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The Emotions: Focus on Inter-Male
Aggression and Dominance Systems
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AGGRESSION AND DOMINANCE SYSTEMS: SOME COMMENTS

by

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I. INTRODUCTION

In a conference with a central theme of the unity of the sciences, it is perhaps redundant to affirm the value of bringing together persons of different disciplinary backgrounds to discuss intellectual issues which we have in common. But we should also recognize that our disciplinary differences have their purposes too, and that different approaches to explanation do not necessarily invalidate each other. In focusing upon the aggressive behavior of humans and higher primates we should be alert to the possibility of multiple explanations of what we observe. But this does not mean that all explanations are equally useful for our understanding. For some purposes of understanding one mode of explanation may be clearly more helpful than others--but without detracting from the value of such other modes for other purposes.

Let me illustrate my general point with special reference to human aggressive behavior. If we have a very large time span for our analysis of variations in aggressive behavior shown by humans--such as a hundred thousand years or more--I see no alternative to viewing such behavior in relationship to the biological survival of the group or species. We thus come to understand differences in terms of biologically-based propensities which are rooted in genetic determinants and developed through interactions with other life forms in the particular niche occupied by the group.

Suppose, however, interest is more in a time span such as that represented by a few decades or a century. Differences in aggressive behavior by members of a society within such a time span are unlikely to be very well explained by biological differences. What we might broadly call sociological variables--including those of cultural background and access to technological resources as well as social organization variables--are much more likely to help us explain the variations in

aggressive behavior between different persons and groups in society and between different periods of time.

Let us narrow the time span now to one or a few days and take the individual as the focus of our analysis. We need not rule out completely the value of biological or sociological variables in order to recognize that another set of variables is much more likely to help us explain the differences in the individual's behavior. These variables have to do especially with self-conceptions, including a sense of social identity, and how different events are seen as enhancing or thwarting these conceptions of the self. Thus the variables which are especially important for explaining aggressive behavior in this context are those which might be broadly labeled as social psychological.

In my comments on other papers presented, I will be attempting to use frameworks parallel to those indicated in the above discussion. That is, I will first discuss the paper which provides observations most relevant for considerations of the biological evolution of aggressive behavior. I will next discuss the paper for which sociological considerations appear most relevant. Finally, I will comment on the social psychology of human aggression, drawing on whatever papers suggest important material for this level of analysis.

Before I proceed to the comments of particular papers, however, let me make one more point of a general introductory nature. This also bears, in a rather perverse way, on the subject of the unity--or disunity--of the sciences. All the sciences like to formulate neat summaries of their findings which belie the complexity of the facts actually observed. Nature itself is often inclined to contradict our neat summaries. And those embodiments of human nature (and almost-human nature among the higher primates), whether viewed in the form of species, social groups, or of individuals, are apt to be much more opportunistic in exploiting their specific conditions of life than our preconceptions, even when based

on scientific generalizations, are apt to prepare us for. I believe that I will be able to point to several illustrations of this within the papers at hand.

II. AN EVOLUTIONARY PERSPECTIVE

As I turn to Professor Itani's paper, I do so not primarily as a student of animal behavior; it is rather in search of the understanding of human behavior that my main interest is drawn to this paper. My main question is: what do these studies of non-human primates suggest to us about the course of human evolution?

Of course we can study the dominance relations of Japanese monkeys simply to understand the social life of these monkeys. Or we can examine the way pygmy chimpanzees share food--or study the mating practices of common chimpanzees--primarily to understand their special adaptations to African habitats. We can compare primate behaviors across species simply to observe and catalog these natural variations. All these are perfectly reasonable ways to approach the data included in Itani's papers. As I read it, however, I cannot but wonder about the human implications. My comments will therefore be directed toward questions of human evolutionary significance.

Early in his paper, Itani gives us a useful taxonomy of forms of primate social organization. There are "elemental" societies--without continued relationships between adult males and females except for mating and breeding--and societies with stable family groups. Those with stable groups can be further subdivided into:

- (1) groups established by one male and one female, both of which leave their birth groups;
- (2) groups established by one male and several females, with the females constituting the home group (matrilineal);

(3) groups established by one male and several females, but with the females moving from group to group (nonmatrilineal and nonpatrilineal);

(4) groups with several males and several females, with the males leaving their natal groups (matrilineal); and

(5) groups with several males and several females, with the females leaving their natal groups (patrilineal).

We may note in passing that despite this great variety of forms, some logical possibilities are not represented at all. We have, for example, no standard patterns of one female with several males. From a human point of view this might seem parallel to the relative absence of polyandry among human societies-- interpretable in terms of avoiding the problems of intense rivalry between males who must share one female as their mate. But if such a human parallel makes this gap understandable, what about the absence of male-female pairs associated with either patrilineal or matrilineal ties? Here is, in term- of human experience, a remarkable absence indeed. Does this suggest that a more or less permanent monogamy ties to an extended family of either the male or female line (a very common human pattern) has had its origins in the complex patterns of distinctively human development?

Be that as it may, most of Professor Itani's paper deals with groups of the last two types, represented by Japanese monkeys and African chimpanzees respectively. The monkeys are described as having a very clear linear dominance system--both for males and for females and both within and between troops. The system of social organization here is described as "coexistence based on the inequality principle." This system of social organization seems to be based on two main forms of identity for individuals--one based on kinship and the other based on rank within the dominance system.

In contrast we have the case of the chimpanzees. Here dominance orders are still important, but they do not so rigidly control behavior as with the monkeys.

Much more do we see elements of behavior within the group based on what Itani calls "the equality principle" and what I might prefer to call a shared sense of social identity within the primary group. It shows most notably in the sharing of food; even one's favorite foods may be shared among chimpanzee associates--and this is not necessarily limited by kinship circles.

Although the beginnings of the equality principle may be seen in the behavior of Japanese monkeys--in grooming relationships and play behavior, or even by some high ranking males "baby sitting" certain young monkeys--it never seems to be applied to the entire group. Among chimpanzees the applications are more numerous and more extensive, and the entire group may share more fully in this sense of mutuality. There is also the strong suggestion that the equalitarian society may be somewhat more fully realized among pygmy chimpanzees than among common chimps.

Although the point is not developed in his paper, Professor Itani indicates that he sees early human hunter-gatherers as something of an extension of this trend toward equality. What therefore seems to be implied is a clear evolutionary pattern as we approach the emergence of man, and this pattern emphasizes the relaxation of rigid dominance hierarchies in favor of a greater mutuality of behavior within the group.

But why should this direction of development occur? What correlated factors are involved in this transition, and how might they have been shaped through forces of natural selection? It is the relative absence of attention to questions such as these that I feel is the most serious omission in Professor Itani's fine paper. Not having any expertise in primatology or the study of human origins, I would add little by my speculations, but I hope that here is an area which will be filled in by others at this conference.

But before we tie too neatly together the main strands of inference about

evolutionary development, let us take note of some paradoxes, ironies, and nagging doubts.

Professor Itani recognizes the paradoxical links between humanistic characteristics and what he calls "negated coexistence" as he concludes with a specific discussion of aggression. Aggression within the group seems relatively well controlled in most primate societies--either by the dominance system, by the mutuality fostered by the equality principle, or by both. But aggression against those outside the group is something else; it is often bitter and occasionally murderous. And here we come to the paradox: as cooperation becomes more fully developed within the group, the potential for systematic attack upon outsiders seems to grow. Of course this is only a paradox if we expect the mutuality within the group to be extended beyond it. Our studies of the chimpanzees at Gombe show that these usually peaceful primates can be quite ruthless in seeking the extermination of a rival group. Itani suggests that only groups with a strong male bond can successfully pursue or resist in such violent intergroup contests, leading to the natural selection of those not only inclined to band together within the group but also willing and able to fight against outsiders. Is this another significant clue about our human origins?

With our focus on inter-male aggression, it is easy to see in the patriarchal chimpanzees--with their intelligence, playfulness, and mutuality within the group; but also with their readiness to resist outsiders by force--some key insights into early human evolution. However, we must recognize some further complications as we speculate about the significance of chimpanzee data for human origins.

First, let us recognize that the focus on Japanese monkeys and chimpanzees may give us a false sense of evolutionary progression from a typical monkey pattern of social organization to that of the great apes. Both monkeys and apes show

important variations to confound the implied progression. Not all monkeys organize themselves into tight hierarchies; for example, the red colobus monkey in western Africa has been described as having a highly fluid pattern of social organization, with females moving freely into and out of home troops led by males.¹ On the other hand, not all apes are well represented by the patriarchal common chimps. There is some evidence that in pygmy chimpanzees the females may be more important in the forming of alliances within the group than the males;² gorillas appear to be more flexible in forms of family organization than are chimpanzees; and the solitary orangutan has clearly a different kind of society. Let us not generalize too freely from our monkey and chimpanzee comparisons.

Also, let us recognize that our focus on inter-male aggression may--with memories of "nature red in tooth and claw"--give us an extremely biased base for speculations about early human evolution. Not only the aggressive male point of view needs to be considered in examining the possible forces of natural selection. There is also the position of the female and that of the child to be considered. The notable sexual dimorphism of human forms and the absence of estrous in the human female may have a significance in human evolution which far outweighs the quality of inter-male relationships. The more permanent sexuality associated with these markedly human characteristics may be extremely important in the formation of the human family. Even more important may these features be in the protection and nurturance of the young--with a father around for protection and to bring food home; and a mother with a wide pelvis to give birth to a baby with a well-rounded head, large breasts to assist for many months of feeding, and without an overpowering estrous cycle to tempt her to forget her maternal duties!

III. SOCIOLOGICAL ANALYSES OF HUMAN CONFLICT³

The paper by Edward LiPuma on "Warfare in the New Guinea Highlands: Theory and Ethnography in Conflict" seems to me to consist of two rather different parts. First we have some general reflections on the nature of human warfare. Then we have a discussion of the particular case of warfare in the highlands of New Guinea, with special attention to the Maring. I will comment on both of these topics in turn.

I find myself applauding the perspective that Edward LiPuma initially lays out for the consideration of human warfare. Although we might quibble over a few of his points about distinctive features of human warfare (some chimpanzees, for example, may engage in conflicts between groups as groups), in general it is good to get out on the table, so to speak, the awareness that organized human conflict is fundamentally different from fighting among other animals. And LiPuma gives us a good listing of the ways that human warfare is different. In so doing he also makes us aware of the variability of human motives and actions included in warfare, and properly warns us against any simple reductionism in explaining warfare.

However, I am a bit disappointed not to have a clearer theoretical statement about the nature of the type of warfare to be examined further in the paper. To say that

Warfare only ignites when societies, predisposed by their own history, aware of their outstanding investments, endowed with compatible dispositions and attitudes, takes an interest in its prosecution.⁴

does not take us very far in predicting or explaining outbreaks in group hostilities. In the end, we have a withdrawal from theorizing in favor of induction. "An adequate, comparative theory of warfare must start with well-developed specific cases, and then synthesize these cases without doing violence to historical truths."⁵

We then turn to the case of warfare among the Maring.

The key question implicit in most of LiPuma's paper is: why is there now a resurgence of warfare between local groups in the New Guinea highlands? Apparently the Maring have not yet returned to the patterns of warfare known before pacification in 1955; but some of their neighbors have shown a return to previous patterns of conflict, and there is at least a concern that the Maring are also headed in that direction. Why?

Before we deal further with the question of why the resurgence of warfare, let us note that the New Guinea highlands are not the same as they were thirty years ago. The modern world has arrived, and many people associate modernity with peace. We have in LiPuma's paper both a good summary of traditional warfare patterns and how the influences of the modern world are now placing new strains in Maring life. The threat of a return to violence, we are told, "is not a return to tradition--the resurrection of an earlier practice-- but that clansmen will enlist violence in the service of modernity."⁶

How could a revival of warfare be used "in the service of modernity"? Here I do not find the paper providing a very clear answer. We have the suggestion that some neighboring tribes have caught the attention of the national government with a return to violence, which assumes that the attention of government is critical in resolving local distresses. And we have the suggestion that litigation over local issues, such as land boundaries, is frequently a very cumbersome process. But I still do not feel that the way warfare could effectively serve modern purposes for the New Guinea highlanders is very clearly indicated in the paper.

Why then do we have a resurgence of warfare in parts of the New Guinea highlands? After first admitting that I don't know (even after reading the helpful papers of LiPuma and Meltzoff), I would join LiPuma in assuming that strains of

population growth and land pressures might be sources of instability and that traditional forms of social organization might still be strong enough to enlist fighting loyalties. However, it seems to me that a major return to a general pattern of warfare could only come if the power of the national government was losing its hold in the highlands and was being replaced by more traditional and localized centers of authority. But this would be quite different from fighting to gain concessions from the national government. The alternative possibility would be that there is a resurgence of sporadic violence, but that this is not really equivalent to the organized warfare of the old days.

I must confess that I am still, in the end, mystified by the resurgence of violence in the New Guinea highlands. Perhaps if I had been there, as LiPuman has, I would understand it better. But not having been there, my mind tends to wander to other forms of conflict with which I am more personally familiar, and in some of these there are similar themes of a resurgence of violence after a period of relative peace. I think of American race relations--of the riots after World War I and during World War II, of the relative calm, and then the outbreaks in the late 1960s. I think of conflict in Northern Ireland, which returned to violence about fourteen years ago after a generation of relative stability. And I think of my own studies of "county seat wars" in America--where local conflicts erupted (mostly toward the end of the nineteenth century) into a sporadic violence over the issue of county seat location. Of course none of these forms of conflict is properly called a war; they have little in common with either modern international wars or more traditional and localized forms of warfare. But I do at least see some common threads running through these various forms of conflict, and I wonder if some of these might also apply to the New Guinea highlands. One common thread is that of social change unleashing new popular aspirations. Another is that of mobilization (using both traditional and nontraditional channels) for group action

to achieve new aspirations. A third is the divided or uncertain role of governmental authority in relation to either supporting or repressing the movements afoot. Here is not the place to develop a theoretical structure for these considerations, only to wonder if they might not also apply to instances like the revival of violence in the highlands of New Guinea.⁷

Finally, let us recognize that the question we have been pursuing can be refocused in another way. Given the strength of the warfare complex in the highlands of New Guinea and its significance for manhood--as is well indicated by both the LiPuma and Meltzoff papers--we might perhaps better ask: how can we explain the generation of relative peace? How can we explain why these tribes generally abandoned warfare? An answer is partly given by LiPuman, but I am still not satisfied that I fully understand the pacification process and why it was generally effective. The understanding of this question is, I think, the key to the secondary question of why we see some of the patterns of warfare returning. That is, what particular elements of the pacification process may no longer be working, and why? It is within the framework of this question that I believe we can most profitably examine the return of warfare in the New Guinea highlands.

IV. THE SOCIAL PSYCHOLOGY OF AGGRESSION

Two of the points I especially appreciate about LiPuma's paper are (1) his insistence that warfare must be seen not only in terms of collective institutions but also as the actions of concrete human beings, and (2) his pointing to the importance of the linguistic component in warfare. Perhaps the point is obvious once made, but we still need to be reminded that the institutions of a society do not speak for themselves but only as they become expressed in the beliefs, motives,

and actions of living people. Warfare too requires an incorporation into the everyday life activities of men and women--especially of men. And linguistic forms may be critical in this process of incorporation. A man becomes a "soldier," and immediately a whole host of new associations--actions, motives and, yes, even beliefs--are triggered by this entry into a new category of personhood. We cannot fully understand the actions of persons engaged in structured social conflict unless we understand the linguistic conventions that define the conflict and the primary roles played by its participants.

Sarah Meltzoff's paper nicely complements that of Edward LiPuma by showing how dance forms continue to accentuate the themes of warfare for highlanders of New Guinea. She starts with a concrete illustration of a dance, then proceeds to point out the ramifications of participation in such dances. We are led to see such ritualized dances as extensions of the same themes of individual and groups competition which apply to ceremonial exchanges between New Guinea big men, as well as to warfare itself.

I mentioned above the importance of the linguistic component in human warfare. Meltzoff's paper cuts even deeper on a similar theme to assert that ritualized actions may have much more symbolic power than linguistic forms alone. As she puts it:

The characteristic decorations of dancers and the forms of dance, which emphasize power and strength, communicate more and in a different way than words alone. Dance and dancers do not simply state that they are powerful, but perform acts of power and strength. They index a social state and are thus that much more convincing than language alone.⁸

I am reminded that much of the symbolic equipment which accompanies and supports combatants in contemporary conflicts in Western nations has a similar power beyond words. Soldiers marching on parade with band and bugle, the standing at attention before the flag while the national anthem is played and sung,

the bright colors of the uniforms of football players as they line up in dramatic confrontation, the cheerleaders performing their gymnastics at the sidelines--these are just a few of the images which come readily to mind. They confirm the importance of symbolic forms which go beyond mere words; such forms are indeed significant ingredients in the structuring of human conflicts.

Symbolic actions are important especially in the confirmation of identities. The dramatic character of symbolic actions helps to induce the individual to fill the mold his actions represent with the appropriate beliefs and aspirations. The public character of symbolic actions is critical for adding social cement to the glue of individual commitment. The dramatic and public character together help the individual to become what he once only imagined--for example, a front-line representative of his group in pursuing a conflict with its opponent.

I will not comment in detail about Meltzoff's discussion of New Guinea warfare. Her paper is in many respects similar to that of LiPuma, and much of what I said about LiPuma's paper is applicable to Meltzoff's as well. It is the role of ritual which I see as the main focus of Meltzoff's paper, and here I have only one main criticism. After reading her paper I am still not sure whether the dance forms which she describes function more as a support for warfare or as a substitute for it. This is admittedly a difficult question to unravel, but I think that it has considerable significance for the search for alternatives to violent conflict. If the dance serves primarily to support the warfare system, the Australians were wrong in developing the Mt. Hagen and Goroka competitions. On the other hand, if such dancing serves as a sublimation for clan hostilities, it may provide an almost ideal example of what William James once termed the "moral equivalent" of war. Perhaps the question I have here cannot be answered by data at hand, but it would be interesting to see the results of a study that compared the dances of groups continually engaging in warfare with those of groups essentially at peace.

Going now beyond the particular papers at hand, I want to suggest a few points of general significance for the subject of the social psychology of aggression.

First, I think that we must distinguish two main types of aggression, which I will, with deliberate over-simplification, characterize respectively as "antisocial aggression" and "prosocial aggression."

Antisocial aggression represents those acts of fighting and attack which are generally condemned within the society. Such actions range from the fist fights of pre-adolescents to barroom brawls of adults and, ultimately, to murder. Although it is not universally the case, such actions often occur in the heat of passion. When this is the case, they are often triggered by personal strains and frustrations which erupt into violent anger. Whether or not anger is centrally involved, the actions do not gain general social approval--and even the individual(s) involved must go to some lengths to justify their actions to themselves.

Prosocial aggression represents acts intended to harm others done in the name of one's own group and its collective interests. Here the white heat of passion is less often observed. What anger and passion are present is often painstakingly aroused and orchestrated by social authorities, and the socially organized nature of action is much more apparent than its impulsive base. Warfare, of course, almost always belongs in this category of prosocial aggression, and the highland tribes of New Guinea give us some excellent examples.

The distinction I have just made is admittedly a rough one. No doubt some social approval can be engendered, within at least small groups of people, for acts of aggression most would brand as criminal. And there are often dissenters within the group pursuing its course of organized aggression against another group. But the main distinction still holds: in one case the action is evaluated negatively by most of the relevant observers (including both peers and those with formal authority), while in the other case there is general social support.

It seems to me that explanations for aggression must be fundamentally different for these two kinds of human aggression. It also seems to me that the forms of human aggression which are most problematic for us--and most capable of resulting in massive destruction--are predominantly of the prosocial variety.

How do we explain the acts of prosocial aggression? We have to start with examining the nature of social organization and the cultural traditions involved. But there is also the need to explain how the conflict goals of the group become embodied in the motives of individuals. This is a very broad subject, and I here only wish to outline some of the questions which help to organize its pursuit:

1. How are the values of the larger group represented in the everyday actions of men and women, and how do these become accepted by children?
2. How are the social divisions of adult worlds recognized and assimilated by children?
3. How are rewards (material and otherwise) provided for certain roles in society, such as those of soldiers or political leaders? How do persons come to learn of these rewards and the behavior required to obtain them?
4. What happens when people are inducted into special social roles, such as that of a soldier? How does the conception of self change with the incorporation of this role? What new ranges of actions become morally permissible as part of this new role?
5. How are political leaders selected, and what characteristics help them to achieve success? How do these characteristics influence the values they pursue in their positions of leadership? What particular values must they be seen to affirm--and what must they be against--if they are to maintain support for their leadership?

Such questions, it seems to me, point the direction in which we must pursue a general understanding of the social psychology of (prosocial) aggression in humans.

V. CONCLUSIONS

In my introductory comments I mentioned that we must be prepared for

surprises, that many generalizations that we find ourselves leaping to make--even on the basis of scientifically derived data--may be more neatly stated than the full array of facts can support. Some of these surprises have been the subject of comments by the authors of papers I have reviewed. Itani points to the "paradoxical links" between the cooperative behavior of chimpanzees and their propensity for infanticide and intergroup violence. LiPuma points to the irony of a resurgence of warfare in the highlands of New Guinea in apparent association with modernization--even while modernization is mentally associated by these people with pacification. And the very title of Meltzoff's paper suggests another irony; "lethal dance"--how indeed could a pleasant recreational activity such as a dance carry deadly overtones? On all of these matters we are enlightened by what these authors have to say; our surprise is at least somewhat allayed as we understand a little more fully the complexities which lie behind these patterns of behavior of humans and their nearest relatives.

The paradox which I find most noteworthy about human aggression is that the most notable and destructive forms are not those we most easily imagine when we think of the term "aggression." Our first associations are apt to be those of a murderous impulse to kill which rises up out of anger. Pain, fear and frustration can readily give rise to anger, and aggression seems a most natural consequence. Of course there are many instances of such angry aggression among us; the police records of major cities such as Chicago can give us an almost endless list. But still I maintain that this most primitive form of aggression (which in its basic expression is probably quite continuous with what is seen in monkeys and apes) is neither the most common nor the most destructive form of human aggression. Certainly another form is more characteristically human. This more distinctively human form is what I have called "prosocial aggression"; it is aggression in the cause of one group in conflict with another. It is in such socially patterned

behavior that our greatest destructiveness of human values can occur. War is the most obvious example of this. And the supreme irony is that these forms of prosocial aggression depend on the highest expressions of human cooperation. They are based on a comprehensive coordination of highly socialized human beings--pitted against other well organized and socialized humans.

This basic paradox of greater cooperation and mutuality of behavior within the group being associated with a greater potential for intergroup conflict is not alone a human feature. Itani points out that it is also a part of the fundamental features of life among chimpanzees. But we have some reason to believe that the basic irony of cooperation and prosocial aggression growing side-by-side is even more strongly a part of the human condition.

This brings us some distance from simple dominance systems represented by individuals within a troop of monkeys. Of course there are dominance systems in human society, but these are not as powerfully expressed through simple face-to-face interactions as they are in other ways. Much more are these the dominance systems of organized social systems--such as (in the modern world) corporations, unions, churches, schools, and governments

NOTES

1. E.D. Starin, "Monkey Moves," Natural History, September 1981, pp. 36-43.
2. Paul Raeburn, "An Uncommon Chimp," Science 83, June 1983, pp. 40-48.
3. I use the term "sociological" here with its broadest connotations. Thus I include not only the characteristic concerns of sociologists but of all social scientists who study human institutions and patterns of social organization. This of course includes most social scientists.
4. Edward LiPuma, "Warfare in the New Guinea Highlands: Theory and Ethnography in Conflict," p. 2.
5. Ibid. p. 6.
6. Ibid., p. 6.
7. I more fully discuss some of these matters in the following: Schellenberg, "The Northern Irish Conflict and Sociological Theory," Western Sociological Review, 11 (1980), pp. 44-52. Schellenberg, "County Sear Wars: Historical Observations," American Studies, 22 (1981), pp. 81-95. Schellenberg, The Science of Conflict (New York: Oxford University Press, 1982), especially pp. 60-145.
8. Sarah Keene Meltzoff, "Lethal Dance," p.3.