Conflict in the Conflict Theories: Ethological and Social Arguments

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RÉSUMÉ

Après un examen des publications récentes sur l'éthologie du conflit, on nous présente une nouvelle théorie sur ce sujet. L'étude vise à démontrer l'utilité de l'éthologie et de l'anthropologie sociale pour une meilleure compréhension du conflit dans notre monde moderne.

I

Recent years have seen considerable interest in the anthropological study of conflict[1]. For example, studies on this subject have appeared in earlier issues of this journal (e.g., Vol. 3, No. 2). At the same time, there is considerable recent interest in the biological study of conflict, particularly from the field of comparative ethology. By and large, the work in these two fields has been done independently; nevertheless, explanations of conflict from the two fields show points of convergence. In the following review, I shall discuss several recent books from ethology and a recent anthropological study of social conflict.

II

I shall consider first two rather popular books on ethology, *African Genesis and Territorial Imperative,* both by Robert Ardry. My copy of *African Genesis* is a new paperbound edition of Ardry's earlier (1961) book on the development of human agressive drives. His more recent book, *Territorial Imperative* (1966), asks, "Is Homo sapiens a territorial species?"

[1] The title of this article was suggested by Richard Frucht.

Baldly put, the thesis of *African Genesis* is that "Man is a predator whose natural instinct is to kill with a weapon" (p. 322). Evidence for this thesis is drawn from the East African fossil record. Ardry presents a convincing argument that the australopithecines were hunters. In this, he follows Raymond Dart's interpretation of bone deposits at Makapan and elsewhere. Nevertheless, in discussing aggression there is a tendency to confound it with predation, as Julian Huxley notes (in his preface to Lorenz 1966). Ardry is not immune to this tendency. Having established that the African man-apes were hunters, he concludes that they were "killer apes". The australopithecines' apparent use of bone implements is cited as evidence of the "systematic use of weapons" (p. 195). Here is an implicit confusion of predation and aggression. The discussion of the effects of a hunting life on proto-human populations is more clearly presented in *Territorial Imperative* (pp. 253-263). Nevertheless, this book cites (p. 258) Washburn and Avis's (1958) rather confusing interpretation of the evolution of human predatory drives:

> Unless careful training has hidden the natural drives, men enjoy the chase and the kill. In most cultures torture and suffering are made public spectacles for the enjoyment of all. The victims *may be either animal or human.*[2]

The distinction between predation and aggression is now better understood than it was in 1958, when the lines quoted above were first printed. Nevertheless, it is still worthwhile to underline this very important distinction. A hunting club is a "weapon" only if it is turned against members of the hunter's species. It is precisely to avoid this sort of confusion that Lorenz urges the terms "aggression" be restricted to intra-specific fighting, thus distinguishing it from predation. In fact, leaving aside their use of hunting implements, the evidence that the australopithecines were "killer apes" is quite slender. It rests primarily on a fractured adolescent australopithecine jaw. Ardry regards this jaw, found near Makapan, as the "remnant of antique assassination" (1961:190). Perhaps it is, but it provides little evidence for Ardry's claim that the australopithecines commonly used weapons.

[2] Italics added

Ardry makes this jaw a basis for speculation on proto-human and human weaponry. As other reviewers have remarked, he regards the term "tool" as a euphemism for "weapon". For example, he takes Kenneth Oakley (1956) to task for not calling his book "Man the Weapon-maker": "With few exceptions, they [Oakley's illustrations] are pictures of weapons" (p. 319). If one recalls that even nuclear explosives may be "tools" in excavating canals, the difficulty of making judgments of this kind is shown.

Ardry is not diffident in urging the implications for social science of the argument summarized above. Let us consider briefly what he considers these implications to be. In the author's view, most social scientists are misled by the romantic fallacy. Briefly put, explanations of human behavior which ignore aggressive drives are incomplete or "romantic". Social thinkers whom Ardry signals for criticism in terms of this paradigm are Rousseau, Marx, Freud, and contemporary pacifists.

He regards Rousseau as the author of the romantic fallacy, and specifically of the "illusion of original goodness". This is the belief that children and primitive retain a morality lost to the civilized. Among subscribers to this view he counts Thomas Jefferson, J. B. Watson, Elliot Smith, Ashley Montagu, Franz Boas, and in fact most anthropologists (some are mentioned in *Territorial Imperative*). To this list might be added the name of Konrad Lorenz (1966:221), who maintains that effective morality must have some basis in natural inclination:

> The man who behaves socially from natural inclination normally makes few demands on the controlling mechanism of his own moral responsibility. Thus, in times of stress, he has huge reserves of moral strength to draw upon.

Karl Marx and early socialist thinkers are the subject of considerable discussion in Ardry's books. Briefly, Marx's opposition to private property is said to counter aggressive instincts, as expressed in the "territorial urge". Further, when aggression cannot be expressed through this urge, as it cannot in socialist societies, it will be expressed in dominance. This argument is further developed in *The Territorial Imperative* (1966). Production in socialist countries, in Ardry's view, runs

afoul of the "territorial imperative". In societies not organized under socialism, the small proprietor is aided in his productive labors by instinctive feelings toward his property. Socialist agriculture, particularly, is unsuccessful because it is not organized on the basis of the pair territory (pp. 112-116). This argument proceeds from Ardry's interpretation of the findings of comparative ethology. First, a territorial species is one in which all males "bear an inherent drive to gain and defend an exclusive property" (p. 3). A territory, on the other hand, is "an area of space ... which an animal or group of animals defends as an exclusive preserve" (p. 3). As the author points out, there are many species which have territories as defined here. It is further reasonable to suppose that man may have shared in the evolutionary process producing territoriality in other species. Thus, he concludes that man has an "inherent drive to gain and defend property". It follows that socialism, pacifism, and internationalism run afoul of this "inherent drive" in various ways. This conclusion, however, rests on a false premise. Ethology does not maintain that there is an instinct to acquire and defend property (cf. Service 1966:21, 24-25). Ardry has simply substituted in his definition the word "property" for the term "territory". He thus establishes by definition what he ought to prove by argument.

A similar kind of semantic sleight of hand is found in Ardry's (1966:191-192) concept of the "biological nation". This term is used by Ardry in much the same sense as the concept of the territorial animal community is used in ethology (e.g., Lorenz 1966: Ch. 10). The definition itself is unexceptionable; obviously there are animal communities which conflict over territory. What is objectionable here is the use of the term "nation" to describe communities. The implication follows that human nations are much like territorial animal communities — like those of rats, for example. Perhaps they are; nevertheless, Ardry only obscures the argument by choosing terms loaded in his favor.

Ardry considers that classical psychoanalysis provides a suggestive but incomplete explanation of human aggressiveness. In "Beyond the Pleasure Principle", Freud (1920) explained aggression as the death wish turned outward. Both Ardry and Konrad Lorenz object to the Freudian view of human aggressiveness.

Lorenz's (1964) comments at a symposium on the natural history of aggression are cited with approval:

> There cannot be any doubt, in the opinion of any biologically-minded scientist, that intraspecific fighting is, in man, just as much of a spontaneous instinctive drive as in most other higher vertebrates. The beginning synthesis between the findings of ethology and psychoanalysis does not leave any doubt, either, that what Sigmund Freud called the "death drive" is nothing else but the miscarrying of this instinct which, in itself, is as indispensable for survival as any other.[3]

The view presented above seems preferable to that of classical psycho-analysis, since it avoids the dualism of Freud's explanation of aggression. If the ethological view can be substantiated, therefore, one can only welcome the promised synthesis of ethological and psychological explanations.

Although its proponents are not named, Ardry's books speak as much to pacifism as to socialism and psychoanalysis. He might have argued that instinctive drives make the abolition of war difficult, but this point is not stressed. He argues, however, in both books reviewed here that the end of war would leave man suffering from undischarged aggressiveness. Lorenz concurs, arguing that a human equivalent of phylogenetically ritualized animal fights is needed (1966:242-243). To an extent, Lorenz believes, we have this in regulated international competition in sport and other fields. Ardry (1966:301), for his part, repeats Anthony Storr's interesting suggestion that competition to explore outer space is a form of ritualized combat. The search for alternative institutions on which man can expend his aggressive energies did not of course begin with Ardry and Lorenz. Over half a century ago, William James (1910), proposed a search for the moral equivalent of war. James, a pacifist, nevertheless believed in the "bellicosity" of human nature. It is interesting to note the similarities between Lorenz's and James's speculations on the problems of ending warfare. Both see, in the struggle with nature, a means of diverting nations' energy for warmaking.

Konrad Lorenz, as noted earlier, believes that there are inherited aggressive drives in man. Though he agrees to that extent

[3] Cited by Ardry 1966:302.

with Ardry, his argument seems truer to the mark than Ardry's. Recall that for Ardry, man is a "biological invention evolved to suit the purposes of the weapon" (1961:318). On the contrary, Lorenz maintains that human inhibitions to aggression have been unable to keep pace with weaponry. Such inhibitions, from the time of the australopithecines on, could not cope with the possibilities for aggression which culture provides.

Nor has man a "predatory mentality", as Ardry and others maintain:

> ...One can only deplore the fact that man has definitely not got a 'carnivorous mentality'. All his trouble arises from his being a basically harmless, omnivorous creature lacking in natural weapons with which to kill big prey, and therefore also devoid of the built-in safety devices which prevent 'professional' carnivores from abusing their killing power to destroy fellow-members of their own species. (Lorenz 1966:207)

If Lorenz is correct, as I believe he is, Ardry has misread virtually the whole of the proto-human fossil record. Far from being the "killer apes" of Ardry's account, the australopithecines possessed an enhanced capacity for intra-specific violence because they lacked a predatory history. As ethologists have shown, large carnivores have instinctual inhibitions which effectively control intra-specific fighting. Man is perhaps unique among large predatory animals in the weakness of such inhibitions. Furthermore, such checks on aggression as early man inherited were thrown out of kilter by the projectile:

> The distance at which all shooting weapons take effect screens the killer against the stimulus situation which would otherwise activate his killing inhibitions. (Lorenz 1966: 208)

The works considered here do not give an encouraging view of the possibility of controlling human violence. At the same time, their central thesis, that aggressiveness is an inherited drive, is untestable. Why, then, consider it? The reader will doubtless answer that question for himself, but I note below a few instances in which this thesis is useful in the explanation of social phenomena.

First, Lorenz's hypothesis provides a parsimonious explanation of spontaneous outbreaks of aggressiveness. Consider a phenomenon variously called Polar disease, Expedition Choler,

or less elegantly "cabin fever". Polar disease or cabin fever attacks small groups of men who become completely dependent on each other during arctic expeditions, imprisonment, or the like. Lorenz experienced it himself while interned in a prisoner-of-war camp. He explains such spontaneous outbreaks of aggression and irritability as follows. During the course of expeditions, imprisonment, or whatever, people are prevented by isolation from discharging their naturally occuring hostile impulses on strangers and outsiders. At the same time, it is not readily discharged on companions because of mutual dependence. Since aggression, in Lorenz's view, is a natural drive like any other, it must be discharged in some way. What occurs is a gradual lowering of the threshold for the release of hostility. In time, members of the group are angered by formerly unnoticed stimuli. As Lorenz (1966:45-46) puts it:

> One reacts to the small mannerisms of one's best friends — such as the way in which they clear their throats or sneeze — in a way that would normally be adequate only if one had been hit by a drunkard.

Finally, ethology provides for a welcome restatement of the classical psychoanalytic theory of instincts. As noted above, Freud believed that aggressive behavior could be explained by the "death instinct". This instinct was primarily self-destructive, a sort of anthropomorphizing of the second law of thermodynamics. Aggression itself was explained as a turning outward of this instinct. Whatever we may say in defense of this theory, it is difficult to believe in an instinct for self-destruction. Instincts, like other parts of the organism, presumably arise through natural selection. It is difficult to see how selection for self-destructiveness could have taken place. The ethological view, on the other hand, holds that aggression is a primary, not a derived phenomenon. It benefits the species in defense against predators, in spacing out individuals over territory, and in establishing a rank order among social species. Like other instincts, it also has dysfunctional aspects, particularly among social beings. Like the sex drive, and other instinctive drives, it must be controlled in the interests of social order. And like other drives, instinctive inhibitions against its release may be thrown out of kilter by cultural developments. Of that, of course, we need scarcely be reminded in the age of the doomsday machine.

III

In ethology, then, as in sociology, conflict may have a pathological outcome. Damage done in conflict with other species may be eufunctional, as that done to conspecifics never is (e.g., Lorenz, 1964:40). As might be expected, then, means of controlling violence to conspecifics have evolved in many species. One of these in particular gives a striking parallel to human custom. This is the mechanism to which Julian Huxley has given the name "ritualization". Ritualization serves, among other things, to control fighting in a number of animal species. Lorenz (1964) discusses a particular kind of ritualization in his essay on "Ritualized Fighting". As the title implies, these are sham fights of various kind — threat displays, wrestling matches, and so on. Of particular interest here are his (1964:48-49; cf. Storr 1964) views on the ritualization of fighting in man:

> ... One of the most important functions phyletic ritualization has to perform in the interest of a species' survival, is the one discussed in this paper, the controlling of intraspecific aggression. It is to be hoped that cultural ritualization will prove able to do the same with that kind of intraspecific aggression in man which threatens him with extinction.

Ritualized conflict, of course, is well known to ethnography. In fact, it is a critical part of Beals ans Siegel's (1966) recent classification of social conflict. In the view of Beals and Siegel (1966:18) conflict obtains when "two parties belonging to the same organization exchange behaviors that symbolize opposition". There are two kinds of conflict. These are divisive conflict and nondisruptive conflict — "pseudoconflict". Pseudoconflict includes individual and team competition. I gather from subsequent discussion that it is much like Lorenz's concept of ritualized fighting.

The term "pseudoconflict" serves to underscore an important issue. As the authors maintain, there is considerable loose thinking in anthropology concerning the purported eufunctional aspects of conflict. Conflict is often viewed as eufunctional in that conflicts among subgroups may promote solidarity in some larger system. Gluckman (1955) exemplifies this viewpoint in chapter headings such as "The Peace in the Feud" and "The Bonds in the Colour-Bar". Coser exemplifies it for sociology; he (1956:80)

maintains, after Simmel, that social systems "tend to be sewn together by multiple and multiform conflict". Many of the anthropological explanations of conflict seem to rest on what Kenneth Boulding calls the functional fallacy: if a thing exists, it must be good for something. As Beals and Siegel point out, fights do not start because individuals wish to maintain the social system. Furthermore, there is always some point of view from which a conflict is desireable: "Presumably an atomic war on earth would be functional for Martians" (Beals and Siegel, p. 24).

Gluckman, Coser, and their students are modern interpreters of Simmel. Many of the contradictions which Beals and Siegel find in their writings stem from Simmel himself. Simmel dealt in epigrams and paradoxes; these are stimulating and thought-provoking, but Beals and Siegel (1966:83-84) are quite right to demand testable propositions. I doubt, for example, that it is generally true that "conflicts in one set of relationships lead to the establishment of cohesion in awider set of relationships" (Gluckman 1955:164). As Beals and Siegel (1966:24) point out, any conflict has functional and dysfunctional aspects; nevertheless:

> If conflict is described in terms of such concepts as breach, disruption, and regulation, it is possible to describe exactly what has been breached, disrupted, or regulated.

Another criticism which the authors level against followers of Simmel is a confusion of psychological distress and social conflict. This is in line with the authors' (1966:24) own preference for "social and cultural analysis", rather than psychological explanations. To be sure, some confusion of psychological and social variables is found in the writings of Simmel. Consider, for example, the following passage:

> A certain amount of discord, inner divergence and outer controversy, is organically tied up with the very elements that ultimately hold the group together... The positive and integrating role of antagonism is shown in structures which stand out by the sharpness and carefully preserved purity of their social divisions and gradations. Thus, the Hindu social system rests not only on hierachy, but also directly on mutual repulsion of the castes. (Simmel 1955:17-18, cited by Coser 1956:33).

Obviously, anthropologists are only interested in "mutual repulsion" where such feelings are part of a regular social pattern. This distinction is not always made clear in Simmel's work,

though I think it is maintained by most of his modern followers. Perhaps Beals and Siegel (1966:88) have some such distinction in mind when they warn against the "unsystematic mixing of levels". If they mean simply that sentiments are of interest in the anthropology of conflict only when they are recurrent and patterned, well and good. If, on the other hand, they intend that anthropology exclude sentiments in considering conflict, then I cannot accept their position. It is impossible to explain conflict without recourse to hostility, fear, and other sentiments. Gluckman (1955) repeatedly makes this point by example, and Lorenz would obviously endorse it. Beals and Siegel's (1966:26) pronouncement on the importance of sociological explanation could be interpreted in either of the above senses:

> The data we have accumulated so far appears to be amenable to social and cultural analysis rather than to a biological or physiological analysis. Perhaps other definitions will prove useful for other purposes.

In fact, the authors' position is ambiguous when speaking of anything but sociological explanations of conflict. For example, they say, "Aggression is a psychological motive that can be understood as aggression only when properly interpreted" (by an alter?). In the same passage, they cite with approval a paragraph from Simmel. This says, in part:

> No matter how much psychological autonomy one may be willing to grant the antagonistic drive, this autonomy is not enough to account for all phenomena involving hostility... (Simmel 1955: 33, cited by Beals and Siegel 1966:25)

Simmel's position, which the authors endorse, is unexceptionable. One can only add a warning from ethology: there is some danger of slighting human aggressiveness in explanations of conflict (cf. Coser 1956:48 49). Lorenz's discussion of "cabin fever", if nothing else, underscores the importance of what Simmel calls the "antagonistic drive".

If I may digress somewhat, the danger of discounting human antagonisms is greater in a related field, the "strategy of conflict" (Schelling 1963). This field applies the calculus of game theory to the study of "conscious, intelligent, sophisticated conflict behavior — of successful (winning) behavior" (Schelling 1963: 3). Clearly, the restriction of research to *successful* behavior in

conflicts is an arbitrary restriction of nature. To be sure, Schelling (1963:201; 248-249) acknowledges that conflict (war) can start inadvertently, through "faulty information, faulty communication, misunderstanding, misuse of authority, panic, or human or mechanical failure". With the exception of panic, all departures from rational, calculated competitive behavior are laid to communication breakdown. Experience testifies, however, that departures from a "rational" conflict strategy are often due to emotional reactions, particularly hostility, in the competitors. A boxer may become angry, a poker player overbet as a matter of pride, and a nation attempt to punish a hated rival. One searches in vain for a recognition of emotional reactions in the analysts of the strategy of conflict. What ethology might add to the view of these analysts is the recognition of the important part which emotional reaction, particularly aggressiveness, plays in conflict.

IV

As I noted above, Lorenz (1966:208) maintains that culture tends to shield man from the consequences of his aggressive actions. I believe that this statement has implications for further anthropological research. Anthropologists should study the ways in which culture shields man from the consequences of his aggressiveness. Patriotic myths, national legends, beliefs that others are less than human, all serve in this way. They all should be studied.

Furthermore, it should be recalled that studies of conflict are themselves part of culture. We should consider the possibility that studies of conflict may themselves help to shield men from the consequences of conflict. Here I am thinking particularly of studies based on the calculus of game theory. Anthropologists must be not simply "strategists of conflict", but students of conflict. These include the whole of conflicts, including irrational motives and pathological consequences. Ethology may be of help in this task by reminding us of the "animal origins and nature of man" (Ardry 1961). The anthropology of conflict must include all conflicts, from personal disputes to international wars. As the editor of a recent sourcebook on the anthropology of conflict

notes, warfare is little represented, and poorly done, in the ethnographic literature (Bohannan, 1967:xiii). And yet it must be done, if we are to have a relevant and humane science of man. Only by meeting the problems of warfare (and peace) head on can anthropology achieve its central task, "the creation of an image of man that will be adequate to the experience of our time" (Wolf 1964:94).

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