For evolutionary psychiatrists, psychologists and others interested in sociophysiological Integration

ASCAP

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"It is sobering to be reminded how readily in matters of life and death we relied, until so recently, on the personal judgment of fallible but often highly opinionated physicians. It should be of some comfort that today we are protected by rigorous clinical trials."

Ray Porter[1]

Across Species Comparison 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.

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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 millionsofyearsandthathaveresultedin 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.

Contents

Don't forget to mail in yourduesfor1996!

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

ADDRESSED TO & FROM ...

SPONSORSHIP & SOCIOPHYSIOLOGY

Jim Eaton from Washington, D.C. is a clinical psychiatrist in private practice who has an interest in ASCAP matters though to this point he hasn't subscribed. But we meet regularly in the course of our business and he tells me of having owned basenjis, for instance, and provided detailed accounts of their interactions with other dogs. He knows both psychopharmacology and psychotherapy issues. He trained at Tulane with Robert Heath as one of his teachers. Dr. Heath is one of the true originators of the socio-physiologicai theme in psychiatry though he hasn't called it that. (After our discussion, however, I resolved to contact Dr. Heath and attempt his support of *The ASCAP Newsletter.)* But I digress.

As you-all know by now, I am attempting even more than before to drum up *ASCAP* business so I gave some recent issues to Jim in the attempt to extract from him a charitable contribution even if he didn't read the issues received after sending in his $35. He read the ones provided, however, and responded with interest. He had not previously known the story of Eli Robins's encounter with psychoanalysis although he had known Eli Robins and had experienced the repercussive power of what happened decades ago in Boston. We discussed how the vividness of the story caused us to recall the related matters with greater power. Minutes later I encountered an article in the 28 Sep 1995 issue of *Nature* which presented data on memory processes. A patient with a condition called Urbach-Wiethe disease was presented with a learning task. This is an extremely rare hereditary condition in which the *amygdala* on both sides is absent. Memory expert Larry Cahill and his colleagues tested the person named BP, who had the condition, but also normal intelligence and intact short term memory.[1] They used a story they had used with many other people with normal amygdalar structures on both sides. The story involved several phases of how a boy went with his mother to visit his father at work. Most people remember one phase of the story best, however, as it involves the emotionally compelling depiction of a terrible car accident in which the boy is injured; graphic pictures were shown. BP *said* he emotionally reacted, rating it even slightly higher than other people did on average. But he did *not* show the usual increase in memory for the events associated with the accident.

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LEADER CHILDREN

Here are some thoughts engendered by Russell Gardner's description of Sarah and Miss Minchin in *ASCAP* (October, 1995).

Sara's confidence is echoed in the description of leader type children studied both by Vernon Reynolds and by Hubert Montagner and which you have already published in *ASCAP* where I presented the evidence for a Socio-Mental Bimodality (August, 1994, issue).

Note how opponents of hedonic individuals, i.e. people who try to dominate them, are acutely aware of the hedonic person's backer. Thus Miss Minchin became more dominating when she realized that Sara's father had died in the war.

I'm preparing a note on the relationship of language, cerebral laterally and the two modes, which bear on all this. Meanwhile I hope to hear soon what proposals you adopt for coping with "the storm in the Gulf of Mexico". Good luck.

Michael Chance
Birmingham, ENGLAND

The ASCAP Newsletter welcomes contributions. Please E-mail to ascap@beach.utmb.edu. or mail hard copy and 3.5" HD diskette to Russell Gardner, Jr., c/o Linda Crouch, Dept of Psychiatry & Behavioral Sciences, University of Texas Medical Branch, Galveston TX 77555-0428, USA. WordPerfect, Microsoft Word or ASCII format preferred. Diskettes will be returned to you. Thank you.

ARTICLE:

by L Sloman

Response to Stevens, Price & Gardner

I am writing in response to the recent thought-provoking articles by David Stevens on *The Essential Nature of Bimodal Theory*[1] John Price on *Agonistic versus Prestige Competition,*[2] and Russell Gardner on *Two Modes in Two Narratives.* The distinction between hedonic and agonic interactions, based on a comparison between chimpanzee and baboon societies is intriguing. However, it raised some questions which I would like to share.

When I tried to apply the above model to children and families, I felt that it had only limited relevance to most of the cases that I was seeing. I thought this might be a result of my lack of understanding of the model. However, I wondered whether the parallels drawn between human "agonic" and agonistic interactions in baboon colonies might create a misconception. When baboons in the wild interact, I presume that their agonistic mechanisms generally function reasonably efficiently. However, many of David's and Russells' descriptions of "agonic" behaviours refer to agonistic mechanisms that were operating in a dysfunctional manner. That is they were fixed and rigid as opposed to functional agonistic mechanisms that are flexible and efficient and generally serve to bring agonistic interactions to an end by leading to acceptance and submission.

When I see families I pay particular attention to how the family handles agonistic interactions. When I observe bullying and victim interactions, I consider this to be an example of dysfunctional agonistic interaction. In victimiser-victim interactions, submission is not effective, because the victimiser continues to escalate. The victim cannot accept his/her unacceptable situation. If he or she did, he/she would not be a victim. I also view bullying behaviour as dysfunctional agonism, because it arises out of insecurity rather than true dominance.

Stevens says "psychopathology is a derivation of Agonistic Relations." It would be a truism to say that the presence of psychopathology is a reflection of mechanisms that have become dysfunctional. It would be relevant to ask why agonistic mechanisms become dysfunctional. When individuals have received adequate care and loving, their agonistic intermechanisms are more likely to function efficiently. When the parents' mechanisms function efficiently, the parents are more likely to be caring and supportive. Children need parents who have sufficient self-confidence to provide both calmness and empathy and also to set adequate limits.

If psychopathology is, as Stevens says, derived from "Agonistic Relations," it might be concluded that high competitiveness is pathogenic. However, the important issue may not be whether the culture fosters competitivity but whether there is a failure to accept realistic limitations. In competitive families, problems are more likely to arise when the parents' aspirations for the child and the childrens' aspirations for themselves are unrealistically high. When families foster hedonic interactions, this too can cause problems by creating difficulty in separation and individuation for the adolescent members of the family.

In practice, the issue may not be so much a case of whether the culture is hedonic or agonic, but whether the child receives contradictory or unclear messages so that the child cannot develop a clear sense of direction. Symptomatic behaviour in a child is more likely when the parents cannot be assertive enough to give their children clear messages about what is expected from them. It is also more likely when parents cannot resolve their differences through negotiation. The problem is not that the parents are too competitive. It is that when one point of view prevails, the other parent cannot accept the situation and therefore continues the struggle by trying to undermine the opposite parent. This would be another example of dysfunctional agonistic interaction.

In support of the bimodal theory there is evidence that children brought up in democratic families do better in this culture than those brought up in autocratic families. However, intergenerational conflicts will be intensified if immigrants with an autocratic tradition raise children in a country with a democratic culture.

David Stevens speaks of competition for control as characteristic of the agonic mode. My observation would be that a need for control is a characteristic feature of those who are, in general, insecurely attached, and this, in turn, contributes to dysfunctional agonistic behaviour. For example, Troy and Stroufels have shown that children who are insecurely attached are more likely to become victims or bullies than those who are securely attached.[4] Coming back to David Stevens' claim that psychopathology is derived from agonistic behaviour, I would argue that psychopathology is derived from insecure attachments to one's main attachment figures, and from rigid agonistic mechanisms.

John Price proposes that competition can exist in the hedonic mode as in competition for positive regard or approval in contrast to competition for control. John demonstrates that love and aggression can be closely intertwined. I believe that there can be many variations on this theme. For example, sibling rivalry can be very playful. However, sibling rivalry can become so intense that it could both be an indication of family dysfunction and also a cause of further family dysfunction. If one views sibling rivalry as a competition for love, then hedonic competition could also be very pathological. I wonder whether the distinction between hedonic and agonic competition is, in practice, always clear-cut. Another example of the overlap would be that some women find that the possession of power is an attractive trait in a man. In this case agonistic success could also therefore contribute to hedonic success.

If any of my comments have been based on a misunderstanding of bimodal theory, I hope you will set me straight. To conclude, as ASCAP members, we all have in common an interest in delineating the genetically based mechanisms of primitive origin that shape our behaviour. When we observe these mechanisms operating in an efficient and effective manner it might behoove us to marvel at their elegance and simplicity.

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ARTICLE:

by R Gardner

Daniel G. Freedman Festschrift Report

Daniel G and his celebration. After heading the Human Development Program at the University of Chicago for two decades, Daniel G. Freedman has retired to New Mexico. But some of his former students hosted a celebratory gathering to honor his work and mentorship on October 27-29, 1995. The name of the occasion was "Genetic, Ethological, and Evolutionary Perspectives on Human Development." The organizers were Nancy L. Segal, Glenn E. Weisfeld, and Carol C. Weisfeld all of whom had obtained their Ph.D.s under his guidance. Another Weisfeld, Mimi, a budding artist, drew the excellent picture of DGF that graced the program cover. The location was Ida Noyes Hall at the University of Chicago.

While the proceedings will eventually be published, I thought that I would provide a summary, not only of the formal proceedings, but other things that I learned along the way, especially since they were in line with the sociophysiological themes of *The ASCAP Newsletter* (I was also prodded a bit by Nancy Segal). The organizing former students are ASCAPians as indeed DGF himself is. How good to see in the biographical note in the program that the most recent article he has published was in *ASCAP,* Volume 8, earlier this year.

At a coffee break, the honoree noticed my attendance with pleased surprise. I am not a former student (at least formally) as I had graduated from the University of Chicago two years before he arrived. Moreover, I had been in the medical school. But in his characteristic manner, he extended his warm greetings and I felt something of why his former students hold him in such high esteem. I found myself telling him of some of our *ASCAP* woes and need for a membership drive. With quiet alacrity he zeroed in on a solution that hadn't yet occurred to me nor to the other brainstormers to that point: obtain the mailing lists of sibling publications and organizations and send each subscriber/member a copy. He suggested that a certain number of such recipients will be surely hooked, as he was. He himself reads each issue with interest. This dovetailed with Nancy Segal, who had beforehand E-mailed that I bring a stack of issues for the display table. When Linda Mealey heard that I had inadvertently left them at my house (they got only from office to home), she suggested that I nevertheless put a notice about the newsletter on the table - which I did with takers, people subscribing for 1996.

I tell you here some summaries of the stories personal and scientific that the various people there told. There were many personal ones about Dan himself, of course. One unfolded before our eyes. A brief horn concert got the proceedings underway on Friday night. Afterwards he talked of how the horns felt to him, including an allusion to a lugubrious bass trombone that caused many of us to laugh a little, although timorously, given that a dean had just spoken. Also, a few of us didn't understand why the horns were there and thus felt perhaps we shouldn't laugh as we meant no disrespect. After all, they were playing Brahms or, as DGF himself explained, they played a *cartoon* of Brahms. The little performance and his reaction to it seemed prototypic Dan Freedman: ceremonial mixed with the informal, appreciative, nuanced, not too serious, yet serious enough, thoughtful and feeling-filled too. He made masculine and feminine behaviors topics of discussion, not matters taken for granted. Humor was expressed but not at anybody's expense. Rather he put into action a subtle example of the bonding function of the powerful human signal of laughter.

He also spoke in this little preamble to the conference of how his travel to China showed him the power of psychological ideas. When people there learned he was a psychologist, they asked him about Abraham Maslow. Maslow it turns out was the most apolitical of academics - he had been enraged at his daughter's participation with the U.S. civil rights movement. But he has provided with his ideas a major impetus to Tianamen Square. His discussion of self-realization as a goal in life meant that frustrated Chinese have felt a legitimization of this human aim otherwise minimized and squelched in their authoritarian state.

Evolutionary biology and behavioral genetics. Then scientifically in the lead off talk, J. Michael Bailey brought forth a major critical point, how *do* evolutionary theory and behavioral genetics dovetail? His answer was that they don't much, at least yet. Evolutionists interested in biological adaptive programs that pertain to all in a species have trouble relating to genetic studies that essentially address individual differences. (Let me also note that this relationship between the two fields has been a recent theme in the electronic discussions of the HBES list; Nancy Segal's contribution to that thread referred to Bailey's talk).

Bailey reviewed Freedman's dog stories with their evidence of gene-environment interactions. He also told us with humor his Bailey's law of heritability, that $h^2 = .40 \pm .20$. All studies of heritability, that is, show results between 0.2 and 0.6! He also illustrated that group-wide generalizations, while true, may conceal great variability amongst the individuals within them. David Buss, for example, has shown that cultures tend to be highly similar with respect to the traits that males and females value, which biological sex differences may be valuable as adaptive biases. While ratings by Bailey's own students reflected the Buss findings, they *also,* however, showed many of the two sexes rated in the opposite directions.

Bailey seemed pessimistic about the cross-fertilization of the two domains of behavioral genetics and evolutionary biology. As I listened to him, I reflected on a just read article about domains in evolutionary biology of a very different kind. Now the other exposition seemed very relevant to the problem of how can the two fields eventually relate. It was an article on "The multiplicity of domains in proteins."[1] People working in Bailey's two areas are not investigators who work directly with the mechanics of the genome's DNA nor with what different genes actually make, promote, or modify. In fact these bottom (as in bottom-up) or down (as in top-down) concerns seem far from the outputs of behavior that both of the two fields involve (mostly top or up).

Dr. Doolittle's continent. The bottom-down author of this article, Russell Doolittle, nevertheless made me think of another Dr. Doolittle of whom I read when a child: that English Dr. D. was a savant who knew how to talk to animals and made great use of this in his travels to Africa. He knew how to communicate between groups that ordinarily didn't have a common lingo. I believe biochemistry will eventually mediate an ultimate communality. The current Dr. Doolittle mentioned that "The domainal nature of proteins is well established" which meant that the proteins have unique and stable structures. Sometimes these rely on their amino acid sequence but sometimes on their three dimensional configuration (with amino acids substituted so long as the structure remains the same *via* Darwin natural selection). Both are biochemical versions of basic plans. Basic plans are components that stay the same in organisms, but are not generally interesting to geneticists who are defined as involved with differences rather than similarities. But many geneticists now are intensely interested in the chemical basis of such differences so Dr. Doolittle may have a role.

Russell Doolittle described extracellular mosaic proteins in animals, concluding colorfully that "many are involved with cell recruitment, others in protein gathering or the assembling of other extracellular aggregates.... aggregates seem to operate as a kind of macromolecular wolf pack, waiting in swarms for the inevitable microbial attack." Dr. Doolittle's domains were continents away from what Bailey and the other evolutionary biologists at the conference discussed, but he provides some of the language of cells and molecules that will eventually, I believe, allow the two fields to evolve a *lingua franca.*

Genes have conflicts of interest. Preoccupied with this end of things as I was, I had been pleased upon preliminarily reading the program, that Robert Trivers was to present, as I know that he is highly familiar with such molecular details. In the opening sequences of the conference, his identity became clear as the eager participant wearing a brimless tribal hat who enthusiastically facilitated the proceedings at times, but at other times behaved non-traditionally. He abruptly left the room, for instance, even as Dan spoke, and Dan alluded to it, patiently explaining to the group that Robert was sensitive to some issues such as whatever he was talking about at the time. As he did so, we felt DGF's central themes even more: always gentle, but also direct, honest, not ducking issues. Dan noticed things, did not ignore the interesting if quirky behaviors of this important figure in evolutionary biology who was certainly himself displaying Trivers-specific individuality and creativity. I had heard Trivers before and appreciate his iconoclastic approach. I felt that here is someone who understands the continent of Dr. Doolittle. He said at some point, for instance, "Genetics without biology is a bad idea."

Trivers, of course, is someone who had made the powerful inclusive fitness predictions that there are selfish gene conflicts of interest with predictable behavioral consequences, as between siblings and parents and between the different sexes. Now he works with biochemical gene realities as well, articulating the bottom-up significance of new findings. In the Toronto meeting of HBES (1994), he had discussed Dean Harrier's NIMH data supporting a possible homosexuality gene on the X chromosome and how this could be understood as a hypersexuality-aimed-at-males strategy independent of the sex of the current carrier of the gene.[2] It may misfire in homosexuals in that they reproduce less. But potential reproduction may increase in females with the same hypersexuality-aimed-at-males genes. Women moreover with their two X chromosomes would have more chances to express such genes.

Tired at some later point in the conference and deciding to not watch some late afternoon videotapes, I put on my overcoat to go when Dr. Trivers saw me make the preparation. He had been napping beneath the display table, but like me at that point really wanted to leave. I knew the way back to the motel so we walked together, gaining acquaintance and discovering a common interest in the chromosome 15q12 deletion syndromes of Prader-Willi (PWS) and Angelman (AS). These exemplify genomic imprinting. PWS patients miss their father's genome component for a key segment of 15q12. AS patients don't have their mother's.

Trivers noted to both me and the audience at Ida Noyes the next day that David Haig, the Harvard scholar who has made great strides using Triver's selfish genes extrapolations, feels that genomic or parental imprinting ranks in scientific importance only under Hamilton's inclusive fitness. Selfish genes operate on the molecular level, with mother's and father's genes in standoffs ordinarily, both contributing and neither overmuch winning, as with people who don't have either syndrome (most of us). Significantly, without their mother's contribution, the AS patients are much worse off! When the father's genes for this area are missing, one has an insatiable appetite; when mother's are gone, one never learns to speak and laughs all the time!

When he inquired about why I have gotten interested, I confessed my interests have been less on this level of theory, but rather on the actual missing genes, their behavioral consequences, and the mechanisms that lie in between! For instance, imaging research on the AS brain shows likelihood that certain usual neuronal migrations and perhaps normal cell death (apoptosis) probably doesn't take place. Thus, genomic accidents of nature interact with behavioral functions but one must be careful about extrapolations made too easily.

Triver's enthusiasm, information and creativity were captivating for me on that nice walk in Chicago's Hyde Park. Walking in that area was also nostalgic for me because there many years ago, for a cut in my rent, was where every morning I had walked my landlord's Mr. Chips, an Irish setter. I then mused about what that dog was thinking many years after reading the original Dr. Doolittle and as yet not acquainted with Charles Darwin's *Expression of the Emotions* then less than 100 years old, nor with John Paul Scott's and Daniel G. Freedman's studies ongoing even then at Bar Harbour.

Dr. Trivers and I became friendly on this day for an additional reason. We discovered we had the same reaction to an offensive mocking attitude of a fellow conference attendee, displayed separately to each of us. We felt bonded as fellow targets of this person's use of the powerful *other* facet of laughter as a human signal. Dan G. skillfully used it to bond the group. But the other person used humor with both of us as a catathetic social tactic that put us in our place, by implication distinctly less than his. It felt restoratives to speak about it to someone with similar experience.

Also, I dimly felt Dr. Trivers to be the person at the conference closest to speaking the integrating mystery language of the Drs. Doolittle for which I felt us all to be searching. And he has stayed fluent on the up levels of the discourse. He was proud, for instance, in a later response to discussion, that his concept of parental investment has held up. Parental care clearly and unequivocally helps the young and would be good to foster in these troubled times of family dissolution. He also told me on our walk that he is planning longitudinal research on developmental features of human attractiveness as indicated by bilateral symmetry.

In the later talk in the lecture hall, he noted that paternally active genes would be predicted to foster 'look out for the self' behaviors whereas those maternally active will foster group cooperation. Offspring are probably more maternally active in some of their body parts and more paternally active in others. To investigate this, chimeric mice have been formed by putting two paternal, or two maternal, gametes in the same eggs. While neither variant survives to parturition, they do survive for some time and their different developmental features can be examined. Results show the skeleton and hypothalamus are more evident in the paternal-paternal chimeras and the neocortex in those with the maternal-maternal combination.

This loosely fits with my own hypothesis that language (surely a cortical function) probably evolved in good part from mother-child patter and story-telling. How interesting that the non-speaking Angelman Syndrome patient has unopposed paternal genes on 15q12. Laughter here possibly a paternal contribution can be either bonding or alienating. How will such observations eventually bear fruit in the ever weirder story of parentally imprinted genes and their by-products? I learned subsequently that gelastic epilepsy (laughing seizures) stems from hemartomas of the thalamus also implying that something is disregulated in the diencephalon to produce this behavior.

Subsequent to the conference, David Haig's ideas have been featured in the December issue of *Scientific American*[3] with nice attribution to Trivers as well, based largely upon Haig's recent article in the *Quarterly Review of Biology.* Haig describes how maternal and paternal genes war in the fetus. Moreover, perhaps, the pre-eclampsia of pregnancy is a paternal strategy to get more investment for the baby from the mother! Read it and enjoy. In the meantime, back to the formal proceedings of the Festschrift.

Back to Ida Noyes Hall. The schizophrenia geneticist Irving Gottesman spoke on a developmental-genetic perspective on aggression. From the display table, I purchased his 1991 book, *Schizophrenia Genesis: The Origins of Madness.* It summarizes landmark research better than any other source I know.

John Paul Scott — whom Robert Trivers described as a living legend -- described the pioneering work he did decades ago on agonistic behavior in dogs including their critical developmental period for connecting with people.[4] He concluded, "Behavior is never inherited as such, but is developed under the influence of genetic and environmental stimuli." He mentioned the University of Chicago researcher, W.C. Allee, who brought Schjelderuppe-Ebbe's work on social rank order into scientific respectability. Allee showed that agonistic behaviors lead to social organization in the vertebrates. Scott noted humans are the sexiest of animals (rivaled only by the bonobos). Most importantly he concluded that social organization diminishes violence and that the current spate of violent crime likely stems directly from the childhood disorganized home life of young men now 16 to 25 years of age.

Nancy Segal noted her recent work on singleton grief, that is, the sadness felt by a twin when the other is lost. She found the intensity is greatest for the monozygotic twin. She is now accumulating pairs of non-genetically connected people who are raised together as the best control for twin effects. For instance, she found in 106 such pairs that the interpersonal correlation coefficient for IQ is much lower than for either monozygotic or dizygotic twins. Specifically, the results she showed are full IQ $r = .17$, verbal IQ $r = -.01$, and performance IQ $r = .29$. Also likeness converges overtime; older such pairs are more alike than when younger. DGF in discussion noted with much interest the greater congruence of performance versus verbal measures.

Robert Marvin summarized across-cultural modes of child-parental contact, documenting that toddler closeness to parents correlates to dangerousness of the environment. Dangerousness also means toddlers explore less. In current work with cerebral palsied children whose motility is limited, he showed how the mother becomes the child's exploratory surrogate. He also noted that 31 % of 36 such handicapped children went into trance when the mother wasn't present. He suggested that this may be the same as the clinical symptom of dissociation, but that for these children it may serve a normal protective function. Roumanian orphans adopt the strategy of indiscriminant friendliness. In non-orphans, however, mothers foster children's wariness of strangers.

Ritch Savin-Williams from Cornell University described classroom data on sexual minorities. He teaches a class on this topic that many humanities and social science majors attend so that scientists as evil entities come up frequently. Both the radial right and left inveigh against scientific work on these loaded issues.

Bostonian Peter Wolff described work on the flushing response to alcohol in Asians which seems to be mediated by histamine and is caused by not having an enzyme that in most people detoxifies the aldehyde by-product of alcohol. Not that the flush inhibits the occurrence of alcoholism: he presented data that with the advent of U.S. soldiers in Thailand, the disorder skyrocketed amongst the natives. He noted that Darwin showed that the face and neck are the sites of emotional blushing and that the same area is affected by the Asian alcohol response.

Nicholas Blurton-Jones showed data on how "children have their own agenda." He noted that Darwinian psychologists may overmuch ignore variability within populations and moreover that variability may in itself be adaptive. He showed that in Africa Hadze children forage more in safer environments than do !Kung children in their more dangerous surroundings. The research measured how much food was gathered by whom and what happened to it. Girls found safer unexciting food and shared it more than did boys. The young males played at hunting or capturing honey with less overall success but with payoffs in reputation that correlated with later numbers of wives and children.

Paul Ekman noted that Daniel G. Freedman was influenced (as he himself had been) by Gregory Bateson. He quoted Simpson's comment that language is the most diagnostic trait of humans. He summarized some of his own work by stating that though many things cannot be communicated by the face, there is good evidence that one can often tell whether an expressor is lying. He is editor of a new book, *What the Face Reveals,* due out next year, which summarizes amongst many things new work on the important Asperger syndrome. As a tip for poker games, he noted in discussion that gamblers watch not the face of their opponents but the hand-card behaviors of fellow gamblers.

I found myself caught up in informal discussions outside the lecture hall as Jerome Barkow began to speak on human happiness and so regrettably did not grasp his central points, but in what I did hear, he approvingly mentioned ASCAPian Tim Miller's book, *How to Have What You Want.*[5]

Glenn Weisfeld discussed emotions. He noted that psychology as a field skipped a descriptive phase typical of other sciences. Paying special attention to pride and shame, he used them to exemplify a number of points he made in the form of a series of assertions:

- Emotions are important
- Emotions are universal
- Few, if any emotions, are unique to humans
- Each emotion has its own affect
- All emotions are biologic
- Emotions have identifiable biological functions
- Reason subserves emotion: the limbic system increased in size even as the neocortex did
- Ontogeny (usually) recapitulates phylogeny
- Emotions are elicited by prepotent and learned stimuli

Frank Lloyd Wright Country. The University of Chicago is located in a part of the city that possesses Frank Lloyd Wright houses. One, the Robie House is immediately on the campus and famously typifies the prototypic prairie style of the mature architect. The Heller House of an earlier vintage happens to be where I roomed for the first two years of medical school. The owners then owned as well the Irish Setter named Mr. Chips that I walked each morning. (Moreover, I realize even as I write this that they had an adopted daughter and a natural bom son within months of each other's ages, people whom Nancy Segal will want to know. As a side note to her, I know whom to contact for more information, a slight payoff for having organized a superb conference!) I walked by the Heller House and am pleased to report that it looks in fine shape, distinctively reflective of the Wright style.

Daniel G. Freedman, his teacher John Paul Scott, and many colleagues gathered at Ida Noyes Hall have pioneered the convergence of fields. They examine human behavior with the unblinking knowledge that there is nothing unbiological about it. Concern about genetics (individual differences and now within-individual gene competitions too), adaptation, human communication, and development have held center ground at the unique University of Chicago program. There was nothing of the disembodied psyche haunting the names of psychology and psychiatry in the way that the Department of Human Development carried out its affairs. The gentle intellectually forward approach of Freedman now gives all of us something of the design, of the right style, of how this scientific house should be built.

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ARTICLE:

by J Pearce, H Caton & K Browne

Excerpts from E-mail on killing

From John Pearce (jkp@world.std.com):
On Killing: The Psychological Cost of Learning to Kill in War and Society, by Dave Grossman, Little Brown 1995, is a must read. Amazing that nothing like this has been done before.

1. People don't like to kill people, and they like it less and less as they get closer and closer and face to face (except the lethal 2% who like to kill).
2. People don't like to stick bayonets in people or be stuck. They would rather swing rifles as clubs.
3. People who are running away are another matter-the inhibition to kill lifts. (One is reminded of the man-eating tigers of the south India salt water swamps where forest workers wear hats with face masks on the back of their heads. Tigers don't like to attack in front.)
4. People will kill when those in authority says they must. Those in authority suffer terribly from having taken the role of causing the deaths of their soldiers.
5. People and other animals will kill when in groups but if a military unit is 50% killed off they become increasingly unwilling.
6. The face-to-face unit, and loyalty to the unit, is fundamental to getting people to kill. They do not want to let each other down.

This book is a great guide to evolved human predilections as unmasked by that most desperate of human conditions, warfare. The author is not part of our intellectual community (makes group-selection assumptions, etc.) but he ought to be.

This book is a find!

From Hiram Caton (h.caton@hum.gu.edu.au):
I've ordered Dave Grossman's On *Killing* on John Pearce's recommendation. It hasn't arrived, so I know it only through John's remarks. Since this is a subject that I've examined closely, I'd like to respond to John's precis of Grossman, maybe to instigate further discussion.

"2. People don't like to stick bayonets in people or be stuck." Change "people" to "young males recruited from the working class" and you've got a good approximation to experience of about 4 centuries (the bayonet was introduced in the late 17th century). Even the Scottish working class males, who will hack English soldiers to pieces with his Claymore sword, shy from the bayonet. The Japanese infantry training put a premium on the bayonet because of the terror it holds from the enemy AND for own troops. The Marines are the only US troops ever to receive serious training in the bayonet, and it was never a favored weapon. Its use was phased out of Marine basic training some time ago.

"1. People don't like to kill people, and they like it less and less as they get closer and closer and face to face (except the lethal 2% who like to kill)." As a generalization, this is useless. There is a facultative adaptation to killing. When it is triggered, young males set to it with a feeling of the sublime. There is abundant anecdotal testimony to this feeling of sacredness in killing, diachronic and cross-cultural. British soccer, American football, and Canadian hockey are three arenas where this adaptation goes right to the point of ejaculation, so to speak. How about females? The evidence comes from community violence, such as revolutions, vendettas, vigilante attacks, public executions. It shows that females also share this adaption, although it configures differently in them. Mostly they cheer the killings by their men (in blood-curdling language), but some are natural-born killers. Like men, they will attack men, women, and children.

To trigger the facultative killing frenzy, the killing group (or individual) has to be suitably "framed" (or in popular idiom, psyched up). Frames are, in most cases, rituals that prepare for the action to follow. Military rituals exemplify; so do the rituals of public executions. See Desmond Morris on the rituals of the soccer tribe.

Grossman is right that there is a powerful inhibition on killing; and right again that proximity to the potential victim triggers a proximate inhibition. There is an interesting experiment going on right now in lifting this inhibition. You will find it among bioethicists advocating euthanasia. Essentially they are conducting a self-indoctrination that enables the mercy killer to see the other with a label reading dead for their own good. None of the bioethicists known to me address the psychology of this type of killing.

"4. People will kill when those in authority says they must." Not a useful statement until the psychosocial content of "authority" is spelled out; and until individual variation is taken into account. Drive-by killing, vigilante killings, etc. occur in defiance of authority. On the other hand, there are many examples of military units that kill efficiently despite holding authority in contempt. This happened on a large scale in World War I.

"Those in authority suffer terribly from having taken the role of causing the deaths of their soldiers." If military commanders are meant, it applies to very few. It is part of the military character to be indifferent to own's one's own death. It is part of military leadership to exhibit to the men one's own fearlessness, because it inspires trust in the men.

"6. *The face-to- face unit, and loyalty to the unit, is fundamental to getting people to kill. They do not want to let each other down."* This is very true.

*"The author is not part of our intellectual community (makes group-selection assumptions, etc.) but he ought to b*e."OUCH! ARE GROUP SELECTIONISTS STIGMATIZED FOLK?.

As I understand it, serious human killing is always by groups of young males, whose association in the warrior/hunting band is based on facultative eusociality. The attractive power of this bond (is it a chaotic attractor?) is such that they constantly stimulate one another to risk-taking behavior (e.g., playing chicken in cars) just to exhibit this curious mingling of emulation and mutual threat. In the *Descent of Man,* Darwin discussed this behavior and thought it a sufficient basis to attribute group selection in the evolution of the human moral sense.

Thoughts, anyone?

From Kingsley Browne (kbrowne@novell.law.au.wayne.edu):
On the point that people do not like to kill, especially up close:

It is noted in Connolly & Anderson's book First Contact that the Highland natives of New Guinea (who killed with axes and spears) were perplexed by the Australians' (who killed with guns) reluctance to kill (and their feelings of anguish when they did kill).[1]

Again from Hiram Caton (h.caton@hum.gu.edu.au):
Kingsley Browne wrote: *"On the point that people do not like to kill, especially up close:*

It is noted in Connolly & Anderson's book First Contact that the Highland natives of New Guinea (who killed with axes and spears) were perplexed by the Australians' (who killed with guns) reluctance to kill' (and their feelings of anguish when they did kill)."

The Australians who made contact with PNG highlanders in the Twenties were a combination of civil servants, anthropologists/geologists and like professionals, and free booters scouting for resources for export. Definitely not the killing type. However, the Australians who met the Japanese in war were of the opposite disposition; young working class males officered by men disciplined to the British officer tradition. The part of that tradition taken from colonial experience is that unarmed civilians may be shot in cold blood to deter them from arming and rioting.

ARTICLE: Hanky panky in the pharmaceutical industry

by S Fisher

The following represents a widening of ASCAP's never narrow focus. Sy Fisher published his thoughtful piece on E-mail in three installments aimed mostly, I believe, on prescribing physicians. But I was interested for ASCAP because it represents a high position on top-down bottom-up gradation of understanding biological systems: human-specific, group oriented, concerned with subtle influences of people working for drug companies on the behavior of other people who give drugs that alter the way that people called patients think and feel. What happens to prescribers through influences on them is hardly unbiological behavior. Deception and its variants are hardly unbiological. Charles V. Ford has recently written soon-to-be-published *Lies! Lies! Lies!* which describes the varieties of human deception.[1]

So that's my focus. Read Sy Fisher's piece for whatever value it has for you but among your various thoughts consider the intricacies of how we operate. And if stimulated, write a reaction.

Russell Gardner, Jr.
Editor

July 23, 1995

Does the pharmaceutical industry want clinicians and patients to learn more about possible side effects of newly marketed drugs? I think the time has come to make public just one egregious example of how individual drug companies can influence the publication of clinical research results that are not in their best financial interests. (I also have documented instances of how insidiously the pharmaceutical industry can influence publication of other manuscripts and even NIH support of research projects dealing with adverse drug reactions of newly marketed drugs. But that's another story for, perhaps, another time.)

First, however, for those who do not know me, I'd like to point out that I have nothing to gain personally by going public with this issue. I'm just about 70 years of age, and my academic credentials and career don't need any embellishing (a brief resume can be found in *Who's Who in America).*

Next, I urge you to read the article on "Postmarketing Surveillance by Patient Self-Monitoring: Preliminary Data for Sertraline versus Fluoxetine" in the July issue of the *Journal of Clinical Psychiatry* (1995;56:288-296). This paper is based on large-scale data indicating that many adverse reactions known to be induced by fluoxetine (Prozac) were being reported with even greater frequency by sertraline (Zoloft) patients; the tables also include suggestions to the clinician for age and gender patient types most at risk. Zoloft is manufactured by Pfizer Incorporated (Roerig Division).

The manuscript was accepted for publication on May 12, 1994. On December 8, 1994 the Editor wrote me to say that he had become *"concerned that our largely clinician readership might interpret the results more literally than our investigator colleagues. This apprehension led me to draft the accompanying commentary, which I would like to publish along with your article."* Although none of the Journal's three reviewers who had originally recommended publication voiced this apprehension, the Editor's proposed commentary was entitled "What will this drug do to me, doctor?", and tacitly implied that our results and conclusions might be spurious.

I replied to this letter on December 20, showing that most of the substantive criticisms he raised in his proposed commentary were simply not valid, suggesting instead that the research results along with the article's carefully qualified discussion of the results should be able to speak for themselves.

Letter from the Editor dated December 30: "I have revised and (I hope you will agree) 'softened' some of my comments. I hope you will be more comfortable with the current draft." His revised commentary included sentences such as *"It would be simplistic and premature, however, to treat this report as gospel and conclude that in reality sertraline produces a higher frequency of unwanted reactions than does fluoxetine."* And the final paragraph was to be: *"The report by Fisher et al. is thought-provoking and can frame hypotheses for additional testing. The actual incidence of side effects of these two SSRIs will become clearer with time and additional study."* (Similar caveats were actually included in the discussion section of the article, but without the pejorative flavor of the proposed Editorial.)

By February of 1995, when we had not yet received page proof nine months after acceptance of the article, I phoned the editorial office for information. I was told it was scheduled for the May issue. However, in April when we had still not received either page or galley proof, and when a follow-up phone call elicited the information that the publication date was now postponed until July, I undertook a quickie "research project." This led to a letter I wrote to the Editor on May 1, in which I expressed the view that publication of his proposed Commentary would be grossly unfair unless I was also given the opportunity to respond to the Editorial. What follows was my proposed rebuttal:

COMMENTARY ON
"What will this drug do to me, doctor?"
In this issue, an article by Fisher et al. presents data from more than 2,700 fluoxetine and sertraline patients using a well-validated postmarketing surveillance method developed to signal possible adverse drug reactions (ADRs).[1] *The preliminary results indicated that many adverse reactions known to be induced by fluoxetine were being reported with substantially greater frequency by sertraline patients. The article is accompanied by an Editorial Commentary, admonishing readers not to "conclude that in reality sertraline produces a higher frequency of unwanted reactions than does fluoxetine."*[2] *Certainly this could be a premature conclusion to draw. But a legitimate question can be raised as to why this particular paper is being singled out when the implied "conclusions" in more than 90% of the papers published in this Journal and in other psychiatric journals are also generally subject to alternative interpretations, not all of which may be equally plausible.*

The Editor notes that a bias could have been introduced because we relied "on a comparatively small percentage of volunteers [almost 20%] out of an approached population." But all postmarketing surveillance studies use only a minute sample of the total population of interest.[3] *The more salient question is whether there is reason based on empirical evidence to believe that the final selected samples favor one drug group over the other. If selection causes a bias in our method, we should not have been able to detect in our validation studies so many of the commonly accepted ADRs for various drugs.*[4,5] *However, it is always possible in any postmarketing surveillance method that volunteer subjects (including physicians who are urged to report possible ADRs to the FDA) or even medical record samples could introduce a bias. Similarly, although the Editor questions "whether this technique is well suited for comparing incidences of adverse events between a newer and an older agent," he also acknowledges that results from our past studies along with the statistical controls used in the data analyses suggest that what we were seeing in this sertraline study is not simply a "newer drug" phenomenon.*

So, again, why the red-flag editorial? A review of 119 articles published in this Joumal from July 1993 through April 1995 (excluding supplements, monographs, and the October 1994 issue, which was unavailable) offers some clues. The mean article length was slightly less than six pages (skewed upward by a few longer papers); the mean publication lag, defined as the number of months between the date of acceptance and the published issue date, was eight months - for which most authors are grateful to the Editor. Only two of the 119 articles were not published until 11 months after acceptance, and none *had a lag of one year or more. There was no relationship between the length of an article and the publication lag.*

While some issues of the journal included a "commentary" on a specific paper, none of them were signed by the Editor. In fact, a cursory search through issues dating back to 1990 found only one previous Editorial, which also focused on adverse drug reactions.[6] *Yet, our sertraline paper not only prompted an Editorial, but publication was delayed more than a year after it was formally accepted on May 12, 1994.*

During the past four years of the 10-year development of our postmarketing method [continuously supported by the National Institute of Mental Health along with other funding sources], we have became acutely aware of the fact that, once a new drug has been marketed, many pharmaceutical companies clearly do not want their drugs to be carefully monitored for possible ADRs -- in particular, not by any method that can systematically and sensitively compare possible ADR profiles. The Editor of this Journal is to be commended for having the courage to publish our sertraline/fluoxetine paper, but one cannot help wonder to what degree external pressures may have contributed to both the publication delay and the need for a cautionary Editorial.

Presently, the ultimate clinical preference for one psychopharmacological agent over another is mainly determined not so much by true differences in therapeutic efficacy (most antidepressants in most situations are about equally effective) but by presumed differences in their ADR profiles.[7] *Systematic health services research carried out in the real world of postmarketing pharmacotherapy is of paramount importance for clinicians to be competent to practice empirically-based, rational patient care. The real bottom line here is that, although publication of our paper could have an adverse effect on company sales, sertraline seems to have a more troublesome ADR profile than fluoxetine, particularly in respect to those known ADRs that appear to be common to the SSRI class. But only continued astute clinical observations and systematic research will judge whether these preliminary results based on patient self-monitoring indeed help provide more accurate answers to the patient's question, "What will this drug do to me, doctor?"*

About two weeks after I had sent the above proposed Editorial Reply, I received a phone call from the Editor while he was attending the American Psychiatric Association meeting in Miami, saying he could not possibly publish my commentary in its proposed form. He agreed that I was entitled to space for rebuttal, but informed me that I would have to modify its contents. I said I would consider this. Then, just a few days later, he phoned again to say that he had decided to drop his proposed editorial, and that therefore I could forget about the rebuttal. In our discussion, he admitted that this decision was made after conferring with Pfizer representatives at the APA meeting.

So you will not find in the July issue of the *Journal of Clinical Psychiatry* any Editorial Commentary or reply to accompany the Fisher *et al.* article -- finally made available to clinicians (and their patients) a full 14 months after acceptance.

Some obvious questions arise from this sequence of events.

1. Why did the Editor wait a full seven months after the paper's acceptance to decide that he should write an editorial to accompany publication of the article? Did it take a few months for word to get back to Pfizer, who heavily subsidizes the Journal, that the paper was in press?
2. Why was final publication of the article delayed for 14 months, when most articles were being published in about eight months and no other article in that Journal between 1993 and 1994 had had a lag more than 11 months? Did Pfizer want to see publication put off as long as possible while its Zoloft sales were going strong? (Odd coincidence: note that the lead article in this July, 1995, issue of the Journal is not only authored by the Editor but claims to have been accepted way back in April 1994.)
3. And why did the Editor decide at the last moment, after consulting with Pfizer representatives, to drop the idea of writing the Editorial? After reading the proposed rebuttal commentary, was Pfizer now concerned that

publishing the editorial and the rebuttal might actually serve to increase the readers' awareness of Pfizer's role in the publication process, in addition to perhaps emphasizing the apparently unfavorable side-effect profile for Zoloft?

I would welcome your comments.

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September 7, 1995
Part II: Hanky-Panky in the Pharmaceutical Industry

About one month ago I posted to a number of different Internet mailing lists and newsgroups a fact-based chronology involving publication of an article on postmarketing surveillance, showing how one drug company (Pfizer Incorporated) blatantly stretched the ethics envelope when its profits were threatened. The article, published in the July issue of the *Journal of Clinical Psychiatry* (1995;56:288-296), was based on large-scale data indicating that many adverse reactions known to be induced by fluoxetine (Prozac) were being reported with even greater frequency by sertraline (Zoloft) patients. Zoloft is manufactured by Pfizer. So far I've received about 100 replies (many more from mailing lists than from newsgroups), all highly supportive.

Some readers interpreted my original post as appropriately taking aim at the Editor of the *Journal of Clinical Psychiatry.* I had hoped it was clear that my frustration was directed not so much at the *Journal* as at drug company advertisers, who can make life very uncomfortable for even the most conscientious and scrupulous journal editor.

Now I'd like to offer as Part II the following additional related facts, in the hope that more of you - whether clinicians, scientists, ethicists, or simply concerned consumers of prescription drugs - will become aware of the many ways the pharmaceutical industry attempts to unethically influence the acquisition and dissemination of knowledge about their drugs to medical practitioners and their patients. Colleagues who know me will, I believe, agree that I'm not prone to either "sour grapes" reactions or paranoid ideation, but what has gradually emerged over the past four years is a not-too-pretty picture of concerted unethical efforts being made to influence psychiatric publications, education, and even research support.

In our systematic research on measuring the frequency of adverse reactions for newly marketed drugs, this is not the first time my colleagues and I have obtained evidence of drug companies exerting pressure upon journal reviewers and editors. And it wasn't just one company, Pfizer. Lilly did essentially the same thing when we first attempted to publish our fluoxetine vs. trazodone paper, which was finally published more than two years after we had first written it (Fisher S, Bryant SG, Kent TA. Postmarketing surveillance by patient self-monitoring: Trazodone versus fluoxetine. *J. Clin. Psychopharmacol.* 1993;13:235-242).

Nor is the NIH research grant review process immune from similar viruses. Recently, after 10 years of continuous funding from the National Institute of Mental Health (NIMH), further support was abruptly terminated, when at least two members of the original study section - one being the chairman (!), another being the primary reviewer- had been conducting numerous company-supported drug studies, substantially contributing either directly or indirectly to the reviewers' overall income. Incredibly, NIH claims there was no conflict-of-interest in the review because "Research that focuses on other than assessing the efficacy of a particular pharmacologic agent(s) do not represent financial conflict of interest situations" (grammar exactly as written).

Having personally served on NIH study sections, I know that some committee members who review grants do let drug companies know (unethically) about research grant applications that might be inimical to the industry's best interests (e.g., postmarketing surveillance of new drugs). Now, if a member of the committee is also receiving part of his/her total income by conducting clinical trials and/or extensive consulting for that company, then I submit that s/he has a potential conflict-of-interest when it comes to discussing and voting upon the grant application. There are always leaks about who voted for or against a particular application, and a committee member would know that an "approval" vote for a grant that the pharmaceutical industry would like to see killed could lead to loss of income.

The bottom line here seems to be that, in NIH's view, a committee member can be in conflict-of-interest if personal income GAIN might result from his/her vote. But if an external source should exert subtle intimidation, NIH says there is no conflict-of-interest even though a vote in one direction might result in a LOSS of personal income. Does this make sense to you?

Want more food for thought? Are so many of our hospitals and medical school departments so broke that their residents have to get free lunches accompanied by drug company representative sales pitches rivaling some of the best infomercials seen on TV? Of course, this ensures that "truthful" information about the efficacy and side effects of their product(s) compared to other similar drugs will be imparted to the residents without any need for those hard-working physicians-in-training to bother to consult the published literature.

I've also been struck by the ubiquitous but unobtrusive presence of drug company representatives at Grand Rounds and other lectures where the speaker's large honorarium comes from the company. Does Big Daddy/Mommy watch and listen to make sure that the home office will know if the speaker says anything bad about its products?

One reply I received to my original post suggested that groups like the APA (American Psychiatric Association) and NCDEU (NIMH's annual New Clinical Drug Evaluation Units meeting) should publicly air the issue. To this, Dr. Ivan Goldberg commented online:

Maybe I am more cynical than I like to think, but with the important role played by "industry" at both the APA and NCDEU, I'll bet you $100 that no matter how hard any of us push, that no symposium on drug company influence over the funding and publication of post-marketing studies of psychopharmacologic agents will be scheduled at APA or NCDEU.

Most of the key psychopharmacology players at APA and NCDEU also have power roles in the American College of Neuropsychopharmacology (ACNP). Since I'm a past president of the ACNP, I have some first-hand knowledge of how courageously the College (whose members comprise drug company representatives as well as academicians and government representatives) acts when the pharmaceutical industry is unhappy about something:

At its 1993 meeting the ACNP had scheduled a luncheon meeting on the very subject mentioned above in Dr. Goldberg's post. However, at the last minute the topic was changed to something much more important, like "Can SSRI antidepressants be used to make basset hounds act like German shepherds?"

For its 1994 meeting the ACNP Council was considering having the editors of a number of psychiatric and psychopharmacological journals participate in a study group session open to all members of the College. The topic was to be a discussion of the ethics of grant and journal reviewers informing drug companies of submitted grants and manuscripts with content inimical to a company's interests. As a corollary, the study group would also be expected to discuss drug company advertisers' pressures upon journal editors. This proposed study group session was never held.

So there you have more of the story. If you missed my original post, please contact me by E-mail and I'll be happy to send you a copy.

We're talking here about a third-rail issue that no one officially wants to touch. Aren't there more people out there willing to get involved by contacting their colleagues, elected representatives, and any media friends they might have?

Somehow we ought to be able to loudly and clearly warn the "ethical" pharmaceutical industry that, while we admire and are grateful for their many therapeutic accomplishments, we will not tolerate their placing dollars before truth. Excelsior!!

If you've read this far, many thanks for your interest. I hope you'll take some action.

As before, I'd also welcome your comments: sfisher@utmb.edu

September 20, 1995
Part III: Hanky-Panky in the Pharmaceutical Industry

During the past month an assortment of Internet replies have been pouring in following the Part II posting, some simply requesting copies of Part I, some offering advice, and others expressing reactions. The many threads under the headings of "Hanky-Panky" and "Unethical Practices - Drug Industry" in different mailing lists and newsgroups (e.g., sci.med., sci.med.psychobiology, sci.med.pharmacy, psycho-pharm@netcom.com) make for fascinating reading. A few posts came from loonies (some with doctorate degrees!) with knee-jerk reactions to any suggestion that there might be some problems with ethics within the "ethical" pharmaceutical industry. One particular reply, however, came from an obviously well-read and thoughtful individual. Since his position was so clearly stated and probably represents the view of many others, I believe it warranted a separate response. The following is essentially what I sent out on the Intemet September 10-12:

At 09:33 PM 9/9/95 -0400, William (Bill) Boyer, M.D., wrote: *Dr. Fisher raises important points in his well-written update. However I wish to take issue with (what I see as) the implication of his article: that there is, somewhere, somehow, some highly ethical yet practical solution to the influence of pharmaceutical companies. As Dr. Fisher implies, research costs money and as he shows, there are two sources of money, pharmaceutical companies and the ever-dwindling resources of the government. Without funding, little research of any merit will get done. Period.*

The best, though not perfect, solution resides in the tree enterprise system and competetion. This is how undue influence is prevented in other areas of the economy. As long as there are several pharmaceutical companies competing with each other to obtain profit and avoid loss, as long as there are several scientific publications competing with each other for prestige as well as advertising, there will be built-in limits to how much influence any one of them can exert. This will happen not only because their power is diluted, but because undue actions by any one of them may open up an opportunity for their competitors. I also firmly believe that anyone who graduates from a professional school is (or was) smart enough to appropriately evaluate material presented over a "free *lunch."*

I see Dr. Fisher's call to "do something", including contacting our legislators, as a de facto *call for increased governmental control, which history shows is the surest way to stifle science.*

Again, the status quo is not necessarily good, but all of the alternatives look worse.

'If you prefer Cogito ergo sum *to* Non sum qualis eram *you are putting Descartes before Horace' - J. Thurber*

I sincerely wish it were true that, as Dr. Boyer views it, the competition among pharmaceutical companies is sufficient to solve some of the serious ethical problems that have been facing the industry for many years. Because he says he sees no "practical solution to the influence of pharmaceutical companies," he is content with the *status quo.* Well, the present *status* isn't exactly anything to *quo* about; therefore, I believe (a) some action is called for, and (b) there are viable alternatives.

I too am strongly in favor of competition - which in most areas can result in economic, technological, and material benefits for the consumer. However, history shows that unrestricted competition always leads to disaster for the consumer. I don't think anyone would argue that the FDA should be abolished and that individual companies should be left to decide which drugs to market: Do we expect that competition among drug companies would ensure that only safe drugs were marketed? And where were the beneficial effects of competition to the consumer in the days before the Kefauver-Harris amendments, when a drug didn't even have to be shown to be effective to be sold in the market place? It is simply untrue that ethical behavior will prevail "because undue actions by any one [company] may open up an opportunity for their competitors." Letting drug companies (or other companies) compete among themselves unfettered is roughly comparable to having Mark Fuhrman appointed head of L.A.'s Internal Affairs division. To save space, I won't elaborate on this point right now, especially since it doesn't directly bear on the question of "What should we be doing now?"

So let's cut to the chase. As I noted above, there are indeed alternatives to the *status quo,* but Dr. Boyer has badly misread my hope. Contrary to his fear that my "call to 'do something'" calls for increased governmental control, increased governmental controls are certainly NOT what I'm either advocating or hoping to achieve (heaven forbid!). The optimal level of federal regulations on business should be exactly the same as for good pharmacotherapy: the smallest dosage necessary to achieve the desired effect (i.e., maximum benefit for the patient/consumer).

I believe Dr. Boyer grossly underestimates the power of the "people" to effect behavioral change - even in the highly competitive world of business. I could list a long string of events where pressure from the people (possibly including perceived threats from legislators who, after all, do speak for the people) has led to changes in corporate policies. To mention just one, look how the drug companies have cut back on their exotically located "seminars" after the "60 Minutes" segment on paid vacations for physicians and their wives. (And please, let's not have semantic arguments here over the use of the term "vacations." If you think the underlying major purpose of those "seminars" was genuine continuing medical education, then I suppose you're similarly susceptible to all the tobacco industry's arguments.)

Yes, Dr. Boyer, I believe that if enough people raise their voices, the resulting publicity can bring enough pressure on the pharmaceutical industry to take a higher moral road - and we will see some changes.

By the way, why are we all so prone to believe that WE, the fortunately better educated, the logical thinkers, obviously cant be bought so easily? This doesn't stand up too well under close examination, when one "free lunch" after another, whether small or large, is piled up over time and friendships are developed. Furthermore, we shouldn't be taken in by the argument that since we're exposed to reps from ALL the different companies, we're not going to favor just one or two -- isnt there a strong need for lobbying reform in Washington despite the fact that our elected representatives are continually meeting with so many varied and competing special interest groups?

I suspect the only real difference between our views concerns optimism vs. pessimism about corporate nature. I believe that if enough of us speak with a forceful voice, the pharmaceutical industry will elevate their ethical standards. Dr. Boyer, why not join me in seeing whether a bit more optimism is justified? Can't hurt.

As always, I welcome comments and requests:

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ABSTRACTS & EXTRACTS...

Paul L, Foss MA & Galloway J: Sexual jealousy in young women and men: Aggressive responsiveness to partner and rival.

Guerra NG, Huesmann LR & Zelli A: Attributions for social failure and adolescent aggression.

Paul L, Foss MA & Galloway J: Sexual jealousy in young women and men: Aggressive responsiveness to partner and rival. ***Aggressive Behavior*** **1993;19:401-420.**

Abstract: Two studies compared judgments about aggressive components of jealous reactions to the partner and to the rival, specifically, emotional (anger), cognitive (blame), and behavioral components. The first study randomly assigned 172 young women and men to two questionnaires on jealous reactions to mild (flirting) and serious (cheating) transgressions. One questionnaire assessed standards for appropriate behavior and perceptions of how people usually react. The second questionnaire asked people to report how they had reacted or, if not experienced with a sexual transgression, how they would react. The second study asked 113 people to imagine a situation in which they knew their partner had been sexually unfaithful.

There were three major findings that were interpreted in the context of courtship, a time when attention is focused on the qualities of one's potential long-term partner. First, the jealous individual's anger and blame were focused more on the partner than the rival. Second, mean anger and blame scores given the partner were well matched. In contrast, the rival received more anger and blame than deemed appropriate and considerably more anger than blame. These data suggest that, in the context of courtship, a rival is not simply a competitor. Third, men were more inclined to *think* about aggressive action against the rival but women were more emotionally and behaviorally reactive to the rival. The latter result implies that, in the context of competition for an established romantic partner, a rival is more salient for women than for men.

Guerra NG, Huesmann LR & Zelli A: Attributions for social failure and adolescent aggression. ***Aggressive Behavior*** **1993;19:421-434.**

Abstract: In the present study, 199 high school boys and 79 institutionalized delinquent boys of the same age range were assessed on their own aggressive behavior and on their tendencies to attribute social failure to controllable, external, stable causes, anticipate a hostile affective response, and endorse aggressive behavioral responses to hypothetical social situations. While the two populations of boys did not differ detectably in their attributional tendencies, the *relations* between an individual's aggressivenes and an individual's attributions differed considerably across the two populations. In particular, among delinquent but not among nondelinquent boys, the tendency to attribute one's social failures to stable and controllable causes predicted stronger hostile emotional responses to failure and a tendency to endorse physically aggressive responses following such failure. These hostile emotional responses to failure and this preference for a physically aggressive response, in turn, predicted greater actual aggression within the population of delinquent boys. Neither of these links could be demonstrated for nondelinquent boys. However, in the nondelinquent sample, attributing social failure to external and controllable causes predicted endorsement of aggressive responses only indirectly through increased hostile affect. It was concluded that the specific relations between cognitive and affective responses to social failure may be a contributing factor to the serious physical aggression displayed by some delinquents and to the less serious aggression of nondelinquents.

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