

# ASCAP

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*"[H]uman language with its levels higher of description and argument and its mathematic questioning ability ...is exhibited even by 3 year-old children with their torrent of questions ...By contrast, apes do not ask questions."*  
John C Eccles<sup>1</sup>

### Newsletter Aims

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

### ASCAP Society Executive Council

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### ASCAP Society Mission Statement

The society represents a group of people who view forms of psychopathology in the context of evolutionary biology and who wish to mobilize the resources of various disciplines and individuals potentially involved so as to enhance the further investigation and study of the conceptual and research questions involved. This scientific society is concerned with the basic plans of behavior that have evolved over millions of years and that have resulted in psychopathologically related states. We are interested in the integration of various methods of study ranging from cellular processes to individuals in groups. The ASCAP Newsletter is a function of the ASCAP society.

### Contents

- To & From the Editor.....page 2
- *Ethological studies of subjectivity: The Internal Working Model* by Daniel G Freedman.....page 3
- *The rat resident/intruder paradigm: A model for the Involuntary Subordinate Strategy (ISS)* by John Price.....page 12
- Abstracts & Extracts: This issue features the special issue of Ethology and Sociobiology that focuses on mental disorders in an evolutionary context.....page 17
- Announcements.....page 21
- References.....page 22

ASCAP society  
 annual meeting in  
 Santa Barbara, California,  
 Tuesday, June 27, 1995:  
*see page 2*

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

# ADDRESSED TO & FROM ...

## RESPONSE TO TIM MILLER

I was very surprised by Tim Miller's conclusions concerning psychotherapy, as expressed in his article on the Big Five personality factors (ASCAP. January 1995).

While I have no cause to disagree with him about the stability of the five personality factors over time and despite the vicissitudes of life, it is beyond me how he could have concluded from this that "psychotherapy may also have little consistent effect".

The assumption seems to be that psychotherapy seeks to change personality. In the history of psychotherapy, such delusions have existed and still do. But does that mean that psychotherapy itself is a delusion?

This assertion fails because psychotherapy is not (or should not be) about changing personality. Various psychotherapies aim to reduce anxiety, correct incorrect assumptions, provide new information, develop new strategies, and reduce isolation. Among the strategies one might certainly count: a) How to cope with the personality you have; and b) How to mesh your personality with your particular socio-economic and cultural surroundings. None of this involves changing a client's personality.

In my view, one of the major

advantages of evolutionary psychology is that it helps clinicians to distinguish between what can be changed and what can't. It seems to me, therefore, that evolutionary psychologists should focus on the process of developing effective therapeutic attitudes, rather than on attacking the enterprise.

Kalman Glantz  
Cambridge MA, USA

## 1995 ASCAP MEETING

Your calendars should be marked Tuesday, June 27, 1995, for the 1995 annual meeting of The ASCAP Society. It will take place in Santa Barbara, California. Date, timing, and location are designed to mesh - yet not interfere ~ with the Human Behavior and Evolution Society (HBES) meeting Wednesday, June 28 through Saturday, July 2.

There has been confusion about the relationship of ASCAP with HBES. The two are united only in that many in The ASCAP Society are also members of HBES and of course there are overlapping interests; in the past, we have woven ASCAP open-ended evening discussion groups into HBES proceedings and ASCAP members have presented in various ways. Ascapians have abiding interest in the physiology of social communicational states relevant to psychopathology whereas the HBES group is concerned with the application of evolutionary theory to human

behavior of all sorts. Ascapians are often clinicians; while HBES has some clinicians, anthropology predominates.

The functions of The ASCAP Society meeting will include that we conduct our group's business, discuss our mutual concerns and hear the first Aaron T. Beck ASCAP award winner present her/his paper. Other agenda items are yet to be determined with finality though the executive committee is working on it (last year then-President Paul Gilbert's message to the group was read).

An added component just now developing stems from the feature article this issue by Daniel G. Freedman of the University of Chicago. In it, he considers the basic plan of "the internal working model". It is a basic plan (our term, not his) because imprinting in a whole variety of species may represent other versions of a same developmental neural program. President Pearce has appointed a committee of me, the two vice presidents (Dan Wilson and Kent Bailey) and two members-at-large (Nancy Segal and Tim Miller), to organize multidisciplinary responses to the article, bearing as it does on a potential basic science of psychiatry. Discussion of elicited responses might organize a plenary session.

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# ARTICLE: Ethological studies of subjectivity: The internal working model<sup>1</sup>

*Abstract: The hypothetical construct introduced by John Bowlby the Internal Working Model (IWM), is seen here as the subjective side of imprinting, which in turn is seen as a phyletically widespread phenomenon aiding a process that can end in speciation. In humans, as in geese and dogs, early social experiences form relatively permanent internal representations, and in each case the genetic natures of the socializing organisms enter the equation. The IWMs subsequently guide our social interactions, and mate choice, and they are always culture specific, helping to perpetuate a culture. Internal Working Models are deeply habitual and resistant to change, and this is seen as having had great evolutionary significance.*

Ever since my childhood, which I experienced as an only child, I have been actively introspective. When I think of my early years, it is not so much the events I remember but rather my *reactions* to the people, the animals and the events in my life. While this was doubtless not an entirely healthy situation, it has determined the type of research I have preferred, and it seems to have prepared me for the current postmodern era in which "objectivity" is met with suspicion and in which acknowledgement of the subjectivity or biases of the investigator is an integral part of the equation. Feminist recasting of primate behavior exemplifies this well.<sup>2</sup>

Given this propensity for interiority, my greatest intellectual attraction in college was, initially, to Freud and Jung. But there was a darkness there, a solipsistic murkiness which I found disturbing. I needed the clarity of scientific method, and that was eventually provided by two of Jacob von Uexkull's students, Kurt Goldstein and Konrad Lorenz. Von Uexkull, with his concept of *Umwelt*, had opened the door to the scientific investigation of animal subjectivity, and Goldstein transferred those principles to his work with brain injured patients, experimentally explicating their unique apperceptions of the world. He was my teacher at City College of New York, and his book The Organism became my bible. Lorenz

was, of course, the founder of modern Ethology, and when I first saw and heard him at Harvard in 1955, I knew immediately that American comparative psychology was due for major revisions.

I wish to return to Lorenz' work on imprinting and introduce a more recent idea, that imprinting is a mechanism that facilitates the speciation process. Jerram Brown, for one, has conducted computer-simulated studies, and shown that, in all likelihood, imprinting has greatly aided this process by guiding mating choice and thereby increasing genetic distance between competing homozygous populations. Bird song learning by local populations seems to have had a similar role.<sup>3</sup>

I don't want to get into the units of selection argument today, but suffice it to say that my position holds imprinting and imprinting-like phenomena to be phylogenetically widespread, appearing in both precocial and altricial birds and mammals, and that their function is the creation of relatively homozygous sub-populations that, in Sewall Wright's terms, enable a population to reach evolutionary valleys and peaks, extinction or expansion.<sup>4</sup> Thus, imprinting in birds, critical periods for socializing dogs, and the lifelong effects of social experiences in the first years of life in humans, I now see as comparable phenomena with comparable evolutionary function. Furthermore, the point will be made that there must be an internal socialization experience, and I am using John Bowlby's term, the "Internal Working Model" for internal representation at all the phyletic levels wherein social imprinting occurs.

The concept as used in "attachment theory" can be easily re-worded to include animal imprinting, so that IWMs can be defined as long lasting psychophysiological representations of social interactions to which an organism is exposed in early life; comparable experiences later in life fail to have the same lasting effect. In humans, Internal Working Models are frequently outside of awareness and we can

speak of their residing in the Unconscious. In general, Internal Working Models become guides for later behavior so that, for example, Lorenz' greyling geese considered him the flock leader even after they were fledged and flying as a family group.

In humans, an example of an IWM might be a beloved child, one who has experienced great affection in her first two years, and who then expects and forecasts positive relations with the persons she meets throughout life; further, she might shun persons who are negative and ungiving, since she has developed no coping mechanisms for such encounters. Conversely, persons abused as children seem to unconsciously forecast more abuse, and thereby more often than not attract it. Thus, whether in geese or man, the early experience has life-long consequences.

Not surprisingly, my Ph.D. work with dogs featured similar ideas, although I wasn't then concerned with the phylogenetic continuity of IWMs. The study was instead an attempt to deal experimentally with David Levy's hypothesis that the origins of human psychopathy lay in either parental over-indulgence or over-rejection of the young child. In the study, puppies became the young children, and over 3-8 weeks of age, a period determined by other work to be highly sensitive for social relationships, I raised four breeds of dog in either indulged or disciplinary conditions. Here is a summary of the initial study:

8 litters of 4 pups each were used. These included 2 litters each of Shetland sheepdogs, basenjis, wire-haired fox terriers, and beagles. Following weaning at three weeks of age, each litter of 4 was divided into 2 pairs which were equated as closely as possible on the basis of sex, weight, activity, vocalizations, maturation of eyes and ears, and reactivity to a startling stimulus. Thereafter, each member of one pair was "indulged" and each member of the second pair disciplined, during two daily 15 minute periods from the third to the eighth week of age.

Indulgence consisted of encouraging a pup in any activity it initiated, such as play, aggression, or climbing on the supine handler. These pups were never punished. The disciplined pups were at first restrained on the experimenter's lap, and were later taught to sit, stay and come upon command. When still older they were trained to follow on a leash. The experimenter handled all the pups and tested them individually; they lived with the identically treated littermates in isolation boxes the rest of the time.

An "incorporation of punishment" test, modified from Whiting,<sup>5</sup> was initiated at eight weeks of age: when a pup ate meat from a bowl, it was swatted on the rump with a shout of "no", and after three minutes the experimenter left the room. Observing through a one-way glass he recorded the time before the pup again ate. The results are presented in Figure 1.

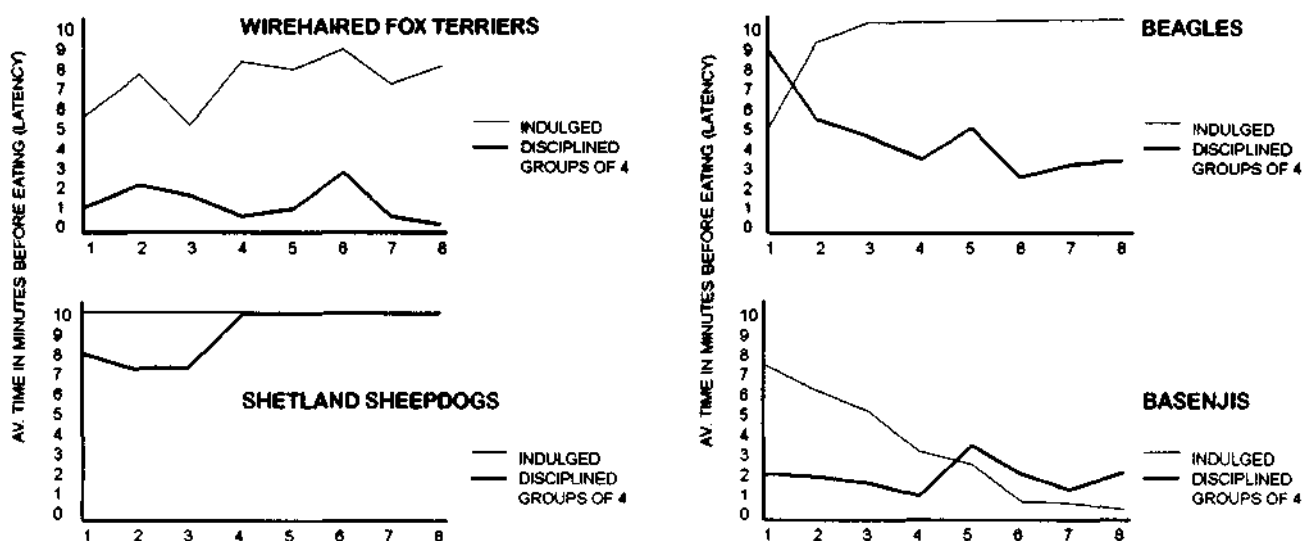


Figure 1. Puppies were punished for eating meat over a three-minute period whereupon the experimenter left the room. These are the average times before puppies ate the forbidden meat after E left. Scores of 10 minutes mean puppies did not eat.

*The conditions of rearing were then continued for a second period, when the pups were 11 to 15 weeks of age, and tests were re-administered with essentially the same results. All pups were then placed with littermates in outdoor pens and followed on a monthly basis through approximately one year of age. (Most of the important findings from this period are incorporated into the description below.)*

This, then, is a brief precis of the studies, done some thirty-seven years ago at the Jackson Laboratories in Bar Harbor, Maine, and now revived with Internal Working Models in mind. In what follows, I am asking about my subjective experience of the puppies and their experience of me, the lone handler, with an emphasis on the permanences of the early learning. I have taken the occasional liberty of making judgments about a puppy's subjective state, usually based on a mix of test data and repeated observation of films,<sup>6</sup> but most importantly I am taking the liberty of presenting my own subjective impressions and intuitions.

The following exercise in narrative, then, is an attempt to get "beneath" the plentiful measures and objective demonstrations reported in my 1957 thesis. To conserve space, I will discuss three of the four breeds.

a) Basenjis, though raised by me from three weeks of age, were largely indifferent to me as a meaningful organism in their lives. When raised in the "indulgent" condition, they rarely sought active contact with me, and I felt I was of no greater interest to them than, say, a balloon; similarly in the "trained" condition, their indifference made training to come to me "Come!" the most difficult item. Instead of approaching me, they often preferred to wander away.

All the disciplined pups fawned and stayed between my legs when punished on the "incorporation of punishment" test, but in neither rearing condition were punishments internalized so as to act as a guide for future interactions, and in Figure 1 we see the evidence for that. After the fourth day of testing, basenjis of both rearing conditions tended to eat the

"forbidden" food soon after the experimenter left the room.

Later in life, from six to twelve months, all basenjis greeted persons entering their pen with vigor, and the "indulged" could be distinguished from the "trained" by their rather greater interest, turned-back ears, and more persistent physical attention. I was actually surprised to see that the early experiences in rearing had made a visible difference in later behavior, although that difference was relatively minimal compared to the other breeds. As for my attachment to them, I could remember none of the basenjis individually, nor did I experience joy with any of them. They were interesting creatures of God, but I loved them only at the rarefied level.

The basenji IWMs of me and my species, on the basis of these experiences, would go like this: humans are naturally dominant given their size and control of resources, but of little social interest beyond that; they are not companions in play (there was little interest although play was encouraged), nor is affection an issue (for the most part I was merely tolerated). The "incorporation of the punishment" test seemed to deal with: "Was the handler present as an internal representation in his physical absence?" For the basenjis, out of sight was literally out of mind, and we assume the mental/emotional representation of the scolding handler was weakly present in the indulged and entirely absent in the disciplined. Although I have no doubt that a pet basenji raised with a great deal of affection would take more seriously a handler's commands, basenji owners know them as only marginally obedient.<sup>6</sup>

All of this fits with the facts that basenjis were originally bred in the old Belgian Congo where they were used to drive game into nets. They typically lived in packs on the edge of villages and were widely used as pets only after import to Europe in this century - that is, they were not bred for close interaction with humans, so that in retrospect their behavior is no surprise.

b) The wire-haired terriers provided the clearest demonstration of continuity. The indulged quickly

grew wildly enthusiastic, showering my face with licks and nips. Although they inhibited themselves from actually biting (I could feel their jaws quiver in restraint), I had to wear gloves after they started to playfully tug on my hands and fingers. The trained group on the other hand, did not retain the enthusiasm of the indulged terriers, and grew reserved, although not aloof. The two groups remained respectively enthused or reserved with all humans all through the follow-up period. When eight months old, four months after differential treatment ceased, the pups were once again brought inside and filmed: the indulged were still wildly enthusiastic with the supine handler, whereas the trained animals were only mildly friendly; however, they remembered their training well, and would sit, stay, and come on command.<sup>6</sup>

The terriers thus provided straightforward evidence that the human relationship was of great importance. They initiated play, and with minimal encouragement or discouragement deeply imbued behavioral patterns were formed. For the indulged, humans represented playfulness and (do I dare say it) love, whereas for the disciplined they represented a sort of dour demandingness; and these differential early experiences apparently firmly shaped subsequent expectations and interactions with humans. For my part, the indulged terriers, especially one enthusiastic little female, became a joyous memory of the experience at Bar Harbor, and apparently she *and* I were both permanently affected.<sup>6</sup>

c) Finally, the Shetland sheepdogs, at least this breeding of them, were nervous and high strung, and I became extremely cautious with these friendly little creatures lest I injure their delicate spirits. Training was almost too easy and each overt signal of my disapproval had a definite effect. Clearly, the laboratory was no place for such an animal, and the few punishments necessary to train the indulged on the "incorporation of punishment" test were sufficient to make them ambivalent to humans throughout the follow-up. Their message to me was, "play gently and I will adore you and do your bidding," and although on the follow-up the differences in rearing were seen to have lasting effects, neither rearing

condition favored optimal development in this breed.

What generalities about Internal Working Models do these accounts suggest? As we have said, the human data are geared to the notions that early experiences around attachments directly affect one's perceptions and expectations of others and one's self, that IWMs are normally acquired from parents, that they are enduring and beyond conscious control, and that they influence emotional reactions in a broad spectrum of relationships. Although a dog study cannot speak to the uniquely human aspects of these claims, certainly the experiences with a handler over 3-8 weeks, for most pups, appeared to affect in an enduring way how they afterwards interacted with all humans. Coupled with the related "critical period" studies in dogs,<sup>7</sup> this seems reasonable evidence for imprinting in an altricial mammalian species. Furthermore, we have here introduced the importance of genotype (at both the individual and group levels) in the formulation of IWMs, an idea we will develop further in the final section on humans.

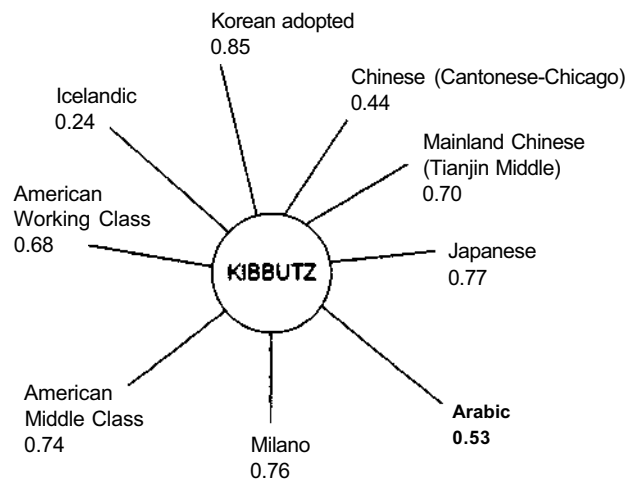
To summarize, to this point, it is a reasonable contention that in imprinting in birds and in what appears to be homologous behavior in dogs, non-conscious internal representations occur (at known neuro-physiological levels in birds<sup>8</sup>) based on initial social experiences. This process seems to be shared as well by humans so that in each case we can speak of the development of Internal Working Models.

### **Transmission of culture**

It seems a reasonable extension of attachment theory that the interpersonal aspects of *culture* become part of anyone's Internal Working Model, since that is also acquired in the first years. We have been investigating this contention in cross-cultural studies of mother-toddler interactions, and more recently by cross-cultural studies using Mary Main's Adult Attachment Interview (unpublished). We have developed a Q-sort of 100 items (see Appendix 1) on the basis of watching videotaped mother-toddler interactions from eight cultures. In the Q-sort we have an instrument that allows the viewer to

empathically enter the mother-toddler interaction and to direct this temporary "identification" into the act of Q-sorting. This is as close as one can come, I believe, to joining the subjective apperception of an event with a numerical record of it. From there on it becomes a matter of reliability, and despite using a culturally wide range of Q-sorters, *reliability* has proven quite good (average .81).

I will illustrate with the findings from the Kibbutz-Arab comparison.



Mean Correlation 0.60

Figure 2. Correlations based on averaged Q-sorts on mothers and toddlers in nine cultures

The overall correlation between the two averaged sorts of each culture-group was 0.53, a relatively low correlation as one examines Figure 2, which suggests the groups are substantially different. In general, items differing by three points were maximally differentiating, and such differences occurred on three items: Arab mothers were judged more dominating, more intrusive, and more discouraging of the child's independence. At the level of two-point differences the trend was the same, and Arab mothers were seen as more often acting as if the child is incompetent, as being bored with the cooperative task, and as uncertain and focused on themselves.

By contrast, kibbutz mothers were assessed as more patient. At the two-point level, kibbutz children were judged higher on showing independence, asserting

desires, starting new routines, being inventive, showing joyousness, clowning around, verbal interaction, uncontrollability, and refusing to give up the experimenter's toys. On the other hand, Arab children were judged higher on self-inhibition and on a tendency to "phase out", that is, for the eyes to gloss-over. The dyadic items were then no surprise given these trends: kibbutz children were seen as relatively unhurried by their mothers, less inhibited by them, less compliant with their directives, and kibbutz mothers were judged less distant and less mis-attuned.

With regard to attachment theory, we would have to designate the kibbutz, with its unusually high level of respect for the child, as fostering more secure attachments than do the Arab homes. However, cultures must be seen as a whole and not judged via criteria originating elsewhere, most especially criteria originating within the judge's own cultural group. Thus the above Q-sort results become truly understandable only when seen in their full cultural context.

Briefly, in an Arab village, as in many tribal villages (e.g., Hopi villages), the eldest is the most respected, and age *per se* lends one status. One has to earn respect by living a long life, and every child somehow gets a sense of being at the beginning of a long road. The kibbutz offers a striking contrast: conceived out of a reaction to the nuclear family of the European *shtetl* (small Jewish town), kibbutzism proceeded to denature the role of parents and to elaborate the Jewish "worship" of the child. At this level, secure versus insecure is entirely a secondary issue for culture-context dictates, the value given different modes of attachment.

Please also note on Figure 2 the additional cultures with relatively low correlations with the kibbutz: Chicago Cantonese ( $r = 0.44$ ) and Iceland ( $r = 0.24$ ). Although very different one from the other, all three samples (Israeli-Arabs, Icelandic and Cantonese) appear to share the adult-centered attitude that children must bide their time before becoming worthy of respect. With the possible exception of the American working class, where some of these attitudes are

present, the remaining samples are strongly child-centered.

What about Internal Working Models and their relation to the transmission of cultural patterns? An attachment theorist and psychiatrist, Charles Zeanah, has reconstructed the transmission across generations of the "avoidant" attachment pattern, and we can use it as a paradigm for cultural transmission.<sup>9</sup> Here is what he says:

*Bids by the child for comfort or nurturance are turned away or rejected (by the caregiver), especially at times when the child is distressed. This rejection ranges from a subtle disengagement from the child to frank aversion to her. She learns to suppress her need for comfort by turning her attention away from her caregiver. Instead she maintains an outward appearance of self-reliance and strength, in order not to elicit rejection from the caregiver by a direct display of neediness... The self-organization underlying this pattern likely includes a powerful, self-reliant, dominant self-representation fused with a needy, weak, submissive self-representation... Because it is less painful, the former self-representation will be more conscious. For it to be maintained, however, the individual will be especially averse to stimuli (e.g., neediness) that threaten to elicit its more painful, less conscious counterpart. Thus the adult in this pattern rejects his needy child as he shuns his own neediness. The child who experiences the rejection defensively constructs a similarly split self-representation, and the cycle continues.*

We think that something very much like this goes on in many instances of cultural transmission. Let us examine several of our groups with some necessarily simplified examples, starting with the Arabic sample. There we saw an average mother-toddler interaction in which a mother treated her child as if it were inadequate to the shape-sorting task, and where she went so far as to manually direct the child's hands. Correspondingly, the toddlers tended to watch their mothers for clues as to whether they were doing the task correctly. Under these circumstances, the Arabic children appeared to develop self-representations of inadequacy *vis-a-vis* the mother (and other adults),

and since the entire community is hierarchically organized, this self-representation is constantly reinforced. Since the parents are imbued with this hierarchical view of relationships, their prejudice or forecast is that mere children will probably not succeed in the task at hand, and so the cycle is repeated.

The Cantonese mothers in our sample were also quite controlling, but they were also seriously task centered, and their clear intention was that the child actually master the task. Thus, in the second half of the task, after their toddlers had gotten the idea under the critical tutelage, these mothers became attentive watchers. Our collaborator, Denise Xiaohong Chen, reports that such mothers (critical of a child's first attempts at mastery) are reasonably common in China, and that a common self-representation among Chinese children is, "I am not up to the task, and I must do better." Later this amounts to comparisons of the self with others, via a gauge that has been internalized. The result is a general sense of uncertainty about one's mastery coupled with high levels of accomplishment. When these children are themselves parents, their own uncertainty is passed to their infants in the form of "premature" demands for mastery, and again the cycle repeats. Chinese mothers' demands for mastery, by the way, start even earlier than toddlerhood. Kuchner found that Chinese mothers consciously avoided picking up and consoling their crying four-month olds in order to "teach" them not to cry.<sup>10</sup>

The Icelandic mothers in our sample were notable for their reserve and for what appeared superficially as disinterest in their toddlers. (See, for example, the low correlation in Figure 2 with the kibbutz, our paradigmatic child-centered culture.) Olafsdottir, however, proposed that Icelandic mothers are working with the following internal model: "I love you, but I can only allow myself to love you so much because otherwise I become vulnerable to rejection. In a certain way I feel really helpless, but I have to act as if I am strong and unapproachable because that's the side of me I trust. I have to be independent and, incidentally, so do you." What follows is a subtle rejection of both dependency and willfulness by the



mothers, and this places their children in something of a double bind.

The average Icelandic toddler in our study, not surprisingly, often seemed confused about the relationship, and doubtless had developed a self-representation of not being lovable, or at best an uncertainty about it. They were repeating their mothers' experience and were learning to trust only the lonely safety of ambivalent independence. In our initial comparison of 30 Adult Attachment Interviews from Bengali, Korean and Icelandic mothers, the Icelandic mothers, significantly more than the others, exhibited major doubts about the trustworthiness of their own parents. We might then speculate that pessimistic forecast around the issue of trust plays a part in the substantial interpersonal distance present throughout Icelandic culture.<sup>11</sup>

Here is a reasonably typical summary of an Icelandic Adult Attachment Interview:

J is a 28 year old mother of three children. She grew up in an unsupportive environment with an alcoholic mother, no father, but a good grandfather. There was no structure in the home, no real family life, instability, rejection and insecurity but the main theme in J's story is: *"I still did o.k. I don't think these experiences were all bad. They taught me a lot of important things about life. I did feel insecure and rejected at times, but there were a lot of other things that made up for it. When my mother was away and we were in foster care we got acquainted with good family life and met some really good people. Mother was good and warm when she was there, but she was also controlling and threatening. She worked a lot, was unavailable and unreliable, but easy to love."* J has never seen her father. Her grandfather is an important attachment figure, who took care of her and her twin brother the first years of their lives. He was affectionate, warm, reliable, but old and sick so they had to take care of him. Currently her relationship is better with her mother. J has a lot of hopes for the future, and consciously tries to give her children security and love.

Out of the eleven subjects, seven described little or

no relationship with their father, two described the relationship in an idealized fashion. Two of the seven described not having any memories of their fathers, the others described them as strict, distant and unavailable. One of the eleven had never seen her father and one of the eleven described her father as fun to be with, calm and quiet, *"very special, spent a lot of time with us, no father I knew of was like my father."*

My colleague, Vala Olafsdottir, has noted that the following passage from Iceland's Nobel prize winning writer, Haldor Laxness, is well known and captures an important part of a culture-wide IWM:

*A wise man once said that next to losing their mother, few things were better for young children than to lose their father. Although I am far from agreeing with all aspects of this statement I am not one to oppose it completely. I would probably have worded this statement without the bitterness or rather without the sting that is inherent in the words themselves.*

*Irrespective of what you may think about this statement it became my lot in life to be without parents in this world. I do not claim that to be my fortune, that would be an exaggeration. I would not call this a misfortune either, at least not my misfortune, because the consequence was that I acquired grandparents. However, it could be argued that this was my parents' misfortune, not because I would have been such an exemplary son, quite the contrary, but simply because children are more useful to their parents than their parents are to them.*

Vala summarizes her work as follows:

*The Icelandic culture is a perfectly viable culture, in which children are largely insecure and in which adults are infrequently autonomous as defined in Van Ijzendoorn.<sup>12</sup> The fact that the adults are most frequently rated as enmeshed/preoccupied or disoriented appears to be the mirror image of the children, and we therefore get the picture of an entire cultural system that reproduces itself, generation after generation, despite a "low" average quality of*

*attachment. It is for this reason that we conclude that, in evolutionary terms, the propensity for attachment is so great that even under cultural conditions in which the average quality of maternal attachment is poor, reproductive success appears unaffected.*

### **Evolutionary implications**

In a recent article,<sup>13</sup> we refer to Neel and Ward's momentous paper<sup>14</sup> in which they propose lineal fissioning, as in the antagonistic tribal village fissioning described by Chagnon among the Yanamamo, as a major evolutionary mechanism, particularly in humans.<sup>15</sup>

Neel and Ward estimated that evolution, as judged by blood group shifts among six Central and South American Indian tribes, is minimally ten times more rapid in humans than in any other mammal thus far examined, and that with a single fission a village has covered half the genetic distance towards forming a new tribe. Such rapid change is clearly aided by the human ability and propensity to track kin, and the tracking of kin is a major preoccupation in every known culture.

With regard to Internal Working Models, we need only repeat the point that group integrity must depend, in part, on some kind of imprinting mechanism - one that assures permanent membership in at least the immediate extended family. The attachment literature makes the case for the unconscious learning of modes of relating over the first years, and we need only add that early exposure to the social ways of one's family is, at the same time, early exposure to the social ways of one's subculture. As we noted above, without allegiance to a kin group and the mechanisms which enable it, there would be no stable sampling at the group level, and no chance for achieving evolutionary perks.<sup>4</sup> Thus, both attachment and antagonistic fissioning probably facilitate the production of new surviving lineages and sub-populations (evolutionary changes). There is not time to discuss this more fully, but I believe we are here dealing with a true case of group selection.<sup>16</sup>

Before closing, let me add a word about temperament and inherited behavioral propensities. An

example is perhaps best: when I worked with infant twins, a number of years ago, there was one pair of fraternal boys that make the point quite well. They were the 5th and 6th children of a young Catholic couple, and were most often left to fend for themselves by the beleaguered parents. By 10 months one of them was climbing out of his crib and joining the other children, while his less active twin simply watched as best he could from where his crib was. They thus "made" different experiences for themselves, rather totally dependent on intrinsic physical abilities and temperament. In follow-up visits at five and ten years, similar differences were still the most essential distinguishing characteristics, e.g., at five, one was a sensitive watcher, a concentrator, a creative drawer, while the other was called by the family "smiling Irishman". By ten, child A was doing relatively well at school, while B was still a specialist in conviviality.

The internal representations of infancy were doubtless very different in these two boys, even as they are doubtless more generally alike in identicals, even identicals reared apart,<sup>17</sup> than in fraternal. Furthermore, there is the probability that some group differences,<sup>18</sup> and Hebb's dictum still holds: behavior, including its subjective, interpretive side, is 100% hereditary and 100% environmental.

### **Summary**

There is an apparent continuity in the imprinting phenomenon across phyla, and it is present in gallinaceous birds, goats, canids, man, and probably most mammals. The function of such fixed early learning about relationships seems to involve evolutionary sampling across competitive but related homozygous sub-populations, *a la* Sewall Wright's "evolutionary landscape".

We now know that first experiences with parental figures cause permanent neuro-physiological changes in the hyperstriatum ventrale in chick brains, indicating that internal representations may become neurologically fixed. In the same vein, the hypothetical construct, the Internal Working Model, helps us to take cognizance of the familiar clinical observation that there are relatively permanent

internal representations of early socialization experiences, and try as we might to change them, ignore them, or tame them, they play themselves out in our lives.

A personal note. I recently took a month to write out, as honestly as I could, my own major Internal Working Models and those of my wife, including how they intersect and cause us both joy and pain. I have since read that manuscript to two classes, and in each class a number of students decided to write something like that for their term paper. It is remarkable to document for oneself what we are stuck with from our personal past, and how little is the effect of will, conscious decision or, in fact, psychotherapy.

The effects of "culture" also become clear in these reports, and it is on that area, the subjective experience of culture, that we plan to focus.

In general, the issue of repairing the past is a thorny one, and many of us in the healing industry are now talking like theologians, using such terms and phrases as "spiritual pathway", "transcendence", and "entering the mystery of life". Internal Working Models, it would appear, are formidably conservative and require no less an ally than God if they are to be overcome.

*References: page 22*

## Appendix I

Cross Cultural Q-Sort, University of Chicago, October 15, 1988

D = (DYADS) 35

M = (MOTHER) 27

C = (CHILD) 38  
100

1. Engage one another lovingly (D)
2. Mutually accepting (D)
3. Mother continuously anticipates child's intentions (D)
4. Mother creates a safe atmosphere via encouraging sounds (D)
5. Child feels unhurried and unharried by mother (D)
6. Mother directs child and child is compliant (D)
7. Child struggling with issues of autonomy (C)
8. Mother acts as if child is incompetent (M)
9. Mother-child relationship filled with humor (D)
10. Child seems to internalize mother's goal/ to do what mother wants (C)
11. Child actively asserts own preferences and desires (C)
12. Child inhibited in exploring materials (C)
13. Child is inventive (C)
14. Child is charming (C)
15. Child is center of attention (C)
16. Mother acts like a teacher (M)
17. Child concentrates on task (C)
18. Mother's responses totally appropriate to child's activities (M)
19. Mother awed by child (M)
20. Mother upset by child (M)
21. Mother helpless, child willful (D)
22. Child is inhibited by mother from exploring materials (D)
23. Mother wants child to do it on his/her own (D)
24. Mother tries to minimize head-on conflicts with child (M)
25. Child looks to mother for cues (C)
26. Mother indulges child's desires for closeness and warmth (D)
27. Child frequently switches activities (C)
28. Child is very patient (C)

29. Mother is very patient (M)
30. Mother passive (M)
31. Mother very verbal (M)
32. Child clowns around (C)
33. Mother acts the clown (M)
34. Mother and child tend to remain in physical proximity (D)
35. Mother and child typically pay attention to the same thing at the same time (D)
36. Mother initiates affectionate physical contact (M)
37. Child initiates affectionate physical contact (C)
38. Sense of maternal distance from child (C)
39. Child deliberately antagonizes mother (D)
40. Mother appears depressed (M)
41. Dyad is very verbal (D)
42. Confused role distinction between mother and child (D)
43. Child's mood is unshared (D)
44. Child easily bored (C)
45. Child initiates new routines (C)
46. Mother initiates new routines (M)
47. Mother bored or indifferent to task (M)
48. Sense of repressed rage in mother (M)
49. Child totally dominated by mother (D)
50. Maternal intelligence evident (M)
51. Child generally happy (C)
52. Mother generally happy (M)
53. Mother and child behave in synchrony (D)
54. Child masters task with relative ease (C)
55. Mother and child appear to have unique and separate "agendas" (D)
56. Child exhibits temper (C)
57. Child is impassive throughout (C)
58. Child exhibits changes in mood (C)
59. Child exhibits joyousness (C)
60. Child exhibits independence (C)
61. Mother appears uncertain or embarrassed (M)
62. Child outgoing to outsiders (C)
63. Child appears unaffected by camera and experimenters (C)
64. Mother permits child's destructiveness (D)
65. Both partners respectfully acknowledge

- one another's turn (D)
66. Mother concentrates on child (M)
67. Child's attention shifts from activity at hand (C)
68. Mother responds to and shares child's enthusiasm (D)
69. Mother discourages child's independence (D)
70. Mother and child mis-attuned (D)
71. Mother facilitates child's play with timely help (D)
72. Dyad's energy level is low (D)
73. Child is more competent than it acts (C)
74. High level of concentration characterizes mother and child (D)
75. Mother appears annoyed with child (M)
76. Child easily frustrated (C)
77. Mother's tone of voice is supportive (M)
78. Mother is task oriented (M)
79. Mother exudes warmth (M)
80. Mother and child typically avert their gaze from one another (D)
81. Child often uncontrollable (C)
82. Mother focussed on self more than on child (M)
83. Mother and child work/move at same pace (D)
84. Many toys are explored together by mother and child (D)
85. Mother intrudes into child's play (D)
86. Child feels understood (D)
87. Child tends to animate the toys (C)
88. Child neatly puts toys away (C)
89. Mother and child tuned into each other (D)
90. Mother tends to nag (M)
91. Child refuses to give up toy (C)
92. Child combines two or more toys (C)
93. Child exhibits discernment between one toy and another (C)
94. Child seems excitable (C)
95. Child is highly vocal (C)
96. Child seems often to "phase out" (C)
97. Mother reprimands or threatens child (M)
98. Child is usually active (C)
99. Mother seems overly active (M)
100. Child appears depressed (C)

# ARTICLE:

by J Price

## The rat resident/intruder paradigm: A model for the Involuntary Subordinate Strategy (ISS)

### BACKGROUND:

#### The rat

Rats are members of the family Muridae which contains mice and moles.<sup>1</sup> Only a few of the 300 species of the genus *Rattus* are commensal with man. Laboratory rats are all derived from *Rattus norvegicus*. This is a burrowing rat which recently ousted the climbing *Rattus rattus* from temperate zones; sometimes the two species co-exist, *norvegicus* in the basement and *rattus* in the attic. When rats explore a strange environment, they create paths marked with scent; which may help them escape from cats. Wild rats commensal with man avoid new objects, or any change in a familiar environment; this "neophobia" is thought to represent selection for rats which avoid traps and bait.

Rats do not form obvious hierarchies but male rats divide naturally into three status groups.<sup>1,pp125-6</sup>

*Alphas move about without hesitation or any attempt to take flight from other rats. They are the only rats that attack. Secondly, omegas are the result of defeat by one or more alphas. Such rats flee at the approach of an alpha. In confined colonies omegas, after a day or two of persecution, are marked by their slow movements, drooping posture and bedraggled appearance. They lose weight, and die if not removed. A third category is needed for rats which, after defeat, adapt themselves to an inferior role: they have been called betas. They endure defeat and succeed in feeding with enough freedom to gain weight. Omegas and betas associate together without conflict... In unconfined colonies, adult males probably vary in status from alpha to beta; any rat with an omega status would soon die or emigrate. The observations of Telle on natural populations, though not detailed, conform with this.*

Laboratory rats have largely lost the neophobia of wild rats, nor do they struggle or bite when handled. The males are less aggressive to other male rats. It

is probable that the tameness to handling and the reduced intraspecific attack behaviour are mediated by different neural systems.<sup>2</sup>

Male rats brought up together do not usually attack each other. Even strange rats introduced to the same cage at the same time do not fight. But if one male is in the cage for ten or more minutes before the introduction of a second rat, it may attack the intruder. Unless the intruder is much bigger, the resident usually wins the fight! The two rats then have a dominance/subordination relationship. The process completes in a few hours, after which there is little fighting; aggressive acts by the intruder rat fall to zero while aggressive acts by the dominant former resident take up less than 1% of behavioural time.

The defeated intruder rat can be studied while still in the cage of the resident, after variable lengths of time, or the intruder can be removed after the defeat experience and kept in isolation (or with other non-aggressive rats) and then studied after variable periods, and compared with the dominant rats and with rats not subjected to agonistic experience. In the first two sections to follow, the defeated rats were left in the presence of the winners until they were studied; in the last two sections, they were removed from the presence of the winners and studied under conditions of isolation for periods up to two months.

### FOUR STUDIES OF DEFEATED RATS:

#### Shortening of life span

The Blanchards and their colleagues at the University of Hawaii studied groups of rats composed of three males and three females over their total life span.<sup>34</sup> The groups rapidly formed stable male hierarchies of one dominant rat and two subordinates who showed little mutual aggression. The subordinate males showed greatly reduced time on the surface (out of their burrows), and they showed maladaptive responses to the presence of a cat. After a cat had been placed briefly on the surface,

the dominant rats increased their episodes of peeking out of the burrow, but each episode was greatly shortened; the subordinate rats did not increase the number of peeks, but each peek was greatly prolonged, which suggested to the experimenters that "the subordinate males were not obtaining or processing information very effectively".

They found increased plasma corticosteroids and reduced corticosteroid binding globulin in the plasma of subordinate male rats; some subordinate rats failed to give the normal increase in corticosteroid level to restraint stress, and these non-responders had lowered plasma testosterone. The subordinates in general had reduced plasma testosterone, their voluntary alcohol intake was raised, and their life-spans were shortened; they had increased 5-HIAA/5-HT ratios in a number of brain areas. Those subordinates who kept closer to the dominant males and slept in the same chambers lived longer than those subordinates who isolated themselves (cf. the findings of von Hoist in tree shrews<sup>5</sup>). The subordinates who died early showed reduced aggressive and sexual behaviour 200 days before death, even below the reduced levels of those subordinate rats who survived the normal life span.

In other studies, both dominant and subordinate males showed adrenal and spleen enlargement, and thymus reduction, but only the subordinates showed reduction in testis weight. In a study still to be reported in full, mRNA pro-opiomelanocortin increased in the pituitaries of subordinate rats.

*Comment:* The ISS is observed under semi-natural conditions. Subordination has a large effect on the daily behaviour of the rat, even when agonistic behaviour is not occurring. Disordered behaviour of subordinates in response to a cat is interesting, suggesting overlap between social and non-social defence systems. We do not yet know if the altered 5-HIAA/5-HT ratio picks up a same change detected in other ways by other workers.

### **Raised plasma corticosteroid concentrations**

Raab and his colleagues in Bordeaux studied both dominant and subordinate rats following ten days of

cohabitation, and compared these with isolated controls and non-aggressive paired controls.<sup>6</sup>

They classified the changes according to whether the changes in the two experimental groups (of dominant and subordinate rats) differed from the controls in the same or opposite directions.

*Dichotomic changes* occurred when the two groups diverged from controls in opposite directions. In the open field test, dominants explored the area during the whole test period whereas subordinates explored briefly and then withdrew to a corner and self-groomed; controls had intermediate values. Dominants had higher prostate weights than controls, and subordinates lower prostate weights than controls.

*Concomitant changes* occurred when the two groups changed in the same direction compared to controls. Adrenal tyrosine hydroxylase was increased in both groups. This increase was presumably due to the fighting rather than to defeat or victory.

*Specific changes* occurred when only one experimental group differed from controls. The subordinate group lost about 5% of their body weight, and their plasma corticosteroid levels were approximately double those of all the other groups; their lymphocytes showed a reduced incorporation of radioactive thymidine when stimulated with mitogenic agents.

*Comment:* There seems to be an increase in adrenal medullary activity in both dominants and subordinates due to the agonistic interaction, but increases in adrenocortical activity in subordinates only.

### **Insensitivity to morphine analgesia**

Klaus Miczek and his colleagues at Tufts University in Massachusetts and the Scripps Research Institute in La Jolla found changes lasting up to two months in morphine-induced analgesia of defeated rats.<sup>7</sup> This finding is based on work on stress-induced analgesia in rodents. When a mouse is defeated, its tail flick latency to heat stress is raised for several hours; that is, it takes longer to move its tail from a hot object. The analgesia occurs in two phases. First, for ten minutes after defeat, there is a non-opioid analgesia,

thought to facilitate escape, so that reflex reactions to pain during escape do not interfere with the essential business of running away. Then, for about an hour, there is an opioid-mediated analgesia, which can be blocked with naloxone and other opioid antagonists; this is associated with bodily immobility, and may be part of the "tonic immobility" response, in which the animal feigns death when in the jaws of a predator.<sup>8</sup> Both types of analgesia occur in rats in the presence of an aggressive resident: the non-opioid type occurs immediately after the rat signals submission with "defensive upright", but opioid analgesia occurs only after the submitted animal has experienced a period of aggression from the winner.

Morphine also raises the flick latency, and during the few hours after defeat the response to morphine is enhanced in both mice and rats. Then the effect is reversed, and for a prolonged period, up to two months, the rat is relatively insensitive to the analgesic effect of morphine. The rats behave the same as rats fed with morphine, who rapidly develop tolerance. Both defeated mice and morphine tolerant mice show a morphine-withdrawal response to the morphine antagonist naloxone.

The authors suggest that defeat produces a prolonged activity of endogenous opiates in the area of brain subserving pain appreciation. Other actions of morphine are not affected by defeat; for instance, the rats continue to be able to discriminate injections containing morphine at the former level of accuracy.<sup>9</sup> Is this tolerance to morphine abolished by antidepressant drugs? We do not know.

Another effect of defeat is increased "emotionality" manifested by a reluctance to explore the open arm (rather than the enclosed arm) of a raised Plus-Maze.<sup>10</sup> This emotionality seems to be mediated by corticotrophin releasing factor in the central nucleus of the amygdala, because it can be blocked by the injection of small quantities of a CRF antagonist into this site but not into the dorsal striatum which served as a control. The effective dose for injection into the amygdala was 250 nanograms, both 125 and 500 ng being ineffective. Medium doses of CRF antagonist injected into the cerebral ventricles also abolished

the defeat-induced emotionality. Abolition of the defeat-induced emotionality with the intra-amygdaloid CRF antagonist did not abolish the hypothalamo-pituitary-adrenocortical reaction to defeat (which is an activation prolonged more than the brief activation induced by fighting and/or winning). Nor did the abolition of the systemic cortisol response by an immune antagonist to CRF abolish the defeat-induced emotionality.<sup>11</sup>

This work suggests that there are at least three independent long-term CNS reactions to defeat: an increase in endogenous opioid activity resulting in tolerance to the analgesic effects of morphine, CRF activity in the central nucleus of the amygdala causing "emotionality" manifested by cautiousness on an open maze, and the well-known hypothalamo-pituitary-adrenocortical response. Possibly one of these reactions results in the disorganisation of circadian rhythms which follows defeat.<sup>12</sup>

*Comment:* In the brains of these defeated rats and mice, we must be looking at the CNS component of the ISS. Therefore our depressive patients should show the same changes (allowing for species differences). If depressives are equivalent to endogenous morphine addicts, they should have an adverse reaction to morphine antagonists such as naloxone and naltrexone, but "the therapeutic effect of this drug on these diseases [schizophrenia, depression and tardive dyskinesia] was minimal or absent"<sup>13</sup> and there was no mood change in alcoholics treated with 50 mg of naltrexone daily.<sup>14</sup> Nevertheless, Miczek *et al.* findings indicate the need to study carefully the endogenous opiates in depression.

Since a CRF antagonist abolished defeat-induced emotionality, and had essentially the same effect as a benzodiazepine, we should look at the possibility of blocking amygdaloid CRF activity in depressed and anxious patients. The problem would be to cross the blood-brain barrier, hopefully without inducing a systemic corticoid deficiency. And would a CRF antagonist have the same disadvantages as the benzodiazepines, such as the development of tolerance? Perhaps benzodiazepines act by blocking central CRF, possibly indirectly.

### **Passivity reversed by antidepressant agents**

Jaap Koolhaas and his colleagues at Groningen University in The Netherlands also used the rat resident-intruder paradigm, and they found that a lasting state of 'depression' followed a single defeat.<sup>15</sup> Their main measure of depression was the duration of immobility following sudden cessation of background noise. Normal rats have a brief period of immobility in this situation, but in the defeated rats the duration of the immobility was increased, and this effect lasted for several weeks. Interestingly, the change took two to three weeks to develop - one week after defeat there was negligible increase - at three weeks the time of immobility was double that of controls and the increased immobility was still present at ten weeks. The immobility was reduced to control levels by eight days treatment with clomipramine 5 mg per kg and this normalisation continued for at least three weeks after treatment was stopped. The increased immobility was also abolished temporarily by 12 hours sleep deprivation immediately before the test. The defeated rats also showed lengthened periods of immobility in a forced swimming test carried out ten weeks after defeat. They also lost weight for two weeks after defeat, but subsequently regained weight; and this shorter time scale suggests that there may be more than one separate process set in motion by the defeat experience. The increased immobility noticed by the Groningen group may be the same as the "reduced open arm of maze exploration" which the Tufts group called "emotionality"; although it is interesting that the Tufts group found the acoustic startle reflex to be unaffected by defeat, morphine and naloxone.

*Comment:* Passivity after defeat is not surprising. What is interesting is the time-scale. The passivity was not apparent one week after defeat, but increased gradually over 2 to 4 weeks and remained steady for a further 6 weeks. This is the time scale similar to that seen in human depression after stressful events. Also of interest is abolition of the passivity by an anti-depressant drug, and its temporary suppression by sleep deprivation, which is known to have a similar temporary effect on human depressive states. In later work the same authors found that a single defeat experience altered

glutamate receptor binding in hippocampal CA3 area of male rats.<sup>16</sup>

### **Overall comment**

All this work is promising, and should be replicated on many other species. For me there are three main underlying questions:

1. To what extent are we dealing with a specific ISS and to what extent with a non-specific response to "stress"? Would the same changes follow the experience of being savaged by a cat? We know the response to a powerful conspecific is different to that to a powerful predator: for instance the rat does not show the submissive responses of "defensive upright" and "crouch" in response to a predator; is there an equivalent difference in CNS responses? And what about the stress of "inescapable shock"? Into which functional module does the rat classify this experience? We should not forget Keith Dixon's exciting, and, I think, still unpublished finding that amitriptyline blocks the hormonal response to social stress but not to non-social stress in rats. This suggests a differentiation of the social and non-social defence systems.

2. Is the rat ISS likely to be homologous to the human ISS? We know that useful things tend to evolve many times over, sometimes using very similar mechanisms but sometimes totally different ones. I have always been wary of rodents because it seems to me that in the rodent lineage the phasic switch which operates the ISS has been used up in dealing with climatic stress, and controls hibernation in those rodents that hibernate. In non-hibernating rodents it is probably inactive. This is just a hunch. It would be really useful to see the rodent work replicated in sugar gliders and better still in primates.

3. Tonic immobility is a predator avoidance strategy not present in primates. Responses to intraspecific aggression may have evolved out of predator avoidance behaviour.<sup>8</sup> Have primates adapted the neural substrate of the tonic immobility reaction to create the ISS, and thus fashioned one of the strategies which have made group living possible?

### Individual variations in defeat behavior

The Groningen workers noted that the development of a severe defeat reaction depended on the intensity of the conflict leading to defeat:

*Defeat of an aggressive, i.e. potentially dominant, male rat results in the development of long-term behavioral depression, whereas non-aggressive rats, i.e. potential subordinates, fail to develop this.*<sup>17</sup>

This may be the rat equivalent of the difference between active and passive submission. If you submit actively, possibly having what John Birtchnell calls a need for lowerness, you do not need to get depressed; only if coerced into submission does the submission become passive and the ISS appear.

There are two clear rat phenotypic responses to defeat, in one the defeated rat seeks the company of the winner and adopts a submissive role; in the other it tries to get far away, and if prevented, it declines and dies. These two defeat behaviour patterns are also described in the tree shrew by von Hoist.<sup>5</sup> We do not know whether these two "yielding strategies" reflect genetically determined individual differences, social experience, or the severity of the defeat inducing the yielding.

It would not be surprising if there were two fundamental defeat strategies in mammals as a whole. One would aim for escape, and search for new territory; we would expect this to be commoner in "territorial" species such as the tree shrew. If escape is blocked, which presumably seldom occurs under natural conditions, these animals suffer severe reactions: their physiology becomes disordered, and they die. Keith Dixon calls this "Arrested Flight".<sup>18</sup> If their ISS is qualitatively the same but more intense than animals allowed to escape, then they are good experimental subjects for studying the physiology of the ISS.

The other basic defeat strategy is based on what Michael Chance has called "reverted escape". The pulls of the group are greater than its pushes, so when defeated the animal seeks the company of the winner and adopts a subordinate role. If escape is

impossible, this is clearly a more advantageous strategy. It is this strategy which makes group living possible, and we would expect it to be commoner in group living species such as *Rattus norvegicus*.

A third strategy has been observed in laboratory rats, and is presumably an artifact of inbreeding:

*Laboratory rats put in a strange cage with a resident wild male may not evoke typical attack; but if they do, they tend not to respond at all: instead of showing the signs of disturbance observed in wild rats, they may continue their exploration of the cage. The same behavior has been observed in rats of the second generation derived from a cross between wild and laboratory rats. It is a remarkable sight to see a laboratory or hybrid male moving slowly around a cage while a resident wild male vigorously postures or leaps at it without producing any evident alteration of behaviour.*<sup>1, p127</sup>

We do not know whether this "coolness" on the part of the laboratory rat protects it from the various changes of the ISS. If so, the laboratory rat seems to have evolved the genetic basis required for a truly hedonic culture.

### Individual variation in attack behaviour

There is a lot of genetic variation in attack behaviour; in many strains, there is a proportion of "attackers" and a proportion who do not attack under any circumstances - even before the rats have been given the opportunity to differentiate into alphas, betas and omegas. Miczek *et al.* state that only 60-70% of male Long-Evans residents fight with an intruder.<sup>19</sup> Barnett states: "If a strange male approaches a non-aggressive male on the latter's territory, the resident may crawl under the stranger instead of attacking".<sup>1 p107</sup> Attack behaviour may be altered by drugs; for instance, small doses of alcohol or benzodiazepines enhance attack behaviour in mice, not by reducing latency to attack, but by increasing the duration of each bout of fighting.<sup>18</sup>

There is room for much research into these matters, both in the Muridae and in other families.

References: page 22



# ABSTRACTS & EXTRACTS...

**McGuire MT, McKinney WT, Ness RM & Troisi A (Eds.) Special Issue Introduction: Mental disorders in an evolutionary context.**

**Marks IM & Nesse RM: Fear and fitness: An evolutionary analysis of anxiety disorders.**

**Feierman JR: A testable hypothesis about schizophrenia generated by evolutionary theory.**

**Kerber KB: The marital balance of power and quid pro quo.**

**Schelde T & Hertz M: Ethology and psychotherapy.**

**McGuire MT, Fawzy FI & Spar JE: Altruism and mental disorders,**

**Hill EM, Young JP & Nord JL: Childhood adversity, attachment security, and adult relationships: A preliminary study.**

**Nesse RM; An evolutionary perspective on substance abuse.**

**Schelde T: Ethological research in psychiatry.**

**McKinney WT, Gardner R, Barlow GW & McGuire MT: Conceptual basis of animal models in psychiatry: A conference summary.**

## **SPECIAL ISSUE INTRODUCTION:**

**Mental disorders in an evolutionary context. In Michael T McGuire, William T McKinney, Randolph M Nesse & Alfonso Troisi (Eds): Ethology and Sociobiology 1994;15:p245.**

This is a special issue of Ethology and Sociobiology which focuses on ways in which evolutionary theory and ethology can inform our understanding of mental disorders. The contributions cover a variety of topics. Some are theoretical. Others are databased.

One might ask if psychiatry has a need to consider alternative ways of describing and explaining mental disorders. For those not familiar with psychiatry it may come as a surprise to find out that psychiatry lacks a theory of behavior, that it pays minimal attention to the details of function, that it has no general conceptual framework within which to organize and prioritize its data, that it has no accepted theoretical system to guide its research, that the signs and symptoms associated with mental disorders as well as disorders themselves are seldom examined from an adaptationist perspective, and that it is divided into "schools" each of which advocates different causes for disorders. While evolutionary biology and ethology can not resolve all of these issues, they may well resolve some and inform others. Hence, this issue.

**Marks IM & Nesse RM: Fear and fitness: An evolutionary analysis of anxiety disorders. Ethology and Sociobiology 1994;15:247-261.**

This article reviews the evolutionary origins and functions of the capacity for anxiety, and relevant clinical and research issues. Normal anxiety is an emotion that helps organisms defend against a wide variety of threats. There is a general capacity for normal defensive arousal, and subtypes of normal anxiety protect against particular kinds of threats. These normal subtypes correspond somewhat to

mild forms of various anxiety disorders. Anxiety disorders arise from dysregulation of normal defensive responses, raising the possibility of a hypophobic disorder (too little anxiety). If a drug were discovered that abolished all defensive anxiety, it could do harm as well as good. Factors that have shaped anxiety-regulation mechanisms can explain prepotent and prepared tendencies to associate anxiety more quickly with certain cues than with others. These tendencies lead to excess fear of largely archaic dangers, like snakes, and too little fear of new threats, like cars. An understanding of the evolutionary origins, functions, and mechanisms of anxiety suggests new questions about anxiety disorders.

**Feierman JR: A testable hypothesis about schizophrenia generated by evolutionary theory. Ethology and Sociobiology 1994;15:263-282.**

A specific hypothesis *about* schizophrenia, which is generated by an application to behavior of the Darwinian theory of evolution by natural selection, is presented. For reasons that are developed herein, the hypothesis was derived from the known behavioral-physiology differences between nocturnal and diurnal mammals. The hypothesis is being published prior to its testing to facilitate the acceptance or rejection of the specific evolutionary-derived theory from which it was drawn and also to focus attention on the more basic, yet often clinically ignored, question, "What is schizophrenia?"

**Kerber KB: The marital balance of power and quid pro quo: An evolutionary perspective. Ethology and Sociobiology 1994;15:283-297.**

Although the marital therapy literature recognizes the importance of quid pro quo in marital negotiations, there has been little attention to certain important sources of power for men and women in that process. This paper will show how parental investment, certainty of paternity, patterns of mate preference, intrasexual competition, and reproductive status all affect the bargaining positions of men and women.

Both premarital and marital negotiations will be discussed. Put into a developmental perspective, this will help clarify what is at stake in the common sources of conflict between the sexes. These considerations form a central part of the socioecology of marriage.

**McGuire MT, Fawzy FI & Spar JE: Altruism and mental disorders. Ethology and Sociobiology 1994;15:299-321.**

Data suggest that the theories of kin selection and reciprocal altruism are viable working models to explain altruistic behavior. It remains to be demonstrated if these models can explain the behavior of persons with mental disorders for whom altruistic behavior is reported to be reduced. This paper addresses this issue. Part I reviews proximate factors that are thought to influence both altruistic decision making and interindividual variation in altruistic behavior. The focus is on trait signaling by potential beneficiaries and the evaluation of signals and altruistic decision making and interindividual variation in altruistic behavior. In Part II, points developed in Part I are combined with clinical and empirical findings to analyze data on personality disorders and dysthymic disorder. The analysis leads to three causal hypotheses: Reduced altruistic behavior may be an evolved strategy, a consequence of dysfunctional recognition systems or algorithms, and/or a secondary response to an increase in symptoms. Different disorders and features of disorders are explained by each hypothesis.

**Hill EM, Young JP, Nord JL: Childhood adversity, attachment security, and adult relationships: A preliminary study. Ethology and Sociobiology 1994;15:323-338.**

Several evolutionary theorists have linked early rearing context to later reproductive strategy, hypothesizing that strategies differentiate during development as functional responses to ecological characteristics, by individuals or through parental manipula-

tion. Attachment security has been proposed as a mediator. In this study, 40 young adults were given a multidimensional assessment, including the Hazen and Shave Adult Attachment Questionnaire. Twenty-four subjects were classified as having secure attachment styles, 16 as nonsecure. The magnitude and predictability of parental investment during childhood was classified as lower if there was a brief intersibling interval, parental divorce, fewer economic resources, or less nurturing parents (i.e., more childhood adversity). Several such indicators were present for 17 people, 12 of whom were nonsecure, compared to only 4 of the 23 others. The nonsecurely attached subjects were less likely to have attained enduring marriages. The 6/16 nonsecure who had a marriage or cohabitation began them at a younger age and after a shorter courtship period than did the 15/24 secure with such relationships. Separations or divorces had already occurred in the relationships of 4/6 nonsecure versus 5/15 secure. Attachment security was associated with childhood adversity and adult relationships for both men and women, when analyzed separately. A retrospective study cannot address cause and effect, because poor adult relationship outcomes might bias recall of parental behavior. However, results are consistent with theories that unpredictable early environments foster short-term rather than long-term mating strategies, possibly through affecting attachment styles.

**Nesse RM: An evolutionary perspective on substance abuse. Ethology and Sociobiology 1994;15:339-348.**

This article describes how recent advances in understanding the evolutionary functions of emotions can help to reconcile diverse approaches to substance abuse. Emotions can be understood as specialized states that prepare individuals to cope with opportunities and threats. Drugs that artificially induce pleasure or block normal suffering disrupt these evolved mechanisms, and this should tend to interfere with adaptive behavior, even if the drugs are medically safe. Nonetheless, we routinely use drugs quite safely to block defenses like pain, cough,

and anxiety. This apparent contradiction is explained by the relatively small costs of defenses compared to the potentially huge costs of not expressing a defensive response when it is needed. An evolutionary perspective has implications for substance abuse research, treatment, and social policy. This perspective suggests that the search for etiology needs to address the human tendency to abuse drugs separately from individual differences in these tendencies, that clinical treatments that take account of the broad range of patients' emotional life are well justified, and that social policies need to address substance use and abuse not as diseases to be cured but as human tendencies that need to be managed. To prepare for future drugs that will likely alter emotions safely, we urgently need a better understanding of the adaptive function of the emotions.

**Schelde T: Ethological research in psychiatry. Ethology and Sociobiology 1994; 15:349-368.**

Ethological or behavioral psychiatry is a young discipline -- only 20-30 years old. It is defined as the empirical and evolutionary study of nonverbal behavior observed in patients with psychiatric disorders. This paper gives a short survey of the theoretical and methodological background of this research. At the same time, some trends and results are elucidated. An attempt is made to emphasize both theoretical perspectives and clinical implications. As a new dimension in psychiatry, ethological-psychiatric research can contribute to diagnostics, treatment assessment, staff-patient interaction, doctor-patient interviews, prediction of medication response, prediction of approximate hospital stay, and suggested environmental therapies.

**McKinney Jr. WT, Gardner, Jr., R, Barlow GW & McGuire MT: Conceptual basis of animal models in psychiatry: A conference summary. Ethology and Sociobiology 1994; 15:369-383.**

Future progress in selected areas of psychiatric research will depend upon use of animal models.

The incorporation of such models into clinical psychiatry needs to be done carefully and in a way not only consonant with descriptive psychiatry and cellular-molecular biology, but also with evolutionary biology and related fields. This report summarizes critical concepts of evolutionary biology that provide a framework for relating animal modeling research to clinical issues.

**Schelde T & Hertz M: Ethology and psychotherapy. Ethology and Sociobiology 1994;15:383-392.**

During the last 25 years, ethology has developed systematic and quantitative methods for studying psychiatric patients' behavior, especially nonverbal behavior. Both research results as well as theoretical considerations may be of interest and value for

psychiatric interviews and psychotherapy. Direct observations of endogenously depressed patients on the ward show significant behavior changes from admission to discharge. The depressive phase is characterized by withdrawal, nonspecific gaze, looking down, little self-activity, little motor activity, and a substantial reduction of social interaction. The recovery phase is characterized by verbal and nonverbal communication, that is, nod, smile, laughter, gesticulation, help, and others. Such behaviors are primarily useful for the assessment of a patient's mental state in the ward environment. In addition, they may also contribute to the psychiatric interview and future psychotherapeutic situations. These elements are related to four basic motivation categories: relaxation, assertion, contact, and flight. The theoretical considerations include evolutionary theory, ethological theory, and regulation-deregulation theory.

## *FINAL REMINDER*

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*continued from page 22*

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# **ANNOUNCEMENT...**

**How to Want What You Have: Discovering the Magic and Grandeur of Ordinary Existence** by Tim Miller. Publishers: Henry Holt & Co Tel: (800) 488 5233. Hardback \$19.95.

This new book represents an attempt to write a scientifically and philosophically responsible popular self-help/inspirational book. The premises are explicitly Darwinian, and one chapter quickly sketches sociobiology-

The premise holds that people naturally strive for the prerequisites to reproductive success, which, among humans, both now and in the EEA, can be summarized as wealth, status and love. However, there is no instinctive mechanism that tells us we have achieved "sufficient" wealth, status or love. I call this the "float valve dilemma". We have no float valve to tell us that our tanks are full. "Sufficiency" in this context cannot be defined; the existence of such a mechanism would violate the logic of evolution.

Consequently, the ordinary desire for happiness, or at least serenity, is on a collision course with relentless, insidious and insatiable human desires for ever more wealth, status and love, bequeathed to us by the evolutionary history of our own species, and the essential logic of evolution itself. These desires are quite seductive, even though we all know perfectly well that gratification of desire produces temporary satisfaction, followed by new dissatisfaction and a new crop of desires.

(Are male evolutionary scientists less likely than other guys to trade in their smart, loyal, near-menopausal forty-four year old wives for a pair of less smart, less loyal, fertile-looking twenty-two year olds? I doubt it.)

Old, stable societies probably develop social mechanisms that define and encourage various kinds of "sufficiency" but "progress" and modern capitalism has wiped out most such mechanisms. So modern individuals have little choice but to use their higher cognitive functions to recognize the reality of the float valve dilemma in their own lives, and then learn to moderate their own instinctive desires. (I call this learning to "want what you have".)

Otherwise we moderns are highly vulnerable to various forms of discontent, restlessness and selfishness if not outright misery and greed. I suggest some methods for moderating relentless, instinctive desires in this manner. My methods are slight modifications of old-fashioned Aaron Beck style cognitive psychotherapy. I propose that if cognitive therapy can be used to treat temporary problems like depression, it might also be employed to treat permanent problems like the essential human condition.

It's also worth mentioning that if there is to be much hope for widespread social justice, environmental protection and restoration, and world peace, then large numbers of people are somehow going to have to learn to want what they have. The standard social science model and its spiritual cousin, *secular humanis*, obviously don't have adequate answers, and most traditional religious approaches that moderate desire have been wiped out by education, literacy, scientific and technical progress, advertising, capitalism, and so on.

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## Letter: Sloman ... p 3

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.....continued on page 20