

On War and Peace in Animals and Man

An ethologist's approach to the biology of aggression.

N. Tinbergen

In 1935 Alexis Carrel published a best seller, *Man—The Unknown* (1). Today, more than 30 years later, we biologists have once more the duty to remind our fellowmen that in many respects we are still, to ourselves, unknown. It is true that we now understand a great deal of the way our bodies function. With this understanding came control: medicine.

The ignorance of ourselves which needs to be stressed today is ignorance about our behavior—lack of understanding of the causes and effects of the function of our brains. A scientific understanding of our behavior, leading to its control, may well be the most urgent task that faces mankind today. It is the effects of our behavior that begin to endanger the very survival of our species and, worse, of all life on earth. By our technological achievements we have attained a mastery of our environment that is without precedent in the history of life. But these achievements are rapidly getting out of hand. The consequences of our “rape of the earth” are now assuming critical proportions. With shortsighted recklessness we deplete the limited natural resources, including even the oxygen and nitrogen of our atmosphere (2). And Rachel Carson’s warning (3) is now being followed by those of scientists, who give us an even gloomier picture of the general pollution of air, soil, and water. This pollution is seriously threatening our health and our food supply. Refusal to curb our reproductive behavior has led to the population explosion. And, as if all this were not

enough, we are waging war on each other—men are fighting and killing men on a massive scale. It is because the effects of these behavior patterns, and of attitudes that determine our behavior, have now acquired such truly lethal potentialities that I have chosen man’s ignorance about his own behavior as the subject of this paper.

I am an ethologist, a zoologist studying animal behavior. What gives a student of animal behavior the temerity to speak about problems of human behavior? Of course the history of medicine provides the answer. We all know that medical research uses animals on a large scale. This makes sense because animals, particularly vertebrates, are, in spite of all differences, so similar to us; they are our blood relations, however distant.

But this use of zoological research for a better understanding of ourselves is, to most people, acceptable only when we have to do with those bodily functions that we look upon as parts of our physiological machinery—the functions, for instance, of our kidneys, our liver, our hormone-producing glands. The majority of people bridle as soon as it is even suggested that studies of animal behavior could be useful for an understanding, let alone for the control, of our own behavior. They do not want to have their own behavior subjected to scientific scrutiny; they certainly resent being compared with animals, and these rejecting attitudes are both deep-rooted and of complex origin.

But now we are witnessing a turn in this tide of human thought. On the one hand the resistances are weakening, and on the other, a positive awareness is growing of the potentialities of a biology of behavior. This has become quite clear from the great interest aroused by sev-

eral recent books that are trying, by comparative studies of animals and man, to trace what we could call “the animal roots of human behavior.” As examples I select Konrad Lorenz’s book *On Aggression* (4) and *The Naked Ape* by Desmond Morris (5). Both books were best sellers from the start. We ethologists are naturally delighted by this sign of rapid growth of interest in our science (even though the growing pains are at times a little hard to endure). But at the same time we are apprehensive, or at least I am.

We are delighted because, from the enormous sales of these and other such books, it is evident that the mental block against self-scrutiny is weakening—that there are masses of people who, so to speak, want to be shaken up.

But I am apprehensive because these books, each admirable in its own way, are being misread. Very few readers give the authors the benefit of the doubt. Far too many either accept uncritically all that the authors say, or (equally uncritically) reject it all. I believe that this is because both Lorenz and Morris emphasize our knowledge rather than our ignorance (and, in addition, present as knowledge a set of statements which are after all no more than likely guesses). In themselves brilliant, these books could stiffen, at a new level, the attitude of certainty, while what we need is a sense of doubt and wonder, and an urge to investigate, to inquire.

Potential Usefulness of Ethological Studies

Now, in a way, I am going to be just as assertative as Lorenz and Morris, but what I am going to stress is how much we do not know. I shall argue that we shall have to make a major research effort. I am of course fully aware of the fact that much research is already being devoted to problems of human, and even of animal, behavior. I know, for instance, that anthropologists, psychologists, psychiatrists, and others are approaching these problems from many angles. But I shall try to show that the research effort has so far made insufficient use of the potential of ethology. Anthropologists, for instance, are beginning to look at animals, but they restrict their work almost entirely to our nearest relatives, the apes and monkeys. Psychologists do study a larger variety of animals, but even they select mainly higher species. They also

Dr. Tinbergen is professor of animal behavior, Department of Zoology, University of Oxford, Oxford, England. This article is the text of his inaugural address, 27 February 1968.

ignore certain major problems that we biologists think have to be studied. Psychiatrists, at least many of them, show a disturbing tendency to apply the *results* rather than the *methods* of ethology to man.

None of these sciences, not even their combined efforts, are as yet parts of one coherent science of behavior. Since behavior is a life process, its study ought to be part of the mainstream of biological research. That is why we zoologists ought to "join the fray." As an ethologist, I am going to try to sketch how my science could assist its sister sciences in their attempts, already well on their way, to make a united, broad-fronted, truly biological attack on the problems of behavior.

I feel that I can cooperate best by discussing what it is in ethology that could be of use to the other behavioral sciences. What we ethologists do not want, what we consider definitely wrong, is uncritical application of our results to man. Instead, I myself at least feel that it is our method of approach, our rationale, that we can offer (6), and also a little simple common sense, and discipline.

The potential usefulness of ethology lies in the fact that, unlike other sciences of behavior, it applies the method or "approach" of biology to the phenomenon behavior. It has developed a set of concepts and terms that allow us to ask:

- 1) In what ways does this phenomenon (behavior) influence the survival, the success of the animal?

- 2) What makes behavior happen at any given moment? How does its "machinery" work?

- 3) How does the behavior machinery develop as the individual grows up?

- 4) How have the behavior systems of each species evolved until they became what they are now?

The first question, that of survival value, has to do with the effects of behavior; the other three are, each on a different time scale, concerned with its causes.

These four questions are, as many of my fellow biologists will recognize, the major questions that biology has been pursuing for a long time. What ethology is doing could be simply described by saying that, just as biology investigates the functioning of the organs responsible for digestion, respiration, circulation, and so forth, so ethology begins now to do the same with respect to behavior; it investigates the functioning of organs responsible for movement.

I have to make clear that in my opin-

ion it is the comprehensive, integrated attack on all four problems that characterizes ethology. I shall try to show that to ignore the questions of survival value and evolution—as, for instance, most psychologists do—is not only shortsighted but makes it impossible to arrive at an understanding of behavioral problems. Here ethology can make, in fact is already making, positive contributions.

Having stated my case for animal ethology as an essential part of the science of behavior, I will now have to sketch how this could be done. For this I shall have to consider one concrete example, and I select aggression, the most directly lethal of our behaviors. And, for reasons that will become clear, I shall also make a short excursion into problems of education.

Let me first try to define what I mean by aggression. We all understand the term in a vague, general way, but it is, after all, no more than a catchword. In terms of actual behavior, aggression involves approaching an opponent, and, when within reach, pushing him away, inflicting damage of some kind, or at least forcing stimuli upon him that subdue him. In this description the effect is already implicit: such behavior tends to remove the opponent, or at least to make him change his behavior in such a way that he no longer interferes with the attacker. The methods of attack differ from one species to another, and so do the weapons that are used, the structures that contribute to the effect.

Since I am concentrating on men fighting men, I shall confine myself to intraspecific fighting, and ignore, for instance, fighting between predators and prey. Intraspecific fighting is very common among animals. Many of them fight in two different contexts, which we can call "offensive" and "defensive." Defensive fighting is often shown as a last resort by an animal that, instead of attacking, has been fleeing from an attacker. If it is cornered, it may suddenly turn round upon its enemy and "fight with the courage of despair."

Of the four questions I mentioned before, I shall consider that of the survival value first. Here comparison faces us right at the start with a striking paradox. On the one hand, man is akin to many species of animals in that he fights his own species. But on the other hand he is, among the thousands of species that fight, the only one in which fighting is disruptive.

In animals, intraspecific fighting is usually of distinctive advantage. In ad-

dition, all species manage as a rule to settle their disputes without killing one another; in fact, even bloodshed is rare. Man is the only species that is a mass murderer, the only misfit in his own society.

Why should this be so? For an answer, we shall have to turn to the question of causation: What makes animals and man fight their own species? And why is our species "the odd man out"?

Causation of Aggression

For a fruitful discussion of this question of causation I shall first have to discuss what exactly we mean when we ask it.

I have already indicated that when thinking of causation we have to distinguish between three subquestions, and that these three differ from one another in the stretch of time that is considered. We ask, first: Given an adult animal that fights now and then, what makes each outburst of fighting happen? The time scale in which we consider these recurrent events is usually one of seconds, or minutes. To use an analogy, this subquestion compares with asking what makes a car start or stop each time we use it.

But in asking this same general question of causation ("What makes an animal fight?") we may also be referring to a longer period of time; we may mean "How has the animal, as it grew up, developed this behavior?" This compares roughly with asking how a car has been constructed in the factory. The distinction between these two subquestions remains useful even though we know that many animals continue their development (much slowed down) even after they have attained adulthood. For instance, they may still continue to learn.

Finally, in biology, as in technology, we can extend this time scale even more, and ask: How have the animal species which we observe today—and which we know have evolved from ancestors that were different—how have they acquired their particular behavior systems during this evolution? Unfortunately, while we know the evolution of cars because they evolved so quickly and have been so fully recorded, the behavior of extinct animals cannot be observed, and has to be reconstructed by indirect methods.

I shall try to justify the claim I made earlier, and show how all these four questions—that of behavior's survival

value and the three subquestions of causation—have to enter into the argument if we are to understand the biology of aggression.

Let us first consider the short-term causation; the mechanism of fighting. What makes us fight at any one moment? Lorenz argues in his book that, in animals and in man, there is an internal urge to attack. An individual does not simply wait to be provoked, but, if actual attack has not been possible for some time, this urge to fight builds up until the individual actively seeks the opportunity to indulge in fighting. Aggression, Lorenz claims, can be spontaneous.

But this view has not gone unchallenged. For instance, R. A. Hinde has written a thorough criticism (7), based on recent work on aggression in animals, in which he writes that Lorenz's "arguments for the spontaneity of aggression do not bear examination" and that "the contrary view, expressed in nearly every textbook of comparative psychology . . ." is that fighting "derives principally from the situation"; and even more explicitly: "There is no need to postulate causes that are purely internal to the aggressor" (7, p. 303). At first glance it would seem as if Lorenz and Hinde disagree profoundly. I have read and reread both authors, and it is to me perfectly clear that loose statements and misunderstandings on both sides have made it appear that there is disagreement where in fact there is something very near to a common opinion. It seems to me that the differences between the two authors lie mainly in the different ways they look at internal and external variables. This in turn seems due to differences of a semantic nature. Lorenz uses the unfortunate term "the spontaneity of aggression." Hinde takes this to mean that external stimuli are in Lorenz's view not necessary at all to make an animal fight. But here he misrepresents Lorenz, for nowhere does Lorenz claim that the internal urge ever makes an animal fight "in vacuo"; somebody or something is attacked. This misunderstanding makes Hinde feel that he has refuted Lorenz's views by saying that "fighting derives principally from the situation." But both authors are fully aware of the fact that fighting is started by a number of variables, of which some are internal and some external. What both authors know, and what cannot be doubted, is that fighting behavior is not like the simple slot machine that produces one plat-form ticket every time one threepenny

bit is inserted. To mention one animal example: a male stickleback does not always show the full fighting behavior in response to an approaching standard opponent; its response varies from none at all to the optimal stimulus on some occasions, to full attack on even a crude dummy at other times. This means that its internal state varies, and in this particular case we know from the work of Hoar (8) that the level of the male sex hormone is an important variable.

Another source of misunderstanding seems to have to do with the stretch of time that the two authors are taking into account. Lorenz undoubtedly thinks of the causes of an outburst of fighting in terms of seconds, or hours—perhaps days. Hinde seems to think of events which may have happened further back in time; an event which is at any particular moment "internal" may well in its turn have been influenced previously by external agents. In our stickleback example, the level of male sex hormone is influenced by external agents such as the length of the daily exposure to light over a period of a month or so (9). Or, less far back in time, its readiness to attack may have been influenced by some experience gained, say, half an hour before the fight.

I admit that I have now been spending a great deal of time on what would seem to be a perfectly simple issue: the very first step in the analysis of the short-term causation, which is to distinguish at any given moment between variables within the animal and variables in the environment. It is of course important for our further understanding to unravel the complex interactions between these two worlds, and in particular the physiology of aggressive behavior. A great deal is being discovered about this, but for my present issue there is no use discussing it as long as even the first step in the analysis has not led to a clearly expressed and generally accepted conclusion. We must remember that we are at the moment concerned with the human problem: "What makes men attack each other?" And for this problem the answer to the first stage of our question is of prime importance: Is our readiness to start an attack constant or not? If it were—if our aggressive behavior were the outcome of an apparatus with the properties of the slot machine—all we would have to do would be to control the external situation: to stop providing threepenny bits. But since our readiness to start an attack is variable, further stud-

ies of both the external and the internal variables are vital to such issues as: Can we reduce fighting by lowering the population density, or by withholding provocative stimuli? Can we do so by changing the hormone balance or other physiological variables? Can we perhaps in addition control our development in such a way as to change the dependence on internal and external factors in adult man? However, before discussing development, I must first return to the fact that I have mentioned before, namely, that man is, among the thousands of other species that fight, the only mass murderer. How do animals in their intraspecific disputes avoid bloodshed?

The Importance of "Fear"

The clue to this problem is to recognize the simple fact that aggression in animals rarely occurs in pure form; it is only one of two components of an adaptive system. This is most clearly seen in territorial behavior, although it is also true of most other types of hostile behavior. Members of territorial species divide, among themselves, the available living space and opportunities by each individual defending its home range against competitors. Now in this system of parceling our living space, avoidance plays as important a part as attack. Put very briefly, animals of territorial species, once they have settled on a territory, attack intruders, but an animal that is still searching for a suitable territory or finds itself outside its home range withdraws when it meets with an already established owner. In terms of function, once you have taken possession of a territory, it pays to drive off competitors; but when you are still looking for a territory (or meet your neighbor at your common boundary), your chances of success are improved by avoiding such established owners. The ruthless fighter who "knows no fear" does not get very far. For an understanding of what follows, this fact, that hostile clashes are controlled by what we could call the "attack-avoidance system," is essential.

When neighboring territory owners meet near their common boundary, both attack behavior and withdrawal behavior are elicited in both animals; each of the two is in a state of motivational conflict. We know a great deal about the variety of movements that appear when these two conflicting, incompatible behaviors are elicited. Many of these ex-

pressions of a motivational conflict have, in the course of evolution, acquired signal function; in colloquial language, they signal "Keep out!" We deduce this from the fact that opponents respond to them in an appropriate way: instead of proceeding to intrude, which would require the use of force, trespassers withdraw, and neighbors are contained by each other. This is how such animals have managed to have all the advantages of their hostile behavior without the disadvantages: they divide their living space in a bloodless way by using as distance-keeping devices these conflict movements ("threat") rather than actual fighting.

Group Territories

In order to see our wars in their correct biological perspective one more comparison with animals is useful. So far I have discussed animal species that defend individual or at best pair territories. But there are also animals which possess and defend territories belonging to a group, or a clan (10).

Now it is an essential aspect of group territorialism that the members of a group unite when in hostile confrontation with another group that approaches, or crosses into their feeding territory. The uniting and the aggression are equally important. It is essential to realize that group territorialism does not exclude hostile relations on lower levels when the group is on its own. For instance, within a group there is often a peck order. And within the group there may be individual or pair territories. But frictions due to these relationships fade away during a clash between groups. This temporary elimination is done by means of so-called appeasement and reassurance signals. They indicate "I am a friend," and so diminish the risk that, in the general flare-up of anger, any animal "takes it out" on a fellow member of the same group (11). Clans meet clans as units, and each individual in an intergroup clash, while united with its fellow-members, is (as in interindividual clashes) torn between attack and withdrawal, and postures and shouts rather than attacks.

We must now examine the hypothesis (which I consider the most likely one) that man still carries with him the animal heritage of group territoriality. This is a question concerning man's evolutionary origin, and here we are, by the very nature of the subject, forced to

speculate. Because I am going to say something about the behavior of our ancestors of, say, 100,000 years ago, I have to discuss briefly a matter of methodology. It is known to all biologists (but unfortunately unknown to most psychologists) that comparison of present-day species can give us a deep insight, with a probability closely approaching certainty, into the evolutionary history of animal species. Even where fossil evidence is lacking, this comparative method alone can do this. It has to be stressed that this comparison is a highly sophisticated method, and not merely a matter of saying that species A is different from species B (12). The basic procedure is this. We interpret differences between really allied species as the result of adaptive divergent evolution from common stock, and we interpret similarities between nonallied species as adaptive convergencies to similar ways of life. By studying the adaptive functions of species characteristics we understand how natural selection can have produced both these divergencies and convergencies. To mention one striking example: even if we had no fossil evidence, we could, by this method alone, recognize whales for what they are—mammals that have returned to the water, and, in doing so, have developed some similarities to fish. This special type of comparison, which has been applied so successfully by students of the structure of animals, has now also been used, and with equal success, in several studies of animal behavior. Two approaches have been applied. One is to see in what respects species of very different origin have convergently adapted to a similar way of life. Von Haartman (13) has applied this to a study of birds of many types that nest in holes—an anti-predator safety device. All such hole-nesters center their territorial fighting on a suitable nest hole. Their courtship consists of luring a female to this hole (often with the use of bright color patterns). Their young gape when a general darkening signals the arrival of the parent. All but the most recently adapted species lay uniformly colored, white or light blue eggs that can easily be seen by the parent.

An example of adaptive divergence has been studied by Cullen (14). Among all the gulls, the kittiwake is unique in that it nests on very narrow ledges on sheer cliffs. Over 20 peculiarities of this species have been recognized by Mrs. Cullen as vital adaptations to this particular habitat.

These and several similar studies (15) demonstrate how comparison reveals, in each species, systems of interrelated, and very intricate adaptive features. In this work, speculation is now being followed by careful experimental checking. It would be tempting to elaborate on this, but I must return to our own unfortunate species.

Now, when we include the "Naked Ape" in our comparative studies, it becomes likely (as has been recently worked out in great detail by Morris) that man is a "social Ape who has turned carnivore" (16). On the one hand he is a social primate; on the other, he has developed similarities to wolves, lions and hyenas. In our present context one thing seems to stand out clearly, a conclusion that seems to me of paramount importance to all of us, and yet has not yet been fully accepted as such. As a social, hunting primate, man must originally have been organized on the principle of group territories.

Ethologists tend to believe that we still carry with us a number of behavioral characteristics of our animal ancestors, which cannot be eliminated by different ways of upbringing, and that our group territorialism is one of those ancestral characters. I shall discuss the problem of the modifiability of our behavior later, but it is useful to point out here that even if our behavior were much more modifiable than Lorenz maintains, our cultural evolution, which resulted in the parceling-out of our living space on lines of tribal, national, and now even "bloc" areas, would, if anything, have tended to enhance group territorialism.

Group Territorialism in Man?

I put so much emphasis on this issue of group territorialism because most writers who have tried to apply ethology to man have done this in the wrong way. They have made the mistake, to which I objected before, of uncritically extrapolating the results of animal studies to man. They try to explain man's behavior by using facts that are valid only of some of the animals we studied. And, as ethologists keep stressing, no two species behave alike. Therefore, instead of taking this easy way out, we ought to study man in his own right. And I repeat that the message of the ethologists is that the methods, rather than the results, of ethology should be used for such a study.

Now, the notion of territory was developed by zoologists (to be precise, by ornithologists, 17), and because individual and pair territories are found in so many more species than group territories (which are particularly rare among birds), most animal studies were concerned with such individual and pair territories. Now such low-level territories do occur in man, as does another form of hostile behavior, the peck order. But the problems created by such low-level frictions are not serious; they can, within a community, be kept in check by the apparatus of law and order; peace within national boundaries can be enforced. In order to understand what makes us go to war, we have to recognize that man behaves very much like a group-territorial species. We too unite in the face of an outside danger to the group; we "forget our differences." We too have threat gestures, for instance, angry facial expressions. And all of us use reassurance and appeasement signals, such as a friendly smile. And (unlike speech) these are universally understood; they are cross-cultural; they are species-specific. And, incidentally, even within a group sharing a common language, they are often more reliable guides to a man's intentions than speech, for speech (as we know now) rarely reflects our true motives, but our facial expressions often "give us away."

If I may digress for a moment: it is humiliating to us ethologists that many nonscientists, particularly novelists and actors, intuitively understand our sign language much better than we scientists ourselves do. Worse, there is a category of human beings who understand intuitively more about the causation of our aggressive behavior: the great demagogues. They have applied this knowledge in order to control our behavior in the most clever ways, and often for the most evil purposes. For instance, Hitler (who had modern mass communication at his disposal, which allowed him to inflame a whole nation) played on both fighting tendencies. The "defensive" fighting was whipped up by his passionate statements about "living space," "encirclement," Jewry, and Freemasonry as threatening powers which made the Germans feel "cornered." The "attack fighting" was similarly set ablaze by playing the myth of the *Herrenvolk*. We must make sure that mankind has learned its lesson and will never forget how disastrous the joint effects have been—if only one of the major nations were led now by a

man like Hitler, life on earth would be wiped out.

I have argued my case for concentrating on studies of group territoriality rather than on other types of aggression. I must now return, in this context, to the problem of man the mass murderer. Why don't we settle even our international disputes by the relatively harmless, animal method of threat? Why have we become unhinged so that so often our attack erupts without being kept in check by fear? It is not that we have no fear, nor that we have no other inhibitions against killing. This problem has to be considered first of all in the general context of the consequences of man having embarked on a new type of evolution.

Cultural Evolution

Man has the ability, unparalleled in scale in the animal kingdom, of passing on his experiences from one generation to the next. By this accumulative and exponentially growing process, which we call cultural evolution, he has been able to change his environment progressively out of all recognition. And this includes the social environment. This new type of evolution proceeds at an incomparably faster pace than genetic evolution. Genetically we have not evolved very strikingly since Cro-Magnon man, but culturally we have changed beyond recognition, and are changing at an ever-increasing rate. It is of course true that we are highly adjustable individually, and so could hope to keep pace with these changes. But I am not alone in believing that this behavioral adjustability, like all types of modifiability, has its limits. These limits are imposed upon us by our hereditary constitution, a constitution which can only change with the far slower speed of genetic evolution. There are good grounds for the conclusion that man's limited behavioral adjustability has been outpaced by the culturally determined changes in his social environment, and that this is why man is now a misfit in his own society.

We can now, at last, return to the problem of war, of uninhibited mass killing. It seems quite clear that our cultural evolution is at the root of the trouble. It is our cultural evolution that has caused the population explosion. In a nutshell, medical science, aiming at the reduction of suffering, has, in doing so, prolonged life for many individuals as well—prolonged it to well

beyond the point at which they produce offspring. Unlike the situation in any wild species, recruitment to the human population consistently surpasses losses through mortality. Agricultural and technical know-how have enabled us to grow food and to exploit other natural resources to such an extent that we can still feed (though only just) the enormous numbers of human beings on our crowded planet. The result is that we now live at a far higher density than that in which genetic evolution has molded our species. This, together with long-distance communication, leads to far more frequent, in fact to continuous, intergroup contacts, and so to continuous external provocation of aggression. Yet this alone would not explain our increased tendency to kill each other; it would merely lead to continuous threat behavior.

The upsetting of the balance between aggression and fear (and this is what causes war) is due to at least three other consequences of cultural evolution. It is an old cultural phenomenon that warriors are both brainwashed and bullied into all-out fighting. They are brainwashed into believing that fleeing—originally, as we have seen, an adaptive type of behavior—is despicable, "cowardly." This seems to me due to the fact that man, accepting that in moral issues death might be preferable to fleeing, has falsely applied the moral concept of "cowardice" to matters of mere practical importance—to the dividing of living space. The fact that our soldiers are also bullied into all-out fighting (by penalizing fleeing in battle) is too well known to deserve elaboration.

Another cultural excess is our ability to make and use killing tools, especially long-range weapons. These make killing easy, not only because a spear or a club inflicts, with the same effort, so much more damage than a fist, but also, and mainly, because the use of long-range weapons prevents the victim from reaching his attacker with his appeasement, reassurance, and distress signals. Very few aircrews who are willing, indeed eager, to drop their bombs "on target" would be willing to strangle, stab, or burn children (or, for that matter, adults) with their own hands; they would stop short of killing, in response to the appeasement and distress signals of their opponents.

These three factors alone would be sufficient to explain how we have become such unhinged killers. But I have to stress once more that all this, how-

ever convincing it may seem, must still be studied more thoroughly.

There is a frightening, and ironical paradox in this conclusion: that the human brain, the finest life-preserving device created by evolution, has made our species so successful in mastering the outside world that it suddenly finds itself taken off guard. One could say that our cortex and our brainstem (our "reason" and our "instincts") are at loggerheads. Together they have created a new social environment in which, rather than ensuring our survival, they are about to do the opposite. The brain finds itself seriously threatened by an enemy of its own making. It is its own enemy. We simply have to understand this enemy.

The Development of Behavior

I must now leave the question of the moment-to-moment control of fighting, and, looking further back in time, turn to the development of aggressive behavior in the growing individual. Again we will start from the human problem. This, in the present context, is whether it is within our power to control development in such a way that we reduce or eliminate fighting among adults. Can or cannot education in the widest sense produce nonaggressive men?

The first step in the consideration of this problem is again to distinguish between external and internal influences, but now we must apply this to the growth, the changing, of the behavioral machinery during the individual's development. Here again the way in which we phrase our questions and our conclusions is of the utmost importance.

In order to discuss this issue fruitfully, I have to start once more by considering it in a wider context, which is now that of the "nature-nurture" problem with respect to behavior in general. This has been discussed more fully by Lorenz in his book *Evolution and Modification of Behaviour* (18); for a discussion of the environmentalist point of view I refer to the various works of Schneirla (see 19).

Lorenz tends to classify behavior types into innate and acquired or learned behavior. Schneirla rejects this dichotomy into two classes of behavior. He stresses that the developmental process, of behavior as well as of other functions, should be considered, and also that this development forms a highly complicated series of interactions between the growing organism and its

environment. I have gradually become convinced that the clue to this difference in approach is to be found in a difference in aims between the two authors. Lorenz claims that "we are justified in leaving, at least for the time being, to the care of the experimental embryologists all those questions which are concerned with the chains of physiological causation leading from the genome to the development of . . . neurosensory structures" (18, p. 43). In other words, he deliberately refrains from starting his analysis of development prior to the stage at which a fully coordinated behavior is performed for the first time. If one in this way restricts one's studies to the later stages of development, then a classification in "innate" and "learned" behavior, or behavior components, can be considered quite justified. And there was a time, some 30 years ago, when the almost grotesquely environmentalist bias of psychology made it imperative for ethologists to stress the extent to which behavior patterns could appear in perfect or near-perfect form without the aid of anything that could be properly called learning. But I now agree (however belatedly) with Schneirla that we must extend our interest to earlier stages of development and embark on a full program of experimental embryology of behavior. When we do this, we discover that interactions with the environment can indeed occur at early stages. These interactions may concern small components of the total machinery of a fully functional behavior pattern, and many of them cannot possibly be called learning. But they are interactions with the environment, and must be taken into account if we follow in the footsteps of the experimental embryologists, and extend our field of interest to the entire sequence of events which lead from the blueprints contained in the zygote to the fully functioning, behaving animal. We simply have to do this if we want an answer to the question to what extent the development of behavior can be influenced from the outside.

When we follow this procedure the rigid distinction between "innate" or unmodifiable and "acquired" or modifiable behavior patterns becomes far less sharp. This is owing to the discovery, on the one hand, that "innate" patterns may contain elements that at an early stage developed in interaction with the environment, and, on the other hand, that learning is, from step to step, limited by internally imposed restrictions.

To illustrate the first point, I take the development of the sensory cells in the retina of the eye. Knoll has shown (20) that the rods in the eyes of tadpoles cannot function properly unless they have first been exposed to light. This means that, although any visually guided response of a tadpole may well, in its integrated form, be "innate" in Lorenz's sense, it is so only in the sense of "nonlearned," not in that of "having grown without interaction with the environment." Now it has been shown by Cullen (21) that male sticklebacks reared from the egg in complete isolation from other animals will, when adult, show full fighting behavior to other males and courtship behavior to females when faced with them for the first time in their lives. This is admittedly an important fact, demonstrating that the various recognized forms of learning do not enter into the programming of these integrated patterns. This is a demonstration of what Lorenz calls an "innate response." But it does not exclude the possibility that parts of the machinery so employed may, at an earlier stage, have been influenced by the environment, as in the case of the tadpoles.

Second, there are also behavior patterns which do appear in the inexperienced animal, but in an incomplete form, and which require additional development through learning. Thorpe has analyzed a clear example of this: when young male chaffinches reared alone begin to produce their song for the first time, they utter a very imperfect warble; this develops into the full song only if, at a certain sensitive stage, the young birds have heard the full song of an adult male (22).

By far the most interesting aspect of such intermediates between innate and acquired behavior is the fact that learning is not indiscriminate, but is guided by a certain selectiveness on the part of the animal. This fact has been dimly recognized long ago; the early ethologists have often pointed out that different, even closely related, species learn different things even when developing the same behavior patterns. This has been emphasized by Lorenz's use of the term "innate teaching mechanism." Other authors use the word "template" in the same context. The best example I know is once more taken from the development of song in certain birds. As I have mentioned, the males of some birds acquire their full song by changing their basic repertoire to resemble the song of adults, which they

have to hear during a special sensitive period some months before they sing themselves. It is in this sensitive period that they acquire, without as yet producing the song, the knowledge of "what the song ought to be like." In technical terms, the bird formed a *Sollwert* (23) (literally, "should-value," an ideal) for the feedback they receive when they hear their own first attempts. Experiments have shown (24) that such birds, when they start to sing, do three things: they listen to what they produce; they notice the difference between this feedback and the ideal song; and they correct their next performance.

This example, while demonstrating an internal teaching mechanism, shows, at the same time, that Lorenz made his concept too narrow when he coined the term "innate teaching mechanism." The birds have developed a teaching mechanism, but while it is true that it is internal, it is not innate; the birds have acquired it by listening to their father's song.

These examples show that if behavior studies are to catch up with experimental embryology our aims, our concepts, and our terms must be continually revised.

Before returning to aggression, I should like to elaborate a little further on general aspects of behavior development, because this will enable me to show the value of animal studies in another context, that of education.

Comparative studies, of different animal species, of different behavior patterns, and of different stages of development, begin to suggest that wherever learning takes a hand in development, it is guided by such *Sollwerte* or templates for the proper feedback, the feedback that reinforces. And it becomes clear that these various *Sollwerte* are of a bewildering variety. In human education one aspect of this has been emphasized in particular, and even applied in the use of teaching machines: the requirement that the reward, in order to have maximum effect, must be immediate. Skinner has stressed this so much because in our own teaching we have imposed an unnatural delay between, say, taking in homework, and giving the pupil his reward in the form of a mark. But we can learn more from animal studies than the need for immediacy of reward. The type of reward is also of great importance, and this may vary from task to task, from stage to stage, from occasion to occasion; the awards may be of almost infinite variety.

Here I have to discuss briefly a

behavior of which I have so far been unable to find the equivalent in the development of structure. This is exploratory behavior. By this we mean a kind of behavior in which the animal sets out to acquire as much information about an object or a situation as it can possibly get. The behavior is intricately adapted to this end, and it terminates when the information has been stored, when the animal has incorporated it in its learned knowledge. This exploration (subjectively we speak of "curiosity") is not confined to the acquisition of information about the external world alone; at least mammals explore their own movements a great deal, and in this way "master new skills." Again, in this exploratory behavior, *Sollwerte* of expected, "hoped-for" feedbacks play their part.

Without going into more detail, we can characterize the picture we begin to get of the development of behavior as a series, or rather a web, of events, starting with innate programming instructions contained in the zygote, which straightaway begin to interact with the environment; this interaction may be discontinuous, in that periods of predominantly internal development alternate with periods of interaction, or sensitive periods. The interaction is enhanced by active exploration; it is steered by selective *Sollwerte* of great variety; and stage by stage this process ramifies; level upon level of ever-increasing complexity is being incorporated into the programming.

Apply what we have heard for a moment to playing children (I do not, of course, distinguish sharply between "play" and "learning"). At a certain age a child begins to use, say, building blocks. It will at first manipulate them in various ways, one at a time. Each way of manipulating acts as exploratory behavior: the child learns what a block looks, feels, tastes like, and so forth, and also how to put it down so that it stands stably.

Each of these stages "peters out" when the child knows what it wanted to find out. But as the development proceeds, a new level of exploration is added: the child discovers that it can put one block on top of the other; it constructs. The new discovery leads to repetition and variation, for each child develops, at some stage, a desire and a set of *Sollwerte* for such effects of construction, and acts out to the full this new level of exploratory behavior. In addition, already at this stage the *Sollwert* or ideal does not merely con-

tain what the blocks do, but also what, for instance, the mother does; her approval, her shared enjoyment, is also of great importance. Just as an exploring animal, the child builds a kind of inverted pyramid of experience, built of layers, each set off by a new wave of exploration and each directed by new sets of *Sollwerte*, and so its development "snowballs." All these phases may well have more or less limited sensitive periods, which determine when the fullest effect can be obtained, and when the child is ready for the next step. More important still, if the opportunity for the next stage is offered either too early or too late, development may be damaged, including the development of motivational and emotional attitudes.

Of course gifted teachers of many generations have known all these things (25) or some of them, but the glimpses of insight have not been fully and scientifically systematized. In human education, this would of course involve experimentation. This need not worry us too much, because in our search for better educational procedures we are in effect experimenting on our children all the time. Also, children are fortunately incredibly resilient, and most grow up into pretty viable adults in spite of our fumbling educational efforts. Yet there is, of course, a limit to what we will allow ourselves, and this, I should like to emphasize, is where animal studies may well become even more important than they are already.

Can Education End Aggression?

Returning now to the development of animal and human aggression, I hope to have made at least several things clear: that behavior development is a very complex phenomenon indeed; that we have only begun to analyze it in animals; that with respect to man we are, if anything, behind in comparison with animal studies; and that I cannot do otherwise than repeat what I said in the beginning: we must make a major research effort. In this effort animal studies can help, but we are still very far from drawing very definite conclusions with regard to our question: To what extent shall we be able to render man less aggressive through manipulation of the environment, that is, by educational measures?

In such a situation personal opinions naturally vary a great deal. I do not hesitate to give as my personal opinion that Lorenz's book *On Aggression*, in

spite of its assertativeness, in spite of factual mistakes, and in spite of the many possibilities of misunderstandings that are due to the lack of a common language among students of behavior—that this work must be taken more seriously as a positive contribution to our problem than many critics have done. Lorenz is, in my opinion, right in claiming that elimination, through education, of the internal urge to fight will turn out to be very difficult, if not impossible.

Everything I have said so far seems to me to allow for only one conclusion. Apart from doing our utmost to return to a reasonable population density, apart from stopping the progressive depletion and pollution of our habitat, we must pursue the biological study of animal behavior for clarifying problems of human behavior of such magnitude as that of our aggression, and of education.

But research takes a long time, and we must remember that there are experts who forecast worldwide famine 10 to 20 years from now; and that we have enough weapons to wipe out all human life on earth. Whatever the causation of our aggression, the simple fact is that for the time being we are saddled with it. This means that there is a crying need for a crash program, for finding ways and means for keeping our intergroup aggression in check. This is of course in practice infinitely more difficult than controlling our intranational frictions; we have as yet not got a truly international police force. But there is hope for avoiding all-out war because, for the first time in history, we are afraid of killing ourselves by the lethal radiation effects even of bombs that we could drop in the enemy's territory. Our politicians know this. And as long as there is this hope, there is every reason to try and learn what we can from animal studies. Here again they can be of help. We have already seen that animal opponents meeting in a hostile clash avoid bloodshed by using the expressions of their motivational conflicts as intimidating signals. Ethologists have studied such conflict movements in some detail (26), and have found that they are of a variety of types. The most instructive of these is the redirected attack; instead of attacking the provoking, yet dreaded, opponent, animals often attack something else, often even an inanimate object. We ourselves bang the table with our fists. Redirection includes

something like sublimation, a term attaching a value judgment to the redirection. As a species with group territories, humans, like hyenas, unite when meeting a common enemy. We do already sublimate our group aggression. The Dutch feel united in their fight against the sea. Scientists do attack their problems together. The space program—surely a mainly military effort—is an up-to-date example. I would not like to claim, as Lorenz does, that redirected attack exhausts the aggressive urge. We know from soccer matches and from animal work how aggressive behavior has two simultaneous, but opposite effects: a waning effect, and one of self-inflammation, of mass hysteria, such as recently seen in Cairo. Of these two the inflammatory effect often wins. But if aggression were used successfully as the motive force behind nonkilling and even useful activities, self-stimulation need not be a danger; in our short-term cure we are not aiming at the elimination of aggressiveness, but at “taking the sting out of it.”

Of all sublimated activities, scientific research would seem to offer the best opportunities for deflecting and sublimating our aggression. And, once we recognize that it is the disrupted relation between our own behavior and our environment that forms our most deadly enemy, what could be better than uniting, at the front or behind the lines, in the scientific attack on our own behavioral problems?

I stress “behind the lines.” The whole population should be made to feel that it participates in the struggle. This is why scientists will always have the duty to inform their fellowmen of what they are doing, of the relevance and the importance of their work. And this is not only a duty, it can give intense satisfaction.

I have come full circle. For both the long-term and the short-term remedies at least we scientists will have to sublimate our aggression into an all-out attack on the enemy within. For this the enemy must be recognized for what it is: our unknown selves, or, deeper down, our refusal to admit that man is, to himself, unknown.

I should like to conclude by saying a few words to my colleagues of the younger generation. Of course we all hope that, by muddling along until we have acquired better understanding, self-annihilation either by the “whimper of famine” or by the “bang of war” can be avoided. For this, we must on the

one hand trust, on the other help (and urge) our politicians. But it is no use denying that the chances of designing the necessary preventive measures are small, let alone the chances of carrying them out. Even birth control still offers a major problem.

It is difficult for my generation to know how seriously you take the danger of mankind destroying his own species. But those who share the apprehension of my generation might perhaps, with us, derive strength from keeping alive the thought that has helped so many of us in the past when faced with the possibility of imminent death. Scientific research is one of the finest occupations of our mind. It is, with art and religion, one of the uniquely human ways of meeting nature, in fact, the most active way. If we are to succumb, and even if this were to be ultimately due to our own stupidity, we could still, so to speak, redeem our species. We could at least go down with some dignity, by using our brain for one of its supreme tasks, by exploring to the end.

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