

# ASCAP

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*"[D]isproof is a hard doctrine. If you have a hypothesis and I have another hypothesis, evidently one of them must be eliminated.... Perhaps this is why so many tend to resist the strong analytical approach — and why some great scientists are so disputatious".<sup>1</sup> John R. Platt*

## Across-Species Comparisons and Psychopathology (ASCAP) Newsletter Aims

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

ASCAP Society Executive Council  
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## ASCAP Society Mission Statement

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

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**SUBSCRIPTIONS FOR 1995  
\$25 FOR 12 ISSUES**

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

# ADDRESSED TO & FROM

## SANTA BARBARA MEETING OF THE ASCAP SOCIETY

We met for the day on June 27, 1995. Additional summary of discussions will come in future issues as I made notes but this ASCAP number is already mostly put to bed so I will report only the following business decisions (in addition to the group's resolution on the transition in managing editor):

- The nominating committee of Paul Gilbert, Dan Wilson and Leon Sloman put forth the name of Mark Erickson as our next second vice president which the group resoundingly endorsed.

- With a sense of consensus from the assembled group, the incoming President, Leon Sloman, suggested that we meet next on May 4, 1996, just prior to the APA meeting in New York City. He and Russell Gardner will work on specific arrangements.

- The New York meeting will contain the business meeting and involve handling the gavel at that time (although, because we had no actual gavel, John Pearce innovated another symbol as he gave way to Leon: a double-click mouse indicated transfer of offices). But there will be other meetings in conjunction with which ASCAP members will probably collect for discussions: these include (1) next year's HBES meeting that will

be at Northwestern University in Chicago, commencing June 26, 1996, (2) the ISHE meeting that will be in Vienna in early August, 1996, and (3) The World Psychiatric Association that will be in Madrid in late August, 1996.

- The Aaron T. Beck Essay-Writing Award activity for students or recent graduates was clearly a resounding success and all agreed that it should be continued into future years. Nick Allen's presentation impressed the entire group and Dr Beck presented him with a handsome plaque. Mark Erickson was appointed by President Sloman to be the committee chair again. He, ATB and RG will decide later about specifics. The committee for this coming year will consist (in addition) of John Pearce and Paul Gilbert.

- A committee consisting of himself, John Pearce and Russell Gardner will work on the newsletter byline as there was sentiment that "evolutionary" be part of the face sheet in a less attenuated form than it is at present. For the time being, RG's compromise included placing "For evolutionary psychiatrists, psychologists and others interested in sociophysiological integration" in the place where "Newsletter of the Society for Sociophysiological Integration" had been formerly located. The name "ASCAP" was endorsed (despite its identity

with the musician's union) in view of its name familiarity, having endured for nearly eight years.

## TRIBUTE AND WELCOME

At the Annual Meeting of the ASCAP Society at Santa Barbara, CA, on June 27, 1995, the following notion was made and unanimously passed as a first order of official business:

- *Whereas* Erica Ainsbury has been with great enthusiasm the managing editor of The ASCAP Newsletter for the past 18 months,
- *Whereas* she has improved the newsletter's format, style and readability so that readers all over the world have complemented The Society's effort, and

- *Whereas* Erica Ainsbury has decided to leave her position of managing editor for reasons that befit her life plan;

### **Therefore,**

*Be it resolved* that the ASCAP Society

- Provides a resounding appreciation for all that she has done,
- Signals best wishes that she have great satisfaction in her future efforts, and
- Welcomes any future contributions that she might make as a less centrally located member of the society

### **Moreover,**

- *Whereas* Dena Stringer has been newly engaged as managing editor of The ASCAP Newsletter, and
- *Whereas* she has expressed great interest in providing the same

high level of readability and style,

**Therefore,**

- *Be it resolved* that The ASCAP Society
- Welcomes her with cordial anticipation,
- Looks forward to accurate and complete representation of our thoughts and ideas, and
- Pledges the cooperation of all its members for the superior publication and a smoothly operating organizational process.

**FAREWELL**

More gaffes, I'm afraid: to which I plead guilty but with extenuating circumstances, In an effort to get last month's Newsletter out in a timely manner, I failed to notice that the month and volume issue numbers were incorrect - the June issue went out under May's headline. For this reason we enclose a replacement first page for last month's issue. Also, we had some problems with the references section, and lost some and misplaced others, resulting in Linda Mealey's references unfortunately being placed under John Birtchnell's article. These regrettable errors occurred because I was trying to do too much in too short a time period. In transferring to another position, with reduced hours to spend on ASCAP. I felt an obligation to at least finish last month's issue and try to get it out on time. I hate to sound like I'm making excuses, but time constraints were the pressuring

force behind the errors, and I want to reassure you all that it won't happen again. I am handing over the reins to Dena Stringer, your new Managing Editor, as of this month.

I have really enjoyed my collaboration with Russell Gardner and various members of the ASCAP Society and wish you all well with future endeavors. I respect highly the intellect and openness displayed by the readership, and will miss being a part of the team.

Erica Ainsbury  
ascap@beach.utmb.edu

**GRATITUDE  
AND TRANSITIONS**

We no longer have Erica Ainsbury as The ASCAP Newsletter's managing editor. But how benefitted we have been for in her pioneering efforts in this post! Her expertise in computer programs and efforts in desk-top publishing have been a boon for our organization. Under her guidance, the format has improved to widespread acclaim, her organization skills have augmented our membership, her applause and support have been enormously helpful to yours truly.

Erica was born in England and educated in London (like the newsletter itself with its conceptual origins with Michael

Chance, John Price, Paul Gilbert, Michael Waller and John Birtchnell in England). But with her very active and computer literate husband they came to the USA nearly two decades ago and have made this part of the country their oyster with a lovely house on Galveston Bay.

Trained as a teacher, UTMB has provided Erica with the opportunity to educate again so that she now gives way to Dena Stringer who is the new managing editor. Dena by contrast is a southeast Texas product with her origins in Orange, TX, next to the Louisiana border and brings to us the energy of a competing tennis player. ASCAP seems to attract athleticism (Erica leads aerobic classes in addition to all the other things she does). We look forward to Dena's energy, friendly demeanor and computer-literate hand at the helm and wish Erica well in her new endeavors.

Russell Gardner, Jr.  
Editor  
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**TEXAS ASCAP GATHERING**

There will be a meeting on Saturday, September 16, 1995 in College Station. The host will be David Rosen. He can be reached at the Department of Psychiatry and Behavioral Sciences; Psychology Bldg. Rm 246; Texas A&M University; College Station, TX 77843-4235; (409) 845-2530; E-Mail: dhr@tamu.psyc.edu. *Details will follow in a future newsletter.*

# Aaron T. Beck ASCAP Award Winning Essay

## ARTICLE:

by N Allen  
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### Towards a computational theory of depression: An evolutionary perspective<sup>1</sup>

The vision scientist David Marr developed an approach to understanding information processing devices, including the brain and its cognitive apparatus, which emphasised the essential role that an understanding of function must play in the analysis of these devices.<sup>2</sup> In order to develop such an understanding, Marr argued, one needed to begin with a *computational theory*, or task analysis of the problem to be solved by the information processing device. Such a computational theory will entail an analysis of the goals of the computation, and the logic of the strategy by which these goals will be achieved. Marr argues that it is only after such analysis has been done that one can fruitfully go on to examine how this computational theory is implemented by modelling the relevant inputs and outputs of the information processing device, and the algorithm by which they are transformed. Finally, one may then inquire as to how these representations (i.e., inputs and outputs) and algorithms may be realised physically. Thus, Marr essentially underscored the point that function determines structure, and that it is only by careful functional analysis (i.e., asking "why?") that we will ever fully explicate *how* a device solves a problem.

Cosmides and Tooby have compared Marr's notion of computational theory to what is referred to in evolutionary biology as *ultimate* level explanations, and the representations, algorithms and physical implementation of these to the evolutionist's *proximate* level of explanation.<sup>3</sup> They point out that integrating Marr's computational theory ideas with evolutionary ones provides even further constraints on the hypotheses that are generated regarding the nature of cognitive devices. This is referred to as the *evolvability constraint*, which proposes that a computational theory of any organic information processing device must include the requirements that (1) the information used to solve the adaptive problem was available in ancestral environments, and (2) that the design must evolve, i.e., it will not be selected out by

alternative designs with different properties. In this essay, I will attempt to use these guiding principles to sketch out a rudimentary computational theory of depression, both as an effect and as a disorder.

Emotions suggest themselves as appropriate targets for such analysis in that they are ubiquitous human capacities with a high degree of functional specialisation. By this I mean that they are domain specific (i.e., they solve adaptive problems in one context, but not others) and they are content dependent (i.e., they are activated by specific forms of content, indicating their input is specialised). They also show a complex yet coordinated set of behavioural outputs in terms of physiology, conscious experience, and overt behavioural tendencies. For some emotions and their disorders however, their adaptive significance (i.e., the computational theory) has been more intuitively discernible than has been the case for depression, and therefore these principles have been able to guide research and modelling of the information processing aspects of the emotion state. One example of this is anxiety, which is easily related to the adaptive problem of dealing with threat. In order to do this, threat must be detected early, and appropriate behavioural responses (i.e., fight, flight, freeze, faint) must be instigated as a matter of temporal priority. Such implicit computational theories of anxiety have guided some very fruitful work on the information processing aspects of anxiety, especially attentional biases, as well as analysis of the differing adaptive requirements of differing forms of anxiety (i.e., social anxiety versus animal anxiety).<sup>5</sup> A similar level of clarity regarding the implicit computational theory of depression has not, however, emerged as easily as it has for anxiety. For instance, considerable evidence now supports the view that the essential psychological theme of anxiety is threat, whereas the theme for depression is loss.<sup>6</sup> Despite this useful conceptual distinction, however, it is much more generally accepted as to how anxiety might be an adaptive

response to threat (by facilitating early detection and mobilising protective or avoidant coping strategies) than it is as to how the state of depression might be adaptive in situations of loss. Indeed, it is not even clear what particular kind of loss might be most centrally related to depression. For instance, a recent report on a consensus development conference for cognitive models of depression, did not make any mention of adaptive function or ultimate level explanations of the disorder.<sup>7</sup>

Despite their lack of integration into information processing accounts of depression, there are a number of proposals that have been made regarding the adaptive significance of the depressive response. These include the notion that depression is an "involuntary subordinate strategy" which has the function of facilitating yielding by low status individuals in competitive situations.<sup>8,9</sup> Others have proposed that depression evolved to conserve or manage resources,<sup>10,11</sup> to facilitate the relinquishment of unrealisable goals,<sup>12</sup> or to manage reciprocal exchange.<sup>13</sup> In this essay I will present a perspective on the ultimate level explanation (computational theory) of depression which incorporates features of all of these perspectives, integrating them through clarifying the distinction between the goal of the depressed state, and the strategies used to achieve this goal. In addition to this, some recent developments in social cognition and game-theoretical analyses of the evolution of social behaviour will be used to clarify the computational theory of depression.

In classic Darwinian theory fitness is measured in terms of individual reproductive success, however, neo-Darwinian analyses of biological evolution propose that an individual's fitness is primarily related to the likelihood of their genes being continued over successive generations. This implies that because individuals share genes with their kin, those behavioural decisions that positively affect the survival and reproductive success of an individual's kin will also be selected, a process known as inclusive fitness. Buss has outlined the reproductive tasks facing humans as being (1) intrasexual competition to attract desirable mates, (2) choosing a mate with

the greatest reproductive value, (3) successful conception, (4) mate retention, (5) reciprocal dyadic alliance formation, (6) coalition building and participation in cooperative groups, (7) ensuring survival and reproductive success of one's offspring, and (8) investing in kin other than one's offspring.<sup>16</sup> Buss points out that the solutions to some of these tasks (coalition building and reciprocal alliance formation) also contribute to individual survival as well as reproduction. Each of these problems will subsume a host of subproblems, but in general it is expected that any genetically determined mental or physical design feature that assists in solving these problems or their subproblems, or that solves them more effectively than competing design features, will be selected by the evolutionary process. Of course, some selected design features will also be relatively domain general and contribute to the general goal of individual survival (i.e., the capacity for visual perception), an obvious prerequisite for the solution of these adaptive problems, or may assist in the solution of a number of the problems simultaneously.

Ethologists have developed a concept of self capacity known as Resource Holding Potential (RHP), which represents an estimate of an individual's fighting capacity, and therefore its capacity to control resources (food, territory) that will aid individual survival and the survival of kin. Assessment of an animal's RHP, both by itself and its competitors, will obviate the need for actual fighting. This aids in the survival of lower RHP animals who live to fight another day, and conserves the energy of the higher RHP animals, who can attest their superiority without actually needing to fight to prove it. Gilbert has pointed out that competition by attraction is more relevant to human evolution than competition by overt aggression.<sup>9</sup> For instance, Buss has demonstrated that across a wide variety of cultures it is features of attractiveness unrelated to strength and aggression that are the chief determinants of mate choice.<sup>18</sup> Females tend to seek mates who are good protectors, are reliable, and have control of resources, whereas males seek mates whose genetic potential is demonstrated by their youthful appearance and beauty, as well as those who will remain faithful, ensuring that paternal resources are not

invested in non-genetically related children. Gilbert has suggested that in humans, therefore, the chief variable used to estimate personal resources (and therefore rank for the purposes of intra-sexual competition, mate attraction, alliance formation and coalition building) is what he refers to as Social Attention Holding Potential (SAHP).<sup>19</sup> Thus it is an individual's capacity to hold the attention of others when necessary that is taken as the chief index of that individual's social resources, and therefore their capacity to attract mates and enter into favourable alliances and coalitions. This bestowing and recognition of social status through the mechanism of social attention will, in group living species such as humans, have direct relevance for inclusive fitness through facilitating the solution of the reproductive problems outlined by Buss.

The relevance of this to depression, is that SAHP can be understood as a psychological operationalisation of an individual's social resources; a form of self perception that could be used to modulate strategic aspects of behaviour in order to adapt to different circumstances. In general, it is better to have higher SAHP than lower, and humans who are motivated to raise their SAHP should have a greater level of inclusive fitness than those that do not. However, the difficulty is in how to raise one's SAHP. In general, SAHP will be determined by a *priori* estimates by others of one's capacity to control social resources. This would include estimates of one's personal capacities and control of material resources and personal capacities. Thus the methods to raise one's SAHP would include (1) enhancing one's own personal capacities and control of material resources, (2) entering into reciprocal alliances and group coalitions with those who control resources or with whom one can act to gain communal resources, and (3) having dominant (non-reciprocal) relationships with others where one can influence how they control resources, or (4) having submissive (non-reciprocal) relationships with others whose social resources outweigh one's own to such an extent that what is required from the submissive partner costs them less than what they gain from the small (relative to the dominant partner's total resources) contribution of the dominant partner to the alliance.

The difficulty with the latter three strategies is that they involve some form of social exchange, and social exchange entails risk. The risks inherent in any social exchange relate to the possibility of being cheated in reciprocal relationships, defeated by subordinates in dominance relationships, and exploited in submissive relationships, all of which would have an effect opposite to that desired; to lower SAHP. Furthermore, if one is going to increase one's social resources, one would look to engage in personally favourable forms of social exchange. This means that ideally one would engage in exchange relationships with others where the benefits to one's self are greater than one's contribution, i.e., reciprocal relationships where one does not give as much as one appears to, dominance relationships over those of as high social resource as possible, submissive relationships with those of extremely high social resource. In the case of social resources and SAHP, what is exchanged is usually more apparent than material. An analogy perhaps can be drawn with the value of shares on the stock market. The value is essentially determined by the ratio of supply and demand for the shares. This is in turn determined in part by the inherent value of the investment, and in part by what others appear to think is the value of the shares. If a stock of low inherent value is thought to be of high value by the community of stock market investors, then it will, in effect, have a high value. It is the same with social resources. If one can convince others that one has a high level of social resources, then the deception can become the reality if others begin to treat you as such.

In general therefore it is useful to present one's self as having a higher level of social resource than is actually the case in order to convincingly attempt up-hierarchy manoeuvres. As one will be most convincing in such self presentations when one is convinced of this one's self, the most effective way to facilitate up hierarchy social exchange manoeuvres is to "deceive" one's self and actually overestimate one's SAHP. However, if one overestimates SAHP too much, others will not be convinced and one will run the risk of being exploited or cheated by higher resource individuals when making SAHP raising manoeuvres. We would therefore expect humans to

systematically overestimate their SAHP to a level where *on average* they are facilitated in entering into new personally advantageous arrangements. In short, the individual plays a percentage strategy, where setbacks are allowed as long as the strategy pays off in the long run. The consequence of occasional exploitation or rejection is not critical so long as over a number of attempts to engage in such exchanges one is advantaged, and as long as one can continue to attempt such exchanges even after the occasional setback.

What would happen, however, if there were a critical level of social resource below which one would not be able to engage in further social exchange? In such a circumstance the optimal strategy would change, if one's social resources were near this critical level, the best strategy would be one that stated, "Engage in beneficial social exchange where possible, but do not *in any circumstance* allow your social resources to drop further". It is my contention that this is exactly the goal of the depressed state. Depression is a strategic behavioural response that has evolved to ensure an extremely conservative (i.e., low risk) approach to the management of social status in situations where one's estimate of one's social resources (SAHP) indicate that these resources are at a critically low level. The strategy should remain in place until one's perception of SAHP raises to such a level as to suggest that there is enough reserve in social resource to permit a more risky (expansive) social resource enhancement strategy.

Before going on to outline the ways in which such a strategy would be implemented, it seems important to consider why there would be a critical level on SAHP. SAHP is an index not only of one's capacity to achieve various inclusive fitness goals (such as those outlined by Buss)<sup>15</sup> but is also an index of one's perceived usefulness to the group or society as a whole. Hirshleifer and Rasmusen have pointed out that ostracism is an action that has costs for both the individual who is ostracised, and the group who shuns them. This is because the group loses the resources brought to collective tasks by the ostracised individual. If an individual is high in social

resource then the group will be loath to ostracise them, even when they break other social rules, because such ostracism will be too costly to the group. However, if an individual is considered to be low in social resources, it will cost the group little to shun them, and may even be advantageous. It should therefore seem possible that the point when a person's estimate of their SAHP indicates that it will cost the group little to ostracise them would be a critical level of this variable. Those who do not change their SAHP enhancing strategies to low risk ones at such a point will create a situation where their social resources may drop below a critical level and ostracism may follow.

In the hunter-gatherer societies that predominated in the human species' environment of evolutionary adaptiveness such ostracism would not only remove opportunities for further SAHP enhancement through social exchange and reduce inclusive fitness capabilities, but also make it very likely that the individual concerned would not survive. It is for this reason that ostracism is proposed as a key concept to explain the "critical level" aspect of this hypothesis Buss has referred to the notion that humans are essentially "inclusive fitness maximisers" as the "sociobiological fallacy".<sup>16</sup> The reason for this is that inclusive fitness cannot be tracked over an individual's lifetime, and as such inclusive fitness cannot be a psychological goal in and of itself. Organisms that are designed to behave in ways that happen to solve the myriad of adaptive problems inherent in inclusive fitness will be preferentially selected. Thus, modern evolutionary psychology would propose that the human mind will contain a large number of specialised adaptations, designed to solve adaptive problems that happen to increase inclusive fitness, but inclusive fitness will not be a motivation as such. In light of this, we can propose that humans will be designed to behave in ways that will increase their social standing, and to find low social standing aversive. But if there is to be a critical level in SAHP, it must be because it connotes a critical event that is perceptible to the individual; i.e., lower inclusive fitness potential cannot be the cause of such a critical value. Ostracism, however, because of its personal salience, is a more likely

trigger for such an evolved psychological mechanism.

At this point it will no doubt be observed that not all individuals who get depressed are in a position of low social resource. The point is, however, what they perceive their SAHP to be. SAHP is no doubt calculated in a complex manner. It might be best to think of it as a weighted mean of a range of previous experiences that are used to estimate SAHP. For instance, this data would be likely to include estimates of ones' own socially desirable capacities and resources, as well as information regarding particular episodes of social interaction, and the esteem in which one was held. However, given what is known about the limitations of the human cognitive system, there are clearly too many such instances to allow all such data to be included in calculations of SAHP; some degree of selection will be applied to the autobiographical database. Cognitive research has consistently shown that non-depressed individuals tend to recall a greater proportion of positive autobiographical memories, and recall them more quickly than negative autobiographical memories, whereas these trends are reversed in the case of those with induced and clinical depressed mood.<sup>21,22</sup> Clearly such biases in availability of self referent biographical material would be likely to influence one's estimate of SAHP, which would in turn influence behavioural strategies energised or inhibited by levels of SAHP. Thus recent life events, because of their salience (especially if they bring about a sudden drop in one's estimates of SAHP), would be likely to create a perception of critically low levels of social resource, even in those for who:) this is not the reality. Alternatively, in those for whom particular social investments (i.e., relationships, care) are crucial for maintaining an otherwise low perception of SAHP at adequate levels, losses of such crucial resources may bring about the perception that SAHP has reached a critical level. Such situations would correspond to socially dependent and autonomous modes of depressive vulnerability.<sup>10</sup> Clearly more needs to be said about the influence of cognitive biases and heuristics and individual differences in information processing patterns on the vulnerability to perceive one's SAHP as critically low, but for the

purposes of our argument here it will suffice to say that because of these factors one's perception of one's own SAHP can vary dramatically from the more objective reality.

This approach is basically consistent with most of the other theories of the ultimate cause of depression that were mentioned above. For instance, Beck's notion that depression serves the function of conservation of resources is clearly consistent with the proposals here, although I would stipulate that in humans the resources being concerned are specifically social in nature, and the fear of ostracism defines the critically low level of SAHP that the depressed state is designed to avoid. Nesse's notion that low mood is chiefly designed as an adaptive response to situations where investments are not paying off is also consistent with the hypothesis advanced here, although I would argue that it is one's assessment of oneself that is the chief determining variable, not the assessment of external circumstances. Clearly, assessment of the propitiousness of circumstances is altered in depression, but I would see this as a strategy to facilitate a conservative social interaction strategy, rather than a causative factor at the ultimate level. Theories that emphasise ranking such as those of Price *et al.*<sup>8</sup> and Gilbert<sup>9</sup> are also consistent with the hypothesis in that SAHP is essentially a ranking variable in humans, and it is the maintenance of rank that is important to depression. Glantz & Pearce emphasise the role of reciprocity, although I would see the effects of depression on reciprocity as a means to an end, that is to maintain rank.<sup>13</sup> Likewise, disengagement from goals is seen here as a strategy used to facilitate the higher order goal of low risk social interchange.<sup>12</sup>

One reason why the above approaches have emphasised different aspects of the adaptive value of the depressive response is because they tend to see relationships between people as essentially emphasising either rank or reciprocity. One exception to this is Gilbert's model where he describes a number of relational "mentalities" (care eliciting, care giving, competing and cooperating) which will influence the form of thinking and behaviour in



interactional contexts. Recent research in social cognition has also provided impressive support to the notion that humans mentally represent relationship according to a finite number of categories. Fiske's "relational models" theory proposes that there are four basic social frames that people use to coordinate and evaluate social behaviour. These are communal sharing, equality matching, market pricing and authority ranking. Communal sharing relationships are characterised by collective membership of a group where individual distinctiveness is not relevant, and resources are seen as communal property. Equality matching and market pricing are both egalitarian relationships, where matching of exchange is emphasised. The difference between these two is that in equality matching the social contract implies that the resources that are exchanged should be repaid in kind, i.e., one favour given implies a debt for an identical favour; whereas in market pricing goods are exchanged with reference to a common scale of ratio values, such as money or perhaps SAHP. Finally, authority ranking relationships are asymmetrical in nature, and are based on command and deference.

Recent empirical research has provided impressive support for the strongest version of Fiske's hypothesis; that humans use these four categories to mentally represent their relationships with others. Fiske, Haslam and Fiske found that in naturally occurring person naming errors, people tended to substitute others with whom they had the same category of relationships according to Fiske's model.<sup>24</sup> Furthermore, Fiske's categories showed more congruence in these naming errors than did personal attributes or role descriptions. Other research using latent class methods to assess categoricity of subject's descriptions of relationships have also shown that Fiske's relational model fits subject's implicit organisation of these relationships as well or better than alternative categorical theories<sup>25,26</sup> or dimensional models.<sup>27</sup> Furthermore the estimated base rate of these categorical classification of relationships demonstrated that the four categories proposed by Fiske were each applied to between 18 and 28 percent of the relationships assessed. This implies that any socially-oriented

theory of the function of depression that does not take this heterogeneity in social cognition into account, risks only describing a proportion of the changes in social behaviour related to the depressed state.

If we therefore accept that humans cognitively organise their social relationships according to these implicit relational models, we can hypothesise how depression might differentially affect strategic behaviour in each of these different relational contexts. To restate, the essential goal of the depressed state is to raise SAHP without risking any loss of SAHP, however temporary or minor. In communal sharing relationships the risk of losing further social resource through rejection or exploitation is very low, and thus a person critically low in SAHP might make excessive calls on the social resources available to the group. This would evidence itself in terms of help seeking behaviours and other communications of helpless status to communal partners. Exchange relationships, such as equality matching and market pricing, due to the inherent risk of being cheated in exchanges of social goods, would need to be approached very carefully, if at all. Only those relationships where the reputation of the other as trustworthy is excessively well established, would be safe to enter into. By and large the best strategy would be to withdraw from all exchange relationships where any uncertainty of outcome may prevail. In authority ranking situations, the strategy would depend on whether one was up rank or down rank in a particular relational context, as has been pointed out by Price *et al.*<sup>8</sup> In relations with up rank individuals, a depressed person would do well to submit to the hierarchy in such a way as to avoid all risk or aggression or exploitation. This would involve not challenging the hierarchy through confrontation, attempts at exchange with dominants, or other up rank manoeuvres. In the case of relations with down rank individuals the strategy is likely to be very different. For instance, attempts to exploit down rank individuals would be a relatively low risk method of raising SAHP. However, it is not a *no risk* method due to the potential for lower status individuals to attempt up rank manoeuvres themselves, such as aggressive or exploitative acts towards a superior. In

such cases a SAHP conserving strategy would dictate that only in those relationships where rank differences are large enough to render such a risk quite minimal would exploitative strategies be safe for a person with critically low estimates of their SAHP.

In order to achieve these strategic goals, a number of psychobiological features of the depressed state would have evolved, and as I hope to demonstrate, these correspond quite closely to the known features of depression. Evolution is a conservative process; it will make bricks with whatever straw is available. This means that evolution by natural selection will not necessarily design the most efficient possible adaptation, but rather will tend to use variations in current design features to adapt to new contingencies. In order to achieve the strategic goals in interpersonal interactions outlined above there are a number of mechanisms that could bring about the desired behavioural change. For instance, lower levels of sensitivity in approach-related psychobiological systems would help to reduce the person's motivation to seek new rewards, ensuring more conservative social interchange. Indeed some recent theories have proposed that depression may be essentially defined by a deficit in approach related positive affect systems.<sup>28,29,30</sup> One recent experimental study which makes this point quite neatly was reported by Henriques, Glowacki and Davidson. In a signal detection analysis of subjects' performance under reward, no-reward and punishment conditions they showed that dysphoric subjects had a significantly more conservative response bias in the reward condition than did control-subjects, indicating they required more certainty before they would respond. This was not as a result of a general behavioural deficit in the dysphoric subjects, but an effect specific to the reward condition.

Another mechanism whereby socially conservative strategy could be maintained is through conservative estimate of one's capacity to achieve social goals. This would be achieved by self evaluations, both in terms of SAHP and other relevant capacities and abilities, that guard against overestimation, and may even underestimate, a person's true social resources

and capabilities. In this regard it is interesting to note the experimental literature on so-called "depressive realism" (see Akermann & DeRubeis, for a review).<sup>32</sup> This literature reports findings that depressed person's judgements about themselves, both in terms of the degree of control they have over particular outcomes, and self evaluative material, is actually more accurate than that of non-depressed individuals, who consistently display an "illusion of control". This literature has often asserted that depressed people are "sadder but wiser".<sup>33</sup> These findings are clearly consistent with the hypothesis advanced here, in that they indicate that the non-depressed state of mind is characterised by overestimation of personal capacities and resources, whereas the depressed state of mind, through making a more sagacious appraisal of such matters, would be less likely to enter into high risk interactions. This would be an appropriate strategy when no errors can be tolerated. An interesting caveat to this is that this literature could be interpreted as saying that those in mild depressed mood states make the most accurate judgements in this regard, whereas clinically depressed subjects actually underestimate such resources considerably. This may suggest, as I shall argue below, that it is the depressed mood state that has been selected by evolution, whereas the clinically depressed state is a pathological aberration based on this adaptive emotional mechanism.

It is not possible to discuss each of the depressive symptoms in this essay, but these major symptoms mentioned here; lack of responsivity in reward seeking psychobiological systems, and lower than usual assessment of one's own capacities and resources, are clearly consistent with the hypothesis. The high degree of anxious symptomatology associated with depression is also consistent with this hypothesis in that depression can also be characterised as a situation of threat (i.e., threat of ostracism) as well as a situation of loss (i.e., loss of SAHP).<sup>29</sup> Furthermore, with regard to the issue of exchange oriented relationships, it is interesting to note the data recently reported by McGuire, Fawzy, Spar, Weigel & Troisi which showed that dysthymic subjects believed that they helped others consider-

ably more than they were helped by others. These subjects also downplayed the value of others' help, and were sceptical of others' intentions to help or reciprocate help. Such attitudes, when engendered in the depressed state, would clearly help to motivate a person to withdraw contribution of resources from relationships, and feel justified to "call in favours", both of which would raise SAHP without much risk. Further research needs to be done on these attitudes to exchange oriented relationships in depression.

One question that is raised by these conjectures is why the social behaviours of the depressed person (i.e., generally being "miserly" in their interpersonal interactions) do not become even more socially undervalued and lose further SAHP. This is a particularly important question in light of the game theory analyses of the evolution of cooperation, where Axelrod's computer tournaments of the iterated prisoner's dilemma have shown that the "tit for tat" strategy finished first. This is a strategy which dictates that an individual will treat another individual exactly the way they were treated by them during their previous interaction. As this paradigm is often seen as the most convincing explanation of how cooperation can evolve in biological communities, it poses a problem for our argument that it can be adaptive for individuals to "defect" (to use the prisoner's dilemma term for an individual who does not cooperate) in relationships for a while while they allow their resources to rise above the critical level. According to the tit for tat logic they would be at risk of entering into a continuing spiral of noncooperative interactions out of which they could not hope to gain much, nor from which they could emerge until they resumed a higher risk cooperative strategy. One point to be made here is that not all forms of social relations will be amenable to a tit for tat strategy (i.e., communal sharing). However, even for more rational and self interested exchange-oriented relationships there may be more forgiveness of non-cooperation than first thought. Recent work on the iterative prisoner's dilemma, however, has shown that this might not be so rigidly the case. Nowak & Sigmund have demonstrated that in a situation where player's responses are probabilistic rather than fixed (a paradigm much more able to deal with mistakes and

errors by players; a situation which more closely resembles biological reality), the most evolutionary stable strategy is what they refer to as "generous tit for tat".<sup>36</sup> This is a strategy where players will always cooperate with previous cooperators, but will give previous defectors the benefit of the doubt on a certain proportion of cases. This strategy outlived and outpopulated the strict "tit for tat" strategy. Clearly there are major limitations that must apply to any attempt to use such analyses as a model for human cognitive processes, but they do make the general point that under conditions of uncertainty generosity is a strategy that pays off. In such an environment, a depressive strategy may well flourish for a minority of individuals whose social standing is low, and who can make use of a degree of generosity in the population at large to allow themselves to raise their SAHP to a level where they can resume a more prosperous, but risky, cooperative strategy.

It is my contention that what the above considerations suggest is an adaptive role for the depressed mood state. It seems to me that it is no more necessary to posit a functional role for clinically depressed states than it is necessary to posit a functional role for panic disorder or autoimmune diseases. Both of these are examples of pathological states based on the inappropriate activation of evolved mechanisms, and I would argue that the same is true of depression. However, depression is a particularly common clinical disorder, implying that the depressive evolved mechanism is particularly vulnerable to getting "stuck in the groove". It seems that although the depressive response is perhaps an improvement on those strategies that maintain a high risk SAHP enhancement strategy regardless of the circumstances, and thus would be selected by evolutionary processes, it remains a highly unstable form of adaptation to low social resources. This is because, as has been noted above, the depressive response can have the opposite effect to that desired under certain circumstances. For instance, if a person has few communal relationships from which to gain resources, there are precious few avenues for low risk SAHP raising. Under such circumstances a depressed mood state may become entrenched. In short, if the biological, cognitive and behavioural

features of the depressed mood state do not achieve their goal of raising SAHP in a low risk fashion within a relatively short time span, they may well begin a vicious cycle of deepening depression that can develop into a clinical disorder. This especially so because it is proposed that lowering one's estimate of SAHP is both a strategy used to facilitate the depressed strategy, *and* the controlling variable of the strategy. This creates a situation where negative self evaluation can easily form a maintaining feedback loop. It may be for this reason that variables such as social support and social skill are often related to prognosis of depression; they will in part determine the SAHP raising success of the depressive strategies. Indeed the depressive response to low social resources may have been a more adaptive strategy in the hunter-gatherer societies in which it evolved than it is in our current form of social organisation. Hunter-gatherer people lived in small kin-based bands of 20 to 100 people. In such close knit groups depressive mood state strategies may have been much more reliable in achieving their goals than they are today, when people can become quite socially isolated (relative to the hunter-gatherer context) without risking individual survival. This notion is consistent with Seligman's assertion that it is the "waxing of the individual and the waning of the commons" that explains the apparent rise in the incidence of clinical depression over the last century.<sup>37</sup>

In summary, this essay has attempted to demon-

strate how a clearly articulated computational theory of a psychological phenomena can guide our approach to understanding its underlying information processing characteristics. It has been proposed that depression evolved as a context dependent adaptive strategy, which instigates a low risk strategy for raising social resources in a context where perceived levels of social resource are so low as to indicate that any further loss can result in ostracism. This strategy evolved in the intensive social context of hunter-gatherer societies, and remains with us today, even though the threat of ostracism no longer necessarily implies dire physical consequences. This view implies a number of hypotheses about the application of differential strategies in different relational contexts (i.e., communal, exchange and authority relationships) which can be tested in future research. As well as this the hypothesis could be further refined by game theoretic artificial life modelling of these strategies to establish which could contribute to an evolutionarily stable strategy for those low in social resource. In any case Marr's proposed that the computational theory of a device was only the first stage in the research program.<sup>2</sup> Our research into depression has for too long been hampered by a nonexistent or poorly articulated computational theory, and I hope that this perspective will contribute to the efforts to bring this state of affairs to an end.

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## **ARTICLE:**

*by M Power*

### **Chimpanzee Racists**

Current understandings of the social behavior and organization of the common chimpanzee as a species are based to a very high degree on the more than 30 years of field study, begun by Jane Goodall in 1961, of a single group of free-ranging chimpanzees inhabiting the Gombe National Park in Tanzania. Due to the high authority accorded the Gombe studies, particularly Goodall's, the perceived view of the common chimpanzee as a species is that this

great ape is typically aggressive, dominance-seeking, and fiercely territorial. However, when the many Gombe publications are read in chronological order, there is much solid evidence that chimpanzee group underwent a drastic negative social change, which spread in ripple fashion, until all aspects of the social order were affected.

The catalyst for this change was a particular method

of artificial feeding, which involved the researchers taking control of access to a desired food away from the apes, through use of what might be called "the closed-box" feeding method. There are elements in the changed behavior of the Gombe chimpanzee group which we would not hesitate to call racist, if this was a human group.

In the era of heaviest artificial feeding of the Gombe apes, the late 1960's and 1970's, there were also a number of far shorter independent field studies of wild chimpanzee groups living in other locations, in which unobtrusive "Naturalistic" study methods were used. Naturalistic methods are those in which the researchers strive to keep interference with the studied group to an absolute minimum. Artificial feeding is never used. These naturalistic studies have been ignored, essentially because the behavior they all report does not fit within the framework of the then-reigning dominance paradigm, while the behavior of the Gombe apes does fit the aggression-based model.

I suggest that we must look beyond the Gombe. In polar contrast to the Gombe reports, quite separately but with striking unanimity, all of the early researchers using naturalistic methods report fluid, open groups of nonaggressive chimpanzees living peacefully on home ranges open to all. Goodall's reports, prior to 1965 when the closed-box feeding was begun, are virtually the same as the unanimous naturalistic reports. The then-little disturbed Gombe apes and wild chimpanzees of both sexes were observed to freely enter, feed, travel and mate in each others' group, without any sign of hostility or exclusion.

It should be kept in mind that when the young and then inexperienced Goodall began her study of the Gombe group in 1961, very little was known about chimpanzees. However, in the past 3 decades, much knowledge has accumulated. It is now known that chimpanzees and humans have such exceedingly close morphological, physiological, genetic, and behavioral affinities as to be considered a sibling species.

In view of this new knowledge of chimpanzee psychology, I suggest that the use of some psychological theory, usually restricted to understanding human behavior, is useful in understanding the changed behavior of the Gombe apes. Before pursuing this further, the human controlled closed-box method of feeding and my reasons for targeting it as the catalyst for negative change, must be explained.

Variations of this unique feeding method were used at Gombe at least several times a week, for a period of 10 years (from 1965-1975). They involved the use of steel-lidded cement feeding boxes, which were sprung open by the researchers pulling on concealed, underground wires attached to the lids at irregular intervals deemed opportune. Goodall reports that during the indeterminate wait for the boxes to be sprung open, "tremendous tensions and frustrations built up in the waiting apes, and they began turning on each other in sudden, explosive, competitive fighting."<sup>3</sup>

While some Gombe observers agree that the closed-box feeding frustrated the chimpanzees, most assume that the effect on the animals' behavior is insignificant, and that therefore their behavior accurately represents the typical behavior of the species generally. However, as theorists (and experience) tell us, tension is the required state for competitive interactions among humans. Also being part of a directly competing group heightens latent hostility towards others.<sup>4</sup> And as we now know, chimpanzees share much the same psychology as ourselves.

Frustration, a state known to be shared by humans and animals, is usually defined as being a condition that results from the interruption of an organism's habitual sequence of acts directed toward attainment of a goal.<sup>5</sup> Theorists hold that states of powerful, repeated or accumulating frustration almost invariably results in some form of aggression, not always expressed overtly. As frustration theorist Janis explains, this definition involves the presence of two key components; a motive (an aroused need, drive or tendency to action) and a barrier (in this instance,

the closed feeding boxes) preventing the motivated organism from reaching the desired goal.<sup>5</sup>

Quite without such intention, and without anyone recognizing it, the closed-box feeding method amounts to being a classic frustration-aggressive schemata, closely paralleling the famous Barker, Dembo, and Lewin psychological experiment, which firmly established the components of frustration-aggression situation back in 1941.<sup>6</sup>

I suggest we must look beyond the Gombe findings for answers as to the usual behavior and social organization of undisturbed wild chimpanzees.<sup>2</sup>

To to make another point, chimpanzees (like the human foraging or hunting-gathering groups) follow a pattern of fission and fusion into small, fluid, very temporary subgroups to forage, and come back together, from the time to time, to socialize. This pattern involves indirect competition, the separate, simultaneous non-confrontational seeking of the same resources.<sup>7</sup>

Characteristically, when an undisturbed subgroup located an ample source of ripe wild food, members 'food-call'; hooting and beating on trees, which attracts other roamers--both of their group and outsiders--to 'fuse' with them for a time, to share the bounty and to socialize, thus renewing old bonds and forming new ones as a larger society. The local people call these constantly recurring chimpanzee gatherings 'Ngoma' (meaning drum dance), but as Reynolds points out, the term 'carnival' perhaps better conveys to us the highly positive, pleasurable 'social climate' of these intensely social gatherings.

The aspect of the extensive negative change in the Gombe society, which is of special relevance to our present interest, is the apes' radically changed behavior toward 'outsiders'; chimpanzees who were in most cases not strangers, but familiar members of neighbouring wild groups with which they had formerly interacted amiably. This particular change came about approximately 2 years after the closed box feeding was introduced, when groups of Gombe males began what Goodall indicated was a new

pattern: tense, silent patrolling of the peripheries of theirs and the neighbouring groups' home ranges, seeking out and attacking encountered 'outsiders', both male and female.<sup>8</sup> These gang attacks are described by Goodall and associates as being "vicious and brutal".<sup>8</sup>

Most animal behaviorists agree that the striking thing about animal conflict is its conventional, non-injurious nature. Animals usually seek to drive away, rather than kill, their competitors. But it was the Gombe observers' impression that the aggressors were deliberately trying to kill their victims, and they succeeded in doing so many times. Goodall (1986) even suggests that the victims were treated as prey animals.<sup>9</sup> They were, as she put it, "dechimpized."

In stark contrast to the highly positive attitude of the peaceful, undisturbed wild chimpanzees towards neighboring groups, the negative behavior of the Gombe apes toward familiar local groups--no different from themselves--contains many of the elements characteristic of the phenomenon we call racism when seen in our own species: rejection, exclusion, hostility, fear, persecution, de-individualization, and the ultimate form of assigned inferiority we refer to as 'dehumanization' which, in this instance (and following Goodall), might be dubbed (rather awkwardly) 'dechimpization'.

The question this raises is: How might this racist attitude and behavior be understood in terms applicable to both chimpanzees and humans? I am neither biologist nor psychologist, so I offer the following suggestions only tentatively, for your consideration.

Biologists hold that no animal species is wholly or solely aggressive, or peaceable, by nature. Rather, they suggest that equal potentials for aggressive or peaceful behavior are both part of the genetic constitution of all animals, including ourselves. Arousal of either potential is stimulated by environmental or social circumstances. (Once roused, aggressive behavior, like racism, may be culturally learned).

Frustration theorist, M.B.F. Maier points out that

through natural selection, animals are endowed with the genetic means of developing different sets of behavioral systems.<sup>10</sup> Shafston suggests further that one or another of these varied behavioral systems, adaptive in the situation, is activated over time by cutting in or cutting out of a related mind set, composed of a coordinated array of perceptual biases, motivated tendencies and motor potentials.<sup>11</sup> These differing mind sets function to allow the animal to deal adaptively with situations that recur naturally in either social or asocial circumstances.

The many years of aggressive responses and direct competition that were activated by the frustrating feeding method are certainly not a natural experience of wild chimpanzees, but they have been the recurring experience of the Gombe group since

1965. The artificial feeding has been discontinued, but recent reports indicate that the negative behavior of the Gombe apes is not much changed.

Was a latent negative mind set with its related negative perceptual biases and behavioral tendencies aroused in the Gombe group through the decade of prolonged human-imposed frustration? so, this strongly suggests that such negative mind set is either a fundamental element~or at least ideal mental state~underlying the appearance of racism in our human primate species also.

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*by F Knobloch*

## **ARTICLE:** Interpersonal meaning of music and ethology

My study of the significance of music has been slowly progressing since the early sixties<sup>1,2,3</sup> as a by-product of my psychotherapeutic work, particularly in a therapeutic community viewed as an ethological laboratory.<sup>4</sup>

### **Musical experience as interpersonal process**

The cliché "music is expressing feelings", though true, always has seemed to me to be a mind-stopper, preventing a broader understanding of the meaning of music. As in a dream, music creates a fantasy world. It elicits in us tendencies towards others, and lets us feel the impact of their tendencies. In other words, music creates interpersonal situations in which we participate, although the narrative is only schematic and does not describe any concrete situation. Suppressing the tendency to speculate - not unknown in the field of esthetics -- I designed a simple experiment to test the hypothesis that *music has interpersonal meaning which is basically the same for different people.*

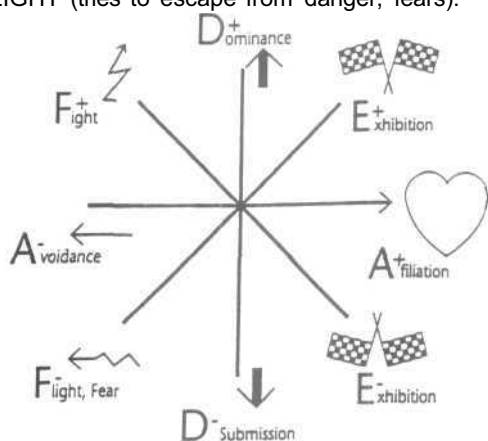
A military march played to soldiers going to war is designed to support their optimistic attitude of self

confidence, dominance, pride, showing off and aggression. Such music makes us to *identify* with these tendencies. The result could be quite different if these tendencies would be presented instead in posters -- such as pictures of aggressive men who shoot. Rather than identifying, we may have a *complementary* reaction - being pushed into submission, feeling attacked, feeling fear and running away. The same may happen when we hear such music in sleep: we do not *identify* with it, we feel the dominant and aggressive impact which pushes us into *complementary* reactions of submission, avoidance, fear and flight. In waking state, music can create both identification and complementation - but most of the time identification.

### **Hypothesis testing: the musical circumplex**

In the first experiments, Leary's circumplex with its eight tendencies was used.<sup>5</sup> But soon, listening to music and inspired by ethology (e.g., I missed flight, fear), I changed it. The circumplex (Tab and Picture) has a *horizontal* (affiliation-avoidance) and a *vertical* (dominance-submission) axes. The tendencies are:

D+: DOMINANCE (directs, dictates, advises, controls);  
 D-: SUBMISSION (asks for or accepts direction, dictate, advice, control);  
 E+: EXHIBITION + (displaying a supposedly valuable quality: I am powerful, attractive, intelligent, have high status, etc.);  
 E-: EXHIBITION • (displays a lack of some quality: I am needy, weak, suffering, ill, depressed. - Sympathy-, care-, pity-soliciting.);  
 A + : AFFILIATION (approaches, seeks cooperation, closeness, friendship, attachment, love);  
 A-: AVOIDANCE (avoids contact, seeks isolation, distance, detached).  
 F+: FIGHT (attacks, shows anger, hostility, aggression);  
 F-: FLIGHT (tries to escape from danger; fears).



Since the hypothesis tested deals exclusively with the question of the interpersonal meaning of music, questions about the psychometric properties of the circumplex are not important here (such as: Is it psychometrically well ordered? - as, e.g., J. Wiggins tries to achieve<sup>6</sup>; or the question: Is the basic idea of interpersonal circumplex psychometrically sound?, raised by J.D. Jackson<sup>7</sup>: "the circumplex structuring is too simple ... inconsistent with much of modern measurement theory"<sup>6</sup>),

Two tests were used, consisting of 24 and 17 fragments respectively of European music from the 17th to 20th centuries. They were presented to experimental subjects (ESs) who independently answered which tendency or tendencies they found in each fragment. In the first experiment there were 3 ESs, in the second 59 and in the third 3 again. In the statistical evaluation the null hypothesis ("the answers are random") was rejected on 5% level of confidence in the first and on 1% level of confidence

in the second and third experiment. Numerous subsequent unpublished experiments both from Europe and North America showed the same results. The same has been reported by others (e.g., the musicologist J Doubravova<sup>8</sup>, etc). *It is concluded that in the populations studied, music has basically the same interpersonal meaning for all people.*

### Cultural and biological programming

What gives the music its universal meaning? One part of the agreement is, no doubt, cultural. If one hears organ music, one thinks about church, something sacred or religious - but only in certain cultures of the world! On the other hand, some aspects of music are likely universal. Nobody will think about lovers in moonlight or sleeping baby, when hearing the loud and quick beats of drums or sounds of loud trumpets. Some sound patterns, such as laughter, crying, male and female voice, are believed to be evolutionary programmed *social releasers* (Lorenz) and are imitated in music by different instruments (flute, violin, violoncello, etc.), possibly as *supernormal stimuli*. Supernormal stimuli are artificial stimuli which have greater impact as social releasers than natural stimuli, (e.g., an artificial egg four times larger, but with a clearer black-and-white pattern than the real eggs of the bird ring plover is preferred by the parent bird, which throws out of the nest its own eggs in favor of the artificial eggs. A gape of the parasitic cuckoo nestling has a higher stimulus value for the parent birds than of their own nestlings. Music has in some respects similar complex structure as language - possibly sharing with it the "deep structure" (Chomsky). The interpersonal character of music is, of course, not the only important aspect of music. Though this will be likely different in future, the present, rather primitive interpersonal analysis is not in a position to contribute to the assessment of the esthetic quality of music. Also, the interpersonal reactions of people of different cultures have to be studied much more extensively than has been possible so far. However, the method can be used already now for studying objectively, e.g., how the musical interpersonal spectrum of a composer changes during his/her life, and how the spectra of different composers and of different musical epochs



differ. Also the difference between poetry and music can be studied. For example, there is a discrepancy in renaissance madrigals, e.g., those of Orlando di Lasso, between the text expressing strongly E- ("I am suffering"), and the lack of E- in the accompanying music. And only three decades later, the E- tendency is strongly expressed both in the text and music in the madrigals of Carlo Gesualdo, that passionate partisan of chromaticism and jealous killer of his wife.

### What would Nietzsche say today?

When talking about biological programming of music, one must think about Friedrich Nietzsche, who claimed that esthetics is "*nothing but a kind of applied physiology*" and explained:

*What is it that my whole body really expects of music....? I believe, its own ease: as if all animal functions should be quickened by easy, bold, exuberant, self-assured rhythms; as if iron, leaden life should lose its gravity through golden, tender, oil-smooth melodies. My melancholy wants to rest in the hiding places and abysses of perfection: that is why I need music.*"<sup>10</sup>

Today, when evolutionary biology and physiology invaded the territory of interpersonal relations, Nietzsche might talk about esthetics as *applied ethology* or *sociophysiology*. Encouraged by what he said at different times, but still with trepidation

and apologies to Nietzsche, I will take the risk and say loud my guess:

*I, Zarathustra, descended from my mountain because I love people, that rope between the ape and overman. I am with people all the time - whether they are real or my phantoms. We are connected — indirectly by the illusive bridges of words, and directly by music, dance and laughter. What is it that my whole body expects from music? Its own ease: as if all animal functions in me and my friends - love, anger, pride, sadness - were quickened, and I could reach the essence of people as never before. Music makes me slip into the dramas and tragedies of others' lives - and for a while I am even seduced into believing that these concrete scenes reveal the meaning of music. But that is how music teases me. The scenes are only illusions, reflections on water. The paradox of music is that it is a universal language - and yet, the universality is not an empty universality of abstraction. Music talks directly to my senses and my body and reveals the true nature of human beings.*

Interpersonal analysis of music may bring us not only closer to the understanding of music and arts, but also to the understanding of the depths of human nature.

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by C Reichelt

## **ARTICLE:** The Evolution of Desire: Strategies of Human Mating.<sup>1</sup> A review.

This provocative book epitomizes the quandary of the sociophysicologist: how to tell a difficult truth tactfully. Even though Dr Buss cites study after study to support his thesis that human sexual strategies are adaptations that have evolved through millennia and are extensions and elaborations of similar strategies of our nonhuman ancestors, I feel certain that he's raised the blood pressure of some readers (including mine at times). The book is readable, informative, carefully based in

research, often witty, engrossing and unsettling. I can only imagine the reaction of the non-scientist reader!

At core, Dr Buss outlines something akin to a "selfish gene" argument: an exceedingly broad range of human behaviors are aimed at the single goal of reproductive success, sometimes obviously, sometimes subtly. Specific sexual strategies evolved to solve specific adaptive problems;

psychological mechanisms developed to enhance and support the strategies. Because human cultures vary extensively, the context of our sexual behavior varies, but our ultimate goals remain the same. Thus, while the details of human sexual behavior in Botswana may not be the same as in London, the most basic strategies will be. Love, jealousy, commitment, desire, infidelity, all stem from the adaptively oriented sources aimed at the same end.

As a scientist, I enjoyed and applauded the book (as a woman, I was often uncomfortable). Dr Buss makes a good case and a high percentage of ASCAP readers will find themselves mostly persuaded. (Of course, we need little persuasion. These ideas are generally familiar and agreeable to us.) The book is well-researched and valuable; it should be widely read. In fact, I think it should be reworked into a small text for required junior high school education courses; it would be illuminating for youngsters afflicted with raging hormones. But admiring as I am, I believe Buss sometimes goes beyond his data. He explains too much with too little and what he omits may be crucial.

Even though what women want has been a matter of controversy - at least among certain males -- for centuries, Buss has the answer: an intelligent male with plenty of economic resources to care for the offspring in which she invests so many of her own resources. Everything else women want and need in a male companion, including health, sincerity and love, are related to commitment (read: of resources). Biologically, that makes perfect sense and is well supported by his data; but it seems so *limiting*. My feminine hubris would like female wants to be more, well, *elevating!* That all seems so grasping. Males, on the other hand, are portrayed as really interested in casual sex, but the only way they can have a good selection among young, healthy, attractive mates is to offer or feign commitment. That's not awfully elevating either, as a matter of fact. Ah-h-h, the pain of scientific truth!

Buss seems to believe that women, although lacking in structural power, hold most of the cards

in the reproductive power game. Female choice rules and, accordingly, males have evolved to be larger, stronger and driven to achieve status in order to fulfill the preferences of women.

That is the basic argument. The rest of the book compellingly expands and explains. For example, attracting a partner is a complicated business achieved through tactics designed to mesh with the fantasies of the attractee. Effective attractants are related to the desired adaptive result. Men want young, healthy mates able to bear them many children; thus, women wear lipstick and the Wonderbra to give an appearance of radiant youth along with good equipment for suckling. Women want physically strong, self-confident husbands with money to support their babies; so men lift weights and strive for nice cars -- and in Tanzania and Papua New Guinea, equivalent behaviors go on. His examples and research cover the world. Both sexes want a mate to be kind, sincere, attractive, loving and faithful. Interestingly, these preferences hold true among homosexuals too, even though reproduction is not an issue for them. Is that fact a weakness in Buss' argument or simply related to homosexuality itself?

Fidelity is important to both males and females all around the world; female fidelity ensures her mate's parentage; male fidelity ensures dinner now and a college education for the kids later. Dr Buss lays out the advantages of long-term committed relationships along with adaptive reasons for jealousy and other psychological and physical methods of keeping mates. Continuing to supply that which attracted the mate in the first place is highly effective, because it is biologically sound. Thus remaining healthy, attractive, young, strong, loving, wealthy, kind and so forth will keep mates, but that caution certainly is a dismal thought for people foolish enough to become ill or old. Despite the attractions of other females, fidelity is adaptively advantageous for males, who have more certainty of parentage when they are present to control the mother of their children; committing resources to another male's child would be a biological waste.

Buss describes sexual conflict including deception, rape, abuse and harassment as arising from the differences in male and female sexual strategies and lays out fairly convincing explanations for its existence. Marital failure, changes in desire and the value of marital harmony also receive attention.

This is necessarily a sketchy description, but it gives an idea of the breadth and depth of the book. So why my uneasiness? Because some of his ideas are pure stereotypes and others don't seem to fit reality. For example, my hubris notwithstanding, it is extremely stereotypical for women to be described in such "diamonds are a girl's best friend" terms, even though diamonds may be biologically sound. And then he describes women as "judicious, prudent and discerning about the men they consent to mate with because they have so many valuable reproductive resources to offer. Those with valuable resources rarely give them away indiscriminately". Where women have substantial control over their lives, perhaps they sound prudent and discerning in interviews, but in actuality, many give away those reproductive resources to a lot of losers! Which is *injudicious*. In Third World countries, female control of their own reproductive lives is extremely limited. Consider the Thai girls forced into prostitution; consider the mutilated African women; consider the women of India murdered so that a new dowry may be extracted from a father eager to get a girl child off his hands; consider the women victimized by male religious leaders. What is "judicious, prudent and discerning" about that? What female choice is there for these women? And, according to Buss, male control of women, including jealousy and violence, has perfectly rational biological roots. He does make an effort to draw the line between the reasonable and the pathological. And he certainly makes it clear that society can and must control the latter, but will his argument nevertheless be construed as "boys will be boys" by some? So while he has written a worthwhile, pretty well-thought-through book and I'm glad he wrote it, I fear it slightly. I hope that it won't be used against us, so to speak.

Some further comments, questions, and quibbles:

oddly, I'd never read much about possible reasons for menopause. He discusses most interestingly several past and current hypotheses. I'd like to add another. Since males have a tremendous proclivity for finding younger and more attractive wives as they grow older and accumulate more resources, does menopause serve to protect women from having children late in life when they are more likely to have no male help in rearing them?

Dr Buss suggests that women enter the work force, particularly when there are fewer males around, to enhance their desirability by increasing their monetary resources implying that women work outside the home only to attract males and provision young. My experience has been that women work for many reasons ranging from intellectual stimulation to love of the activity to needing to provide for *themselves!* Sometimes a cigar is just a cigar.

Finally, there is the question of the apparently strong male interest and indulgence in casual sex, which Dr. Buss feels has some adaptive advantages for both sexes. This seems to contradict somewhat his assertion that fidelity is adaptive. Casual sex gives males access to more females; it gives females immediate access to resources and permits them to evaluate potential husbands. For males, I can understand why broadcasting one's seed widely would increase the number of births. But isn't having offspring for whom one does not provide biologically unsound in humans? Out young require such long term nurturing. Won't many children die without paternal care? Consider the inner cities of the USA. With the increasing death rates there, one wonders whether casual sex has many adaptive advantages. STDs sterilize and kill their victims as well as producing defective children. Monogamy or something close to it really seems more adaptive for both genders. And there's some evidence for that point of view, I believe. Rather than searching immediately for a new mate, elderly widowers often sicken and die after the death of a spouse. Has anyone studied the effects of spousal loss on younger widowers? Is there increased morbidity? What are the effects on

# ABSTRACTS & EXTRACTS...

Papavassiliou AG: Molecular Medicine. Transcription Factors.

Schoenherr CJ & Anderson DJ: The neuron-restrictive silencer factor (NRSF): A coordinate repressor of multiple neuron-specific genes.

Szathmary E, Maynard Smith J: The major evolutionary transitions.

**Papavassiliou AG: Molecular Medicine. Transcription factors. *NEJM* 1995;332(1):45-47. (Article "crunched" by Beverly Sutton, M.D., Austin)**

*Summary:* Gene expression governing cell differentiation and maturation is regulated by transcription factors. Cells designate certain genes to be active in response to stress, hormones, growth factors, infection and other external stimuli. Most cancers begin by abnormal expression of the genes governing growth and differentiation (proto-oncogenes).

Recent advances in finding out how genes switch on and off include the discovery of a class of proteins, the transcription factors, that regulate gene expression by binding to regulatory parts of the DNA known as promoters and enhancers. They also alter formation of messenger RNA through interaction with DNA. Usually, the DNA-binding part (domain) of a transcription factor is  $\alpha$ -helical in shape and is near or within positively charged amino acids. This domain binds to the promoters or enhancers in DNA. Factors regulating gene transcription include a separate area (transactivation domain) that mediates cooperative associations. This structure reflects the shape of the DNA double helix coiling into supercoils and chromosomes and the DNA binding and scaffolding proteins, e.g., histones. Particular genes are

accessible to transcription because of the higher-order DNA in which they are found.

These structures in the cell nucleus and extracellular or intracellular signals lead to modification of DNA. Phosphorylation and steroid hormones alter the function of transcription factors. This, in turn, modifies gene regulation during cellular response to neurotransmitters, growth factors, antigens and cytokines.

80% of transcription factors are found in the following forms:

1. helix-turn-helix (HTH). 2. zinc finger. 3. leucine zipper. 4. helix-loop-helix (HLH)

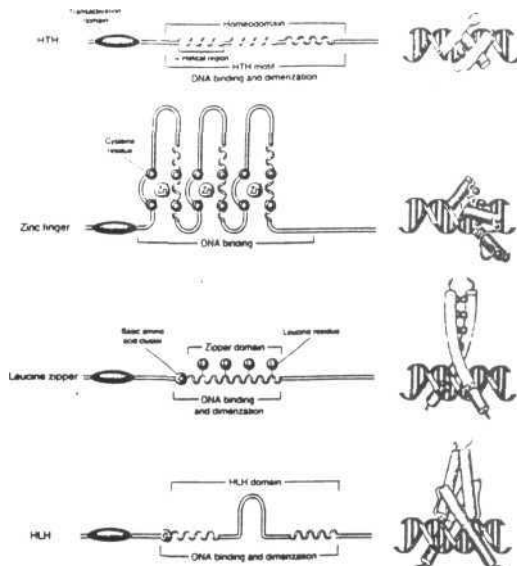
The HTH transcription factors are important in embryonic development, differentiation of hematopoietic precursor cells and neoplasms. These factors consist of 3 linked  $\alpha$  helices and one of these interacts with DNA at multiple sites. Pit-1, Oct-1, Oct-2, and Unc-86 (POU) proteins play an important role in gene expression during immune and central nervous system development.

The zinc finger has a pair of histidines and a pair of cysteines or two pairs of cysteines that hold a zinc ion. This makes the polypeptide between the amino acid pairs loop out. The specificity for the DNA sequence in the amino acid is at the base and body of the finger (not the tip). These transcriptions bind as dimers to elements in the upstream region of the gene under control. Steroid or thyroid hormone receptors are a class of zinc fingers and are involved in hereditary vitamin D resistant rickets in one family and in cancer. WTI is a zinc finger protein (a tumor-suppressor) that is inactivated in Wilms' tumor. GATA-1 helps control globin gene expression. Other zinc finger proteins control the gastric proton pump (H<sup>+</sup>/K<sup>+</sup>-ATPase genes).

Leucine-zipper proteins have an extended  $\alpha$  helix with leucine in every 7th position. The leucine along

one side of the a helix interacts with the leucine on the corresponding helix of its partner - "zips up". This protein underlies interactions that determine cell lineage and growth (including carcinogenesis). Another activity governed is CREB (cyclic AMP response element-binding protein) involved in gluconeogenesis, neuronal excitation, circadian rhythms, pituitary proliferation, and opiate tolerance.

The HLH proteins bind to muscle-specific transcription factors controlling myogenesis and to myc genes involved in growth regulation. There is a region of positively charged amino acids that contacts DNA and an adjoining region mediating dimer formation. Both types of transcription factors play major roles in differentiation and development.



**Figure 1. Transcription Factors and Their Interactions with DNA.** The general protein structures of the four major classes of transcription factors described in the text are shown on the left. The structures include a transactivation domain linked to a DNA-binding domain and, in certain cases, a dimerization domain. The types of transcription factor take their names from the characteristic motifs (helix-turn-helix [HTH], zinc finger, leucine zipper, and helix-loop-helix [HLH]) involved in DNA binding and protein dimerization and are shown on the right interacting with DNA. The cylinders represent  $\alpha$ -helical regions, and the areas that contact DNA directly are green. Zn denotes zinc, C cysteine, and L leucine. The plus sign indicates a positive charge.

**Schoenherr CJ & Anderson DJ: The neuron-restrictive silencer factor (NRSF): A coordinate repressor of multiple neuron-specific genes. *Science* 1995;267:1360-1363.**

**Abstract:** The neuron-restrictive silencer factor

(NRSF) binds a DNA sequence element, called the neuron-restrictive silencer element (NRSE), that represses neuronal gene transcription in nonneuronal cells. Consensus NRSEs have been identified in 18 neuron-specific genes. Complementary DNA clones encoding a functional fragment of NRSF were isolated and found to encode a novel protein containing eight noncanonical zinc fingers. Expression of NRSF mRNA was detected in most nonneuronal tissues at several developmental stages. In the nervous system, NRSF mRNA was detected in undifferentiated neuronal progenitors, but not in differentiated neurons. NRSF represents the first example of a vertebrate silencer protein that potentially regulates a large battery of cell type-specific genes, and therefore may function as a master negative regulator of neurogenesis.

**Szathmary E, Maynard Smith J: The major evolutionary transitions. *Nature* 1995;374:227-232.**<sup>1</sup>

**Abstract:** There is no theoretical reason to expect evolutionary lineages to increase in complexity with time, and no empirical evidence that they do so. Nevertheless, eukaryotic cells are more complex than prokaryotic ones, animals and plants are more complex than protists, and so on. This increase in complexity may have been achieved as a result of a series of major evolutionary transitions. These involved changes in the way information is stored and transmitted.

**Table 1. The major transitions**

Replicating molecules to populations of molecules in chromosomes  
 Unlinked replicators to chromosomes  
 RNA as gene and enzyme to DNA and protein (genetic code)  
 Prokaryotes to eukaryotes  
 Asexual clones to sexual populations  
 Protists to animals, plants, and fungi (cell differentiation)  
 Solitary individuals to colonies (non-reproductive castes)  
 Primate societies to human societies (language)

**Extract.** There are common features that recur in many of the transitions [listed in table 1]: (1) Entities that were capable of independent replication before the transition can only replicate as parts of a larger unit after it. For example, free-living bacteria evolved into organelles. (2) The division of labour: as [Adam] Smith pointed out, increased efficiency can

result from task specialization... For example, in ribo-organisms nucleic acids played two roles, as genetic material and enzymes, whereas today most enzymes are proteins. (3) There have been changes in language, information storage and transmission. Examples include the origin of the genetic code, of sexual reproduction, of epigenetic inheritance and of human language. ...

Grammar enables a speaker with a finite vocabulary to convey an indefinitely large number of meanings, just as the genetic code enables DNA to specify an indefinitely large number of proteins. We accept Chomsky's argument that grammatical competence is unique, both in the sense of being peculiar to humans, and of being special to language, and not merely an aspect of general learning ability. But we are puzzled by the reluctance of many linguists, including Chomsky himself, to think about the evolution of this competence. The objection takes the form of asserting, not only that human language is different in kind from animal communication, but that no intermediate is possible between the two.

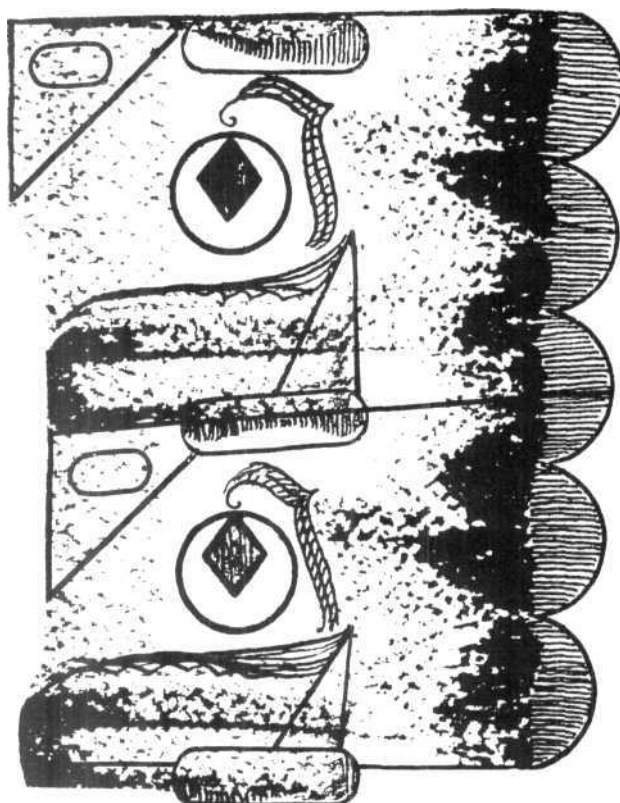
It is argued that any rudimentary form of grammar would not allow one to generate some types of sentence. This is true but irrelevant: by analogy, it is better to have some light-sensitive cells than none at all; a perfect eye is not the only useful solution to the problem.

It is in fact rather easy to think of intermediates between protolanguage and true language. There remains the question of the evolutionary origin of grammatical novelties. It is reasonable to assume that this happened by genetic assimilation, new rules being made up by individuals as non-genetic innovations, then learnt by members of the community, then hard-wired into the 'language organ' subsequently. It has been demonstrated that learning and selection can lead to such as assimilation in extreme cases when the latter alone could not get anywhere: learning can transform an initially flat fitness landscape with a needle-like peak into a well-behaved Fujiyama-like one.

Perhaps the most convincing evidence both for the

belief that grammatical competence is to some degree independent of general learning ability, and for the possibility of functional intermediates between no grammar and perfect grammar, comes from studies of hereditary variation in linguistic competence. One remarkable case involves a family in which a so-called feature-blind dysphasia seems to be inherited in a mendelian fashion, a single dominant gene being responsible.... it demonstrates that there can be something useful between perfect grammar and protolanguage: it also holds out the hope that we will in the future be able to dissect language genetically, as we are today dissecting development.

Conclusions: A central idea in contemporary biology is that of information. Developmental biology can be seen as the study of how information in the genome is translated into adult structure, and evolutionary biology of how the information came to be there in the first place. Our excuse for writing an article concerning topics as diverse as origins of genes, of cells and language is that all are concerned with the storage and transmission of information. The article is more an agenda of future research than a summary of what is known. But there is sufficient formal similarity between the various transitions to hold out the hope that progress in understanding any one of them will help illuminate the others.



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## Abstract... p 20

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