

# ASCAP NEWSLETTER

Across-Species Comparisons And Psychiatry Newsletter

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"In the early days of ethology, Niki Tinbergen introduced the term ethophysiology and stated that the causal analysis of innate behavior must be carried out from the high levels of behavior down to the level of neurophysiology, and 45 years later one must add, down to the level of neurochemistry." Ploog<sup>1</sup>

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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: The Yesavage Depression Scale is offered as a candidate instrument for the study planned on subordination and depression. .page 4

I Zhdanova provides us with a summary of her work . . . . . page 5

A Randrup and G Sorensen summarize their work . . . . . page 9

J Pearce follows controversies generated by Glantz's report and questions psychoanalysts. . . . . page 9

## Letters

6-Mar-90

Thanks so much for writing back [with plans]...

..I am really looking to seeing you soon. [We'll finally] meet after all these years. 1984/5 was it we started communicating?

Apr 90

Urgent! 1) I am not able to make Tues 15th because that is the exact time of the Social Anxiety Symposium, [in which I am scheduled to participate] 2-5 on Tues (in your APA Program it is No. 31 on that day)...

2) I really hope you could change it.. As you know I want to talk about

our study. Your meeting is the second major reason for me coming over.

3) The Social Anxiety Symposium might be of interest to our group since a couple of presentations are on cross-cultural aspects of social anxiety and depression and their relationship. Assessment requires asking for culture-typic methods and can't be described by us. Satorius et al (1983) has found low selfworth to be a main/core symptom in five cultures (W.H.O. collaborative study). See also Satorius et al in Psychological Medicine 1980. We should also write to Morsella and I have a contact in c/cultural studies in Honolulu...

I do hope you can change it. There are a few things I'd like to add as well as meet everyone.

Paul Gilbert, Derby, England

As you know the meeting time was changed to Wed am 16 May 90.

## Letters: (continued) 27 March 1990

It was a real pleasure meeting you, and old as well as new friends [at the Boston planning meeting]. On my drive home, I thought about the meeting and the project. Here are some ideas, but only as background guides to your conjoint clinical expertise.

1) The evolutionary relationship of low-status and depressed behavior, as derived from birds, etc., I believe to be moot for humans. The growth of that "balloon brain" has altered, radically, the morphological, physiological, and genetic underpinnings for this behavior.

2) There is much evidence with regard to exceptional mental qualities being correlated with extreme adaptive as well as maladaptive behavior: Bi-polar syndrome--high status (intelligence), Autism--high status in family, Dyslexia--high status in family, Asthma, lefthandedness, allergies, myopia--math intelligence, incipient high status. Also, violent criminality, alcoholism, schizophrenia, kleptomania, hypertension, and stress diseases, all relate to genetic and intellectual anomalies.

3) The real evolutionary issue in terms of abnormal social behavior, highly adaptive as well as maladaptive, may lie in the radical selective premium that undergirds orthoselection (selection in a straight line). The explosion of the hominid cortex after about 0.5 million years ago is critical. Given the highly variable nature of human brain structure and behavior even at that point in human evolutionary history, as well as as complex genetic and chromosomal linkages (tell me more about chromosome 15) has brought to the fore kinds of anomalous brain-behavior relationships that psychiatrists, social workers, criminologists have to deal with today.

My prediction is that an inverse relationship will be found between low dominance status individuals and clinically diagnosed bi-polar syndrome. Let me know more details about the NY meeting. I would like to attend if possible.

Seymour W Itzkoff, Smith College,  
Northampton, MA, USA

Letters: (continued) Mar 30, 1990

Just a note to let you know that I would be able to come to a meeting in NYC at the APA [time] only on a Fri or a weekend.

I enjoyed the meeting, hope something will come out of it.

Kalman Glantz, Cambridge, MA, USA

Letters (continued)

4/26/90

...[W]hat prompted this letter was the new ASCAP. On the subject of your cross-cultural study, which I think very exciting, two things came to mind that I want to discuss for the May 16 meeting: The first was in response to Leon Sloman's saying that biological mechanisms may be only loosely linked to societally manifest status. That's very true and can't be emphasized enough, I believe.

There are many people who are dominant in their social milieu but who are not dominant in an overall social hierarchy..."big fish in small ponds". There are also people who aren't dominant anywhere but who have a positive self-image in that they value themselves in the quiet role they play in life. Then there are people who would be dominant in an overall hierarchy, but who are constrained by lack of education or whatever.

And let's look at the reverse of that: I know the retired treasurer and chief legal officer of a major company. His father had wanted him to stay home on the farm after eighth grade, but he and his mother thought otherwise. He is as much of an alpha as you could find, very dominant. I know a local physician with an identical experience. Socially, both came from humble beginnings. Despite these origins, both now rank high in their environments. So this is relative and it is probably necessary to make some kind of adjustment to take these things into account. Somehow your questions must elicit the internal without reference to the external, although the external must be recorded also.

Also, we're talking about biologically influenced propensities here. You and RA Hinde both use that word and it is a good one. Hinde does a good job of laying out how those propensities may appear in the case of innate fears, which may have been

adaptive in our environment of evolutionary adaptedness. And you're talking about something that may exist that influences certain kinds of adaptive behaviors. But with our cortical development and extraordinarily complex cultures, those biological mechanisms must be subtle in their influence ... or at least in the way the influence is expressed.

Related to the above is the question of how you "test" in a variety of cultures. When we did the GERIAT project, we used an instrument in which I interviewed patients about their "level of contentment" in the nursing home. These patients are certainly part of our culture, but thinking about them in light of your proposed study, they almost could be called a subculture ... especially the people in Menahga, Minnesota, who were of recent Finnish extraction. Well, I discovered something very interesting in interviewing them with that instrument. These were simple questions addressing how they felt to which we were trying to get some answers graded on what we thought of as a simple clear scale. Many of the residents ... even the most alert ... and especially the men ... had a hard time responding to these questions. There was a lot of "I don't know ... I never really thought about it." With patience ... giving them time to adjust to the new idea of thinking about such things as happiness and loneliness and if it's interesting here ... they were able to answer, although the gradations were very difficult. I concluded ... with the help of a physician colleague, because he is so attuned to these patients ... that these are not introspective people. It has never even occurred to them that thinking about feelings has value. They can't express feelings; many don't even recognize them. ... As I said, the Finns had the most trouble with this.

So I suggest that somehow your in-

strument must have a common sense core to it that can pick up on feelings without "feeling" wordings, because I strongly suspect that there are other cultures that would give you more blank looks than even the "subculture" that we tested did. ..

C Reichelt, Wadena, Minnesota, USA

Please examine the Yesavage instrument for depression (below) that was originally designed for elderly patients with "yes-no" answers, but that may be more translatable across-cultures than instruments with a range of answers per question. Your criticisms of the Snaith scale in the March ASCAP were most pertinent.

Letters; (continued) Apr 27, 1990

Tomorrow I am off on a two week trip to Greece [hence can't make the NY planning meeting]. Thank you...for the ongoing enlightenment you provide in your publication which I enjoy and look forward to receiving.

One [issue] concerns what terms to use in differentiating between subordinate behaviour which occurs while losing an agonistic encounter (which can all occur in one's head) and the final act of submission or yielding which brings the encounter to an end (which can also occur in one's head). I am no longer finding the distinction between voluntary and involuntary yielding as being all that helpful as I believe that, when a biological mechanism is involved, it incorporates both "conscious" and "unconscious" processes. I don't feel we have as yet achieved a final resolution about what would be the best terms to use. The important distinction between "subordinate behaviour" and behaviour that leads to resolution of conflict, let us call it "yielding" is that yielding is self-terminating. ...

Leon Sloman, The Clarke, Toronto, CA

Letters; (continued") 2-Apr-90

The enclosed [manuscript not yet published] replaces that piece I sent you in the form of a letter to the British Journal of Psychiatry - I never sent that letter but hope to get it published in the BJP as an article and I think some comments from ASCAP readers might help to lick it into shape.

Have you thought of having peer reviews (to supplement editorial comment) in the way they do in Brain and Behaviour Sciences and Current Anthropology? They send the article to "n" reviewers and then publish all together: the article, the reviews and the author's reply to the reviews. For instance, I would like to read comments on the enclosed by Michael McGuire, Randolph Nesse (possibly even Gerald Klerman!) - so far the response to what I've had in ASCAP has been rather thin - one good reply by CRR and one by Lubo Kanov about homeostasis - which certainly made me think. Probably the preoccupation of most readers lies with general sociobiological concepts rather than with basic plans such as yielding routines, and so some solicited peer reviews would be helpful to direct their attention more to our interests.

I am looking forward to hearing news of your meeting in Boston.

Best wishes.

John Price, Meynard Keynes, England

Your idea for a peer review mechanism seems timely for this stage in ASCAP's development as well as an exciting new venture! What a good idea to send a generic letter to "n" potential reviewers, some subscribers and others, indeed, who may have had no reason to have heard of ASCAP previously. I am assuming your fellow Birminghamers are well acquainted with your views and that they will already have provided input to you,

but if you wish, we could send it to them as well.

A variation from the other publications is that from space constraints, the article and responses may have to occupy multiple issues

Let's experiment with this format with your article as the pioneering effort, consider the results, and if positive, think about other articles that would organize solicitation of peer reviews.

As you know from a letter I wrote to you personally, your idea of peer review may have come after writing the article which seems aimed at ASCAP readers who already know the terms and core concepts. Since some of the "peers" may be persons who don't ordinarily read ASCAP, I have (as you know) sent back your article for amplification and revision to aim at the more general audience.

**\*\*Featured Report\*\***

Yesavage Depression Scale<sup>5</sup> by RG

In the study being planned in the March-Boston and May-New York meetings, an instrument to measure depression simply, reliably and convincingly needs to be found. In addition, it needs to exhibit characteristics that would foster its usability in multiple cultures and after translation.

The scale that I am here describing as the Yesavage Depression Scale (YDS) was developed as a Geriatric Depression Screening Scale "that would be simple to administer and not require the time or skills of a trained interviewer." The instrument was methodically derived by taking 100 items that had face validity for depression and demanded of respondents only that they answered yes or no (rather than the usual four or six point scale with more discriminations needed on the respondent's part). Older people with more cognitive impairment have greater trouble making

finer distinctions . which is also a point made by C Reichelt about persons from other cultures to be approached in the study being planned (see letter above).

The 100 items were reduced to 30 by selecting those that correlated best with the total score after administration to normal and depressed persons over age 55 years. In a validation phase, all respondents were also rated with the Hamilton Rating Scale (HRS) for Depression (a commonly used behavior rating scale that represents an "industry standard") and with another self rating scale, the Zung Self Rating Depression Scale (SDS). Clinically depressed patients were also classified according to the Research Diagnostic Criteria (RDC) as "severe" (meeting criteria for RDC major depression) or "mild" (depressed but not meeting RDC criteria).

Four measures of internal consistency showed that the YDS was equal to (one measure) or better than the HRS and better in all cases than the SDS. Correlations with HRS and SDS was high,  $r=.83$ ,  $r=.84$  respectively, both  $p<.001$ . (Correlation between HRS and SDS was  $.80$ .) The instrument accurately classified the patients (with RDC as the standard). Moreover, with specificity held constant at 80%, the sensitivities of the YDS, SDS and HRS were found to be 90, 82 and 86% respectively.

Later Sheikh & Yesavage<sup>7</sup> developed a short version of the YDS, selecting 15 items with highest correlation with depressive symptoms in the validation studies. Normal and depressed elderly were administered both long and short forms. Results correlated well,  $r=.84$ ,  $p<.001$ .

The short form of the instrument is replicated below with the direction of a "depressed" response indicated by bold print. Each answer counts one point; scores greater than 5 indicate probable depression.

1. Are you basically satisfied with your life?..... yes/no
2. Have you dropped many of your activities and interests? . . . . yes/no
3. Do you feel that your life is empty?..... yes/no
4. Do you often get bored? . . . .yes/no
5. Are you in good spirits most of the time..... yes/no
6. Are you afraid that something bad is going to happen to you? . . . .yes/no
7. Do you feel happy much of the time?..... yes/no
8. Do you often feel helpless? yes/no
9. Do you prefer to stay at home, rather than going out and doing new things?..... yes/no
10. Do you feel you have more problems with memory than most? . . .yes/no
11. Do you think it is wonderful to be alive now?..... yes/no
12. Do you feel pretty worthless the way you are now?..... yes/no
13. Do you feel full of energy yes/no
14. Do you feel your situation is hopeless?..... yes/no
15. Do you think that most people are better off than you are? . . . . yes/no

**\*\*Featured Essay\*\***

Summary of Work in Russia on Social Rank Hierarchy by Irina Zhdanova

In belief that the main features of the affective diseases are the disturbances in evaluation of selfpower (what you call R) in the social environment, work with the cerebral spinal fluid (CSF) of affective patients was started with two models of animals' intraspecific communication. The first model was that of "emotional resonance." The scheme of this experiment was as follows: in the dark small compartment of the cage (highly preferred by the rats), the experimental animal heard the cry of the rat "victim." This was another rat, previously unknown to the test rat, that had its paws stimulated electrically in the open lighted area. When the test animal emerged

out of the dark and into the illuminated larger compartment, electrical stimulation of the partner's paws stopped. Rats could see, hear and smell each other. All of them were male but had never before lived together. The time spent inside the dark compartment and the number of enterings into it within the first 5 minutes of experiment were measured. Rats showed several types of stable behaviour strategy:

1. All the time in the light.
2. Short time in the dark compartment, but several enterings.
3. Long time in the dark, but also several enterings.
4. All the time in the dark.

I tried to name these groups in some way which could reflect their specific features. First it was altruists-egoists, then extra-introverts. Now after I have learned about the terminology of ASCAP, they can be called agonic-hedonic. All these names have a common feature reflecting what seemed more important for the animal (or for what will it spend its R?) in concrete situations its own stability and internal (within body) order or the stability and order of the social environment. Of course it is not an absolute strategy for each animal, but depends on the intensity of the stimulus, physical status of the animal, previous experience, etc.

The second model was "social hierarchy" and it showed the behaviour of 3 males and 2 females without previous contacts, but that now began to live together. After observing their communication during several days, a provocation was made by putting a new male-rat into the cage for 1/2 or 1 hour. The sum of observations gave the basis for the conclusion about social roles and type of hierarchy in the group. Before the injection of CSF, all the males were tested in both models, so there was a sort of social portrait

that was evident for each.

At the beginning I worked with outbred albino rats, but since then I've gotten inbred Tryons Maze-Bright and Maze-Dull rats. They are really very interesting for social models, as they have specific individual and social behaviour and have strong capability to organize different types of hierarchy. Initially their selection was done according to the ability of food finding in the maze. Successful animals were called Brights and unsuccessful, Dulls. But later it was showed that the main reason of this difference situation was the relative low significance that the food reward had for the Dulls. Indeed, in other situations that tested learning, they were even more successful than Brights. Remarkably, during long food deprivation, Bright-rats became passive very soon and moved rather rarely but lived much longer than Dull-rats which were active for a long time but then died. It seems to me that those specific features of social behaviour that were demonstrated in our experiments have some points in common with these facts as shown in specific tendencies of adaptation strategy.

In our experiments Dull-rats showed high level of motor activity, a lot of hedonic communicative patterns, and low aggressivity. They organized only into a hedonic hierarchy, and its organization and stabilisation happened mostly without fighting. Sometimes it was really difficult to know which animal was the leader of the group. Even the provocation with a new, strange male in the cage could not clarify it, as there were no evident aggressive behaviour that the dominant usually demonstrated to the stranger. As for the rest of "emotional resonance," Dull-rats were mostly in the 1st and 2nd groups, demonstrating altruistic behaviour. Bright rats on the contrary didn't move a lot, had rather rare contacts

with each other, were afraid of people (cried while handling) and in many of the groups organized agonistic hierarchies, which I called despotic.

In such cases the dominant was very aggressive to the submissives including females and the stranger rat. But in case of a mixed group including Bright and Dull rats, as well mostly Dulls, Dulls became dominants, but they didn't organize despotic hierarchy. That is, even in the agonistic hierarchy of Bright rats, if the stranger rat was Dull, the despotic dominant didn't attack it all the time, but produced redirected aggression to its submissive Bright-rats.

On the other hand I read in ASCAP (1 Nov 1989) about reverted escape. I hadn't heard about it before, but I also observed it in the groups of Bright rats, though seldom. It was a strange behaviour when the submissive animal, which had just been attacked by the despotic dominant, lunged for it with widely opened mouth and eyes. But when the dominant looked back, the animal cried and posed submissively again.

Most of Bright rats demonstrated agonistic behaviour in the "emotional resonance" mode (60-70%), but some of them, including many dominants were in the 3econd group with "altruistic" behaviour. There was no despotic dominant in this group. All of them were in the third group - so had agonistic, but "unstable," behaviour (because they entered the illuminated compartment several times).

After we had such behaviour portraits of the rats we made the injections of patients with CSF either intraventricularly through chronic cannula or into cisterna magna. Control experiments included injections of saline, rats' CSF and CSF of patients without psychiatric disorders and 24 hours social deprivation after it, as in experiment.

The main problem in evaluating the

results was with patients' CSP. A first issue concerned drugs. In our experience, if the patient had taken drugs within 15 days before the puncture, the effect to be described below did not occur or was opposite to the predicted one, in spite of drug use, the affective syndrome was still evident in the patient. So the only possibility was to take CSF from newly coming patients, about whom we knew that they hadn't taken psychotropic drugs.

The second problem was that even if we were sure about the lack of drugs, CSF of some manic patients didn't influence rat behaviour, and even some times produced depressive-like effect instead.

It seemed to us that there was a tendency (when CSF was used from patients with "pure" manic-depressive disorder) that the rats' behaviour was influenced in a prognostic way. But CSF of the patients with "schizo-affective" disorder in manic phase had unpredictable effects. I am not that much familiar with your terminology for psychiatric disorders, that is, I am not sure ours is the same as yours. What is more we had insufficient data from patients: for example, we had CSF of only 19 nondrug patients altogether.

So now I'll tell about the effects which were observed in "positive cases" only. Rat behaviour in the "emotional resonance" model under the influence of CSF of manic patients was reliably changed to the hedonic one. These rats spent more time in the illuminated compartment. On the other hand, CSF of depressive patients produced the opposite effect - rats spent the time of experiment in the dark compartment, though victims were crying. Changes of behaviour were observed for 2-3 days in most cases, but some rats had prolonged effects from the injection - several of them died after 8-10 days, others had cyclic changes of

behaviour for several weeks. Before injection these rats had belonged to the groups 2 and 3, unstable altruists and egoists (animals that did not stay in either dark or light all the time).

The influence of patients' CSF on the group hierarchy depended on the social roles of the recipient and on the type of hierarchy. The effect of depressive patients' CSF was most dramatically evident in the dominant rat. He became submissive with decreased motor and sexual activity. The recipient demonstrated subordinate poses and did not attack the stranger-rat. After 5-7 days its behaviour became more active, but the dynamics of the social communication during the given period depended on the initial type of hierarchy in the group. In the case of the hedonic hierarchy, the recipient became "rehabilitated" – dominant in the group again; in the case of the despotic (agonic) hierarchy, the recipient was persistently attacked by the new leader of the group and failed to return to its previous rank; in some groups this rat was killed.

Injection of CSF of depressive patients into the submissive rat made it less active (diminished food consumption, motor-sexual activity, and frequency of intragroup communication); some of them cried if a member of the group tried to contact it. After 4-8 days the behaviour became normal.

Injection of the CSF of manic patients to the dominant of the group did not change the social rank of the recipient, but decreased its aggressivity. This became obvious when the agonic hierarchy changed to the hedonic type; some of the despotic dominants became non-aggressive for a long time after injection (up to 2 months), but the most of them became aggressive again after 3-10 days. The injection of the CSF of manic patients to the submissive rats of

the group increased its motor, sexual and communicative activity, but did not change the hierarchy of the group. None of them were aggressive. The initial behaviour returned after 4-6 days.

I do not describe all the peculiarities of these experiments (there were many). It will take a lot of space and I am not sure it will be very interesting for you.

I have not had the possibility so far for a serious analysis of those CSF components which influence animals' intraspecific behaviour. The biotesting of CSF fractions showed that the activity is connected with the low molecular fraction (M.M. under 2kDa) and disappears after the treatment with trypsin. So it may be an effect of small peptides but to answer this question a serious and correct work should be done.

These results were published in several articles (1-5) but they are all in Russian, so I am afraid you can not read them. I tried to summarize the data, but decided not to discuss it myself now but to listen to your opinion first.

Now I begin to work with monkeys but will continue experiments with rats as well because they give the opportunity to test many animals and to choose subjects more vulnerable to the emotional disbalance.

These are extraordinary efforts and interesting, novel results. I am pleased to provide a response in a future issue. There are many issues to raise, ranging from methodological details to exciting conceptual possibilities. Too little space remains in this issue to respond now. I did some editing so please review carefully that this description is accurate. Also I'm sure other readers, such as Michael Chance, father of this entire endeavor, will be interested in these results.



**\*\*Featured Essay\*\***

On animal behaviours that may be related to obsessive-compulsive disorder by A Randrup & G Sorensen

In recent years there have been some attempts to relate the symptoms of obsessive-compulsive disorder to certain types of repetitive or stereotyped animal behaviour (note abstract by Pitman, quoted in ASCAP Vol 2 #11, Nov 1989, p.1).

In our laboratory, we have studied stereotyped behaviours of animals that have been elicited by a range of doses of amphetamine or by inadequate (restricted) cage environments. And we want to underline that although the term "stereotypy" is in common use in all these cases, it covers very different behaviours, ranging from a few repetitions of a quick and short behavioural element of automatic appearance to a continuous stereotypy lasting several hours. Common features are repetition, apparent uselessness and perhaps an important role of dopamine in the accompanying brain processes. Perhaps we are dealing with a continuum of abnormal behaviours. A similar range of behaviours is seen in the psychiatric clinic, where they are designated stereotyped or compulsive. Amphetamine induces stereotypies in man in conjunction with syndromes mimicking various common psychiatric diagnoses including paranoid schizophrenic psychosis (the most common), hypomania, depression and obsessive-compulsive reactions.

Apparent uselessness is one of the hallmarks of stereotyped and compulsive behaviours. But may these behaviours still have adaptive or survival value at least in their milder and temporary forms? Perhaps in the same way as fever, though debilitating, may still help to combat infectious disease.

Our experiments with bank voles indicate that even severe stereotypy interfering with normal functions may

relieve stress and protect against lethal effects of an unfavourable (restricted) milieu<sup>10</sup>. Confirming evidence has appeared, particularly from the experiments of Wiepkema et al<sup>11</sup>, which indicate that stereotyped behaviour protect veal calves in a confined environment from abomasal damage (gastric ulcer).

This is interesting relevant work: motor functions and their disorders pervade the experience and description of psychiatric disorders - including gross side-effects of their treatment ranging to subtle phenomena, eg, Sloman's study of the gaits of depressed vs undepressed persons.

**\*\*Featured Essay\*\***

Contribution on The Kalman Glantz controversy by John K. Pearce

Every group of human beings develops splits. How seriously they are taken depends, I suppose, on the mental health of individuals. But, all organizations have their feuds and ideological divisions. The Human Behavior & Evolution Society is no exception.

We have seen in print the debate between evolutionary anthropologists and those who doubt the legitimacy of their research. Another split, one that has not emerged in open discussion, is the split between the evolutionary psychoanalysts and their colleagues who doubt that evolutionary theory is going to provide much support for psychoanalysis.

The issue, which really doesn't amount to very much, may be made to seem more significant by some of the facts of history. In the beginning, in the early half of this century, psychoanalysis was a movement that seemed to embody the bright light of reason. It saw itself as attacked by dark forces, evil anti-psychoanalytic forces. Psychiatry appeared to be

polarized. With passing time, fewer people think that psychoanalysis is a bright light and hardly anyone bothers to attack it. Still, criticisms of psychoanalysis are regarded as pretty suspicious stuff. That's too bad.

To get to the point of contention: Evolutionary psychoanalysts hope that some set of psychoanalytic principles can be shown to be rooted in specific brain structures, structures that were selected by evolution. If that were achieved, and if psychology moves in the direction of integration with biology (we hope it will) than psychoanalysis would get a badly needed shot in the arm.

We think that would be nice, in fact it would be terrific, but we don't think it's very likely. We would say that the practical methods of all kinds of psychotherapy are surely based on the basic facts of brain functioning, but we see no reason why psychoanalysis would be particularly favored. In fact, quite the opposite. Psychoanalytic theory has always been strongly driven by current scientific beliefs. The gap between psychoanalytic theory and practical experience has always been unusually wide. The culture of psychoanalysis has always been characterized by intense loyalty to theoretical ideas.

Aside from general doubt about evolutionary psychoanalytic prospects, I can say no more. I find the papers of my perfectly smart, perfectly agreeable psychoanalytic colleagues to be virtually unintelligible. At our meetings, I have given up and gone to another part of the program.

I suspect obscurity. I suspect there is less than meets the eye.

It will come as no surprise to hear me say that I think that in our book, "Exiles From Eden", Kalman Glantz and I took the best approach to beginning work in clinical evolutionary

psychology. We took what we (or accurately, my senior colleague, Kalman) thought were the parts of human psychological functioning that were most clearly evolved adaptations: reciprocity and gender roles.

Our book is not obscure.

Let me challenge the evolutionary psychoanalysts: In clear language show us how psychoanalytic principles can be the result of evolutionary selection, i.e., give us an account of the origins of your particular list of domain-specific information subsystems.

I think this requires a plausible narrative, based on all that we know about the life sciences that might turn out to be relevant, e.g., primatology, ethology, archaeology, climatology, anthropology, molecular genetics, neuroscience, brain imaging, physiology and developmental psychology.

As Jerome Barkow says in his wonderful book "Darwin, Sex and Status", our movement is about "dissolution of discontinuities between ..social-behavioral and biological sciences". It is vital that evolutionary psychoanalysts widely embrace biology to remove such discontinuities.

It would also help if evolutionary psychoanalysts embraced what Barkow calls the Tinbergian framework (also used by Daly & Wilson and Blurton Jones) for multi-level explanations:

1. an adaptationist account of the natural selection those behaviors they will study,
2. an account of the related neurophysiological mechanisms,
3. an account of ontogeny--development on the individual level,
4. and the comparison of homologous behavior systems in species sharing a common ancestor.

Are evolutionary psychoanalysts up to such a challenge?

And, are they up to making their ideas clear?

1. Ploog O: Human neuroethology of emotion. Prog. Neuro-Psychopharm. & Biol. Psychiat. 1989;13:S15-S22
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 Hind. 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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