

ASCAP NEWSLETTER

Across-Species Comparisons And Psychiatry Newsletter

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"Psychiatry shares with other medical disciplines..formal or generic premises regarding what disease is, but it is unique in that to a very large extent the content or material that clothes these diseases is rooted in social behavior." Fabrega¹

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

For the philosophy guiding this newsletter, predicated upon combinations of top-down and bottom-up analyses, see footnote on p11³

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

Features; Some highlights of the Human Behavior and Evolution Society (HBES) meeting in Los Angeles p2

Summary comments of 3rd meeting of the Basic Plan Group aiming at collecting data to test the yielding/defeat-depression hypothesis. . . . p4

Responses to Dr. Price's stimulus essay (ASCAP Vol3#7 July) include:

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Letters: 30 hug 90

ASCAP readers might like to know that Randall Collins, Prof of Sociology at Riverside Campus U of California wrote to say Social Fabrics of the Mind was in his opinion a significant step forward for social science and has set out his reasoning why it helps to clarify the theory of social science in an article in the Am J Sociol 1990(Jul);96:32-68 entitled "Dimensions of Microinteractions." Reprints can be had from TD

Kemper (joint author)⁴.

Through ASCAP may I add my heartfelt congratulations to Lionel Tiger on his appointment to the Chair of Charles Darwin Professor of Anthropology at Rutgers. This is a remarkable vindication of his resolution to bring ethology and social anthropology together summed up in his own words "it can be done" after he, Robin Fox, myself had met together at King's College in the early 1950s.

Michael Chance, Birmingham, UK

Letters (cont.): 20 June 1990

During my contact with Prof Ch Badcock (London School of Economics) he informed me about your publication and courteously sent me the Feb issue of the ASCAP Newsletter. We in the USSR should be very interested in obtaining copies of your interesting publication and to take part in the work of the newsletter. ...

I think that you are well informed about the present situation in official soviet psychiatry, but I can note that [even] with an official very political [state of the specialty] in our country, another psychiatry has always existed. [However] representatives of this "underground" science have had a lot of difficulties with publication of their works. [This is] first of all [due] to the absence of wide circles of special[ty] [publications].

The general direction of my own study is evolutionary psychiatry. I

am trying to unite the data of clinics, genetics, history of the mind, ethology and ecology in [a] clinical-evolutionary study of mental disorders which in principle helps to build the evolutionary conception of pathological behaviour. [This may be similar] to sociobiological psychiatry. If you [are] interested in details of our work, I with great pleasure [will] present them in an essay. Furthermore, my own manuscript of the book "Evolutionary Psychiatry" (about 700 pages in Russian) is ready, which according to long publishing time in our country will be printed only in 1994. ...

Prof V Samohvalov, Dept Psychiat,
Simferopol, USSR

Thank you for your cordial letter. We respect your distance from the politicizing of psychiatry which must be very distressing to the conscientious practitioner.

We are excited to hear about your book and look forward to learning more of your thinking and findings. How frustrating the long delay on the publication of your book.

We are convinced that new insights can emerge from thinkers and scholars other than those immersed in the everyday science of the west; sometimes we are rigid in our thinking and constrained maladaptively by our paradigms. Indeed, this is a primary reason for the ASCAP Newsletter.

As you will see upon reading ASCAP, another psychiatrist from USSR, Irina Zhdanova has contributed to ASCAP her findings that rat social behavior may be altered by injections of spinal fluid from affective patients.

Please do contribute one or more essays to ASCAP that would describe your work. Moreover, would you respond to the essay by Dr J Price? Dr Price has pioneered linking the biology of social rank hierarchies with affective states.

Letters (cont.): 28 Aug 1990

I was so very pleased to meet you at the HBES meeting last week! I was particularly interested to hear about your ASCAP Newsletter and am very eager to get on your mailing list.

Enclosed is the talk that I gave at the meeting, wildcat speculation that it is. I would be very grateful if you have any citations concerning the "runaway" evolution that I call "selection inclusion" in my paper. Also, have you ever heard anything about gene couples that I talk about in the paper? I am looking forward to hearing more about Angelman's syndrome next year.

John Wylie, Washington, DC

HBES annual meeting (Aug 17-19) by RG

Kalman Glantz wasn't there! At my first UMich meeting, our last year's ASCAP reporter was one of the first persons met and I have enjoyed his excitements and his challenges since.

But old and new friends were there, eg, J Wylie (see letter above) has sent his paper for a future ASCAP and the active questions he poses should cause us to anticipate it. Moreover, I learned that SJ Ferrara from Oak Park, Ill, USA is a fellow science-watcher. Over some decades of organizational involvement, I have evolved a people-watching/organization-watching avocation (PWOVA). The acronym has an aboriginal sound, doesn't it? This is apt as the third plenary session featured detailed anthropological studies of primitive societies. PWOVA said "Anthropologists are important in this organization!" Hearing them I felt educated about the hypotheses and careful methods of this discipline.

The out-going president W Hamilton showed his PWOVA in a long letter he sent to us; he didn't come to the meeting as he was in the Asian far-east searching for beetles under the bark of dead breadfruit trees. He

said "...wishing long life for the society and wishing it to have free, truthful discussion may not be compatible." Also "..all people while they are with us should dare at least to try out ideas that would estrange colleagues and ruin careers elsewhere." How interesting. He doesn't seem to expect many of us to be with the organization over time nor that the organization will itself exist very long, at least excitingly.

The many disciplines of HBES are a challenge for it of course. Concerning ASCAP's interests, only a handful of psychiatric researchers, psychiatrists, and other clinicians were around, but perhaps because R Nesse, the incoming president and founder of the organization, and M McGuire, the local host, are both psychiatrists, the handful that did make it were treated with respect and our contributions with courtesy. (I for one was appreciative; also Dr Nesse seemed interested in pursuing K Norberg's idea that future meetings confer Category I AMA continuing education credits that US physicians need).

Richard Alexander, like Hamilton originally an insect biologist, led off the entire conference fittingly because he has been the inspiration and conceptual leader of the UMich group and hence the HBES itself. He suggested that the calculation of personal benefit that the moral person must carry out to warrant his moral behaviors has to be made unconsciously; if such calculations become conscious, they become obviously self-serving and not moral .

This idea is interesting juxtaposed to my reading of Caro's biography of the Texas politician and former US president, LB Johnson, who independently discovered and made practical use of this algorithm; indeed, Caro seems to organize his story about LBJ's obviously conscious and *tightly secret* calculations, which deeply offend Caro, clearly a highly moral per-

son. LBJ from childhood knew how to alter the actions of others with an appearance of morality.

Moving on, PWOWA noted hints from podium-conduct that strongest antagonisms between people at HBES were *within* not between disciplines. Subtle interdisciplinary issues existed of course, not best described as overt conflicts but perhaps creative tensions. Such were best revealed by the second day's plenary session of young presenters with a cash prize awarded to the "best." Results for the audience was delightful: Four obviously well prepared, interesting and well presented talks all deserved the prize so the winner told us more of the judges than the presenters. So guess from the summaries below who won... (Guess also the unofficial ASCAP winner, which also tells you more of the judging than the relative merits!)

Mark Erickson, a psychiatry resident, presented a literature review on the primacy of the incest taboo across-species and across-cultures and noted that this has implications for the psychoanalytic concept of the Oedipus conflict which posits that the incest taboo is secondarily developed *after* sexual attraction, not primarily. ME suggested that familial bonding (FB), attachment connections between birth and six years, is highly important; sexual attraction between family members is evident only when FB has been disturbed.

The second paper by Bruce Ellis and Don Symons produced considerable audience discussion (DS, much in the audience, wrote a book a few years ago on the evolution of sexual differences). His junior colleague Bruce has collected more data and presented more facts about how women and men have different sexual fantasies, as evident from preferred readings (romances vs pornography).

A third paper by Xian-Tian Wang and V Johnson (VJ not an obvious presence

in the audience) juxtaposed endocrinology (progesterone peaks at ovulation) with electrophysiology and psychology. Sensory evoked potentials are evident milliseconds after stimuli are given (a computer averages the EEG after multiple stimuli). P300, a positive wave occurring at about 300 msec, was previously known to be germane to cognition. This data showed that pictures of babies and nude males caused p300 amplitude to be greatest in women when progesterone is highest.

Finally, a presentation of Stanford psychology graduate student Geoffrey Miller energized us from his sheer effrontery: Psychology has no basic theory and is a bankrupt discipline without grants from the federal government. Psychology should embrace evolutionary theory by using Darwinian algorithms. Several of us in the audience interested in alpha psallic had to admire his implementation of that communicational state: he was funny, irreverent, entitled, and outrageous. Flaunting his lack of data, he asked and answered rather proudly, "Data? I have no data."

Who won? Not the psychiatric resident with the very important formulation for psychiatry; not the data juxtaposing three disciplines with a new finding; not our Darwinian algorithmologist. PWOWA says from Chimpanzee Politics that he who is strongly allied with power in the organization gets high rank in tech, high panache, high importance for psychiatry did not win, but rather sex differences in sex fantasy held the day. Well, maybe that's nice in a way. Low key data collection and alliance with the powers and ideals of the organization maybe should determine such prizes. Congratulations to Bruce Ellis.

However, our non-cash non-important ASCAP prize goes to Mark Erickson with Xian-Tian Wang in second. Congratulations to both of you as well!

Minutes: Basic Plan Group by RG

Date: Aug 17, 1990; Place: Private dining room, UCLA cafeteria; Time: noon to 2 pm. Present: R Gardner, chair; D Wilson, J Pearce, B Wenegrat, MT McGuire, R Nesse, A Lloyd, M Erickson, D Bach and SM Gardner. The group appreciated the arrangements help of Toby Kronin from UCLA. K van Schoor, S Heisel and K Norberg spoke privately later: each was interested and felt involved in the process, but hadn't able to make it. (Enthusiastic comments came in later conversation on our way to see the famous vervet monkeys of UCLA, whose raised serotonin in alpha males had been first noted by Raleigh and McGuire.)

Back to the group that did meet. Except for DW and JP, this group was new to the project so two agenda items prevailed: 1) acquainting the new members with the previous proceedings, 2) the open-ended discussion of the issues and how the hypothesis of a deep identity between depression and defeat/yielding might be tested.

To state the conclusions that emerged from the group's work: 1) A conference in July 1991 in England is requisite for the next step. John Price, the original author of the hypothesis, and Paul Gilbert, who has done the most data-collection bearing on the issue so far, are there and have made the invitation. The group agreed that a two day conference would be good.) (From M McGuire) Some data, of any sort, must be collected as soon as possible to achieve momentum and to let the results affect a next iteration of data collection.

Some other ideas that emerged included the collection of data with assistance of unions to study persons losing their jobs from plant/company closings. Usage of games might cross cultural barriers. Also a considerable literature exists on cross-cultural facets of depression.

Depression In Pernicious Environments: Comments on J Price's Paper

by A Randrup and G Sorensen

There is ample evidence that unfavourable environments may cause the death of animals and humans by means of central nervous mechanisms, neurogenic or psychogenic death⁸; the comprehensive experiments of D von Hoist with tree shrews^{7d-e} are particularly relevant with respect to Price's paper. Von Hoist put two males of this strongly territorial species in one cage, and in many of the pairs he observed that the one, which became subordinate, also became extremely passive and died within a few days. Some subordinates, however, developed a strategy involving active avoidance of the dominant (self-effacing?) and survived longer. The psychiatrist Bettelheim apparently made analogous observations in humans, when he was prisoner in a German concentration camp^{7f}. Maybe depressive and other psychiatric symptoms are sometimes parts of a process leading to death (eg, suicide), but in other cases constitute defense reactions, pathological modes of living in pernicious environments (or perhaps in some cases preferred modes of (limited) living in difficult environments). As described by Price the depressed patients "get their way" (ie, live and survive) in both the agonistic and the hedonic social settings, although in a limited way.

In our own experiments with bank voles we found that stereotyped behaviour, even severe stereotypies interfering with normal functions, may relieve stress and protect against lethal effects of an unfavourable (restricted) milieu^{7a}. Perhaps like fever which, though debilitating, may help to combat infectious disease. A reaction, which appears maladaptive, may still in this way have adaptive or survival value.

Finally we want to mention a book (in Danish) by the psychiatrist J Wil-

ladsen: Depression, your Name is Woman. Male Dominance and Health.

She regards depression in women as an overacting of the normal female role in a society dominated by men⁹.

Price-Reichelt Exchange

Dr. Price has written an elegant thoughtful essay...with a well-reasoned thesis, but it might be instructive to consider some other factors. Firstly, while ritual agonistic behavior (RAB) may usually be a male phenomenon, social rank may still figure heavily in female (yielding) depression. Remember, primate females are heavily invested in social hierarchies, since the rank of their offspring depends on their own. Hence, the female's ability to defend her place in the hierarchy is crucial both to her and to her young. With so much riding on her success in hostile encounters, a loss becomes particularly traumatic .

Secondly, might a slightly different interpretation of what is at work in the demanding depressed patient be helpful? Perhaps the sick role is more than a metaphor of submission. I suspect it may be even more primitive and possibly "healthier." Eliciting nurturance has less to do with social rank than with the expectations of an imprinted infant animal. Nothing is more domineering than that infant cuckoo of whom Dr Price writes. The nurturer is there for the infant's exclusive benefit, so the infant "believes." Thus the "sick role" isn't so much submissive as it is a demand for nurturance just as the imprinted infant demands nurturance from its mother. The message isn't only "I'm out of action"... it is also "Take care of me, as my mother did, when she made me feel valuable." The caretaking reassures the depressed patient that she has value after all and nourishes her needy core in perhaps the only way

believable at that point in her depression. After all, nurturance in infancy caused all of us to feel we were the center of the universe. So it is understandable that at a time of such self-devaluation, the patient may long for the reassurance provided by the caretaker with its echo of the supremacy of infancy. This could have certain healthy aspects as well...a sort of self-medication in the demand for nurturance. Indeed, does it signal an effort in the patient at giving up submission, if the therapist could capitalize on it? Or is it perhaps a sort of distorted effort by a dominance psalic to come forward, to use Dr Gardner's terminology.

Parkes-Price Exchange

I have found that John Price's theory does help us to understand depression and would like to add a refinement or two.

Observations of human animal companions (in particular, my cat, a neutered female) leads me to conclude that moods of apparent depression commonly follow humiliating defeats as when an uninvited Tom has bested her or I have inflicted summary punishment for theft.)\t such times she slinks into a corner of her home territory, adopting a low posture and remains indifferent to offers of food, stroking or other nurturance. Clearly she is still in an 'agonic' mode and will not fight unless cornered in which event she can become vicious and over-step the normal inhibitions against inflicting grievous bodily harm on humans.

Only as her mood lightens will she switch into a hedonic mode in which state she is more likely to seek proximity to my wife than to others and, while she will accept stroking it will be some time before she purrs, requests food or engages in play. Interestingly, rolling on her back or exposing her throat are in-

vitations to aggressive play which she never exhibits when 'depressed'. Thus it appears that nurturance, like other complex behaviour patterns, has a number of sub-routines each of which requires different internal and external stimuli for manifestation.

Studies of depression in humans suggest that this too can be switched on by humiliating defeats as well as by losses. John Price has made the ingenious suggestion that one of the reasons that people get depressed after a loss is to facilitate the decline in status which inevitably follows the loss of an ally. By becoming depressed the bereaved individual can move down the social hierarchy without having to fight every step of the way. (This does not, of course, explain why mothers who lose children are so susceptible to depression since the possession of a child is not associated with high status).

Seligman's theory of learned helplessness provides us with an explanation for the peculiar vulnerability of some people to depression since they expect to be defeated in all encounters and have a predisposition to yield. Genetic and endocrine factors may also play a part.

The suggestion that 'nurturant' behaviour towards the sick may be an innate rather than an acquired human characteristic I believe to be unlikely. Sick individuals certainly exhibit behaviours that inhibit aggression and evoke nurturance: they are passive, pale, incoordinated and often literally lower than the healthy. They may also adopt a plaintive tone of voice like that of the anxious child in need of protection and/or attention. What we are witnessing here is a part of the individual's range of attachment behaviours which do not disappear at the end of childhood. Like all other innate behaviour patterns they are modified by learning from the moment of their inception and 'sick role'

behaviour is not difficult to explain if we recognise that parents and parent substitutes vary greatly in their responsiveness to the range of attachment cues exhibited by their children; some may reward the above cues and little else, thereby providing a powerful incentive to the child to behave in this way whenever it feels frustrated or deprived.

This and similar experiences offer us an explanation for the various forms taken by depression and it is hardly surprising that some individuals cling, others rage and yet others become 'sick' when they experience life events which evoke feelings of helplessness.

Negativism, the refusal of nurturance or other offers of help is not necessarily to be seen as an expression of aggression. It may, rather, reflect a situation in which a person is in agonic mode and/or lacks sufficient trust in others to move into hedonic mode.

Hofer - Price Exchange

Russell Gardner, a friend and former colleague, asked for my reactions to Dr Price's article "Metaphors of Submission." I would not ordinarily have commented because my outlook on evolutionary theories of mental illness is essentially a wait-and-see one. I think the approach may well be important in psychiatry and is generally unappreciated at present. My own responses may provide some of the reasons for this.

Thus far there has been very little data gathered specifically to test evolutionary theories of mental illness and in my opinion the level of formulation has not been specific enough to generate crucial tests, counterintuitive predictions, or novel applications. Dr Price's theory is appealing because it accords with a great deal of what we know about

depression and about illness as a communication. But I can think of a number of depressed patients that don't fit very well with this formulation. From everything we know, depression is a heterogeneous disorder and it would seem unlikely that Dr Price's "yielding" hypothesis is going to apply to all forms, except at a most superficial level. Some forms (or levels) of depression may well be due to disease processes of recent origin that fall outside the workings of evolutionary processes. In fact, a strong case can be made that transient depressive responses should be clearly distinguished from depressive disorders; the former being more likely to fit theories such as Dr Price's than the latter.

This is an age in which available methods make empirical work in psychobiology enormously exciting: surprises, novel agents, and new relationships turn up every few weeks or months. This state of affairs takes the glow out of broad integrative theories such as Dr Price's and others I have read in ASCAP. Reading about empirical work is more fun for many scientists and generates a feeling that the terrain of knowledge in the brain sciences is changing so fast that it is the wrong time to try to draw maps. We are just beginning to test theories on the evolution of species-typical adaptive behaviors. But we know very little about the biological nature of mental illnesses, and have not yet established a basis for their cross-species analysis, two serious obstacles for any attempt to reconstruct their possible evolution.

Psychoanalytic and cognitive theories of depression are interesting because they suggest particular kinds of interventions that should help patients. Biological theories suggest experimental tests, new drugs or physical treatments (eg, light therapy). Evolutionary theories, on

the other hand, deal with ultimate questions and should not be compared with theories of proximate causation for such immediate payoffs. Evolutionary theories are most interesting when they bring us to a new view of our own nature, and Dr Price's theory has not yet been taken this far. I await further developments.

McGuire-Price Exchange

I have always liked what John Price has to say and the way he goes about his work, namely, to continually emerge himself in the complexities of his subject and not sit satisfied with first impressions or successes. "Metaphors of Submission" is another example of Price at work.

What issues need to be addressed given Price's view?

(1) Do depressed persons continue to seek goals to the same degree that they did prior to their depression? I couldn't find an answer to this question in the article. It seems to me, however, that it is necessary to assume that this is the case if one is going to postulate that an evolved characteristic of depression is that of manipulating others to behave in ways that increase the probability of the manipulator achieving his/her goals. My own view on this matter is that depression is a physiological/psychological response to suboptimal goal achievement. During mild depression, normal motivation remains relatively intact: namely, achieve those goals that are important to me as efficiently as possible. Gaining others' attention and help is often a way to do this. The problem with such behavior is that one's social environment will help and change behavior only so much. Moreover, empathetic or helpful others can only assist in limited ways. Thus, irrespective of social environmental characteristics, depression would be expected to continue

unless the depressed person changes his/her goals, which is usually the case among those who improve clinically. In instances of severe, debilitating depression, however, motivational priorities appear to change, with hostile motivations taking precedence. This view is not incompatible with what Price has postulated and could fit in with his formulation that different types of depression have different consequences in agonistic and hedonic social environments.

The preceding views about motivational change lead to other explanatory problems. At first glance it is not easy to explain why some persons with severe, debilitating depression are overtly hostile (attack) while others socially withdraw. Further, are these behaviors messages in the same way that mild depression can be considered a message? And, is it the luck of the draw that one gets depressed in agonistic or hedonic social environments or do the environments have something to do with the type of depression one develops?

(2) Given that some persons with severe, debilitating depression withdraw while others attack, the metaphor that seems most relevant to me is "I have yielded and you have not complied with my expectations that you will facilitate my goal-achieving aims, so now you must pay or, I don't want to have anything more to do with you." I suspect the differences in behavior (social withdrawal vs. attack) in association with this metaphor are due to differences in patients' capacities to accept the fact that members of their social environment will only expend so much effort (i.e., change their behavior) in response to depression. Those who can accept this fact as well as the fact that others have limited capacities to optimize goal achievement are likely to withdraw,

that is to leave their social group; those who can't, are likely to attack, that is punish members of their social group. Both types of behaviors remain messages, but now with very different meanings and interpretations. Social withdrawal is a message primarily because persons associated with the depressed person make it so (e.g., "I really worry about him"). It is less a message on the part of persons who withdraw, for they have largely given up membership in the group. This perhaps explains why such persons are so difficult to engage socially when they are withdrawn. Attack is a message whose meaning is clearly understandable to those who are attacked, so clear in fact that we often get angry (attack in return) at such persons even though we are aware they are ill. Persons who attack have not exited from the social environment. As Price points out, responses of observers across different environments to either of these behaviors are likely to differ. I don't think there is any question about this point and it is probably one of the main reasons that many depressed patients get better when they enter a new environment (eg, hospital)•

(3) Is it the luck of the draw that one gets depressed in agonic or hedonic social environments or do the environments have something to do with the type of depression one develops? Like Price, I suspect that there are factors at work other than those he has mentioned. Consider yielding behavior. We all engage in this behavior from time to time and if we don't do it too often depression usually doesn't follow. Persons who are mildly depressed often engage in yielding, eg, letting others hear their story, cook for them, run errands, etc. On the basis of animal data, such behavior (within limits) should result in a trend towards normalization of a typical physiological

state. Among other things, yielding behavior signals that one is still a member of the social system and that one has 'rights' to certain social benefits. Provided one collects these benefits, one should improve clinically. I would agree with Price's prediction that yielding behavior is likely to result in yielders getting their way in hedonic social environments. I also suspect that yielders are likely to get their way the first few times in agonic social environments – after all, such environments generally are not inhumane. However, in an agonic social environment, repeated yielding is likely to result in attack (eg, rejection, ostracism, etc.) directed at nondepressed members of the group. Whether depressed persons withdraw or counter-attack in this environment thus is in part determined by the characteristics of the environment just as their behavior is in part determined by their capacities to recognize their inability to alter the behavior of others beyond certain limits.

Price - Shami Exchange

As a scientist who is a layman to the field of psychiatry, I found the contents of this essay to be very interesting and thought provoking. Many more questions were raised in my mind than that of the single question in the title. Questions of this nature, derived from the original topic would probably be of interest to funding agencies. I note this because the author indicates that there is inadequate funding for this field of study. I do have three criticisms of this essay. These are: 1) The title does not accurately reflect the contents of the essay. 2) The question in the title is never directly answered. 3) The essay could be reorganized such that it would be more clear, concise, and readable.

Addressing the first criticism

above, the essay appears more accurately to contain an explanation of the means by which depressed patients get their own way. For example, in the hedonic mode, they get their own way by acting as children or women or by being ill. This is also one of the key points of the essay which I feel does not receive the attention it deserves. The author even speculates upon the possible mechanisms in terms of brain functions involved in childish or female behavior as opposed to illness (p 8, col 2).

The second criticism regards the title. It seems that the answer is, "Yes, depressed patients do get their own way under many circumstances". Although this answer is implied and much evidence is given for it, it is never directly stated. This should certainly be made clear in the abstract and concluding paragraph. I suspect that a direct answer is avoided because the original yielding hypothesis of depression generally requires depressed patients to be submissive. However, the author clearly states that the paradox will be explained by application of the concept of agonistic and hedonic modes. He then does apply and explain these concepts well. Perhaps a more appropriate title for the essay would be "Mechanisms by which depressed patients get their own way".

Finally, reorganization of the paper would allow the key points to be emphasized rather than lost. For example, the first sentence of the last paragraph on page 10 is important and should be at the beginning of the section rather than at the end. In general, the author seems to write in a pattern which includes much review material before he gets to the points he wishes to make. Although this is written for a relatively informal newsletter, I think it is still extremely useful to readers for the general format of a scientific paper to be adopted. This

would involve clearly stating the hypothesis first and then giving necessary background information. Some of the background information seems irrelevant and distracting to the essay as a whole. In addition, there are several paragraphs or sections which appear to be non-sequiturs. An example is the section on agonistic and hedonic modes on p 4, col 2, after the section on paradoxical power on p 4, col 1. Perhaps the author should describe exactly how he plans to present his material at the beginning of the essay in order that the reader will be better able to follow the ideas. Finally, I question the need for such length of essay. It appears that much material could be condensed and some omitted entirely. For example, direct quotes could be succinctly restated.

The questions that arise in my mind after reading this essay include: 1) How does the hypothesis of depressed patients getting their own way and the means by which they do it differ between men and women? Differences are alluded to in the essay. 2) How does, or is it possible, for depression masquerading as illness to actually develop into a true illness? I suspect that many illnesses, especially autoimmune, include depression in their etiology. 3) What are the biochemical bases of the depressive changes described? Are there biochemical differences between depressive behaviors in the agonistic and hedonic modes? I ask this question because the author alludes to neurochemical changes. To take this question even further, are there individual differences based on genetic makeup which lead one person into physical illness behavior and another to remain as depressed within the hedonic mode? Individual susceptibilities are being found to be significant in many physical diseases such as cancer as well as classic examples such as hemophilia.

1. Fabrega H (1974) Cultural influences in depression. In (Eds) EJ Anthony, T Benedek: Depression and Human Existence Boston: Little Brown, p68
2. For ASCAP Vol 3 (Jan through Dec, 1990) please send \$18 (US dollars) for the 12 issues. Make checks or money orders out to "Department of Psychiatry and Behavioral Sciences, UTMB"
3. ASCAP philosophy and goal. High scientific importance rests on comparing animal behaviors across-species to understand better human behavior, knowing as we do so that evolutionary factors must be considered for understanding properly such behaviors. To accomplish these comparisons, very different new ways of viewing psychological and behavioral phenomena are required. This in turn explains why we need new words to define and illustrate new dimensions of comparisons across species. We expect that work in natural history biology combined with cellular-molecular biologic research will emerge as a comprehensive biologic basic science of psychiatry. Both top-down and bottom-up analyses are needed. Indeed, this must happen if we are to explain psychiatric illnesses as deviations from normal processes, something not possible now. Compare to pathogenesis in diseases of internal medicine.
4. Dr TD Kemper, Dept of Sociology, St Johns University, Jamaica, NY 11439, USA.
5. Alexander R (1987) The Biology of Moral Systems Aldine de Gruyter
6. a. Caro RA (1982) The Years of Lyndon Johnson: The Path to Power NY: Alfred A Knopf
b. Caro RA (1990) The Years of Lyndon Johnson: Means of Ascent NY: Alfred A Knopf
7. de Waal F (1982, 1989) Chimpanzee Politics: Power and Sex among Apes Baltimore: Johns Hopkins Press
8. a. Sorensen G, Randrup A. Possible protective value of severe psychopathology against lethal effects of an unfavourable milieu. Stress Medicine 1986;2:103-105. (Ref in ASCAP Vol 3#5, May 1990.)
b. Overvad E, Bom F: Om psykogen dod hos primitive og civiliseroede mennesker. Nordisk Psykiatrisk Medlemsblad 1957;11:11-20.
c. Carter N: Effects of psycho-physiological stress on captive dolphins. Int.J.Stud.Anim.Prob. 1982;3:193-198.
d. v Hoist D: Renal failure as the cause of death in Tupaia belangori exposed to persistent social stress. J. comp.Physiol. 1972;78:236-273.
e. v Hoist D: Coping behaviour and stress physiology in male tree shrews (Tupaia belangeri). Fortschritte der Zoologie 1985;31:461-470.
f. Bettelheim B: Le Quotidien du Medecin (Paris) March 6, 1990.
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10. Samuels A, Sil JB, Altmann J: Continuity and Change in Dominance Relations Among Female Baboons. Animal Behavior 1987;35:785-793.