Dept Communic Studies, U Mass) entitled "Communication theory in a new key" (pl49-194) gives a definition of relationship in terms of episodes:

"[T]he development of a close personal relationship may be described as a movement from employing episodes as the context for defining the relationship to the evolution of relationship concept that forms the context for assigning meaning to episodes" (pl66). They make a rather incomplete attempt to make a classification of episodes, describing enigmatic episodes, episodes characterised by unwanted, repetitive patterns and episodes of value-expressing ritual. They do not include episodes of agonic interaction in their list, but they do in fact give a case history of a married couple in whom there is a cyclical pattern of "conflict" episodes (about their level of closeness) followed by "confrontation" episodes which feature" self-disclosure, validation and acceptance in which they try to eliminate the conflict episode".

They point out that the pattern of interaction is the same as one described by the Bateson group, in which there is a cycle of intrusiveness and withdrawal, punctuated by the intruder as intrusiveness caused by the other's withdrawal, and by the withdrawer as withdrawal caused by the other's intrusiveness.

This is a case of a switch into the agonic mode due to conflict over the definition of the relationship on Birtchnell's horizontal axis of closeness/distance, another example of the complex interaction of attachment and agonistic behaviour. The same group describes elsewhere another example of switching from mode to mode, mentioned in my contribution to the World Futures symposium.

Before leaving the paper by Pearce *et al,* I should mention their splendid double-bind in which Jan says to Dave, "I want you to make me more assertive". They use a lot of algebra which I find incomprehensible, but the text makes sense without it. They are very concerned with logical levels (one reason for their affinity with the Palo Alto group) and recommend "...the process of tacking back and forth between higher and lower levels of meaning" (p 169). Also they are interested in the interface between interactional description and the description of the mental states of the interactors, which has been a subject of discussion in the Birmingham group: "When two or more persons exchange interactions, the "logic" of the interpersonal system consists of the interface of the logics of each person's intrapersonal system" (p 168). So, here is a group which studies dyadic interaction in long-term relationships, and who have discovered and described the process of switching back and forth between the agonic and hedonic modes.

The volume also contains an important paper by L Edna Rogers (at Dept of Communic, Cleveland St U) entitled "Symmetry and complementarity: evolution and evaluation of an idea" (pp 231-252). "In a complementary transaction the interactors' behaviors are fully differential. The relational control definition offered by one interactor is accepted by the other. In a symmetrical transaction one interactor behaves toward the other, as the other behaves toward him. There is a similarity of control definition between the interactors" (p239).

There are two types of symmetrical relationship, an agonic one characterised, eg, by mutual boasting; and a hedonic one in which "an ongoing reciprocity of asymmetrical exchange produces a symmetry of relationship over time" (p 235). Lacking the two modes concept, the interactional

researchers clearly have difficulty conceptualising these two types of symmetrical relationship.

Since Palo Alto was such an important fountainhead of ideas both for family therapy and for communications researchers, it might be helpful to clarify just what "Palo Alto" was. As far as I can gather, there were three different but related organisations working in/around Palo Alto:

1. Bateson's research group at Menlo Park, operated from 1952 to 1962, when Bateson left psychiatry and went to study dolphins.

2. The Mental Research Institute, founded by Don Jackson in 1958 — (is it still going?)

3. The Brief Therapy Centre, a subdivision of the Mental Research Institute, whose work is described in "Tactics of Change."[14]

In a final chapter entitled "The charm of the scout" (pp 357-368) Stephen Toulmin (Prof Soc Thought and Phil at U Chicago) likens Bateson to an American Frontier Scout, "leaving the safety of the settlement and reappearing unpredictability, bringing a mixture of first hand reports, rumors and warnings about the wilderness ahead--together with a tantalizing collection of plant specimens, animal skins, and rock samples..." He also likens him to Francis Galton--neither got a Ph.D. or held a tenured university post, and so crossed disciplinary boundaries without constraint: "...all of his notable contributions to science have sprung from his habit of viewing the mental life and behavior of creatures as functional, adaptive activities that need to be intelligently related to their evolutionary history and habitat.

Response to Beck by Paul Gilbert

Tim Beck's contribution to our understanding of psychological suffering has been profound and his work will echo in history for a very long time. Not content with introducing one of the most important psychotherapeutic paradigms of this century he has now set sail to explore how various of our evaluative schemata have innate origins and to apply these insights to new conceptualisations of personality disorder.

His first major exploration of this issue came with his analysis of anxiety in which he suggested that various cognitions activated innate defensive responses (e.g., flight-avoidance, demobilisation). The work on personality disorder followed from this and also his concern with the tactics (strategies) of sociotropy (effort to elicit support and reassurance from others) and autonomy (efforts to be self-sufficient and independent). These he sees as two basic dimensions--which is in line with other theories. My comments on his Apr 92 ASCAP paper reflects the fact that these new explorations and developments now need to be opened up.

Points of Agreement

1. That variations in biosocial goal selection and social strategies are expressed among individuals in a population is in line with our understanding of personality differences and the importance of variation on which selection can operate.

2. The phylogeney of core schemata reflect the consequences of evolutionary pressure that make possible a commonality of biosocial goals, evolved stable strategies and innate algorithms. This view can be traced back to Jung's concept of archetype. This idea of internal universal aspects to the human mind can in turn (admittedly stripped of its evolutionary focus) be traced back to Plato's doctrine of universals and Kant's concept of schema.

3. In comparing individuals we can note that some biosocial goals and strategies are amplified, overdeveloped while others seem to be in-

hibited or underdeveloped. This incidently introduces the importance of development and opens up cognitive theory to the study of the maturation of cognitive-evaluative abilities.

These points of agreement greatly outweigh our differences, but difference is the source of dialogue.

Points of difference

There are a number of areas where I would take a slightly different approach. The most general point is that Tim has worked from cognition to evolution, whereas many evolution theorists in this area work in the other direction.[16,17,18]

1. I am not sure it is wise to classify strategies on the basis of DSM. In the first place most research suggests that these DSM classifications (of dependent, narcissistic, etc) do not actually exist in any clear form but rather people show various mixtures. I would lay the same criticism at some of my own work, and I am only just beginning to realise how misleading the DSM may be for our approach.

2. I would make a clear distinction between two fundamental types of internal system. The first codes for threat/harm or the degree of safeness in the environment (defensive and safety). The second are more specific types of social evaluative systems, that utilise particular types of algorithm and code for the interpretation and expression" of social roles (eg, care-eliciting, cooperation). Defense and safety systems are basic organising systems within the brain that shape the maturation, development and organisation of interpersonal schema. In fact, Beck, *et al,*[15] seem to agree on this point and although they give little attention to the safety system they make it clear that anxiety relates to both the perception of threat and the specificity of the (social) domain. Thus, one needs both systems - one for coding for threat and one coding from the more specific social behaviours.

3. Cognitive therapists love to generate lists and they are very good at it too! But I do not think it is sufficient to generate lists of dysfunctional schemata, some of which seem primary and others not and then suggest these are all basic evolved strategies. At times, some seem more like symptoms. For example, Tim suggests that in obsessionals, spontaneity is underdeveloped. But this could be symptom of compulsive self-reliance and intense sensitivity to put down, or fear. Spontaneity (or lack of it) may be a symptom and not a basic strategy - the basic strategy might be avoidance of ridicule and constantly checking before doing.

4. Evolutionary theory is currently concerned with three basic functions (biosocial goals, strategies, and algorithms) which evolved because they solved problems and gave reproductive advantage.

A) *Biosocial Goals:* In general, these turn out to be finite and relate to establishing control over resources. Among the commonly agreed ones are; (i) attachment, (ii) alliance formation, (iii) mate attraction and retention, (iv) dominance and ranking. Goals may be experienced as ideals or wants, something one is aiming for.

B) Strategies: To get any of the above biosocial goals one might use strategies of aggression, deception, appeasement, altruism/helping/caring, or affiliation (or some combination). These seem the most common strategies.

C) *Algorithms:* These represent information processing routines. They help direct attentional mechanisms and are the evaluative of reasoning modules; that is how information is evaluated vis a vis the goal that is being pursued. Typical algorithms are concerned with proximity-distance, reciprocation, social comparisons (e.g., of same-different as

in ingroup-outgroup and inferior-superior as in ranking).

Thus, evolutionary theorists make important distinctions between desires or wants and knowledge beliefs and strategies, which cognitive theorists fail to do.

5. Different goals, strategies and algorithms might link up in certain ways. For example, attachment might use algorithms for proximity-distance/separation. In alliance formation, identifying potential allies might use the algorithms of social comparison of same-different. Alliances are more likely to be developed with those seen as similar to oneself--aggression is different according to whether it is directed at an ally or non-ally. With maturation, the judgements for choosing allies may change and centre on shared values (eg, political, religious, academic, etc). The ranking goals might depend on algorithms for working out relative differences of strength and/or ability and use the social comparison of inferior-superior, weaker-stronger. Thus, we can distinguish among: (i) what I want; (ii) making judgements of how far-close (gaining, moving towards - losing, moving away) from my goal; (iii) what behaviours (strategies) will I use to get to my goal.

To show how this might work consider the following: I want status, money, and prestige (biosocial goal). I judge whether I am getting this by socially comparing myself with others and with my own standards, and being attentive to the type of attention others pay me and what opportunities are coming my way (basic algorithm). I select an optimum mixture of aggression, affiliation and deception strategies to keep me going up. Not too much aggression to alienate others but enough to avoid being done down, enough affiliation to make life comfortable and pleasant and gain allies, and a little deception, but not so much that I could get caught out. We can do the same exercise with other goals like attachment or mate selection.

Now in personality disorders, dysfunctions can show up in either the biosocial goal, the way the algorithms are working (eg, sensitivity to put-down or rejection) and the strategies that are chosen (eg, too much appeasement or too much aggression or too little affiliation, etc). Of course, this is a very crude approximation, but it might get around the problem of lists that are disconnected from evolved goals and strategies.

6. Particular innate systems such as ranking or attachment may give rise to different presentations but may nevertheless share a focus in a specific system. for example, attachment theorists suggest that problems in attachment behaviour can give rise to individuals who are clingy and anxious of separations, while others may show avoidance and distance - yet both types represent attachment dysfunctions. Thus, one can't assume that different personality disorders are related to different problems in different biosocial goals. Thus, in one it might be too much need for attachment and proximity and avoidance of risk of rejection, in another it is too little, or it might be due to denial, ie, some researchers suggest that violent men deny (defensively exclude) emotional and dependency needs and are threatened by them.

7. If one works in this way, then we can begin to classify personalities in terms of poor maturation (over and underdevelopment) of a few specific but basic biosocial goals, algorithms and strategies. For example, the narcissist and the psychopath share heightened sensitivity to rank judgements (and social comparison is a major way of organising social information). Both strive for control over others (the

superior position), but with the psychopath more likely to use aggressive options of response and less likely to feel empathic inhibition to the distress they cause others. Narcissists are more likely to be verbally abusive if their status is threatened, whereas, the psychopath is more physically impulsive. But both show poor alliance formation in the area of empathy for other and are exploitive; that is, there is poor cooperation, mutuality and reciprocal sharing behaviour. And neither openly acknowledge their attachment/ dependency and need for others (well, actually narcissists can, but in a highly idealised fashion). Indeed, the overlap between these two types have given rise to an intermediate category of maligant narcissist.

This dependent and avoidant type also show problems in ranking behaviour but take up the subordinate position. Both have problems with assertive behaviour and engaging in the competitive rituals of life. In regard to attachment, they are very focused on this goal and prone to seeking out others for support.

Thus, we might have Figure 1:

Dysfunctional Biosocial Goal	Underdeveloped (inhibited?)	Overdeveloped
Attachment:	narcissistic psychopath	dependant, avoidant
Alliance:	narcissistic psychopath (non-forgiving?)	dependant over-forgiving?)
Ranking:	dependent	narcissistic

To give the full overview of such an approach, we would have to have the three dimensions of biosocial goals, strategies and algorithms, but maybe this gives a flavour.

Hence, if we consider a rather limited number of possible biosocial goals (eg, (1) attachment, (2) alliance formation, (3) mate attraction and retention, (4) dominance and ranking, basic algorithms (proximity-distance, social comparison, etc.) and strategies (aggression, deception, appeasement, altruism/helping/ caring, affiliation), we may begin to see how various *patterns and combinations* (of biosocial goals, strategies and algorithms) give rise to different types of problems. Thus, although Tim has set us on a journey that holds much promise, and opened up again new horizons, we should now begin to try to 'peel away at this onion' and get at the very basic core of these innate schemata. I suspect that such an approach will offer a rather different understanding of the underlying dynamics of personality disorder than now presumed in DSM.

Review of Gilbert's Depression: Evolution of Powerlessness[20b] by JS Price

This book of Paul's is a major and important work, the first systematic attempt to apply evolutionary thinking to the psychology of mood states. The evolutionary perspective highlights and enriches some existing approaches (I was particularly enlightened by his review of Kohut's Self-Psychology), and brings in some entirely new thinking about depression. His comprehensive literature review throws up the fact that there just is no psychology of ranking behaviour, and practically no psychology of group membership behaviour. These are matters which are too important for people's lives to permit experimentation in the laboratory. If we want to read about them, we have to turn to fiction.

He is particularly strong on evolutionary change in competitive behaviour, and points out the way competition by attraction has come to overlay, but not replace, competition by intimidation--a point that few social scientists apart from Barkow and Kemper seem to have grasped. He has made the original suggestion that in competition by attraction we need a concept to replace RHP (a measure of

intimidatory capacity) and he has suggested Social attention-Holding Power (SAHP)--a measure of social attractiveness, or capacity to elicit prestige. I think this is a useful development, and it needs more thought to define it carefully and delineate its relation to other mental mechanisms. Incidentally, it should be listed in the index under SAHP, as the person who runs across SAHP in the text does not know it is Social Attention-Holding Power until he has looked it up in the index, and he can't do that until he knows what the initials stand for.

Another criticism is the neglect of the ideas of Alfred Adler in the context of a theory of involuntary subordination and inferiority. Adler stressed the importance of the drive to self-perfection, which could be greatly enhanced by feelings of inferiority, such as about a physical defect (as he knew from experience), and he called this enhancement "compensation". But the drive to self-perfection could deteriorate into a drive for superiority over others, and if this drive was blocked, the result could be neurosis. For "drive to self-perfection" we could substitute "aspiration for high SAHP" and for "drive for superiority over others" we could substitute "aspiration for high RHP". Since RHP is part of the mentality entrained by the agonic mode, and SAHP part of mentality entrained by the hedonic mode of interaction, we could say that Adler adumbrated Michael Chance's admonition to cling to the hedonic mode and avoid the agonic.

On p464 there is a paragraph headed "flight into health", which (Paul told me on the phone) means recovery from depression while the underlying problems have not been resolved. I think this is a pernicious idea masquerading under the guise of a clever paradox. Since the act of fleeing is largely voluntary, and the destination of flight is usually chosen by the flyer, the concept of flight into health implies that depressed patients can choose to flee into health if they so wish, which means that, if they have not done so, they have chosen to remain depressed, which puts them into the category of malingering. The implication is the same as exhortations to "snap out of it" or "pull yourself together", implying conscious control over depressed mood. The depressed patient is therefore to blame for the depression, and is made to feel guilty, which is likely to make them feel more depressed, another example of the circular interactive processes which may maintain depressed mood.

Since the book was written over two years ago, it does not reflect our current thinking on the implications of ranking theory for treatment. Depression is seen as involuntary subordination, which occurs because cognitively based behaviour has not managed to get the individual into a social situation in which the triggers for involuntary subordination are not pulled. These triggers are some function of low or falling RHP/SAHP and/or quantity of punishment or frustrative non-reward experienced in unit time. From a ranking point of review, there are two causes for this situation. One is that the person who ranks higher is a bully, and the individual is therefore suffering from what might be called ranking abuse, or excessive down-hierarchy pecking. The other is that there has not been voluntary acceptance of whatever rank the individual finds himself in, and this results in various kinds of rebellious or insubordinate behaviour which elicit putting-down behaviour from others. In a systemic situation, there is ranking mismatch. Let me use again the analogy of temperature control. If we equate putting-down be-

haviour with cold, the depressed patient is like someone who is shivering. He could have avoided the shivering if he had switched on the central heating, or if he had got out of the situation into a warmer place. Therapy of the shivering person then takes the form of exploration of why he did not turn on the central heating. Was he too mean? Did he not understand the switch? Had he forgotten to order fuel? and why had he not been able to go somewhere warmer? These approaches are more effective than giving him curare to reduce the muscular contractions of shivering. Similarly, with the depressed patient, why is he staying in a situation which is causing him grief, in what way is he entrapped? And, if he cannot get away, why has he not yielded voluntarily, to turn away the wrath of whoever is punishing him? If he is being forced into an unacceptable inferior position, or being asked to accept unjustifiable punishment, and if he cannot get away, he may need help in mobilising support for voluntary resistance, in the way that on a larger social scale exploited workers formed into unions - although this may be very difficult in some situations, eg, marriage. In short, the occurrence of an involuntary yielding response means that for some reason the voluntary yielding response, which would have pre-empted the involuntary response, has not been made. It is the task of therapy to find out why, and to explore alternative strategies. I think this is the main contribution to therapy of what Paul calls ranking theory.

Paul's second book[20a] is already influencing sociological thinking about depression in an evolutionary direction. Brian Cooper, for instance , quoting Paul's book, talks of "primary capacity for construing social interactions, analogous to those which have been postulated by universalist theories of language acquisition and moral development...whichever direction such research may follow, one can safely predict that it will have to pay regard to man's evolutionary background." This is quite a shift in sociological thinking. The case is not yet won, as evidenced by Cooper's later comments:

> ...a comparative ethology that served merely to reinforce existing notions about the mainsprings of human social behaviour could for practical purposes be dispensed with as superfluous, on the principle of Occam's razor. If there were no more to it than that, sociological scepticism would be fully justified. The crucial question is: do we find evidence for the existence of phylogenetically determined biosocial drives, whose expression in psychopathology is definable, and cannot be equally well explained in terms of individual development and learning? Only if this is the case does it seem realistic and useful to speak of an ethological psychiatry. For the present, however, this question must remain open.

Hopefully, this third book of Paul's will continue to win converts for evolutionary/ethological psychology and psychiatry.

I can say without hesitation that anyone who has the kind of interest which lead them to read ASCAP would benefit from reading Paul's new book, from cover to cover, at least once.

Bickerton-Bichakjian Exchange

by Derek Bickerton

Thanks for sending me a copy of the June issue of the ASCAP Newsletter, and my sincere apologies for not having answered your letter sooner, as I was away from here from May through mid-August and did not have my mail forwarded. The material was fascinating, although the accidental omission of p2 (including, as I gather from the contents, Bichakjian's letter, which was the trigger for the whole thing) made it harder for me to follow.

With regard to your specific inquiries, yes, you very succinctly sum-

marize the differences between protolanguage and language with only one potentially misleading point: it's not that protolanguage lacks mull elements and language has them, so much as that both have them, but the null elements of language can always be interpreted by rule, whereas those of protolanguage are wholly unsystematic and can only be interpreted by context, pragmatic knowledge, etc etc. I wish everybody else understood me as well.

Bernard Bichakjian's (SB's) claim that I am an essentialist not an evolutionist is the waste product of a male farm animal. He is obviously way behind the times and assumes that an evolutionist has to be a gradualist--not any more, if ever! (Even Darwin is ambiguous on this point, as on so many others). Claiming that I advocate a language-forming mutation 'comparable to a would-be direct mutation from monad to man' the worst kind of disinformation. Given that erectus had protolanguage--just the kind of development that happens constantly in evolution as animals try out new behaviors and genetic developments that enhance such behaviors are selected for strongly and rapidly.

BB is simply mistaken when he says that recapitulationism 'proved to be wrong' without qualification. It proved to be wrong in some things and right in others--it failed simply as a meaningful generalization. Whether it is right in any given case is an empirical question not to be dismissed out of hand by appeals to authority (especially when authority's two-faced!). To accuse me of nineteenth century romanticism comparing 'primitive' societies with contemporary infants is nonsensical, as BB must know--I am comparing modern infants, not with 'primitive' humans, but with members of prehuman and nonhuman species, and I am not comparing them across the board, but with respect to one particular feature--the way in which they communicate. In denying that this is serious scholarship, BB is sailing perilously close to actionable libel.

If BB knew anything about pidgins, he would know they are NOT 'mutilated languages' but attempts to start up communication virtually from scratch which scraps of old languages may be used from time to time in order to eke out basic (and presumably innate and hominid-specific) communication strategies. Would he put up his man-in-the-street mininformation against experts in other fields, or is this just another case of pidgins, Don-Rickles-like, 'getting no respect' because they are usually spoken by the underprivileged and powerless?

Finally, a few words about BB's gradualism. Gradualism is a religion, whereas catastrophism isn't. That is, because gradualists have an a *priori* commitment to the faith that there are no catastrophes in nature. In contrast, no catastrophist ever has or ever will believe that there is not gradualism in nature. Whether a particular process is gradual or catastrophic is a purely empirical question. BB won't accept this, therefore 'as you travel back in time sytactic functions become less... grammatical"! But there's not a scintilla of evidence for this, and as I showed in Language and Species, what (highly indirect) evidence there is points the opposite direction.[22] BB might bear in mind the following. Today, there is tremendous disparity in the degree of development of cultures, from Andaman islanders who never discovered how to make fire to Western civilization with its supercolliders and interplanatary probes. But there is no disparity at all in the degree of development of languages--all, so far as anyone can determine, are equally complex. If both language and culture is now spead right across the spectrum from

the primitive to the postmodern, while languages have, apparently all advanced lockstep to their present level of sophistication.

But evidence seldom shakes a true believer, so I guess BB will continue to excoriate me for not looking at what is not there.

I see that I seem to have satisfied your request for some exchanges on these issues. Please feel free to include this letter, or parts of it, edited as you think fit, in the ASCAP Newsletter, if you desire.

Thanks again for giving me the opportunity to get involved in this discussion — please keep me informeed if it continues.

Derek Bickerton, U Hawaii at Manoa

Communication Amongst Linguistics RG

In the June issue of ASCAP, the conclusions of two well published linguists, Bernard Bichakjian and Derek Bickerton were compared. Both are linguists; both are interested in evolution; both, I conjectured, might be interested in discussing a potential continuum in the development of language, with Bickerton's protolanguage first followed by Bichakjian's ancient-modern language comparison.

Both also from this non-linguist's point of view had highly creative ideas, but how can the non-expert evaluate the evidence for the non-expert as each defends himself vigorously and in colorful language. Perhaps the word to describe best the BB/DB language and thrust of message is 'catathesis', noun version of 'catathetic'. John Price provided a definition in an early issue of ASCAP (Vol 1, #2, p3):

> What I have tried to do, in suggesting the term catathetic (an adjective to be applied to signals, messages and behavior), is to find a term which will cover all kinds of "putting down" behavior, whether they consist of words or blows or whether they occur in man or animals.

Both linguists illustrate use of catathesis--when one enhances one's own RHP by diminishing that of the target; anathesis, its opposite counterpart, occurs when the target has its RHP also enhanced.

In addition to the actual issues, we are perhaps here involved with a tradition, as linguistics seems to be a field often characterized by such colorful damnation.

Here in ASCAP, we seem to make less use of catathesis; we need to feel ingroup with respect to the enemies of ignorance and the disinclination many have to consider evolutionary factors in behavior. Anathesis--for the moment--works better.

Note on Slavin & Kriegman's Adaptive Design of Human Psyche; Psychoanalysis, Evolutionary Biology, Therapeutic Process. by J Pearce

This technical book relates evolutionary thinking to an extensive but technical psychoanalytic literature that I, alas, no longer follow. It would be worthwhile hearing an opinion about how they have done from one of our colleagues who does have a command of that literature.

Slavin and Kriegman address the special problems of doing evolutionariy sophisticated therapy that has some relationship to psychoanalytic traditions; in particular, with frequent sessions, 3 to 5 visits per week, lasting many years. The rest of us now do therapy on a much less visit-intensive basis--a very different practice. They are clearly very smart and experienced—admirable guys. Would they be willing to write a second book, one that would not imprison their wisdom in a psychoanalytic framework? They have, as leaders of an Institute of the Amer Psychoanalytic Assoc, good reason to write a technical book for their psychoanalytic peers. Would that they would write a more process-oriented, descriptive book for the rest of us.

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2. c/o R Gardner, 1.200 Graves Building (D29), University of Texas Medical Branch, Galveston, TX 77555-0429 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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At this time this "informal" organization has no official budget.

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