

THE ASCAP NEWSLETTER

Across-Species Comparisons And Psychopathology Newsletter

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"Nature is indeed diverse, awe inspiring, and beautiful, but we are now well beyond the broad descriptions of entire landscapes and in an era of pragmatism, function, and experimental testing of hypotheses."
George L Gabor Miklos¹

Newsletter aims

1. A free exchange of letters, notes, articles, essays or ideas in brief format.
2. Elaboration of others' ideas.
3. Keeping up with productions, events, and other news.
4. Proposals for new initiatives, joint research endeavors, etc

The ASCAP Newsletter is a function of the International Association for the Study of Comparative Psychopathology²

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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 psycho-pathologically related states. We are interested in the integration of various methods of study ranging from cellular processes to individuals to individuals in groups.

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Announcement

The 1994 meeting of the Human Behavior and Evolution Society will be held at the U Michigan, Ann Arbor, June 16-19. Abstract deadline is 3/15/94..

Time for new subscriptions

Subscriptions are rolling in for The ASCAP Newsletter, Vol 7, No.s 74 through 85, Jan-Dec, 1994. If you have already subscribed, please ignore the cover form; but if you have not, please subscribe now. Send American dollars or use a credit card number: UTMB's accounting no longer accepts foreign currency. For using the credit card mechanism, we will accept Master-Card, VISA, Discover or American Express. We need the card type, number, expiration date, & your signature.

Planning for Philadelphia

The subscription forms include a question whether you desire to be included in a possible block of rooms we may try to reserve in a hotel near the **IAS-CAP** meeting site (5/21-22/94) and not too far from the later

meeting of the American Psychiatric Association (APA) which starts 5/23/94. Positive answers are coming our way. Let us know as soon as possible for an accurate count.

Paul Gilbert and Aaron Beck have corresponded about the suitability of the Center for Cognitive Therapy as the site for the first formal meeting. Unfortunately the Center is moving within the U Penn and so cannot host the meeting. But we are continuing to work on sites even closer to the APA meetings so that attendees may easily go to both.

Letters

October 21, 1993

I had dinner twice with Dr Giovanni Liotti who has been in Toronto to attend the Attachment Conference where he presented. He is at the Association for Research on the Psychopathology of the Attachment System (ARPAS) in Rome. He is a reader of ASCAP. I see him as being more committed than anyone else I know to applying attachment theory and social hierarchy theory to his clinical work with patients. Like Paul [Gilbert], he also sees this approach as being very compatible with the cognitive approach to psychotherapy. I found him to be very pleasant and knowledgeable, and also receptive to our ideas about social hierarchy mechanisms. He also shares our interest in spelling out the precise interrelationship between social hierarchy and attachment.

Both he and I would like to see ASCAP devote more space to issues of child development, recognizing that we should perhaps make contributions.

Leon Sloman, Clarke Inst, Toronto

Letters (continued)

November 9, 1993

I haven't been in touch for awhile but I continue to read The ASCAP Newsletter religiously. I thought that you - and possibly the readership - might be interested in the following letter that I sent to the editor of the New York Times Book Review Section. This section had a series of articles on the seven deadly sins plus an article on

despair which the author thought should be included in the series. I thought that it might be fun to take a crack at this subject from an evolutionary standpoint and so I sent in the letter below.

I presume that other people have taken a similar approach to the origin and control of "sins" from an evolutionary standpoint but I have not found any such references or allusions as yet. If anybody can refer me to such sources, I would appreciate it and of course, I always enjoy having feedback on my various sallies into evolutionary psychology.

Aaron T Beck, Center of Cognitive Therapy, U Penn, Philadelphia

Seven deadly sins plus despair

by Aaron T. Beck

(The following replicates a letter to the editor of the New York Times Book Review Section.)³

Your series [in the NY Times] raises some interesting questions: Why are these quintessentially human traits labeled as sins? Why was despair added to the list?

Actually, the deadly seven can be construed as derivatives of primitive adaptations of our ancestors

to prehistoric conditions in the wild. In the competitive struggle for limited resources, egocentric behaviors fostering survival and reproductions were favored by evolutionary pressures. With its emphasis on individual fitness, a kind of master genetic program directed and rewarded (by conferring pleasure) the expression of the prototypes of what we now call sin. Thus the acquisition of resources (for example, food) would be served by avarice and gluttony; the competitive struggle would be sharpened by pride over success and envy over others' success; conservation of energy would be facilitated by rest (sloth), and reproductive success would obviously be served by lust. Anger would be crucial for individual protection. Similar "sinful" characteristics can be discerned among nonhuman primates.

Why are these evolved traits and behaviors regarded as sins? Simply stated, the egocentric dispositions that serve one individual can hurt others. The excessive expression of one's own evolutionary strategies (for example, feeding, fornicating

and fighting) deprives or injures other people. Preoccupation with personal satisfactions (for example, pride and sloth) ritualizes cooperation and reciprocity. Thus, to preserve the social order, society has imposed sanctions on these biologically based behaviors, particularly when flagrant.

But despair seems to be out of place in this group. It certainly does not detract from other people as does, say, gluttony or pride, nor does it seem to be an evolutionary strategy. However, analogues to despair can be found in the animal kingdom: a dethroned dominant male chimpanzee will display behavior similar to human depression -withdrawing from the band and then returning after a considerable period of time. In humans, mild or time-limited depression could be adaptive in forcing a reconsideration of one's goals and strategies.

The problem with despair is the same as that posed by the other asocial traits: they are detrimental when excessive. Despair could result in irrational suicide (found only in humans, by the way). The elimination of a key member of a group hurts the entire group. Thus, despair (when excessive) could conceivably be listed with the other seven "sins after all. When excessive, despair hurts other people, but, like the other sins, it can be modified with social and psychological assistance.

Letters (continued)

2-12-93

Michael Chance, my early mentor and friend, advised me to write to you concerning membership of ASCAP. After having received a complimentary copy of the ASCAP Newsletter it felt like a breath of fresh air to read it. Having now read my second Newsletter (which graciously included an edited version of my letter to Michael), I am most keen to join the Society and be put on the mailing list for the Newsletter...

Arnold Keith Dixon, Sandoz, Berne, Switzerland

As you see from the cover page, subscribing is all that is needed to join IASCAP. Welcome to the group. Your first contribution was a hit and we look forward to added communications from Berne! Please send copies of your papers so we can

reprint them. Also your experience and perspective may significantly help us towards a new name.

Letters (continued)

6 December 1993

... I enjoyed your comments on the issue of the new name. It will be interesting to see if anybody responds to that.

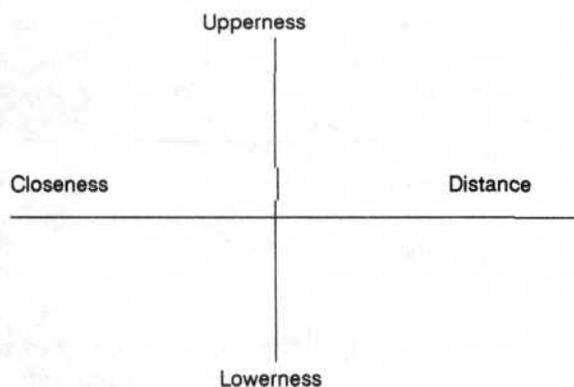
Paul Gilbert, Derbyshire, England

Message from John Birtchnell

... I do not have the same preoccupation with evolution that you [RG], John [Price], Leon. [Sloman], and Paul [Gilbert] do and I don't think I ever will but I discovered via John, Paul and yourself that my theory fits into an evolutionary context and is reinforced by it. I think perhaps this applies also to Aaron T Beck and Isaac Marks.

Up to now I feel that John, Leon, Michael [Chance] and Paul have aligned themselves too much with the vertical axis and have seen evolution too much in vertical terms [see figure]. As I have said before, I believe it to be significant that

Figure. Spatial Model of Interpersonal Relating



there was scarcely a mention of Bowlby, also a great evolutionist, in *Social Fabrics*.⁴ Bowlby is (in my view) essentially a horizontal thinker and sees evolution in such terms. I was delighted when Leon, a few issues back, put in a plea for what appeared to me to be a reconciliation of the two axes. In John's latest contribution to the Birtchnell-

Price controversy, he reasserts his claim that evolution is essentially a vertical process. In their joint contribution in the present issue, John and Leon begin to make some concessions to the horizontal axis, but continue to maintain that the vertical axis is the most important. It's interesting that they avoid using my terminology. It seems that one of my functions in IASCAP is to keep pressing for an integration of the two axes.

I note in the last issue you bracket me with Seymour [Itzkoff] as upholding the need to emphasise the large gap between humans and other animals, and it seems that another of my functions is to caution evolutionists against too easily making connections between animal and human behaviour. Continuing to use terms such as to dominate and to affiliate is a means of maintaining humans at the animal level. One of my reasons for introducing terms like upper and close is that they incorporate behaviours such as love, kindness, compassion, etc which are essentially human. I was interested that you used the term basic plan to describe this eagerness to see humans as slightly modified animals, because I was beginning to see my theory as representing a more complete form of basic plan. The concept that human behavior has grown out of animal behaviour is one that binds us all together. The issue on which we differ is how big the jump has been.

Related to this I think is the extent to which we like to be preoccupied with animals, how much our libido we wish to invest in animals, or even into primitive humans. There is a useful concept in Gestalt therapy that a preoccupation with the past is a way of avoiding the present. In my terminology, I would consider this a form of distancing. Although there is great value in establishing continuities between animals and humans, there are dangers in seeing humans as slightly modified animals. This is related to the kinds of criticisms which humanistic psychologists direct towards experimental psychologists.

I agree with you that the acronym, ASCAP, preferably without the I, does have a pleasant ring to it and has come to be associated with a particular way of thinking and with the Newsletter. It would be a pity to lose it. Personally I would like to get away from the pathology bit. Do we really want to restrict ourselves to pathological behavior? How about simply ASC, Across Species Continuities, or ASCIB, Across Species Continuities in Behavior?

We all like our theories and we are all inclined to try to impose them on others. Many other original

thinkers are phallic narcissists at heart and like to accumulate disciples around them. Freud was such a person, and so was Bowiby. There are those who like to be upper (narcissists) and those who like to be lower (disciples) and they tend to find each other and to form complementary relationships. I think, rather like the authoritarian personality, some narcissists like to be disciples to those they experience as upper to themselves, and some disciples like to be narcissists to those they experience as lower to themselves. It is difficult to form an association of original thinkers (distant people).

On the other hand, original thinkers like to have friends (closeness), and they choose as friends people whom they experience as being sufficiently like themselves in their particular form of originality (distance) to form safe allies. In those kinds of alliances, if you remain too original you lose your friends, but if you make too many compromises in order to stay friends you lose your originality. All these kinds of things are going on in our group and it's a kind of delicate juggling act to provide sufficient common ground to maintain the alliance.

Response to LS & JB

byRG

First, there *should* be discussion of the horizontal axis of Birchnell and the developmental issues of Sloman and Liotti. With the emphasis here on not only phenotypic behaviors but on genomic and neural facets of development, this issue of ASCAP hopefully remedies at least some of this, although I agree with Leon that if more contributions from him and Dr Liotti were to come this way, they would grace these pages. While not specifically on *child* development, the *very important article* by Miklos quoted at the end of the issue has importance for issues of the evolution and mechanisms of development more generally.

Concerning the arguments JB has with those focusing more on evolution: I believe that this is like the nature-nurture controversy. Everyone now agrees that *both* operate in the formation of people and other animals and that the reality is that *both* are more important than the former antagonists could ever have known. I think that you *and* the evolutionists are absolutely and simultaneously correct - there is incredibly more to learn about both spheres than we can now imagine.

To be more specific about both being right: there is no doubt, I believe, that enormously ancient and powerful forces determine our present shape and behavioral possibilities. Similarly, there is extraordinary flexibility and new learning possible with our ancient apparatus, including creative ways of relating that have been amplified in our brains relative to precursor brains. So truth is a matter of emphasis, perspective and appreciation.

Might I suggest that you should deploy closeness in seeing the communalities in the approaches rather than in seeing the conceptual differences and therefore feeling increased distance? I believe the points of John Price are that the upper-lower dimension is a *more ancient* one and more fundamental only in that, not greater in importance nor even in amount of neural programming for this instead of the horizontal axis. See Miklos's discussion in the section dealing with his two phases of development.

Which is more ancient and a more deeply buried and powerfully determinant program is separate from "more important" which is a value judgment and a choice of what one wants to study. A former default position in psychology and psychiatry is that human uniqueness is more interesting; The ASCAP Newsletter default has ancient features as more interesting, partly because there is such resistance to examining them in other venues.

But enormous excitement resides in dissecting which body and behavioral components are the ancient and which are the human-recent? That was my intent in talking of the three-times bigger brain earlier this year. What does it do that wasn't done before? Well, capacity for closeness is fundamental to being human, but that doesn't make uninteresting earlier versions of closeness that other mammals exhibit. It depends on a scholar's interest.

Abstracts by Kent Bailey & associates

1. Bailey KG: Human paleopsychopathology: implications for the paraphilias. New Trends in Exp & Cl Psychiat 1991 ;VII(1):5-16

Summary: The concept of paleopsychopathology is introduced and applied to a reconceptualization of the paraphilias. The model suggests that the paraphilias are basically disorders of phylogenetic maleness where high sex drive and other aspects

of male primate sexuality are displaced, malimprinted, or disinhibited in socially proscribed ways. Specific disorders discussed were voyeurism, exhibitionism, pedophilia, and fetishism.

2. Bailey KG: Psychological kinship: implications for the helping professions. Psychotherapy 1988;25(1):132-141

Abstract: Kinship is the primary organizing principle in human relations. The notion of psychological kinship is introduced to account for those instances where persons treat certain genetically related others "as if" they were family. As in other human relations, psychological kinship is assumed to play an important moderating role in client-therapist and helpee-helper relations in the helping professions. Various implications of psychological kinship for professional helping, "low" and "high" kinship approaches, and characteristics of the "kinship therapist" are discussed.

3. Bailey KG, Wood HE, Nava GR: What do clients want? Role of psychological kinship in professional helping. J Psycloth Integration 1992;2(2):125-147

Abstract: Both empirically and theoretically, the relationship is central to virtually all forms of professional helping and psychological treatment. However, why this is the case remains unanswered. We submit that the helping relationship is a variation of the natural human tendency to form "kinships" with close significant others. Biological kinship is based on degree of biological relationship plus classification as "family," whereas the psychological kinship outlined in Bailey's 1988 article is based on classifying biologically unrelated others as if they were family. The kinship model of professional helping proposes that clients generally want a psychological kinship relationship with the helper or therapist (as they do in other interpersonally close encounters); moreover, the model proposes that the greater the client stress the greater the client need for kinship with the helping agent. Several forms of lay and professional helping are discussed in light of the kinship model. It is concluded that considerable evidence supports the idea that clients often want psychological kinship with their helpers, and stress-kinship link receives circumstantial support as well.

4. Nava GR, Bailey KG: Measuring psychological kinship: scale refinement and validation. Pschol Rep.1991 ;68:215-227.

Summary: *The Kinship Scale was designed to measure the construct of psychological kinship. A revised version of the scale was validated against several measures of intimacy and relatedness. Subjects were undergraduate psychology students (56 men, 144 women) who were administered test packets including the Revised Kinship Scale, the Rubin Love and Liking Scales, the Adolescent Parent Relations Scale (Attachment), and the Revised UCLA Loneliness Scale. The scales (except for Loneliness) were taken relative to two different cognitive sets, Closest Parent and Boyfriend or Girlfriend. As predicted, psychological kinship was positively correlated with attachment, love, and liking; however, sex differences complicated the picture. The prediction of a negative correlation between Kinship and Loneliness was not supported. Factor analysis yielded four factors, accounting for 51% of the variance. The strongest factor ("family love") contained 10 of the 20 items on the Revised Kinship Scale and accounted for 37.7% of the variance. It correlated as highly with the external affiliative measures as did the revised scale, and so may be a good short form of the test.*

Comment on RG's case IA in August ASCAP

by John Price

I think the case of IA (ASCAP Aug 1993) is instructive and raises questions. The boss delivers a "blow" to the subordinate, or at least is seen to do so by the subordinate, and this blow (or catathetic signal) has results which can be described at both individual and dyadic levels.

At the individual level we say that the subordinate became depressed (meaning that he experienced depressed emotion and this led to clinical depression). This depression was the result of an appraisal system which decided between becoming angry or depressed, which are the two alternative strategies available in response to punishment or frustrative non-reward. The main reason for becoming depressed rather than angry is that the boss was higher ranking.

Aristotle discussed this in his Art of Rhetoric, so it is not new. Aristotle pointed out that to receive an insult is painful, and if it comes from a higher ranking person the pain makes you depressed; if it comes from a lower ranking person it makes you angry. In your patient other factors may have been

operative, such as a sensitivity to insults derived from his relationship with his father, and also a tendency to adopt the depressive rather than the aggressive strategy, also derived from his father.

At the dyadic level, can we say that the relationship switches from the hedonic mode into the agonic mode? Would anyone disagree with this? If your patient had become angry rather than depressed, it would have been a different sort of agonic mode, or possibly an agonistic mode with expression of aggression on both sides. How was the episode experienced by the boss? Did he notice that the relationship had changed?

Possibly he just thought that IA had become a bit quiet and probably had something on his mind--it is unlikely that he attributed the change in IA to any action of his own. Can we say that the function of the depression (involuntary subordinate strategy) was to prevent overt aggression in the relation; ie, to keep it agonic rather than agonistic? In general it seems to be true that the agonic mode is experienced much more acutely by the subordinate than by the dominant partner to the relationship (cf Dietrich von Hoist's tree shrews).

Continuing with the systemic view, how did the episode appear to people outside the dyad? We would predict that he would be more irritable with subordinates (such as his secretary) but less aggressive to superiors and equals; at home his response would depend on whether he was the dominant or subordinate spouse: if dominant he would be more aggressive with the wife and children, if subordinate he would just be seen to become quieter and spend more time in the potting shed; and in the latter case he might be more aggressive to the dog, unless it were recognized as the wife's dog, and then might he take it out on the dog when the wife was not present?

It was a pity that the boss left, although IA's recovery when a new boss came confirmed that the depression had been a function of his relationship with the old boss. If the boss had not left, your therapy would have been put to a keener test. Presumably the objective would have been reconciliation, which means a switch back into the hedonic mode. How would you have proceeded to this target? How would you have elicited things like acceptance, forgiveness, atonement-or the hugging and kissing which chimpanzees use? Is our equivalent going out for a beer together?

I am interested in why you said that he did not "need" antidepressive medication. Was this be-

cause the depression was only mild, or because it was reactive to a perceived insult from the boss? Or was it because you felt that the depression would facilitate "acceptance" on his part and thus lead to reconciliation? Might the depression lead to a cognition that "the boss has a right to give me the sort of insult he gave me" and so reduce the apparent disparity in the definition of the relationship between the two men?

On the other hand, if you lifted his depression with drugs, he might have felt well enough to go to the boss and say, "I don't think you have a right to insult me in that way" whereupon the boss might have said, "I agree with you, the insult was unintentional and I apologise," and so lead to reconciliation via another channel. Or the boss might have said, "You are a foci to see an insult when none existed, this whole problem is your fault," which would have been a further catathetic signal and would have intensified the depression.

I think one can conclude that, if it seems possible to achieve reconciliation with reduced or unchanged asymmetry in the relationship, it is better to treat the depression first and go for reconciliation later. But to achieve reconciliation with increased asymmetry, it is better to go for reconciliation first and treat the depression later because one function of the depression is to accommodate the patient to increased subordination. Clearly, in anyone's book, it is better to have reconciliation with reduced or unchanged asymmetry. It is part of the skill of the therapist to assess whether such a form of reconciliation is possible, or whether one should go for reconciliation with increased asymmetry, which is what De Waal calls "conditional reconciliation" when it occurs in chimpanzees, because the reconciliation is conditional upon the subordinate chimpanzee accepting a degree of subordination which either did not exist before, or if it did, was unacceptable to the subordinate (which gave rise to the agonistic episode in the first place).

Application of "Palo Alto" systems thinking

Before the episode of criticism (when we assume the relationship was hedonic) the command or definitional components of the communication between IA and his boss were redundant in that they both accepted the definition which had partly been imposed on them by the institutional structure and partly negotiated between them. We do not know whether the relationship was complementary in the Batesonian sense of IA responding to the boss's aggression with "less of the same or

something different."

In applying the criticism and using the analogy (leaving aside the question of whether it was intended), the boss was putting IA down at three different logical levels. At the simplest level of content, the criticism is in itself a put-down, saying "You are less than perfect." Then at the higher logical level of definition, the criticism defines the relationship as one in which the boss is entitled to give this sort of criticism; and since this definition had not previously been accepted by IA (which is why he was so upset about it), it is a unilateral redefinition of the relationship, which is a sophisticated form of catathetic signal (put-down). At a still higher logical level, the application of the criticism contains the message, "I have the right to make unilateral alterations in the definition of our relationship," which is equivalent to saying, "I hold the power in this relationship," and is also a catathetic signal since it is a non-redundant metamessage extending an existing complementarity.

Application of the 3-level model

Perhaps the case of IA is an opportunity to give the 3-level model another airing (ASCAP Aug 1992). I think we can show that our approach adds something to ordinary clinical help with conflict resolution. Here is the model to remind ourselves:

		Alternative strategies	
		ESCALATING	DE-ESCALATING
	CORTEX (deliberative)	(A) Fight to win	(B) Submit
Brain level	LIMBIC (emotional)	(C) Get angry	(D) Feel chastened
	STRIATUM (thymic)	(E) Mood elevation	(F) Depression

Box A reminds us that IA has the option of being assertive. He could tackle his boss again and refuse to accept the denial of the insult, and demand an apology. Or he could go to someone higher up in the hierarchy and try to get his boss sacked. He would find this difficult in view of the activity in Box F, and he would be laying himself open to further insult or criticism, which would intensify activity in Box F.

Box B reminds us that he could make a voluntary, spontaneous act of submission; for instance, he could go to his boss and say, "I have been

stupid and thought you were getting at me in that allegory, but I realise you weren't, and even if you were, why shouldn't you?" If pride prevents him from doing this, he comes into the category of "blocked voluntary yielding," and we could call him a Boxes-ADF depressive.

Box C reminds us that whatever IA chooses to do, he may still remain angry with his boss, and this will inhibit the activation of Box B and jeopardise reconciliation. We have recently been reminded that a lot of depressives feel angry and this may well be why they are still depressed.⁵ They are Boxes-BCF depressives. Paul's book gives a good example.⁶

Box D reminds us that, in addition to his clinical depression, he may feel the emotion of being chastened or humiliated, and this, unlike the depression, will be person-directed in that he will feel humiliated specifically by the boss, rather than just feel the non-specific humiliation of depressive mood. Box E calls to our attention the fact that IA became depressed rather than manic, possibly reflecting his genes or his early experience, or the action of some randomising device.

Box F is where he is at when he comes for help. We then have to decide whether there is any alternative to a choice between Boxes A and B. Can he get out of the situation altogether, possibly by accepting another job offer? Can we indulge in some reframing so that he ceases to experience the episode as catathetic (putting-down)? If no other options are available, we have to help him make a decision between boxes A and B; and if he chooses A, we need to relieve his depression with drugs and give him assertiveness training; if he chooses B, we must help him to make the submission complete, and to fully achieve acceptance so that activity in Box C can be switched off, hopefully without being replaced by activity in Box D.

I know you have used this model explicitly with patients in the past - did you use it with IA? I think he is a particularly illustrative case, possibly because the "episode" was such an isolated one in an otherwise hedonic relationship -- rather than the history of chronic abuse by a dominant person that so many patients give. Leon [Sloman] has presented a similar case of a trainee teacher who felt insulted by her supervisor sitting in on her classes, and in that case too the depression remitted after the supervisor left to take another job. Usually catathetic bosses are not so obliging.

Differences in explorative behaviour in dominant and subordinate male cockroaches⁷

by Jean Gilbert

Ranking behaviour evolved as a mechanism for the control of agonistic behaviour and as a mediator of resource allocation.⁸ Although dominance is a description of relationship animals located at different ranked positions show characteristic patterns of behaviour. Dominant animals will show increased aggressiveness, explorative behaviour and less timidity as compared to those of a lower rank.⁹

Much of our understanding of social ranks comes from mammals, especially primates. Much less is known of ranking in more primitive species, like the cockroach who evolved many millions of years before the mammals. This study sets out to investigate the characteristics of dominant; subordinate behaviours in male cockroaches, *Byrsotria fumigata*. Cockroaches live together in groups and do seem to form ranked social hierarchies of dominance and subordination.

Explorative behaviour was selected for study in relation to rank. The hierarchy was established and recorded in the following way. Four male cockroaches were randomly selected from tanks containing male only cockroaches. They were then isolated from each other for several days. For identification, three were marked with coloured inks (Red, White, and Yellow) on the dorsal surfaces of their pronotum, and one was left unmarked.

After their period in isolation they were then introduced to each other into a large tank, with only four small cardboard containers for concealment. Their activities and interactions were observed for 30 minutes. For the rest of the study the four cockroaches are referred to by the colours on their pronotums. The four cockroaches were kept together for seven days.

The Unmark cockroach was the first ranked male of the dominance hierarchy. He never backed away from confrontations with another nor did he spend any of the 30 minutes in a concealment area. White was the second ranked male due to his chasing of Red and his mounting of Yellow. Yellow was third as he was an active member of the group but he presented himself to be mounted by Unmark and White. Red was the most subordinate as he avoided the other three as much as possible.

To observe the differences in explorative behaviour between these cockroaches, they were placed in individual empty tanks and were presented with various objects, each for 15 minutes duration. The objects included a 2 inch cylindrical cork, a similar sized foam cork, a 6 inch square of bubble packing paper, a cardboard tunnel and a wad of damp cotton wool which had been rubbed over the body of a female cockroach.

This experiment was carried out in two phases separated by seven days during which the cockroaches remained in their individual containers. During the second phase the same objects were introduced, but with the addition of the wad of damp cotton wool carrying the scent of one of the other males. The scent of the first ranking male was placed in the container of the second ranking male and vice versa. The same process was employed with the third and fourth ranking males. In a final part of this study, a female was introduced into each separate container and a 'time to mating' was observed.

Results

Unmark, the most dominant, failed to show the explorative behaviour expected of his rank. When faced with the scent of the second ranking White he tried to climb out of his tank. However, limited knowledge of cockroach behaviour may have led to misinterpretation of these actions. This may not have been subordinate behaviour.

When a female was introduced, Unmark's 'time to mate' was the fastest, 55 seconds (although he had to have a second female introduced as the first was physically too large!).

The second ranker, White was highly explorative and exhibited cockroach dominant behaviour by climbing on top of the presented objects. He became agitated as if looking for the intruder, when the scent of the dominant, Unmark, was introduced.

When a female was introduced to White, his time to mating was 2 minutes and 30 seconds.

The third ranker, Yellow also showed explorative behaviour which intensified after the seven days in his own environment. His time to mating was 3 minutes (30 sees longer than White). Red showed very little explorative behaviour during the study. He often remained still in the corner of his container, even after the seven days on his own. Time to mate for Red was 4 minutes.

Conclusion

Explorative behaviour did, in general, follow the hypothesis, by being greater in the more dominant cockroaches. At the present time little is known about dominance-subordinate relationships in such species as cockroaches. It was fascinating to see how well behaviour could be classified with this concept. Whether or not the dominate-subordinate dimension is found useful in later work, this study certainly found very clear individual variations in behaviour, that were stable over a long time period and predicted time to mating.

Moreover, as Paul [Gilbert] has suggested, low explorative behaviour and internal inhibition seem to be important components of subordination.¹⁰ Red was far more inhibited than any of the others. New studies on the biology, genes and the early experiences influencing the level of explorative and inhibited behaviour would seem a useful next step.

If any one wants to fund me for Ph.D. work just call!!

The metaphoric gloom of teosinte

by Russell Gardner

This essay stems from two articles published in the same recent issue of Science plus another from Physiology & Behavior (see end of essay for the three abstracts).

A single gene change in a wild Mexican grass affecting a component called glume has apparently allowed the evolution of the modern food crop of maize. The results of the research are clear and illustrate the workings of modern genetics. Most importantly, it provides us with an illustration of basic plans discussed at length last issue.

To explain best the basic plan concept -- or whatever we eventually call it ~ simpler organisms might help. Plants are easier to examine than humans (for example, IA discussed by John Price above) or even cockroaches (discussed by Jean Gilbert). Darwin wrote extensively about plants, Mendel made his famous experiments on peas, and Barbara McClintock discovered jumping genes in maize.

When a teosinte gene called *tga1* is active, it

causes a hard glossy glume (one of the flower bracts) to hold the seed close to the stalk. However, when the Mendelian dominant maize version *Tga1* is present, glume is less obvious and appears duller. Importantly, *Tga 1* causes the seed to be more exposed for easy human harvest.

Ancient native Americans apparently took advantage of this easier harvest and selected for expression of the *Tga 1*, human, not natural selection, was the environmental factor that made for a selective advantage for *Tga1* over *tga1*. The authors conjecture that *tga1* was advantageous for its survival in prehuman times because granivorous animals or molds might digest the un-glummy (might we loosely think of it as euthymic!?) seed more completely.

So the basic plan here is whatever allows the formation of the seed kernel and its glume for protection; this is "tinkered with" (making it less basic) to provide two versions of the basic plan: *tga1* means the kernel is concealed and protected vs *Tga 1* means the kernel is open and undefensive. Both help adaptation in different ways: human vs non-human environments makes a difference for survival and production of descendents (human selection and natural selection).

But still more basic plans are inferred by the fundamentals that the grass is a seed producing, fruiting plant. Many plans must underlie it: eukaryote cells are basic to multicellularity which is basic to the nodularity of the grass/maize stem which is (I presume) in turn basic to the petal-like bracts surrounding the seed (the glume is one kind of bract). We need a concept and name that captures that, emphasizing less what Francois Jacob mentioned as tinkering (with which we are all fundamentally concerned) and emphasizing more the parts tinkered with, the basic foundations.

Depression as analogous to teosinte gene tga 1

To describe an analogy of glume-bound and unbound seeds to involuntary and voluntary submission, similarities include that a teosinte-like gene may cause involuntary submission (heightened glume) and the maize gene the voluntary version. Depression with its separate gloom correlates with being out of sight, protected, defensive, and more impervious to damage. Voluntary submission as with maize and its lesser glume on the other hand recognizes that human interaction allows for better survival: exposure and bending to the harvest may be good.

This (without the metaphor) has been a point made often by Leon Sloman in conjunction with others of us.¹¹

A human example of single gene affecting behavior: MAOA

In this teosinte example, I've compared the presence or absence of glume to depression vs voluntary yielding but have not focused on speculating whether the two alternatives are due to the presence of a single gene: almost surely the propensity for its inheritance is more complex. However, the same issue of *Science* reported the inheritance of an aggressive tendency within a Danish kindred as the result of a mutational change in a single nucleotide base change in the gene that codes the monoamine oxidase A (MAOA) protein.

This well known protein has major effects on the biogenic amine metabolism of the body, and is significant for the study of depression because depleting it, as with the first tranquilizer, reserpine, causes major depression in about 15% of people. Inhibiting it (along with its relative MAOB) results in the reversal of depression. This was the mechanism of action for the first antidepressants discovered by George Crane in the early 1950s. MAO in both its A and B forms is the subject of an expensive new book due out soon for which I have the advertisement but have not read.¹²

Though only one nucleotide was omitted in the genome of the males in the Danish kindred, the small change had major effects because it signaled what is called a "stop codon" that insured that the rest of the protein would not get made. This seemed to result in deprivation of this cellular enzyme with consequent major effects on behavior. The mildly retarded males of this family without it were easily made over-aggressive; they apparently had a lower threshold of provocation.

This summary does not do justice to the complexity of the phenomena - basic plans -- influenced by MAOA. For instance, in addition to --or perhaps in some way via - the breakdown of norepinephrine, serotonin, and dopamine, absence of most of the MAOA molecule may have influenced the development of the child's brain. This echoes the point made by Leon in his letter about a need we have to focus on development: Are particular neurons encouraged or discouraged in their growth? Are factors generally inhibited in most people most of the time not inhibited in these unfortunate family members? What are those factors?

On nearly finishing this essay for this December issue, I happened upon another article relevant for the influence of MAOA on female mouse xenophobic behavior towards stranger females of the same species (see abstract 3). The effect seems opposite that in the Danish men -- a functional inhibitor of the MAOA inhibited aggression in the treated mice rather than exacerbating it! Exacerbation towards them also occurred, which seemed to be mediated through the smell of con-specific urine, however.

Three abstracts

1. Dorweiler J, Stec A, Kermicle J, Doebley J:
Teosinte glume architecture 1: A genetic locus controlling a key step in maize evolution. Science 1993;262:233-235

Abstract: *Teosinte, the probable progenitor of maize, has kernels that are encased in hardened fruitcases, which interfere with the use of the kernels as food. Although the components of the fruit-case are present in maize, their development is disrupted so that the kernels are not encased as in teosinte but exposed on the ear. The change from encased to exposed kernels represents a key step in maize evolution. The locus that largely controls this morphological difference between maize and teosinte, teosinte glume architecture 1 is described and genetically mapped.*

2. Brunner HG, Nelen M, Breakefield XO, Ropers HH, Van Oost BA: Abnormal behavior associated with a point mutation in the structural gene for monoamine oxidase A. Science 1993;262:578-580

Abstract: *Genetic and metabolic studies have been done on a large kindred in which several males are affected by a syndrome of borderline mental retardation and abnormal behavior. The types of behavior that occurred include impulsive aggression, arson, attempted rape, and exhibitionism. Analysis of 24-hour urine samples indicated markedly disturbed monoamine metabolism. This syndrome was associated with a complete and selective deficiency of enzymatic activity of monoamine oxidase A (MAOA). In each of five affected males, a point mutation was identified in the eighth axon of the MAOA structural gene, which changes a glutamine to a termination codon. Thus, isolated complete MAOA deficiency in this family is associated with a recognizable behavioral phenotype that includes disturbed regulation of impulsive aggression.*

3. Wallian L, Brian PF, Haug M: Aggression in female mice: contrasting effects of amiflamine (FLA 336), a selective and reversible MAO-type A inhibitor. Physiol & Behav 1993;54: 411-414.

Abstract: *The effects of FLA 336 on the attack by resident groups of female mice on strange female intruders were assessed. This MAO-type A inhibitor clearly suppressed attack in treated subjects without producing major alterations in other activities but there was no clear dose-response relationship. The substance also increased attack on treated animals, an effect largely mediated via changes in the odor characteristics of the urine. The data emphasize the need for appropriate controls in studies with psychoactive compounds to distinguish direct (CNS-mediated) from indirect (mediated via changed signaling or perception) actions.*

An Important Article

selectively quoted by RG (with quotes in italics)

Miklos GLG: Molecules and cognition: the latterday lessons of levels, language, and *lac*: Evolutionary overview of brain structure and function in some vertebrates and invertebrates. J Neurobiol 1993;24(6) :842-890.

Summary: *The characteristics of the nervous systems of a number of organisms in different phyla are examined at the recombinant DNA, protein, neuroanatomic, neurophysiological, and cognitive levels. Among the invertebrates, special attention is paid to the advantages as well as the shortcomings of the fly *Drosophila melanogaster*, the worm *Caenorhabditis elegans*, the honey bee *Apis mellifera*, the sea hare *Aplisia californica*, the octopus *Octopus vulgaris*, and the squid *Loligo pealei*. Among vertebrates, the focus is on *Homo sapiens*, the mouse *Mus musculus*, the rat *Rattus norvegicus*, the cat *Felis catus*, the macaque monkey *Macaca fascicularis*, the barn owl *Tyto alba*, and the zebrafish *Brachydanio rerio*. Vertebrate nervous systems have also been compared in fossil vs extant organisms. I conclude that complex nervous systems arose in the early Cambrian via a big bang that was underpinned by a modular method of construction involving massive pleiotropy of gene circuits. This rapidity of construction had enormous implications for the degrees of freedom that were consequently available to evolving nervous systems. I also conclude that at the level of*

neuronal populations and interactions of neuropiles there is no model system between phyla except at the basic macromolecular level. Further, I argue that to achieve a significant understanding of the functions of extant nervous systems we need to concentrate on fewer organisms in greater depth and manipulate genomes via transgenic technologies to understand the behavioral outputs that are possible from an organism. Finally, I analyze the concepts of "perceptual categorization" and "information processing" and the difficulties involved in the extrapolation of computer analyses to sophisticated nervous systems.

From the body of the article: George Miklos asks four major questions:

1. *At what level do we seek enlightenment in a given nervous system?*
2. *Which nervous systems allow an integration of molecular actions and organismal actions?*
3. *What do evolutionary comparisons reveal about the likely generality of the results from one nervous system to another?*
4. *What and where are the promising inroads in different nervous systems?*

He points out that his is a pragmatic approach, hoping to make *rapid progress in understanding any nervous system at a given level and then of understanding phenomena between levels? The approach I take is to see what pathways exist from the level of molecules through neuroanatomy to the level of behavior. I attempt to evaluate the bottom-up and top-down approaches in the contexts of forward and reverse engineering as well as in the contexts of evolutionary constraints.*

Later he illustrates from the sound localization-abilities of the barn owl that *organisms could use many different molecular systems to achieve the same end result, and many different circuits could be manufactured during embryogenesis to solve the same computational problem. What is actually used will be the result of the evolutionary history of that particular genome.*

...It is time to discard the yoke of 19th-century naturalism, the imprint of which is still with us. Nature is indeed diverse, awe inspiring, and beautiful, but we are now well beyond the broad descriptions of entire landscapes and in an era of pragmatism, function, and experimental testing of

hypotheses.... Without the tools of modern genomic manipulation, studies of behavior will simply constitute a collection of "just so" stories and we shall remain in a state of organismal gridlock.

This newsletter has recently considered discussions of basic plan whilst simultaneously searching for a better term. Dr Miklos states the following: *There at least 800 species of Hawaiian Drosophila and thousands of species of vertebrates each with their own minor variations on the same developmental themes.*

He goes on to develop a different point, namely that a few genomes could be focused on without loss -- that changes in the neural system occur without adaptive factors entering, as follows: *many biologists still cling to the belief that everything in the brain is functional, has arisen as a consequence of natural selection, is highly adapted, and is vigilantly and assiduously honed at every opportunity by such a force. The lessons from molecular biology, however, are different. Functionless stretches of junk DNA are a way of cellular life and most of the genome and its outputs are a result of unavoidable historic turnover processes.*

In a section entitled **Mountains and Molehills**, Miklos draws attention to two different perspectives. *The first is the brain seen from the molehill of macromolecular and cellular collisions and the second is the perspective afforded by the mountains that constitute the various behaviors of different organisms. Unless these worlds can be made to interface, no matter how inexact, each of us shall be left working in smaller and smaller quarantined areas, compartmentalized both in terms of data and outlook.*

Again, he approaches our issue of basic plan in a section called **The Evolutionary Tapestry**. He quotes from another article written with KSW Campbell, *It seems inescapable to us that the major conclusions from the fossil record are the very rapid appearance of functional morphologies inexorably followed by a period that is basically "morphological fiddling at the edges."* Later he quotes insect investigators, adding his own emphasis (which is ours also), that *although insects appear fabulously diverse "when seen through the lenses of external body form and behavior," they are "astonishingly conservative with respect to the architecture of the CNS which must serve that changing body form and behavior." ...the oldest fossil bee is almost indistinguishable from present-day bees. Because it was a worker,*

sociality and all that entails at the level of nervous systems, namely, learning, foraging, and navigation, can be assumed to have have changed little over this long time span of nearly 100 million years."

Moreover, he points out that the five major brain divisions of vertebrates were already present in fossil ostracoderms and *It is again likely that the basic vertebrate nervous system evolved rapidly, although subsequent embellishments, such as eyes and jaws, came later and at different times (after about 500 million years ago).*

Then in **Prisoners of Circuitry**, Miklos focuses less on the foundations than on the evolution of new behaviors. He asserts that the following are needed: 1. *the evolution of new neurons that can form entirely novel circuits (the corpus callosum in mammals and the accessory lobes of decapods, for example), 2. new neurons could interact with established circuits to yield new outputs, 3. old neurons could be "recycled" by forming new synaptic connections (synaptic reaffiliation)... 4. old neurons and old circuits could utilize newer and different signaling molecules at various times....*

What needs to be appreciated is that once there is multiple usage for anything, be it gene, protein, circuit, or organ, compromise is inevitable and specific optimality is unachievable.... ancestral neurons and circuits that now seem to serve no particular function have not been eliminated simply because they are not in the way (for example, motoneurons with axons that do not connect to the muscle)...

Miklos draws three lessons from across-phylo comparisons: 1. *if one views the phyla not from the jaded perspective of how they are related, namely the taxonomic overkill that has gone on seemingly since time immemorial, but from the complexities of the nervous systems within them and the complexities of the organisms' behavior, then it is clear that complex brains are found in those organisms that have complex locomotory activities....*

2. *morphological and neuronal diversity came early in the Cambrian. This is rather different from the conventional viewpoints that often emphasize an increase in the complexity of wiring diagrams by leisurely descent from a simple ancestor largely bereft of neural horsepower.... The Cambrian was clearly a "cut-and-paste" period of experimentation at many levels.*

3. *we do not need to look at every organism in every phylum in detail. We should be able to glean most of the principles concerning function and behavior by in-depth analysis of but a few organisms. Thus most of what will be discovered from the manipulate D. melanogaster nervous system... will be available for evaluation in other arthropods.*

This is a powerful basic plan point! Should the P in ASCAP -- that John Birtchnell thinks should *not* refer to pathology ~ refer instead to "Principle."

In **Levels of Analysis**, Miklos asks, *At what level do we seek understanding in any nervous system, and at what level is one system a model for another? A method is exploring the understanding the workings of an individual neuron vs. the operation of circuits. ...at what levels can we expect to find communalities of significance between C. elegans, D. melanogaster, and H. sapiens, for example? ...At the level of molecules the ground is certainly solid because it is already known that nearly 50% of the monoclonal antibodies made to the adult D. melanogaster brain cross-react to the human nervous system, including the spinal cord, cerebellum, hippocampus, and optic nerve. The number of genes that exhibit significant sequence similarity between S. cerevisiae, C. elegans, D. melanogaster, representatives of the Mollusca and Cnidariae, and our own genome is growing rapidly.* He then lists 13 examples, of which I will quote only one, in view of the fact that it is a component of a gene missing in the critical region of the human chromosome 15 in cases of Angelman syndrome:

The beta-subunit of the GABA_A receptor from the mollusc L. stagnalis can take the place of a vertebrate beta-subunit and function properly in a heterooligomeric receptor containing alpha- and beta-subunits.

In his letter above, Leon Sloman expressed some feeling on his and Giovanni Liotti's part that developmental issues be discussed more. Miklos provides such discussion on the molecular level: *There are basically two aspects to the development of every nervous system. The first is that of development and is the world of cell lineages, cell identities, cell adhesion molecules, substrate adhesion molecules, induction processes, cell movements, and neuronal degeneration; in short everything that goes on in embryogenesis before the organism is born (or hatches).... This is the world of molecular and cellular collisions where pioneering neural highways are set up and where*

gene hierarchies, growth cones, and adhesion molecules are the merchants and currencies... The successful ending of this wiring process depends absolutely upon both fickle and faithful cellular adhesions and scaffolds that can be torn down suddenly and without warning....

The second phase is one in which the synaptic strengths of different populations of neurons are altered over time. However, the manner in which the cerebral cortex "divides up" into different anatomic and structural areas is largely unknown. Evidence from neurotransmitter receptor research emergences indicates rather surprisingly that "the entire cerebral cortex matures as an integrated network, rather than as a system-by-system cascade."....

The human, mouse, bird, and cephalopod nervous systems, on the other hand, are examples of the second category because these brains significantly increase in mass from birth to adulthood, although the formative stages for the major neuronal populations have been attended to during embryogenesis.

There is much more to this article not additionally summarized here at this time. Subsequent sections are entitled **Myth of the Developmental Electrician, Dark World of Synaptic Strengths, Memories and Language, Perceptual Categorization, Fish and Fashion, Birds and Behavior, Mammals and Memories, Deus ex Machine, Methodologies and Biological Machines, Molecules, Mutants and Melanogaster, The Visual System, Denouement, Memory, and End Game.**

Like our own John K. Pearce, George Miklos illustrates that good teaching comes from vivid metaphor and active voice. I recommend you obtain Dr Miklos's article. Debate of his points in [ASCAP's](#) pages would benefit us all.

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Nedivi E, Hevroni D, Naot D, Israeli D, Citri Y: Numerous candidate plasticity-related genes revealed by cDNA cloning [Nature](#) 1993;363:718-22

Abstract: *Plasticity is a property of the nervous system that allows it to modify its response to an altered input. This capacity for change suggests that there are molecular mechanisms in neurons that can couple stimuli to long-term alterations in phenotype. Neuronal excitation elicits rapid transcriptional activation of several immediate-early genes, for example, c-fos, c-jun, and zif268. Many immediate-early genes encode transcriptional factors that control expression of downstream genes whose products are believed to bring about long-term plastic changes. Here we use a highly sensitive differential complementary DNA cloning procedure to identify genes that may participate in long-term plasticity. We cloned 52 cDNAs of genes induced by the glutamate' analogue kainate in the hippocampus dentate gyrus. The number of these candidate plasticity-related genes (CPGs) is estimated to be 500-1000. One of the cloned CPGs (16C8), encoding protease inhibitor, is induced by a stimulus producing long-term potentiation and during dentate gyrus development; a second, cpg1, is dependent on activation of the NMDA (N-methyl-D-aspartate) receptor for induction and encodes a new small dentate-gyrus-specific protein. Seventeen of the cloned CPGs encode known proteins, including six suggesting that strong neuronal activation leads to de novo synthesis of vesicular and other synaptic components.*

Weisskopf MG, Zalutsky RA, Nicoll RA: The opioid peptide dynorphin mediates heterosynaptic depression of hippocampal mossy fibre synapses and modulates long-term potentiation. [Nature](#) 1993;362:423-427.

Abstract: *The mossy fibre pathway in the hippocampus uses glutamate as a neurotransmitter, but also contains the opioid peptide dynorphin. Synaptic release of dynorphin causes a presynaptic inhibition of neighbouring mossy fibres and inhibits the induction and expression of mossy fibre long-term potentiation. These findings demonstrate a physiological role for a neuropeptide with classic neurotransmitters and demonstrate the very different roles played by these two classes of signaling molecules.*

1. Miklos GLG: Molecules and cognition: the latterday lessons of levels, language, and *Iac*: Evolutionary overview of brain structure and function in some vertebrates and invertebrates. J Neurobiol 1993;24(6):842-890.
2. c/o R Gardner, 4.450 Graves Building (D28), University of Texas Medical Branch, Galveston, TX 77555-0428. FAX: 409-772-6771. (Telephone: 409-772-7029). 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." At this time this "informal" organization has no official budget.
3. Aug22, 1993
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5. Fave M *et al*/: Anger attacks in unipolar depression, part 1: clinical correlates and response to fluoxetine treatment. Am J Psychiat 1993; 150:1158-1163.
6. Gilbert P: Depression: the evolution of powerlessness, Hove: Lawrence Erlbaum, 1992, p252.
7. I would like to thank Professor Russ Beck for his help and supervision.
8. Trivers R: Social Evolution. California: Benjamin/Cummings, 1985.
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12. Lieberman A, Olanow CW, Youdim MBH, K Tipton (eds): Monoamine Oxidase Inhibitors in Neurological Diseases. 270 Madison Ave, Ny, NY, 10016: Marcel Dekker, Inc, (\$150) 400 pages, 1400 references, illustrated.