THE ASCAP NEWSLETTER

Across-Species Comparisons And Psychopathology Newsletter

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"[W]e must recognize how very limited in both scope and precision a paradigm can be at the time of its first appearance. Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners have come to recognize as acute. Thomas Kuhn[1]

Newsletter aims

1. A free exchange of letters, notes, articles, essays or ideas in brief format.

2. Elaboration of others' ideas.

3. Keeping up with produc tions, events, and other news.

4. Proposals for new initia tives, joint research endeavors, etc

The ASCAP Newsletter is a function of the International Association for the Study of Comparative Psychopathology[2]

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

Contents

IASCAP to Meet in 94

President Gilbert has decided to come to Philadelphia in late May, 1994, to join a group of other **IASCAP** members just before the annual meeting of the American Psychiatric Association. **IASCAP** will have both business and plenary meetings, though the exact nature of the latter is yet to be determined and hinges on decisions to be made by in late October.

This planning came about from a suggestion from Michael Alan Schwartz, the current president and one of the founders of the American Association for the Advancement of Philosophy and Psychiatry (AAPP). For several years now after a rejection by the APA program committee, AAPP began meeting as a separate entity. He suggested in conversation with me that the two associations think of a joint scientific session and I have coordinated a proposal. He is not the only decision-maker so the group may turn down the proposal, but the officers of **IAS-CAP** have decided that we will meet either together with them as well as independently.

Mark vour calendars accordingly: May 21 +/or 22, 1994.

Letter from President of IASCAP (or whatever it will be called!)

Dear Colleagues,

I would like to float to you some ideas about the future direction of our society. It seems to me that the future faces us with a number of exciting challenges. Let me outline them as I see them.

We need to reach a much larger audience than we do at present. If we believe that we have a powerful approach to the study of psychopathol-ogy, then we need to advertise and inform more than we do at present. We need to clarify, in as terms simple as possible, what we stand for, and we need to make greater efforts to connect with people.

Second, our clarity should focus on two central messages. The first is that our approach sets out to be integrative in a rather grand way. We are not content to understand variation in psychiatric phenomenology with single-process models. Our natural home is probably in the biopsychosocial model. In fact, this approach has been around for many thousands of years and is true to Hip-pocratic principles of a holistic approach. In suggesting the theory of the four humours, the Greeks believed that the humours (eg black bile, which was seen as the basis of depression) could be disturbed in a *patient for any of* a *number of reasons: life events, diets, or even seasons; that illness was the result of complex interacting processes was clear to them. This complexity became overshadowed by the Platonic and disease-centred approach but was never off stage for long. Adolf Meyer tried to rescue it and then Engel in the 1970s. However, for all kinds of reasons, partly to do with the enormous strides made in biomedicine and partly to do with the clinical implications and problems of using it, the biopsychosocial approach has not fared too well.[3] Many give lip service to it but little more.*

Another problem with the biopsychosocial approach, especially for research, is that it is an approach and not a theory. Of itself, it does not suggest any specific connections. Unfortunately, without a theory, it may have the problem of offering no more than suggestions for the study of inter-aciiuns. Thus, at *best, ii can give no more man a set of correlations.*

But this is where we can come in by suggesting that the interactions between biologic, behavioural and social domains are meaningful in that they relate to what Russ has called basic plans, *or what Jung called archetypes etc. It has always fascinated me that those who work in attachment theory, both with animals and humans, are in fact working with an evolutionary model, yet they sometimes lack interest in the implication of Darwinian principles. The only reason that disruption of salient attachments have the profound effects they do is because of our evolutionary history on brain design. This line of work advanced because it began with the idea of an evolved system and then began to unpack it with animal and human studies. We now know a reasonable amount about attachment: biological, social, developmental and so forth.[4] Probably one of the best books that has taken an evolutionary theory and biopsychosocial approach is Henry and Stephens and though now some years old, I know of no book to beat it.[5]*

Change of Name

Our message is the study of interactions which are informed by an evolutionary approach. Russ does not like biopsychosocial, so we can play with terms (I don't like sociophysiology because it ignores psychology, attitudes and personality variables, and so forth). But I do think we need to change our name. As I see it, the mission statement captures what we are about, more or less, but the title gives tne impression we are more interested in animal behaviour and pathologies. This may put some people off a bit. Also, there are many societies who already cater for that interest (eg the journal of comparative and physiological psychology). Thus, any mention of comparative or across-species should be dropped. The title should more accurately reflect that we are only concerned with animal models in that they point up continuities through time, and offer research leads (like attachment theory did). What I am trying to get a handle on is a title that informs clearly and also excites with the idea, That sounds like an interesting society, I must find out more!" Here are some thoughts I have had, none of which grabs me greatly, but still, here goes:

- *Society for the Study of Evolutionary Ap proaches to Interpersonal Processes (SSEAIP)*

- *The Evolution of Interpersonal Behaviour Society (EIBS)*

- *The Evolution and Psychopathology Society (EPS).*

Okay, I agree - not too inspiring, I guess, but maybe others can think up a few ideas and send them in.

The second change I would like to see is more on therapy implications.

Both Glantz and Pearce and Slavin and Krieg-man (who take a self-psychology approach and give it a fascinating evolutionary treatment) are excellent texts on therapy issues from an evolutionary perspective.[6] Anthony Stevens has recently

written a most engaging book, taking some of the new evolutionary approaches and exploring them in relation to Jung's concept of archetype.[7]

Special Editions & Literature Information

Another possibility for development is to think about special editions and invite pieces. For example, Beck's contribution and Birtchnell were good position statements. This would require future planning.

At present, Russ does literature information without much help from others. But we could have a special section for people to send in notice of papers of relevance.

Overview

Here are just some suggestions for beginning the difficult task of upping our profile. Russ has started us off and maintained us now for some years in communicating with each other. His excellent efforts need building on. I can think of no other newsletter that caters for things this one does. I feel strongly that we have much to offer, but we have to do a much better marketing job. We need a good name for our society and a perhaps some change to the newsletter to make it more accessible and relevant to practising clinicians.

If we do decide to change our name and our format slightly, then I see the next job as writing to all the major journals who have a letters section to inform them of our existence and what they are likely to get out of joining the society.

Please write in if you are in agreement and on what you think might be a better name. If you have any other thoughts about changes and developments, then write in. and let's make 1993/1994 a time of expansion.

Best wishes to you all,
Paul Gilbert, U Derby, UK

September 29, 1993

Tonight I got it up and running. The address of the E-mail ASCAP bulletin board is:

ascap@world.std.com

At this juncture the list consists of me, Kalman Glantz and Randy Nesse.

How do others get on and off?

I will keep the list in the World computer in Brookline, MA.

Send me addresses by any channel - phone, mail, fax, or my email address:

jkkp@world. std. com.

Here's how it works. You send an email message to ascap@world.std.com and it will appear in the mail of everyone on the list. There, the recipient can read it, reply to everyone on the list, reply to the individual sender, ignore it, erase it, or save it for their grandchildren.

Hurrah! it was easy to Start

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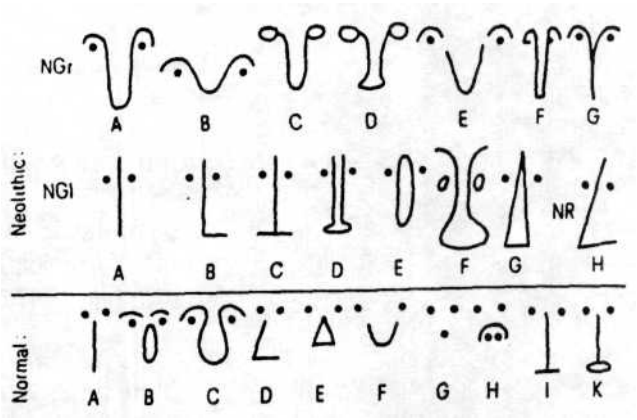
Abstracts by Anneliese Pontius

Pontius AL: Neuro-psychiatric and neuro-biological (limbic?) aspects of homicide, reflecting on normal action. Integr Psychiat 1987;5:116-139

Abstract: *Our heuristic goals here are twofold: to delineate essential components of human action behavior viewed through the distorting "magnifying glass" of homicide, and to compare them with findings of animal experimentation (limbic system kindling by minimal but repetitive stimuli). In addition to eight previously reported cases a ninth* case *described here of a proposed "Psychotic Trigger Reaction' suggests that ego-alien, motiveless (as to drive theory concepts) homicide can be evoked by a highly individualized trigger stimulus under cognitive puzzlement and the vividly reactivated memory of repetitive (though not necessarily highly traumatic) past experiences. Typically, there are autonomic nervous system concomitants, flat affect, (first-time) delusions and/or hallucinations (formed or unformed) without significant alteration of consciousness and full recall, inviting clinical investigation of suggested psychophysiological interaction until objective test methods become generally available for prison populations.*

Pontius AL: Face representation linked with literacy level in colonial American tombstone engravings and Third World preliterates' drawings. Toward a culture-evolutionary neurology. Experientia 1982; 38:577-580.

Abstract: *Among colonial North-American artisans, subgroups of South-Americans, Indonesians and New Guineans, a close correspondence exists between illiteracy rates and specifically spatially inaccurate representations of the upper face configuration, a characteristic also seen in the pre-literate period of 'neolithic' art, in early individual development, and in certain pathological regressions. Common to the configuration both of lexical signs and of the face is a specific spatial-relational ratio and orientation. Accurate representation of both configurations appears to be neuro-developmentally linked, within a cultural context, and consistent with a novel position that the 'ontogeny" of such cognitive functions recapitulates their prevailingly culturally determined 'phylogeny'.*



Frontal view of forehead-nose-eyes sector of face patterns, measured with a ruler. 'Neolithic face[1] patterns 'NGr' and 'NGI': continuation of forehead into nose without any indication of a narrowing, indentation or discontinuity at the bridge (root) of the nose, is, at the area beween eyes. Thus, a ruler placed at the upper and lower borders of the eyes measures the presence or absence of a neolithic face configuration. Subtype NGr is preent if at least at the lower border of the eyes (if not even above it) the nose is as wide there as it is at its tipe, discounting a bulbous enlargement of the car-tilagenous part around th tip of the nose. Subtype NGI is present if the nose extends above the upper border of the eyes into the forehead. A half-profile NR is not rated as it Is intermediary between 'neolithic' and normal faces. Normal face patterns are characterized by the indications of a discontinuity between forehead and nose at the bridge of the nose area through (a) narrowing or (b) indentation, and/or (c) a beginning of the nose design only at or below the lower border of the eyes.

Ethopharmacology

by Keith Dixon

Michael Chance shares with the ASCAP audience this letter from Dr Dixon to him; hence the allusions and the personal informal nature of its composition. We believe readers will agree, however, that this is an extremely worthwhile communication which will stir wishes to know more!

With respect to "approach-oriented" behaviours and flight, we have been very interested in trying to modify these selectively with drugs. We have a Sociotropic-Catadrasic concept of drug action. Basically, the idea came as a reaction to the pressure placed upon us by clinicians to have us label a drug "anti-depressant" or "anxiolytic" etc when we don't really know if it is one of these agents. I felt it better to conceive the preclinical profile of a drug in terms of which a) we can measure, b) we can reproduce and c) we can verify in man.

Communication to non-specialists is important in our game, and so we are sometimes forced to (over) simplify quite complex phenomena in order to get non-specialists to support us. Paradoxically, we also have to oversimplify our concepts for clinicians who have been divorced from *looking* at their patients due to their backgrounds and training. Our Sociotropic-Catadrasic concept is an oversimplification of a more complex set of affairs, but we are aware of this, and by adopting a bimodal concept we were able to take practical steps in tackling drug development from a different stand-point. It brought home to both the drug developers as well as the psychiatrists we have dealt with that there were at least two main ways of improving soda! behaviour (and inevitably improving the cognitive associations as well) other than the monotherapies usually used.

We wanted to develop drugs which reduced various forms of flight (ie the catadrasic action) since we see in our human studies patterns of avoidance which to our eyes are reminiscent of escape patterns in animals. This is work I've been doing with Hans Ueli Fisch at the Psychiatric University Poliklinik in Berne. Depressives and paranoid schizophrenics both show a largely flight-oriented non-verbal response to a standardized interview situation. But, although they both utilize the same elements of behaviour, the patterning is different. Also, at different stages of their illness,

their flight behaviour can be active or passive. Two examples from our own work (partly published and the rest being written) will, I hope, make this clearer.

We assume that social withdrawal, which is the cardinal state of most types of mental patients, arises when flight is high and/or when approach mechanisms are impaired. This suggests at least two ways of treating them. One is to attenuate but not remove excessive patterns of flight, and the other is to reactivate (if not damaged) the mechanisms subserving approach-oriented sociability (we do not need to know what neuronal circuits are involved in order to promote sociability). This, in turn, means that we should tease out the different ways flight behaviour is used and see how the different components affect sociability, which also takes several forms.

In Major Depressives (DSMIII-R) we find that, compared to healthy subjects, the gaze profile in a semi-structured interview, consists of longer "looking-aside" (ie, looking at the face but not making direct eye-contact), longer and more frequent looking down (to the floor), and an almost total absence of looking up and around to their immediate physical environment. Looking-at is slightly reduced. Interestingly, these patients are fully capable of looking around since, when the therapist goes out of the room, the camera shows the patients then actively inspecting their surroundings; ie, the lack of scanning is a socially inhibited effect.

In addition to this gaze avoidance, the depressed patients show fewer movements of the forehead, eyebrows and face which instigate or invite social interactions. They basically lower their output for interacting socially. This fits well with published verbal reports of depressed patients who, contrary to what is often thought, simply do not want to be bothered by well-meaning therapists. They want to be left alone! The elements which we have found to be affected serve precisely that function.

Moreover, we have looked at the "speech-architecture" of our depressed patients and found that they utter fewer words per second (speech density), have more frequent and longer pauses between bouts of speech and, most interesting, they have longer answer-latencies to questions put to them by the therapist. In order to get the necessary information to fill out the Hamilton Rating Scale, our therapists had to ask the patient 17 times whereas healthy patients required an average of 4 questions for the same information; ie the depressed patients are refractory to questioning. Where does all this incipient flight, avoidance, social withdrawal etc lead us? We believe that the depressed patient is in an escape mode which in animals we would call "arrested flight" which, like blocked-escape, arises when an animal is faced with an inescapable situation which arouses flight. The arrested-flight mode consists of three main components: (1) cut-offs (in Michael Chance's sense of this), (2) crypticity (behaviours having low-attraction potential), and (3) little scanning of the environment.

We have seen this in animals (rodents and macaques) under attack when denied access to escape routes, and we think that the depressed patients are showing the same qualities of behaviour (ie gaze avoidance, lower input and output of social stimuli, lack of inspection of the room). Ail of this lowers the opportunity to indulge in social interactions. Since the depressed patient does not seem to want to indulge in social interactions, one can conceive of arrested flight as an overall defence against overload of aversive stimulation which the depressed patient cannot adequately handle.

How does this differ from schizophrenics? Well, here we have too little data for definitive statements, and we are waiting to start a big study with Denmark. However, we have performed a study with a small group of paranoid schizophrenic patients who were also socially withdrawn. They seem to use flight in a different way. Their gaze-profile is especially interesting in that their eye contact with the therapist is not only longer than that of the depressive, but "looking-aside" hardly occurs at all; ie, they stare. But they stare out of the corners of their eyes or look at you with a lowered head (incipient flight is here, signaled by the vector in their head position). However, when they do not stare, they look away, down or even close their eyes (something we rarely saw with depressed patients); ie, there is no "buffering" effect of looking aside.

We have concluded from this that the paranoid patient's behavioural profile is polarized between attending towards the therapist and looking away towards their surroundings (it is not always easy to decide if they are looking away because of interest in the surroundings or if they are avoiding eye-contact; one could serve the other). Paradoxically, these patients will sometimes show contact-elements such as smiles, something that depressed patients rarely show.

It is not easy to predict when these patients will stare, close their eyes or smile. They often talk effusively and sometimes stay still and just shut-up. We feel that the bifurcation of their behaviour into extremes (looking at or staring, smiles, speech vs looking away, down or closing eyes plus immobility) suggests divided attention towards and away from the therapist. We are also wondering if the difficulty to tell in advance exactly what the patients will do, is "protean"[8] in nature. Altogether, we seem to be dealing with someone who behaves ambivalently towards the therapists but feels exposed to threats from the therapist, from within and from the environment. Sorry to be anthropomorphic here but we are still not sure of our ideas-we need more data. To be honest, these schizophrenics remind me of "peripheral males" who have been relegated to the edge of the monkey group but want to stay within it for fear of being predated upon.

If, as it seems from studies like these, flight behaviour occurs in different forms and if, as it seems, flight interferes with opportunities to indulge in sociability, then it makes sense to try and understand these flight processes and see if we can modify the different components of flight with therapies, including drugs. Hence our idea of catadrasics!

Of course, this idea considers social withdrawal as a consequence of activated flight pathways, but that is only part of the story. What about sociability and its different forms?

We believe that there are different forms of sociability and that these become "dis-ordered" in mental illness. Since some of our drugs affect different components of approach-oriented behaviours, we conceived the idea of a "sociotropic" drug action which basically re-flected a drug's action of approach-mechanisms. We have evidence (not released as yet) that certain drugs will promote approach behaviours in animals independent of their effects of flight or offence. Initially, when we conceived this idea we oversimplified; ie, aggression, investigation etc were all "approach-behaviours." Now-a-days, we are much more selective and, clearly, we need more time to dissect these different behaviours out. But we are trying to do this.

It is worth noting that when our depressed patients were subjected to a night of total sleep-deprivation, their depressed symptoms reduced and their elements of social contact increased significantly, and many of their avoidance elements were reduced (though not all disappeared). This is a nice example of a non-pharmacological therapy exerting precisely the kind of "ethological" effect we are striving for with our drugs (60% of the patients felt better, 90% of the patients improved their social behaviour... if only we could have followed them up after several weeks).

Whether we had a true "sociotropic" or "catadrasic" action or both is not worrying us right now. It is enough to say that at the animal level we can separate the two experimentally. We are encouraged by this approach because we can test for the differences in man using the same approach as we use in animals -- providing we pass the ethical committees.

So far I've not said much about "reverted" escape or about social submission. Even in rodents, "reverted" or "rejected" escape occurs. In mice we see it as "scalloping," which describes the path of a subordinate mouse which first escapes but then approaches the dominant mouse and attends to him again. In a close encounter, the subordinate mouse literally rushes into the arms of the attacker. This can be separated from "sociability" in that contact behaviors such as grooming, sniffing, huddling, and sexuality never follow a "reflected escape" towards an attacker (not in our studies at least). Presenting in monkeys might occur but I'm not expert enough to be sure.

Social submission increases in proportion to encounters so that when a drug promotes sociability, it will increase submission to some extent. But it seems as if (in rats, for example) social investigation can be increased separately from affecting submission....

A final point, just to emphasize the complexity of what we do. We found out that diazepam reduces flight when the flight is in the form of defensive ambivalence. By reducing the defensive component, the animals were "disinhibited" and predisposed towards acting along the approach continuum. Occasionally they showed aggression (also seen in man, where, paradoxically it is called "paradoxical aggression") but often sociability. If the animals are put in a low flight situation (familiar males), no sociability occurs; ie, we are not dealing with a sociotropic drug but a type of catadrasic.

Unfortunately, we arrived at this conclusion only after we had to rule on an effect of the drug on urinary odours of the treated animals. We have found that diazepam and other drugs actually alter the pheromonal activity of the recipient animals, and

this too affects social behaviour. Thus, in animal studies, the final picture that one sees is (a) the direct effect of the drug on the treated animal's behaviour, superimposed upon (b) changes in the treated animal induced by its untreated partner's behavioural response to the drug-recipient's altered behaviour, to which one must add (c) the confounding effects of drug-induced changes in behaviourally active odours. Not an easy task, I can tell you. Odours affect behaviour which affect hormones which affect receptors in the brain. My biochemist friends now also have to think of these things (they usually chose to pretend such problems do not exist).

I just wanted to make a final point. I remember you (Michael Chance) commented after my talk in Birmingham that there was something of a contradiction in trying to do the things we are doing whilst the medics have an opposite way of thinking. We realized that, despite our entreaties, the clinicians were *not* prepared to tackle ethology. So we started ourselves, we being me and HU Fisch who is a psychiatrist.

We have now the satisfaction of being invited to speak in most of the Swiss psychiatric Institutes. We are publishing and interest is being generated. (All thanks to you [Michael Chance] and Ewan really). In addition, I trained several people in Ethology who subsequently went back and started ethology up in their home towns. You met Franceska Farabollini who is now Professor in Siena.

Lastly, when I first came to Switzerland, none of the technicians knew how to look at a rat. One of my colleagues, Hans Peter Kaesermann, who also acquired his ethological skills in my Laboratory, stayed with me five years, then took a job as Rector of a newly founded school for biological technicians in Berne, where he has been for the last 8 years. This school supplies all technicians for most Swiss companies and universities. A regular part of the syllabus is ethology, including ethopharmacol-ogy. We are giving courses to medical and dental students and occasionally psychology students. So, you see, we have been very active in "spreading the gospel" at the grass roots. I believe "medical ethology" is something to push because it will have a future. But one has to go out actively and demonstrate its potential. So, Michael, you see that we are very busy with all of this stuff.

In a scrawled letter like this it is impossible to describe every little step. Also, I haven't told you about our forays into ethology and analytical psychology. My last lecture and the title of a manuscript that I am writing up is entitled "Ethological and psychodynamic aspects of defence." My last talk on this topic was at Raymond Battegay's lab.

I'm glad that you are cooperating with Franceska, who is a dear friend of mine. She did a post-doc with me many years ago, and I taught her ethology and she taught me neuroendocrinology. She has become quite famous in Italy, and we have regular contact.

All in all, I must say that "ethopharmacology" was a hard master, and I have earned lots of scars in its pursuit. But today it is becoming an established discipline, and through it I have made some very good friends and some exciting discoveries. Thank you for introducing me to this discipline which has, after all, become my life.

The Process of Cure in Psychoanalysis: An Evolutionary Biological Perspective

by Daniel Kriegman

Though I agree with much of what has been said earlier in an August 7, 1993, symposium at the Human Behavior and Evolution Society meeting, I will take a somewhat more provocative position for three reasons: (1) psychoanalysis is not popular among evolutionary biologists, (2) counterpoint may lead to some interesting discussion and debate, and (3) I am a later born child.[9]

As I turn to consider an evolutionary view of the psychoanalytic model of treatment, I am tempted to paraphrase a philosopher's description of psychoanalysis. So I will: "Psychoanalysis," he said, "is Olympic gibberish."[10] Yet, an enormous amount of effort, work, clinical data, and careful study of the human psyche has occurred in the psychoanalytic field. Before we empty the enormous quantity of filthy bathwater, wouldn't we be wise to make a search to see if there is a baby in the water? Not only would I suggest that we will find a living baby, I would also suggest that the baby has nearly drowned and is in desperate need of evolutionary biological resuscitation. Let us turn now to look at how evolutionary biology may be applied to psychoanalysis for this purpose. We can do this by first asking the question: how should we act as part of our patient's environment to have maximal impact as a therapeutic agent?

In the classical psychoanalytic model, Freud envisioned a central organizing agent, the ego at the heart of a tri-partite psychological system. The ego has to manage pressures impinging on it that cause it to experience anxiety. These pressures emanate from the id (instinctual anxiety), superego (moral anxiety), and reality (realistic anxiety). The compromise operations (defense mechanisms being the sub-group of these operations upon which psychoanalysis focuses) with which the ego manages these pressures operate largely unconsciously. The goal of treatment, then the role of the therapist, is to make the ego aware of its predicament and its defensive system of compromise operations. Without such awareness (insight), the ego blindly repeats self-destructive and useless actions unless much of this system can be brought within the ego's awareness, and only then can new, more productive compromises be arranged. Because instincts conflict with social pressures, they comprise the largest part of the unconscious forces operating on the ego. The goal is to make the unconscious instincts conscious. In the Freudian interpretations that evolutionists are fond of ridiculing, we find patients being told some of the most blatantly absurd things about their psyches, their intentions, and their motives.[11,6b] But, more of this later.

The technique Freud used to uncover the unconscious was *The Basic Rule.* The Basic Rule consists of the patient saying everything he or she becomes aware of without any form of censorship. Freud then began to notice the tremendous difficulty patients had in acknowledging a whole range of feelings they had about the therapist. Some of the most difficult of these were angry critical feelings, disappointment, sexual feelings, longings to be cared about, longings to be treated as special, etc. When strongly encouraged to talk about everything including these terribly embarrassing and frightening feelings without censorship in a relationship that consisted of 4 - 5 regular meetings every week, the patient began to have a strange experience. The therapist took on a role unlike that of any other person in the patient's life. Given the normal constraints of human life, no one enters into another's inner experience to such a great degree. No one, not even our closest relatives and lovers, gets to hear everything. No one gets a chance to come this close to stepping inside of our shoes and seeing what it's really like to have our own unique experience.

Freud noticed that the intense relationship that formed was colored by unique factors in each patient's circumstances. Each intense relationship that developed was different. Yet each such relationship became the central vehicle of the treatment. These unique colorations and the stable pattern of relating they engendered were termed "transference." Psychoanalysis became *transference* analysis.

Since that time, almost all innovations in psychoanalytic theory and technique have been comprised of new theories about human relatedness.[12] Current psychoanalytic theory is far less dominated by absurd symbolic sexual interpretations. There is now far more focus on the meaning of relatedness, the therapist's role in the relationship, and other relational issues. But all too often, psychoanalytic treatment operating outside of the context of a valid scientific view of human nature is now dominated by absurd interpretations about relationships. Again, more of this in a moment. Let's pause now and turn to evolutionary biology and mention just a few crucial points.

We know that humans are extremely social creatures: possibly the most social creatures of all. We know that humans are the most neotenized species with the longest period of childhood dependency in close kin environments. Finally, it is clear that the explosive growth of the human brain had little to do with *technology;* the human brain developed into its current form long before complex technological innovation occurred. *The human brain developed as a powerful social computer that evolved to deal with the incredible complexity of kin and reciprocal relatedness and conflict in a relatively reliable tribal web of social connections.* Humans in solitary confinement go insane. The greatest threat, possibly even greater than death itself, is to be shamed, to be cut off from the tribe. The greatest cause of suicide in physically healthy people is shame and hopelessness about being loved.

Thus evolutionary biology would tell us that the most powerful tool in shaping and *reshaping* (remember, we are talking about psychotherapy) human experience and behavior is a powerful, intimate, and involving relationship. Unfortunately, most psychotherapies, especially evolutionary ones, forego the use of this potent intervention. The use of such a powerful interpersonal experience has fallen into disrepute in the hands of psychoanalysts who operate with misleading biological suppositions that range from the partially correct, through the naive and wrong, to the absurd about the aims and goals of human psychological functioning, that is, assumptions about human nature. Because of this, non-analytic

psychotherapies tend to superficially use the relationship to teach, advise, suggest strategies for action, etc. Meanwhile, only psychoanalysis has consistently tried to tap deeply into an unusually involving and powerful human relationship. For some troubled individuals, herein lies both their greatest hope and gravest danger.

We can apply evolutionary biology to psychoanalysis in two major ways. First, we can weed out the nonsensical ideas that have at various times dominated psychoanalytic theories. Ideas inconsistent with what we know about the evolution of all mammalian behavior must be questioned at least and in most cases discarded.

For example, in our book, The Adaptive Design of The Human Psyche (1992), Mel Slavin and I examine the great debate in psychoanalysis about whether humans are primarily motivated by *instinctual drives,* that is, whether we are primarily selfish, self-serving, pleasure seeking creatures dominated by the need to discharge drive pressures *or* whether humans are primarily *relational creatures* whose overriding goal is to maintain attachments and secure relatedness.

Evolutionary theory quickly makes this debate appear to be an argument between two extremely naive views of any species' nature. If, as Freud argued, we are primarily *self*-interested, then our species would have died out long ago. As Dawkins has shown, we operate in the interest of the reproductive success of our *genes.* Leaving viable copies of our genes *in others* who survive our deaths is the evolutionary definition of fitness. As mammals, then, we must invest in others whose well-being is more important in the end than our own. A prototypically good human life ends with parents relatively spent and exhausted, surrounded by children and grandchildren.

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Yet simultaneously, evolutionary theory, eg, Trivers' Parent-Offspring Conflict Theory, conclusively shows that no two genetically distinct individuals share identical interests. In the most intimate and closest relationships, conflict is omnipresent and inextricably intertwined with overlapping interests.

So, as we show in The Adaptive Design of the Human Psyche, evolutionary theory can be used to deconstruct these two aspects of competing psychoanalytic theories in a manner that allows us to remove the baby from the bathwater prior to discarding the immense volume of dirty water.

Finally, as clinicians, we must use evolutionary theory as our guide if we are to construct and enter into the intimate human relationships that may provide the most powerful vehicles for therapeutic change. One overriding principle of evolution is that each of us is designed to operate in the best interest of our own genetic material. Psychoanalysis is a relationship between two unrelated individuals in which, to a great extent, the therapist has enormous power over the patient. Evolutionary theory tells us that it is likely that such power will be used to further the best interests of the one who holds the larger share of power (the therapist) and that the pursuit of such interests may self-deceptively be disguised as being in the best interest of the patient. This may account for the stories we hear about psychoanalysis: patients in treatment for 10 and 20 years being told the most absurd things about their motives.

One patient of mine (I was his 31st therapist) began his long course of treatment with a six-year psychoanalysis. He entered treatment as a promising psychiatric resident who wanted to become a psychoanalyst himself. Six years later he left as a severely hopeless, crippled, obsessional character who could only hold menial jobs and had to engage in an almost endless series of daily rituals, eg, repeated showering and handwashing to ward off overwhelming anxiety. After a ten-year search through 29 other therapists, he came to me.

It quickly became clear that his former analyst's interpretations consistently made him worse and worse while only the analyst benefitted from the treatment. Yet it was also clear that the analyst had behaved "properly," that is, according to sound analytic technique; the analyst's belief system supported a self-deceptive exploitation of the patient. Through a lengthy new treatment (with yet another therapist) that lasted over 4 years and took *this patient's point of view as the legitimate one* (not the analyst's twisted interpretations), this man was able to slowly regain his functionality, re-enter and finish his residency, and become a significant contributor to the field. This "taking the patient's point of view," similar to Russell Gardner's striving to be an ally to his patients, can be considered from an evolutionary perspective to be trying to ally with the patient's inclusive self-interest (a term that is intentionally similar to inclusive fitness). I believe that this is central to a general trend in evolutionary clinical approaches (as we have heard this morning) to "de-pathologize" our patients as we search for the adaptive strivings underlying their difficulties and symptoms.

As the earlier speakers have pointed out, evolutionary psychology entails a good deal of common sense. For example, over lunch on Thursday, John Price described a woman who had a terrible, crippling fear of public speaking. The clinicians discussing the case all ventured to make grand psychodynamic interpretations. Yet, no one suggested that such a fear was extremely common and may be normal. No one suggested that she consider whether she should be in a job that required her to engage in such a difficult task.

I responded to John that this would be very foolish for the clinicians. After all, they make an enormous amount of money from making such interpretations, and, should she get another job, it would be unlikely that she would continue to pay large sums of money to listen to such pronouncements.

Unlike evolutionary psychologists who focus on what I would call "superficial" advice and guidance, I believe that there is a considerable number of patients who truly need to be able to tap into the power of a psychoanalytic, relationship-based treatment. However, such intimate relationships with such dramatically unequal power differentials must be carefully monitored to prevent the abuse of the patient. Melanie Klein's daughter, trained as a psychoanalyst herself, reported knowing of two leading analysts who treated patients for 12 and 18 years respectively prior to sending them on for lobotomies.[14]

Such horrors can be reduced if not eliminated if analysts are trained from an evolutionary perspective in which every action they take is first assumed to be in *their* interests, not their patient's. One of the dangers of the evolutionary approach John Pearce presented this morning is that he assumes he can use his knowledge of human nature and psychiatry to move a patient in the direction that *he* thinks is best for the patient. But, evolutionary biology suggests that unless this outcome is carefully guarded against judgments of what is best for another, they are highly likely to be *in the judger's* best interests.

As therapists we all try to wean our patients from destructive and self-defeating beliefs. We inevitably try to indoctrinate them into our own belief systems, to present them with *our* "discourse." The evolutionary view I am presenting suggests that while this may, in fact, frequently be helpful to our patients, it will frequently be a way for us to make a comfortable living while affirming our own values, views, and self-esteem~at times, to our patients' detriment. Rather, evolutionary biology tells us that the psychoanalyst must struggle often against his or her own natural impulses to help patients find, define, uncover, and develop their own unique configuration of interests, abilities, talents, and beliefs: to develop their *own* discourse.

In summary, *using an evolutionary biological approach,* the psycho-analytic baby (that is, the extraordinary power of the unusual psychoanalytic relationship and method) can be removed and saved before the dirty bathwater is discarded.

The Itzkoff **Quartet** by

Russell Gardner

Smith College Philosopher Seymour W Itzkoff's four volumes on the biology of intelligence usually reside not more than two feet from where I write: behind the chair to the left - in their special corner. As I received each of his four volumes, I promptly read them, but then except for quick dips or reference searches did not look at them further. I guess I had not thought of myself concerned primarily with variability in human intelligence, but rather in the physiology of intra-specific communications -- the sociophysiology of what John Birtchnell more simply calls relating. So in the press of time I recall that I enjoyed the volumes more as casual reading, interesting but not critical.

Then in connection with recent work I've been doing on the censorship of sociobiology via "politically correct" thinking, I again picked up the quartet -- and found that the four volumes caught me and compelled me. I was unable to put them down despite other important things to do; indeed, they compelled me enough to devote this ASCAP space to them, although I am aware that many already know his work and have alluded to one or another of these books: Seymour Itzkoff is a familiar name in evolutionary biology.

Let me make a caveat about using the term evolution which bears on a later critical point: even though I alluded to it because it is so familiar, I'd rather not use the term. Evolution is emphatically not in the title of the ASCAP Newsletter in accordance with Darwin's early thinking. He didn't like the term because evolution implied perfection. A cumbersome but also more accurate alternative is across-species biological comparisons considered historically. The matter bears on Paul Gilbert's

above interest in a better title for the **IASCAP** society which I strongly support: but I vote *against* evolution as part of it. But this is an aside; now having signaled a position discussed more in the course of this review, let us return from the caveat to Seymour's opus.

The four Itzkoff volumes in chronological order are: (1) The Form of Man. Ashfield MA: Paideia Publishers, 1983. (2) Triumph of The Intelligent: The Creation of Homo *sapiens sapiens*. Ashfield MA: Paideia Publishers, 1985. (3) Why Humans Vary in Intelligence. Ashfield MA: Paideia Publishers, 1987. (4) The Making of the Civilized Mind. NY: Peter Lang, 1990. These are called Form. Triumph, Variance and Mind in the following.

A student learns

As part of my reread, I found myself thinking of the well-known story of the college student near graduation talking with his father. The student remarked with seriousness and amazement on how much the father had learned over recent years.

Thus, since the initial reading, I have read many other things in preparation for a book I am myself composing on the Biology of Leadership. But as I reread the quartet, I was amazed at how dull I had been to not sense better at the earlier stage its importance for my topic. Seymour well knows that intelligence is a social phenomenon, and as Ernst Mayr says on the book jacket, he has read very widely -- relevant quotes jumped out at me such that I could not believe I had previously missed them. Moreover, his writing is clear and pleasing; I conjecture that this perhaps had been a misleading factor the first time around. How can this be serious nutrition? ~ it tastes too good.

Seymour is a student of Ernst Cassirer who in turn had descended intellectually from Kant's philosophical position. Cassirer, brother of pioneering neurologist Kurt Goldstein, had been interested in the origins of symbol and language and Seymour decided to take this to its rudiments: what are the origins seen evolutionarily? So he took upon himself the exploration of biological origins of intelligence and has looked unblinkingly at the evidence, which has included considerable knowledge of the nervous system. This search and his findings permeate the quartet which is like a musical composition, with reiterated themes, repeated specific phrasings, and impeccably lucid prose.

Quartet quotes

To best convey a sense of this prose, a quote from each volume introduces them sequentially:

Form, p133: *"[Human] continued survival testified that [the human] was too evasive, wary, socially coordinated to be easy pickings for predators. Though leopards, lions, dogs are intelligent in their basic living patterns, they seldom seek risky pursuits. (There are no hospitals for wounded predators.)"*

The first book of the quartet, Form, is a scholarly foundation for the three that follow, each briefer than the first plowing of the ground; by additional contrast, they also use larger print, are less specifically referenced, and are written in a more easily digested and vivid style. Form tells the history of organisms and describes how Seymour means intelligence. He reviews what is known of the specific history of humankind. His version of how humans evolved is somewhat at variance with the standard. He has us changing to our present form for an extended period *in parallel with* the other great apes, rather *than directly from* them, similar to the way that the races of mankind separately evolved, though with the difference that the ability to mate has meant cross-fertilization over the millenia. Low intelligence human groups have been brought up to higher standards from such interbreeding.

Triumph. p103: "We *climbed up beyond* Homo Erectus to a *higher cliff. We started to explore it. A dark recess opened up to receive us. When we finally emerged 200,000 years later we were at the edge. In back of us was a sheer drop. Since then we have been busily eliminating any possibilities for other egress. We can only go forward now. To hope for success in the perilous climb, we must rediscover those primeval laws that brought us this far."*

Amongst several things, this quote illustrates orthoselection, the positive feedback effects treated extensively by Seymour which ratchet-like seem to have brought about humans as a unique species. Triumph is somewhat of a reprise of Form though more succinctly phrased -- written in part as though the reader hadn't read the first. Some examples are reiterated, for example, phrases vivid enough in the first go-around that they stay in the mind for recognition in the others -memorable imagery may cause problems with even small amounts of redundancy.

Variance. p235: "We *must realize that our set of*

genetic structures and their phenotypic exemplifications do not work as independent units. Not only in linkages between genes, but during fertilization, there are crossover patterns on the chromosomes that create wholly new relationships. In the process of natural selection, when one surface dimension, structure, or behavior, such as the brain and intelligence, becomes highly charged positively, it pulls along with it an entire complex of functions that have worked their way into genetic association."

Variance also deals with the sticky issues of the political controversy ensuing from Wilson's Sociobioloav in 1975, (for which Seymour has no great admiration).[15] But he is unequivocal about the IQ data: Stephen Jay Gould criticized the early formulations of IQ in his 1981 Mismeasure of Man, but did not provide a command of later information, nor was he unbiased.[16]

Variability in intelligence exists in varied human populations regardless of desired wishes for equality. Seymour asserts that the controversial *g* factor of Spearman is real and useful, as testified to by positive correlations amongst the many intelligence and achievement tests and the innumerable deployments of it in everyday life: from premilitary testing to measures of students at all levels then ubiquitiously used by admission committees of educational institutions and professional accrediting bodies. These real world practicalities indicate IQ and its relatives measure useful things.

Mind. p167: *"Like a mineral invading the rotted arteries of* a *giant aged tree, [Christianity] took over the Roman political and legal structure."* The last of Seymour's quartet is the most definitively his, all the previous themes drawn together in a lovely crescendo. This more philosophical work makes some conclusions about the biology of culture as he details how high intelligence created civilization through stages/attributes discussed in the following order: materialism and technology, religion, literacy, philosophy, science, and music.

One feels Seymour's musician-hood as he gives us an understanding of how he sees that expression of intelligence at a human pinnacle, especially in the last works of Bach, Beethoven and Wagner. Note in the above list that philosophy is *not* at the end, not itself the pinnacle: indeed, Seymour eschews the philosophical tradition of verbal gymnastics and is clearly in the corner of the scientific method for adducing new knowledge.

Some points of critique

I read out loud a quote from Mind to someone who didn't know much of Seymour. The person was caught by it, but was also puzzled, and then asked, "What again does he do? Is he a biologist then?" The source of confusion stemmed from Seymour's erudition; moreover, his knowledge is not only of biology but encompasses a consumate grasp of archeology and history, including prehistoric, classical, renaissance and modern eras. Is there *anything* this scholar hasn't read? Well, perhaps a little.

Thus, I felt mildly triumphant that his writings did not betray an awareness of Schmandt-Besserat's discovery of small clay tokens ubiquitously found in ancient Mesopotamian sites and reported even in the nonesoteric Scientific American long ago as 1978.[17] These were apparentlv used for economic purposes and date back to10,000 B.C. They seem to be the earliest precursors of writing, in that the particular shapes and forms of the tokens are reflected in the earliest cunieform symbols. But this is minor. Seymour is crystal clear about the leading importance of economic matters prior to the development of letters and cultural sophistication.

My more serious problem with Seymour involves what I believe to be his interest in perfection, laudable with music and art, but less so in consider-ing the products of natural selection. In his mind, for example, there is no doubt that more intel-ligence means more perfection, better adaptation, more hope for the world, just as the three com-posers made their most abstract and wonderful compositions at the end of their lives. He is very sophisticated about this. He knows Darwin's idea that things are undirected by higher supernatural agencies, but happen as resultants of chance, the mechanics of inheritance, and natural selection.

But be that as it may, I believe that perfection is something that Seymour idealizes and persuasively argues for, just as his predecessor-philosopher, Aristotle, believed in the *scala naturae.* And not only the two of them: this may be subtly pervasive in the writings of evolutionary biology, caveats notwithstanding. Paul Gilbert points out above in his presidential letter that names are important. Consider that the name evolution embodies a metaphor. In its etymology and in its nineteenth century meaning, evolution has meant to unfold with ever greater perfection, as a flower unfurling perfect petals. Darwin especially disliked

that implication, indeed not using the term evolution until the sixth edition of The Origin of the Species.

As we know Darwin compromised on many things and this finally was one. But as the current day teachers of Darwin use only his first editions of Origin because he diluted the later ones to ac-comodate public opinion, so we too might go with his earliest thinking. Evolution is not the product of a person. There is no preordained goal that a supernatural being had in mind in directing our evolution. Poet Robert Frost pointed out that "[Y]ou [need to know] metaphor in its strength and its weakness. You [need to know] how far you may expect to ride it and when it may break down with you. [Without that] You are not safe in science; you are not safe in history."[18] We need to keep our personal human goals -- including our high appreciation of cognitive and artistic perfection - distinguished from the impersonal operations of biological forces into which we may read our human motives.

I recognize that even natural selection is an anthropomorphic term: (some entity -- Is it Mother Nature? -- seems to do some action known as "selecting"). But while we may have anthropomorphism built in the machinery of our thinking, we should also keep in mind its distorting features. We need to subject our favorite ideas and images to the rude dissections that science deploys: doubtful, critical, indelicate opinions that diminish conclusions except in the face of evidence. Hopefully, we recognize the limits of these metaphors as Frost advises, but when someone as sophisticated as Seymour Itzkoff may have gotten carried away with perfection applying to biological mechanisms, we in what has been **IASCAP** should be careful about perpetuating terms that may enhance this.

Human uniqueness and basic plan

As a philosopher and therefore someone by training and inclination interested in the intricacies of mind and intelligence, Seymour emphasizes how humans are different from other animals. John Birtchnell has articulated a similar emphasis: humans are unique, different from other animals.[19] Both suggest that we not trivialize that difference.

Certainly Seymour does not ignore com-munalities -- they pervade his quartet - but he seems more impressed by, and certainly is more interested in, human uniqueness. Contrariwise, while not ignoring this, many of us in **IASCAP** are more impressed with basic plans, or whatever one wishes to call the genomic foundations upon which the human uniquenesses stand - or feel that the communalities represent a best tactic of investigation. We become less impressed with ourselves if we view our reflections in the mirror as just another example of adaptation with some qualities that happen if big-brains are deployed and then expanded even more.

Not that one or the other perspective is correct. I suggest that rather than controversy, we need ever more and ever better cross-talk between the approaches: I hope on the one hand that Seymour and John Birtchnell not talk scathingly of those who "reduce" human actions to basic plans just as, on the other hand, a Price-Sloman-Gilbert focus on how basic plans are expressed should not trivialize human orthoselection and consequent uniqueness. The two approaches should have mutual respect for each other.

Politics vs *science*

Now back to politically correct censorship: I recently hearkened to the ideas of historian William H McNeill.[20] Since McNeill is removed from the specific politics of sociobiology, I especially appreciated his view of science. He argues that science is *less* vulnerable than other modes of thinking to the bad vicissitudes of leadership. Science has a process of its own that carries to the present the rude and vigorous debate of the Grecian farmer-scholars in their marketplace during the classical period: disrespectful, responsive to data and facts, allowing and encouraging the jostling of opinion. The approach provided methods of resolving the conflict independently of personality and authority. McNeill suggests that science healthily contains correctives for wrong ideas that go beyond political solutions, which are inevitably slower and quite imprecise in their mechanisms of providing change, albeit democratic constitutional mechanisms to contain leaders are similar to those used by science to contain enthusiastic explanations for natural phenomena.

Restraint on scientific enthusiasm happens, even though it sometimes infuriates people who wish for a more applauding audience for their ideas. But even if angry, they respond to the rules of science. The process is not perfect: fraud occurs, false data have been published, self-correcting devices are sometimes imperfect. But the devices exist and function, watchdogs are out there, retractions are published, considerable punishment ensues for fraud.

McNeill argues that the free market of ideas represented by science is still the most reliable social contract. Seymour does too. Censorship does little good for learning more. Political correctness -what Seymour calls "intellectual conformity" -refers to a social censorship in certain locations, even -- indeed sometimes especially -- on college and university campuses, supposed sites of academic freedom. That is, prejudicial attitudes are assumed to be communicated by certain terms which thereby are forbidden. Sociobiology has been such a politically incorrect term, indicating to its critics *biological determinism* - they assert that this is a scientifically fostered but truly political philosophy that holds that since all is caused by biology, there is no political optimism for disadvan-taged people. They believe -- and Seymour presents the counterevidence that this is not based on reality - that all humans are equivalenfly intelligent. Seymour courageously asserts peoples are manifestly *not* equally intelligent but that this doesn't make them politically unequal.

I realized on rereading Variance that one of the reasons that McNeill's ideas had fallen receptively on my ears was that I had been prepared by an earlier, easily ingested Itzkoffian prose that I hadn't consciously retained. He makes the following powerful statements (page 24) that articulate the attitude of many of us in sociobiology and its scientific relatives:

"[I]n our western world of secular, rational, dispassionate scientific study, some 350 to 400 years after the great persecutions, inquisitions, and heresy burnings that ushered in our modern era, we see ... much hysteria and hatred expressed against those dissidents from the established orthodoxy of intellectual conformity....

"Let me affirm in the strongest terms possible that a book whose subject matter is an explanation from the standpoint of human evolution of why humans vary in intelligence does not *have as its purpose to exacerbate the pain or to promote conflict. My belief is that the longer the rational, wise, educated people refuse to face the truth, continue to live in a world of smug, complacent, incense-filled sureties about our own species, the longer the hatred and the destruction of our civilizational goals will continue....*

"No longer can we afford, as has been the case so many times in the past, to persecute the weatherman for the storm."

Does the P factor include philosophy?

I have not seen the civilizing role of the frontal lobes described better than the way Seymour does it in his third volume, Variance. He notes that Ward Halstead labeled it first as the P factor: a power and postponement component of thinking and action stems from the most hypertrophied and recently developed cortex, the brain part that myelinates last and matures slowest in the course of human development. Other meanings of P are planning, practice, perduring, patterning, predicting, all frontal lobe functions.

One might additionally put "phrasing" on the list, for the action component of speech and language. Broca's speech area is in the left lateral frontal cortex and functionally includes subcortical regions just below this . This re'ates to the fact that the fron-tal lobes powerfully mediate social connectedness: without them, amongst the things that happen is that a patient becomes withdrawn, asocial, loses interest in personal hygiene, becomes tactless, without social graces or initiative.

Another quote (Variance. p180): "The postponement factor inhibits the forces in our biology that demand satisfaction -- food, sex, fight, or flight. The power factor, on the other hand, spurs the individual to act in the name of thought, reason, to make a map of the situation."

In Seymour's speaking to us in these four volumes, I sense the artful deployment of all the P words listed plus philosophy. He repeatedly credits his wife Pat for considerable help -- another most important P factor specifically for him! -- but symbolizing not only the particular importance of Pat, but that of important persons, people, the factor of human gregariousness most important to Seymour and to each of us humans more generally. We all need other people to help -- for our best planning and other uses of our brains - functioning especially well together in the hedonic settings of MRA Chance.

In conclusion, I recommend Seymour Itzkoff's volumes as cornerstones for the library of anyone interested in a perfect or imperfect evolution, a philosophy that is more than verbal gymnastics, the psyche-specialties of psychology and psychiatry, the sociophysiology fundamental to both, across-species comparisons in brain functions resulting in behavior and thought, and intelligence in general - as well as an Itzkoffian incisive intelligence in particular.

Geiger G: Evolutionary anthropology and the non-cognitive foundation of moral validity. Biology and Philosophy 1993;8:133-151.

Abstract: *This paper makes an attempt at the con-ceputal foundation of descriptive ethical theories in terms of evolutionary anthropology. It suggests, first, that what human social actors tend to accept to be morally valid and legitimate ultimately follows an evolved pattern of hierarchical behaivour control in the social animal species. The analysis starts with a brief review of Thomas Hobbes' moral philosophy, with special emphasis on Hobbes' "authoritarian" view of moral validity and of the common political origins and ultimate basis of legitimacy of moral and legal systems. Hobbes' philosophical conceptions are then put into the context of Max Weber's influential empirical theory of legitimacy, especially charismatic revelation and authority as the ultimate source of all moral, legal, and religious obligations. Weber's concept of charismatic authority is given a biobehavioural interpretation in terms of ritualised status signals indicating an individual's superior physical and emotional dispositions to control the social actions of others. Various conclusions are drawn concerning the concept of moral validity and its possible evolutionary interpretations.*

Enquist M, Arak A: Selection of exaggerated male traits by female aesthetic senses. Nature 1993;361:446-448.

Abstract: *Darwin suggested that many apparently deleterious secondary sexual characters in males, such as bright colours, elaborate ornaments and conspicuous displays, evolved as a result of female choice. Darwin never tried to explain the crucial agent of selection. Rather, ha took it for granted that females of many species possess a 'sense of the beautiful', akin to the aesthetic sense in humans. The question of why such preferences evolve remains a controversial issue. Here we report that mechanisms concerned with signal recognition possess inevitable biases in response that act as important agents of selection on signal form. The existence of such biases may be sufficient to explain the evolution of exaggerated male secondary sexual traits, and elaborate signals in general.*

From the body of the article: The problem of how females recognize conspecific males is often ignored because recognition is usually assumed to be perfect and without biases.

All but the simplest of recognition systems are unlikely to be perfect.... Generalization is a com mon property of recognition systems that involves classifying novel variations of stimuli into particular categories that the organism has experienced before....

[W]e have studied...examples of recognition mechanisms based on artificial neural networks. Even the most simple of...networks... exhibit... properties [of] animal recognition systems....

[T]he precise course that evolution takes will be highly unpredictable. This instability may explain in part the great diversity in the form of signals observed in courtship behaviour, even within single species....

[T]he process of exaggeration described here is not confined to signals used for mate attraction; it applies with equal force to all contexts of signaling, including interspecific communication (such as warning colouration), and may offer a general explanation for the elaboration of signals that occurs during the process of ritualization.

Enquist M, Leimar 0: The evolution of cooperation in mobile organisms. Anim Behav 1993;45:747-757

Abstract: *Current game theory models of cooperation based on reciprocity do not take into account the active switching of partners made possible by mobility. Since such situations cannot be understood by means of a repeated Prisoner's Dilemma game, new theory is developed which considers both a current partner and the access to other partners, it is shown that mobility seriously restricts the evolution of cooperation: an efficient free rider could move rapidly through a population of cooperative individuals, searching out victims to exploit. Properties of the social environment, such as population size and density, influence the search time for a free rider and thus the possibilities for the evolution of cooperation. Behavioural adaptations, such as initial suspicious-ness towards strangers, and gossiping, may to some extent counteract the effects of mobility and favour cooperation. The possible importance of search time is illustrated by comparative data on the relationship between nest sharing and size of nest aggregations in sphecid wasps.*

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2. c/o R Gardner, 4.450 Graves Building (D28), University of Texas Medical Branch, Galveston, TX 77555-0428. FAX: 409-772-6771. For ASCAP Newsletter Volumes 3 (Jan through Dec, 1990), 4 (same months, 1991), and 5 (same months, 1992), please send $18 (or equivalent) for each 12 issue set. The first two volumes (1988 and 1989) of thirteen and twelve issues respectively are available on request without cost. For subscription to the 1993 set of 12 issues (Volume 6), the cost is $20/year. Make checks or money or ders out to "Department of Psychiatry and Behavioral Sciences, UTMB." At this time this "informal" organization has no official budget.

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