

# ASCAP NEWSLETTER

Across-Species Comparisons And Psychiatry Newsletter

Volume 3, No. 6, 15 June 1990

"... theories at distinct levels often co-evolve, as each informs and corrects the other, and if a theory at one stage of its history cannot reduce a likely candidate at a higher level, it may grow and mature so that eventually it does succeed..." Churchland<sup>1</sup>

(c/o Russell Gardner, 1.200 Graves Building (D29), University of Texas Medical Branch, Galveston, TX 77550)<sup>2</sup>

For the philosophy guiding this newsletter, predicated upon combinations of top-down and bottom-up analyses, see footnote on p11<sup>3</sup>

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

Features: This issue publishes much correspondence about and a summary of the second meeting of the "Basic Plan Group" in NYC on 16 May 1990. ... p.6

K Sharp and CR Badcock provide an essay entitled: Paternity, Identification and Latency: A Possible Genetic Basis for some Freudian Observations. . . . . p.8

This is ASCAP's second essay about Badcock's new book, Oedipus in Evolution, which has now been reviewed in Nature, the 24 May 1990 issue<sup>4</sup> (the full page review was illustrated by a large mid-page Greek picture including the infant Oedipus, the rescuing shepherd and a long spear invading the neighboring print). Congratulations on the recognition!

Basic Plan Group Next Meetings: Many thanks to Lubo Kanov, Jay Harris and the Cabrini Medical Center for their kind efforts for the second (NY) meeting. The basic plan group will meet next over lunch at the Los Angeles Human Behavior and Evolution Society (HBES) Annual Meeting on Sat, 17 Aug 1990, from noon to 2 pm, exact

site to be determined and posted/announced on Friday.

Agenda includes: 1) Minutes of the NY meeting (see below). 2) Next meeting in England (with JS Price) mid-1991. 3) Considerations that stem from the Price essay of next issue and other communications (see Price letter and figure 1 this issue). 4) Learned helplessness project (see P Gilbert letter, conclusions and summary of the NY planning group meeting this issue). 5) Data from Marsella on difficulties in across-cultural comparisons of depression. 6) Other business.

Next Issues: The immediate next issue (July) features John Price's essay entitled "Metaphors of Submission" that outlines his ethological theory of depression. We hope you will join others (including prominent figures not now ASCAP readers) in peer-reviewing it (that is, in providing a 2 page double spaced response to be published in a later ASCAP).

Fall issues will feature such replies to the Price essay, my response to Irina Zhdanova's investigation of CSF from manic patients in two animal test systems of social signals (summarized by her in the May 1990 ASCAP), a condensed summary of T Schelde's "Ethological and Evolutionary View of Endogenous Depression," and excerpts from Tiger and Fox's "New Introduction" to their re-issue of The Imperial Animal<sup>6</sup>.

Quoted Abstract; from J Lee Kavanau  
Conservative behavioural evolution:  
the neural substrate Anim.Behav  
1990;39:758-767

The evolution of neural circuitry-differs from that of other structures in a significant respect. Natural selection cannot physically eliminate most neuronal pathways for obsolete functions, because the individual neurons of these pathways usually are multifunctional, also being components of pathways for adaptive functions. Selection for loss of obsolete neural functions probably operates largely through mechanisms that suppress or neutralize the corresponding neuronal pathways without disabling pathways for adaptive functions. Accordingly, rather than becoming eliminated physically, obsolete pathways not selected against merely cease to be accessed. These mechanisms of loss of neural function provide one of several intrinsically conservative bases for neural ontogeny and evolution. Since the central nervous system encodes and programmes motor outputs for behaviour, the obsolete circuits that are retained include pathways for obsolete behaviour. In vertebrates, such behaviour can sometimes be elicited directly by appropriate stimuli, but when the neuronal pathways for the obsolete behaviour are in an inhibited or blocked condition, they first must be rendered accessible (which can occur adventitiously, pathologically, experimentally or in hybrids). The vertebrate central nervous system also plays a large role in programming the ontogeny and maturation of gametes. Egg formation in many species appears to be highly conservative, extensively retracing ancestral stages. This conservativeness probably derives from influences both of being under neural control and of having an evolutionary history in which the provisioning of eggs was continually on the increase.

Letters: February 5, 1990

*I read your last ASCAP newsletter with great interest. Since there are not many people in the world who are seriously interested in across-species comparisons and psychiatry, I think it might be of interest to you what I have written along these lines. Regarding the top-down and bottom-up analysis you will find something of interest in our 1988 An-*

nual Report.

*I plan to be at the meetings of the Soc of Biol Psychiat and the Amer Psychiat Assoc in NY in May. I hope that we can meet at this occasion.*

*Detlev Ploog, Max Planck Institute for Psychiatrie, Munchen, FRG*

And indeed at the APA we did meet: fortuitously sitting adjacent to one another in the third row to hear Gerald Edelman on "Naural Darwinism; somehow we managed to be in the same place at the same time to hear words relevant to issues raised by Dr. Ploog (DP) long before ASCAP: In 1964 Dr. Ploog published Verhaltensforschung und Psychiatrie<sup>7</sup> (Ethology and Psychiatry) that anticipated our ASCAP concerns.

Manuscripts published in English in the 1980s are: 1. DP: Psychopathology of human emotions in view of neuroethology. (Eds) K Davison, A Kerr (1989) Contemporary Themes in Psychiatry London: Royal College of Psychiatrists, pp441-458.

2. DP: Human neuroethology of emotion. Prog Neuro-Psychopharmacol & Biol Psychiat 1989;13: S15-S22

3. DP: An outline of human neuroethology. Human Neurobiol 1988;6:227-238

4. DP: Foreword. (Ed) JD Newman (1988) The Physiological Control of Mammalian Vocalization NY: Plenum Press, pp.vii-ix

5. DP: Neurobiology and pathology of subhuman vocal communication and human speech. (Eds) Todt, Goedecking, Symmes (1988) Primate Vocal Communication Berlin: Springer-Verlag, pp. 195-212

6. DP, Pirke KM: Psychobiology of anorexia nervosa. Psychol Med 1987;17:843-859

7. DP: Neuroethology. (Ed) G Adelman (1987) Encyclopedia of Neuroscience. Vol II Boston: Birkhauser, pp.780-783

8. DP, Brandt F: The destruction of the countenance; an ethological review of leprosy from the standpoint of gestures and facial expressions. Hexagon: A Roche Journal for Doctors 1987;15:#4, pp.24-28

9. DP: Human neuroethology of emotions. Clinical Neuropharmacology 1986;9:S-154:371-373

10. DP: Chapter 7: Biological foundations of the vocal expressions of emotions. (Eds) R Plutchik, H

Kellerman (1986) Emotion: Theory, Research and Experience. Vol III, Biological Foundations of Emotion NY: Academic Press, pp. 173-197

11. DP, Jurgens U: Vocal behavior of nonhuman primates and man. (Ed) SA Corson (1980) Ethology and Nonverbal Communications in Mental Health NY: Pergamon Press, pp.179-189

Letters (cont): 5 April 1990

In regards to the whole issue of depression and defeat it has been brought to my attention that depressive-like states only show up in reptilians or birds when flight is blocked, ie, flight to a new territory or situation is not possible. In fact, birds frequently contest the boundaries of their territory. This is why I qualify about defeat and social control.

If the above is true then we need to work out in what sense we are saying more than the learned helplessness (1h) model. The 1h model stresses the control dimension, hence "defeat plus no alternative" may both be necessary in order to understand depression. This then leads us to consider the blocking (thwarting) of efforts to move to a new situation, such as taking up a new job or relationship, or otherwise changing achievement structures. These blocks can stem from both external and internal sources, externally by economic dimensions and internally by having self esteem stem only from achievement of unobtainable goals. Hence I think we should be cautious here and focus on both the issue of defeat and sense of failure, plus the question of blocking coping options.

I liked your term "subordinate self-perception" from the abstract for the August HBES conference in LA. But I would add the word involuntary so that perhaps we should be thinking of "involuntary subordinate self perception". This idea opens us up to the issues of shame and concealment which are big issues in the psychol-

ogy of depression now.

The issue of submissive behaviour in depression is also complex but relates in my view primarily to the inhibition of explorative behaviour, thereby to send no challenge signals. Submission evolved as a coping response to allow animals to stay with others to whom they may be inferior. In humans voluntary submissive behaviours would not produce depression but involuntary submission would; "voluntary vs involuntary" is an essential dimension.

The paradox in depression of course is that although submissive behaviour evolved as a coping behaviour to avoid aggression, involuntary submissive behaviour in humans actually blocks them from the very things they need to do which is to get on and explore alternatives (explorative behaviour and problem solving are often essential to help an individual out of depression).

The role of submitting to avoid losing SAHP is also an issue. Many conflicts in depression are related to the inhibition of assertive behaviour for fear of loss of approval (SAHP). I have papers on this (they don't use formally the SAHP concept). Generally, inhibitions on assertive behaviour often have to do with fear of the image the person will create in the mind of the other after such behavior: the other will laugh and ridicule, or abandon the person, or escalate the conflict beyond the degree that can be coped with. Hence in this sense the RHP system and the SAHP system seems to be in conflict. This is a major concern for me.

I think that you can gather that issues of ranking remain crucial for me. I have developed the Buss and Craik idea<sup>8</sup> about submissive acts into a scale. Cronbach alpha and retest reliabilities are very good and we have now used it in both normal and depressed populations. From our work and that of others, social

anxiety is related to depression and we need to explore this in detail and in relationship to submissiveness.

I am concerned because we are not getting [the defeat model] conceptually into the SAHP system. I note that noone from ASCAP has taken up that idea, but I think without it we will be in difficulty. I also agree, however, that SAHP may well recruit gene-neural structures of the RHP system and that the psychobiological mechanisms of response to loss of SAHP are very similar to what occurs in the RHP group situation.

The other issue that I think we must take on board here is that of the hostile depressive, such as a narcissistic depression where any degree of submissiveness or humility tends to activate shame, depression and rage – the person is either not capable of submitting enough in a sense or the submissions tend to be grandiose. I don't think it's the case that all depressive process is necessarily submissive behaviour, but subordinate self-perception may well apply to all. I would also make the distinction between submissive behaviour which is expressed within relationships where the person remains engaged (or has desire to remain engaged) versus those in which the person has a desire to withdraw.

I think the psychobiological paper by Henry advances our model of depression and subordinate self perception.

P Gilbert, Derbyshire, England

Letters (cont): 1 May 90

I will work on the paper "Metaphors of Submission" .. to make it suitable .. to be circulated...

Am writing in a rush to try to catch you before you leave for NY ..

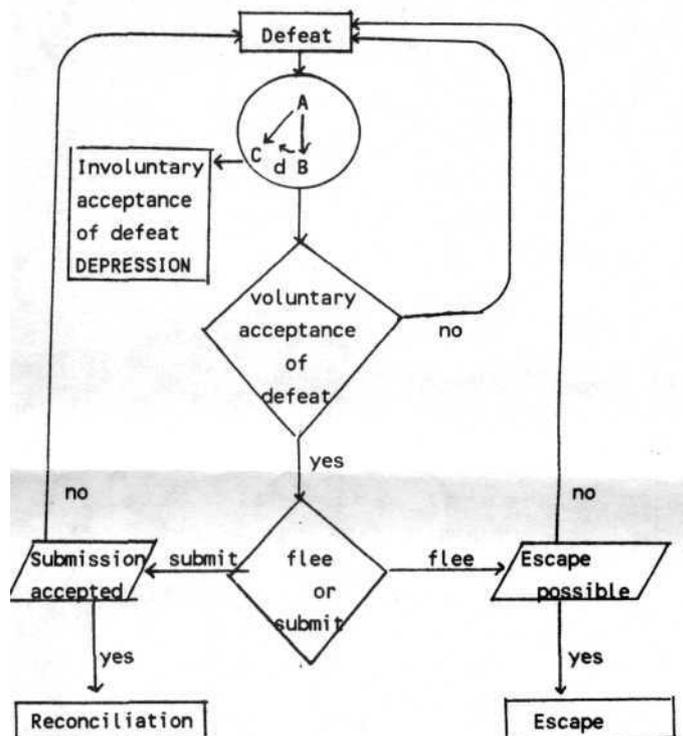
I've been thinking about defeat in response to your comments and I think we ought to try to calibrate our ideas and terminology. I think that

now, I see defeat as a situation which one can respond to by either fleeing or submitting (at the voluntary level) and if both these responses are blocked or not effective for some reason, the involuntary "yielding subroutine" is triggered. I have tried to express these ideas in the enclosed flow chart [figure 1]. Would you go along with it, or do you have a different flow chart in mind?

I hope the meeting is constructive and that you all have a good time too. I am thinking about a meeting here in the summer of 1991.

John S Price, Milten Keynes, Engl

Figure 1. Flow chart of consequences of being in a defeat situation



d = switch moves from B to C after n operations

- ◇ = choice by subject
- ▭ = choice imposed on subject
- ▭ = situation or outcome

As you know, I feel the expanded summary paper is an excellent prototype for peer-reviewed manuscripts that we might utilize more extensively for the ASCAP Newsletter in order to assure authors of such summaries of a reading by a knowledgeable critical audience.

We didn't process the materials furnished the day of the meeting (the May ASCAP issue, this letter of yours, the diagram in figure 1) in such a way that they entered sufficiently into the discussion, but your points (part of the discussions I gather of those of you in England) were also reflected in P Gilbert's discussions (his letter and contributions at the NY meeting - see below).

Letters (cont): 5 May 90

Thanks for your prompt mailing and clarification for the NYC meeting. Looking forward to it.

Meanwhile I mentioned a Hungarian colleague [Dr Tamas Besreczki] and it might be useful to offer ASCAP to him. My experiences with APA Council on International Affairs taught such gestures are not just nice but that colleagues in less developed and (recently?) Stalinist countries are starved for information and literature... whatever ASCAP could send will, I'm sure, obtain multiple readers.

D Wilson, McLean, Belmont, MA, USA

Recent copies were sent with a subscription form.

Letters (cont): 23 May 90

There are a few points I feel I must make in response to John Pearce's "Contribution on the Kalman Glantz Controversy" in the May ASCAP vol3#5, pp9-10. First, Dr Pearce feels that "the practical methods of all

kinds of psychotherapy are surely based on the basic facts of brain functioning," and this seems to be one reason why he doubts the value of the application of evolutionary insights to psychoanalytic theory. I take the view, however, in common I thought with other "evolutionary psychologists", that the whole point of applying evolutionary insights to apparent psychological processes and mechanisms is to assess their validity in ultimate, functional terms. Indeed, it seems to me that only in this way can the true nature of proximate mechanisms, such as the "basic facts of brain functioning", ever be elucidated and understood.

Secondly, I must say I find Dr Pearce's comments that he finds the papers of his psychoanalytic colleagues "virtually unintelligible", and that he suspects them of perhaps deliberate "obscurity" rather puzzling. Since he does not name the offenders I cannot directly refute his claim, but, whilst agreeing that psychoanalytic writing has not always been the model of erudition, all the work I have seen in this area has seemed to me to be perfectly clear and direct. Recent essays printed in ASCAP by Christopher Badcock (Sept. 1989, April 1990), for instance, seem to me to be uncommonly intelligible, elegant in fact.

Furthermore, Dr. Pearce implies that evolutionary psychoanalysts have failed to show how psychoanalytic principles can be the result of evolutionary selection; indeed he offers this as a challenge. It is quite evident, however, that this is precisely what evolutionary psychoanalysts have been doing from the beginning, and I would suggest, with not inconsiderable success. Dr Badcock, for example, in the pages of ASCAP and at much greater length elsewhere<sup>10</sup>, has offered plausible evolutionary explanations of such pivotal psychoanalytic concepts as

the Oedipus complex, penis-envy (April 90 ASCAP vol3#4,pp8-10) and oral behaviour (Sep 89 ASCAP vol2 #9,pp2-7), not to mention repression, identification and the entire classical topography . Further notable examples of the fact that evolutionary psychoanalysts have met Dr Pearce's challenge include Alan Lloyd's evolutionary analysis of classical dream theory, presented at last year's ISHE conference in Edinburgh, a forthcoming chapter by Alan Lloyd and Randolph Nesse detailing the adaptive significance of the mechanisms of defence<sup>11</sup>, virtually all of Daniel Rancour-Laferrriere's Signs of the Flesh, and so on and on. And, what is more, all of this is in "clear language", or at least language clear enough to be fully understood by first year sociology and psychology undergraduates!

So, far from lacking clear statements of the adaptive significance of psychoanalytic principles as Dr Pearce claims, what we are in reality lacking is any coherent challenge to what, to me anyway, seems to be fast becoming the most promising line of enquiry in modern evolutionary psychology. K Sharp, Edinburgh

Letters (cont): 22 May 90

. . .I have enjoyed this Newsletter and sincerely hope it is a sign of the times.

JL Donley, Prairie Village, KS

\*\* Feature \*\*

New York Planning Meeting on defeat-depression This conference took place at the Cabrini Medical Center on May 16, 1990 from 9 a.m. to 12 noon. Participants involved those of the original Boston "Basic Plan Group" --that is, Kalman Glantz (KG), Seymour Itzkoff (SI), Dan Wilson (DW), Russell Gardner (RG - who chaired) along with Paul Gilbert (PG). We were

joined by our hosts Lubo Kanov (LK) and Jay Harris (JH), Department of Psychiatry, Cabrini Medical Center. We appreciated the coffee and fruit that was provided.

As was true of the first conference, the discussion was non-stop and productive. Some conclusions were evident at the end: 1. The minutes in the April ASCAP and the abstract for the Human Behavior and Evolution Society (HBES) Los Angeles Annual Meeting (as phrased by RG) were satisfactory. (Also, happily, the abstract was accepted for presentation by the HBES program committee.)

2. More work needs to be done to clarify both the ethological and the depressive dimensions before plans for data-gathering can be launched, eg, nonverbal data. Moreover, the research project must be more clearly underpinned by already done relevant research.

3. There needs to an exploration of the learned helplessness (lh) paradigm of depression (see Paul Gilbert's letter above) in the form of a position paper that Paul indicated that he would draft for the group to consider.

4. A meeting of this nature needs to have John Price (JP) participate directly (we are appreciative of his willingness to participate in a basic plan group meeting in England next summer). Also see his letter above and his "Metaphors as Submission" manuscript in the July ASCAP issue (that we ask you to peer-review).

Summary; In the abstract, the term "subordinate self-perception" was lauded (see P Gilbert letter above). The issue of considering depression as a categorical vs dimensional concept was discussed and KG pointed out that as was true of quantum mechanics where both the wave and particle theories applied, so too with depression: two simultaneous models may have to be applied. There was agree-

ment that with an eventual study, there would be patients with major depression (as defined in DSM-IIIR or by equivalent criteria), patients without major depression and normals. Exclusion criteria would include patients with psychosis, dementia and other organic disorders.

PG presented data on his submission scale (see his letter above). These showed that self-ratings on it were correlated  $r=.47$  with the Beck Depression Index for normals and  $r=.52$  when the sample was a depressed population. He also found that inferiority correlates with depression  $r=.56$ , but that submission did not correlate with inferiority (for normals  $r=.2$  and for depressives  $r=.21$ ). KG noted that a distinction between submission and conciliatory behavior may be difficult. PG re-emphasized the importance of "blocked escape".

SI noted that self-perception may be misleading; for example, Korean children do well in math but rate themselves low; American blacks, however, do more poorly, but rate themselves more highly.

Reflecting a European ethological tradition, LK felt a non-verbal ethological measure would enhance the best ethological investigation. Such could involve psychomotorics, gaze behavior, interruptive vs listening behaviors (cf with D Ploog references above). DW recalled Leon Sloman's interest in gait and agreed that an ethological study would be optimal but that practicality is important. RG mentioned the work of Morice on the language of the Australian Pintupi. DW felt linguistic analyses may be problematic from his knowledge of this area of anthropology.

PG noted that the learned helplessness (lh) investigators have produced relevant data (which he reviewed in his book<sup>3d</sup>). KG felt that lh needs to be reworked as an "evolutionary biology of defeat." DW noted that lh may have an evolutionary algorithm, that

it could be put on a basic plan foundation. KG suggested that now unexplained variance be accounted for via an evolutionary model.

PG noted that John Price has not extensively reviewed the lh literature so that it hasn't fully entered his thinking as yet. PG further suggested that predation pressure does not explain the evolution of lh sufficiently. KG strongly endorsed the plan of presenting a review of this material for the basic plan group's more sophisticated further work and encouraged PG to lead further pursuit of this project for the group. An article on this could be aimed at a major journal. The group agreed.

John Price's diagram and letter of 1 May (see page 4 above) were furnished the group. KG and SI noted they had problems with the tremendous variety of reactions of humans to defeat. KG noted that humans have moved from relative status to "self-worth" -- indeed, that is the strategy of Beck's cognitive therapy. (Also compare to Carolyn Reichelt's letter in the May ASCAP). "Does one have control?" is very important.

SI asked if mental illness or psychiatric disorder is unique to humans. RG provided the basic plan notion that while such uniqueness may exist (because humans may be more protective of other, disabled humans), the same neurochemistry may apply to both illnesses and counterpart normal states. LK noted that what may be adaptive in animals may be depression for us, but may show psychomotoric congruities.

JH, interested in neoteny and cerebral lateralization, noted that post-traumatic stress disorder may be an apt model; defeat may reflect a lack of adaptability and the inability to come up with new behaviors. PG felt that this was a really important point (cf his letter above). Maybe learned helplessness is a model for PTSD he suggested.

\*\* Feature \*\*

Paternity. Identification and Latency  
A Possible Genetic Basis for some  
Freudian Observations.

by K Sharp & CR Badcock

For the greater part of its history, both supporters and critics of psychoanalysis have been at one in condemnation of Freud's admittedly speculative, but nonetheless determined, search for the phylogenetic determinants of the mind. Whilst schools of psychoanalysis have proliferated, their theoretical divergence has generally been united by a common disregard for the ultimate causal significance of their theoretical constructs. The effect of this has been the emergence of a bewildering array of models and concepts held to describe mental reality, while the rejection of ultimate evolutionary causation has necessarily brought with it an almost exclusive emphasis upon the environment as the sole determinant of mental life. Admittedly, Freud's evolutionary biology is now largely out of date, but his recognition that psychological mechanisms are ultimately explicable in terms of evolutionary biology sets him firmly apart from his environmentalist successors. Indeed, the very fact that Freud, unlike his successors, demanded an ultimate evolutionary rationale for his assertions<sup>12</sup>, strongly recommends these assertions to the attention of modern evolutionary biologists, as one or two of them have recently noticed.<sup>13</sup>

Freud found--and more recent, non-analytic research has confirmed<sup>14</sup>--that fathers seem to be of crucial significance to their offsprings' final sex life role, particularly that of boys. This came about by way of the eventual resolution of the Oedipus complex and the onset of latency thanks to repression of earlier, polymorphously perverse infantile sexuality and identification

with, and internalization of, the values, ideals and demands of the parents. This looks superficially like--and has certainly been widely interpreted as--a crucial instance of classical socialization by means of which offspring acquired the unconscious foundations of their eventual sex roles. Nevertheless, and in stark contrast to the subsequent trend, Freud increasingly emphasized the fundamental importance of phylogenetic considerations in creating the innate, biological foundations on which personal Oedipal ontogeny was built<sup>10b(pp203-10)</sup>. The following arguments, driven entirely from modern, evolutionary theory, suggest a surprising possibility: namely, that what has hitherto been taken largely as an outcome of socialization and environment may in reality be a consequence of heredity.

Whereas maternity is always certain and, in a mammal like a human being, the mother's characteristic taste and smell can be learnt by an offspring within a few days of birth, paternity can only be probable. A possible evolutionary reason for Freud's observations regarding the crucial nature of post-Oedipal identifications may be found in the twin realizations that 1) the polygyny characteristic of most human societies and almost certainly also of our hominid origins<sup>10b(pp47-66)</sup> is a high-risk sexual strategy which may encourage the selection of alternative, lower-risk--if less reproductively successful--strategies, such as 'satellites', female-mimics and sneak-fertilizers; 2) the best indicator of the heritable, biological basis of a male's likely reproductive success (RS) will be the observed RS of his own, biological father.

This suggests that correct recognition of the biological father is crucial. Since direct biochemical recognition is ruled out, the most likely mechanism would seem to be

phenotypic matching, this is, the use of self as the standard of comparison for assessing the degree of likely genetic similarity with another organism. This casts a new light on why Freud saw identification with the father as central to the ontogenesis of adult male sex roles. This is because, as Badcock has suggested<sup>10a</sup>, identification may have evolved as a human, psychological equivalent of such phenotypic matching.

The fact that Freud located the onset of identifications with the parents at the close of the Oedipal period (age 5-7) may also be explicable in evolutionary terms. If, as has been suggested in a previous communication (May ASCAP Vol3#5p9), Oedipal behavior is a means by which both males and females attempt to solicit additional parental investment (PI), then, according to PI theory, there could come a time when the benefits of doing this are outweighed by the costs of depriving younger siblings of it. This would explain first of all why post-Oedipal identifications are accompanied by the general repression of the sexual and aggressive behaviors of the Oedipal period (until their reactivation at puberty)--what Freud called the latency period--and secondly perhaps why such identifications are characterized by a degree of mimicry of parental behavior (perhaps especially in girls) might correspond to 'helper-in-the-nest' behavior (not to be confused with the earlier discussion of transsexuals as 'helpers in the nest' recruited almost from birth) (May ASCAP Vol3#5p9).

Such helper-in-the-nest behavior is a classical instance of kin altruism defined as a behavior which promotes the RS of the recipient at a cost to that of the altruist but is selected by virtue of the fact that  $Br > C$ , where B is the benefit to the gene for altruism in the recipient, C is the cost to an identical copy of the

gene for altruism in the altruist, and r is the degree of genetic relatedness.<sup>16</sup> Identification was originally proposed by Rancour-Laferriere<sup>17</sup> as a possible mechanism producing altruistic behavior in adults, and Badcock further and independently proposed that this may have been selected in small-scale primal hunter-gatherer societies where those most likely to remind one of oneself (phenotypic matching) would also have been those most likely to be kin<sup>10b</sup> (pp71-83). However, here, in this jointly authored publication, Sharp additionally suggests that such adult identifications were in fact prefigured in post-Oedipal, childhood identifications with the parents--what Freud called Oedipal resolution and latency. This explains what has long been apparent to psychoanalysis: namely, that such adult identifications are likely to be transferences, that is, subjective recreations in adult life of earlier, infantile familial relationships.

We could predict that the presence of younger half-siblings, as opposed to younger full-siblings, would delay the onset of latency and dilute post-Oedipal identifications because r, the coefficient of genetic relatedness, has only half the value between half siblings (1/4) that it has for full siblings (1/2). Half-siblings with a common father, such as would be found in a polygynous household probably typical of primal hunter-gatherers, could be seen as belonging to 'overlapping' nuclear family groups, centered on a common father, but with different mothers. Since we assume that each child knows who its true mother is in such a situation, it follows that older offspring are not likely to misidentify either their full siblings or half-siblings who are offspring of different mothers. Where half-siblings are offspring of the same mother, however, they can only be dis-

criminated against on the basis of having different fathers and so the failure to identify with the current father on the part of older half-siblings could be expected to reduce such kin altruism by a factor comparable to the reduction in  $r$  which would apply.

Where a father has not been a successful polygynist, a son may correspondingly fail to succeed in this respect himself because he had not inherited the genetic determinants of successful polygyny from his father. Under such circumstances, the son's best strategy might be to avoid polygyny in favour of a lower risk alternative. This could explain why Freud traced the etiology of male homosexuality--possible alternative strategy--to the failure of a boy to identify with a satisfactory father--i.e., one who possessed and could hand on • the 'right stuff' for polygyny to his sons. (The fact that some homosexuals report a dominant mother, rather than an inadequate father, may be a secondary effect of lacking such a father.)

An analogy is suggested by female mimics and sneak fertilizers in other species, such as the bluegill sunfish, where, along with regular, dimorphic males, both 'transvestites' (who resemble females but have massively enlarged testes in place of ovaries) and 'little sneakers' (who are a paedomorphic form) exist. Although such aberrant types of males do not occur among human beings as a physical adaptation, homosexuals who have a feminine, rather than masculine, sexual identity, might correspond to a psychological, behavioral equivalent. Again, homosexuals who, although masculine in their behavior as subjects nevertheless prefer male (usually young male) objects, could also be seen as avoiding competition with regular males by means of not appearing to be interested in females,

while the paedomorphic homosexuals who do appeal to them might similarly avoid competition by the appearance of being too immature to be a threat. Since it will normally pay females to be selective in their sexual partners rather than be prevailed upon by the male, such apparent behavioral demasculinization could also recommend such males to females. Indeed, other so-called sexual 'aberrations', such as narcissism and fetishism might be part of the same picture and interpreted as further instances of such cryptic sexuality (i.e., sexual behavior which succeeds by means of disguising its true nature and/or objects, in the latter instance by making the narcissist seem to be his own sex object or, in the case of fetishism, by making him appear to misidentify the correct object). However, this theory also predicts that homosexuals in particular, and all such sexual cryptics in general, should have some measure of RS, almost certainly in excess of what their behavior appears to suggest. Finally, the fact that identification of--and, consequently, identification with--mothers on the part of girls seems a much more straightforward matter might partly explain why sexual perverts tend to be overwhelmingly male (the other part of the explanation being that males almost universally tend to be less discriminating thanks to their vastly numerous and mobile sex cells).

If these considerations are in any way to be relied upon, they strongly suggest that the dominant twentieth century interpretation of Freud's findings as reflecting a predominantly environmental basis for crucial identifications may not be the only view of the matter to be held in twenty-first. On the contrary, they suggest that Freudian findings may find an evolutionary, genetic basis, previously unsuspected by nearly all except Freud himself<sup>10b</sup> (pp110-19).

1. Churchland PS (1986) Neurophilosophy: Toward a Unified Theory of the Mind-Brain Cambridge, MA: MIT Press, p284
2. For ASCAP Vol 3 (Jan through Dec, 1990) please send \$18 (US dollars) for the 12 issues. Make checks or money orders out to "Department of Psychiatry and Behavioral Sciences, UTMB"
3. ASCAP philosophy and goal. High scientific importance rests on comparing animal behaviors across-species to understand better human behavior, knowing as we do so that evolutionary factors must be considered for understanding properly such behaviors. To accomplish these comparisons, very different new ways of viewing psychological and behavioral phenomena are required. This in turn explains why we need new words to define and illustrate new dimensions of comparisons across species. We expect that work in natural history biology-combined with cellular-molecular biologic research will emerge as a comprehensive biologic basic science of psychiatry. Both top-down and bottom-up analyses are needed. Indeed, this must happen if we are to explain psychiatric illnesses as deviations from normal processes, something not possible now. Compare to pathogenesis in diseases of internal medicine.
 

Some neologisms that hopefully will help implement these goals are those of:

  - a. MRA Chance: "hedonic" and "agonic" refer to the tone of groupings of conspecifics (members of a same species) i.e., relaxed and fun-loving versus tense and competitive.
  - b. JS Price: "anathetic" and "catathetic" describe conspecific messages. Catathetic messages "put-down" and anathetic "build-up" the resource holding potential (R) of target individuals.
  - c. R Gardner: "psalic" is a 2 way acronym: Propensity States Antedating Language In Communication and Programmed Spacings And Linkages In Conspecifics. This describes communicational states conjecturally seen with psychiatric disorder and normality (human and non-human), ie, alpha psalic seen in manics, high profile leaders and dominant non-human animals. Eight psalics are named alpha (A), alpha-reciprocal (AR), in-group omega (IGO), out-group omega (OGO), spacing (Sp), sexual (S), nurturant (N), and nurturant-recipient (NR).
 

These new or renewed terms are initiated or elaborated in Chance, MRA (Ed) Social Fabrics of the Mind. Hove and NJ: Lawrence Erlbaum Associates, 1988.
  - d. P Gilbert: Social Attention Holding Power/Potential (SAHP) focuses upon the non-aggressive facets of leadership when this is deployed in the hedonic mode. See ASCAP v.2, #1 and his book: Human Mature and Suffering. Hove and NJ: Lawrence Erlbaum, 1989.
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