

Nature/Nurture Revisited I:

A REVIEW OF THE BIOLOGICAL BASES OF CONFLICT

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Biologically oriented approaches to the study of human conflict have thus far been limited largely to the study of aggression. A sample of the literature on this topic is reviewed, drawing upon four major approaches: comparative psychology, ethology (including some popularized accounts), evolutionary-based theories, and several areas of human physiology. More sophisticated relationships between so-called "innate" and "acquired" determinants of behavior are discussed, along with the proper relevance of animal behavior studies for human behavior. Unless contained in a comprehensive theory which includes social and psychological variables, biologically oriented theories (although often valid within their domain) offer at best severely limited and at worst highly misleading explanations of complex social conflicts. The review concludes with a list of several positive contributions of these biological approaches and suggests that social scientists must become more knowledgeable about them.

Until recently, social scientists—including those interested in social conflict—have been able to ignore with impunity biological approaches to social and political phenomena. Perhaps it is a measure of the lack of cross-disciplinary communication that so many of us could have considered the spate of best-selling books by Konrad Lorenz, Robert Ardrey, Desmond Morris, and Lionel Tiger a mere passing fad. In fact, they were the popularized tip of a very serious iceberg, for there has been emerging over the past decade or two a growing set of interrelated fields which have developed biologically based theories of human behavior, steadily spreading into the social sciences, with apparent relevance for the study of

conflict. Such fields as ethology, neuropsychology, psychopharmacology, behavior genetics, evolutionary genetics, and physical anthropology are finding increasing application in areas of the social sciences where social, political, historical, and psychological concepts have traditionally held sway.

In various ways and to different degrees, these fields share Dennis Wrong's (1963: 78) conviction that "we must start with the recognition that *in the beginning there is the body.*" But, Wrong continues, speaking of his own discipline, "as soon as the body is mentioned the specter of 'biological determinism' raises its head and sociologists draw back in fright." Sociologists have not been alone, but the reactions to the biological work have been not so much fear as either carefully reasoned argument or rhetorical hysteria, as well as a variety of intermediate responses. For the dialogue has stirred up the old "nature-nurture" controversy again, this time in more sophisticated form. While both sides now agree that the old form of the controversy is dead and that human behavior is the result of complex interactions between both biological givens and environmental influences, "the extent of disagreement about the modes of interaction and the weight to be given to different elements involved is so sharp and extensive as to constitute almost a 'nature-advocate'-'nurture-advocate' polarity, with a variety of intermediate positions located on the intervening continuum" (Willhoite, 1971: 620). However, all writers, regardless of their predilections, agree on the importance of the issue: that our survival as a species, or the form that survival takes, depends upon our understanding far better than we do now the nature of human beings and the determinants of their behavior (Eisenberg, 1972).

The entree of biological approaches to human conflict is by means of the concept of aggression. This review, therefore, must be at once both narrower and broader than my title implies—narrower because the biologically oriented literature focuses almost entirely on aggression, and this concept is only one small part, theoretically speaking, of conflict; and broader because often theoretical and methodological principles that apply to behavior in general must be clarified. (Wherever possible, I will try to broaden the discussion to considerations of conflict, not just aggression.) However, the biological literature even on aggression alone is enormous, and, as a result, in order to acquaint the reader with the breadth and scope of these approaches, some depth of review must be sacrificed. Consequently, the coverage of some material will seem sketchy, shallow, and abrupt. In evaluating the biological approaches, I have tried to be sympathetic but critical. I see my role not as one of merely passing facts and ideas

uncritically on to the reader, but rather of pointing out both strengths and weaknesses of both the biologically oriented approaches and those of their critics.

Trying to define aggression is often difficult and confusing. Many uses of the term "utterly mix cause and effect, symbol and reality, fact and value" (Davies, 1970: 612). Further, there is little agreement on the scope of definition. Some writers define aggression fairly narrowly (Davies, 1970; Bandura, 1973), while others define it so broadly that it nearly becomes synonymous with "assertiveness" or "achievement-seeking" (Storr, 1968; Corning, 1971; Willhoite, 1971). The broad definitions have several disadvantages. First, they require the concept to do too much, to cover too many kinds of referents, and as a result they carry a lot of connotative "excess baggage." Second, they prejudge empirical questions which demand data, rather than definitional fiat, for their resolution; for example, whether the sources of "aggression" in the narrow sense (attack with injury or damage intended) can, if redirected, lead to achievement behavior, mastery of the environment, assertiveness, and so on. Third, they are more prone to careless use and resultant confusion of cause and effect.

Davies' (1970: 613) definition of aggression, which avoids such difficulties, concentrates only on effects: "an act done with the intent to injure person[s] or damage property." (Violence is synonymous with aggression for Davies.) He defines "aggressiveness" as "the tendency to commit aggression, the tendency to do injury or damage." I will follow these definitions in this review.

It is generally agreed that, among nonhuman animals, predation is not considered aggression. (This latter term is usually reserved for intraspecies attacks.) However, for humans, it is often difficult to know where to draw the line between aggressive and nonaggressive killing, or in some cases even between predation and aggressive killing—for example, hunting for game (Rapoport, 1965).

This review will discuss biologically oriented approaches to aggression in the fields of comparative psychology, ethology, evolution and evolutionary genetics, and several fields dealing with human physiology. The applicability of these approaches to the problem of war will be considered, and the paper will conclude with an overall evaluation of the contributions and limitations of such approaches.

EVIDENCE FROM NONHUMAN SPECIES

In the language of nature-hating, we are informed that man is an animal because of certain black-sheep traits, like insanity in the family closet. He

shares with the beasts: brutality, violence, and inevitable death. Against that 'biological' side of human nature, sensibility, compassion, language, and spirituality are claimed as peculiarly human. . . . [However,] the notion belongs to a particular history of ideas rather than to life's immutable realities [Shepard and McKinley, 1969: 99].

It seems rather hard on other animals to project the failings of mankind upon them, and then blame them for having bequeathed those failings to us. Yet this kind of protective anthropomorphism . . . constitutes an only too common rationalization for the deplorabilities of human behavior [Montagu, 1973a: xvi-xvii].

The belief has persisted, with varying degrees of sophistication, that the study of "animal" (i.e., nonhuman species) behavior will somehow shed light on human behavior. In this section, I will deal with two different approaches to this assumption: comparative psychology and ethology. Considerable material which is relevant to both fields—and, of course, to human behavior—will be discussed in the latter section, so it should be emphasized that the relative length of the two sections does not necessarily reflect on the fields' respective contributions. Although technically incorrect, for ease of exposition I will frequently use the more colloquial term "animal" to mean nonhuman species.

EVIDENCE FROM COMPARATIVE PSYCHOLOGY

Traditional assumptions. Until the recent challenge by ethology, comparative psychologists shared certain assumptions regarding phylogeny, cross-species comparisons, behavior acquisition, and methodology. Some of the major ones have been summarized by Lockard (1971: 169-170) in a somewhat overstated but not entirely unfair fashion, some of which are as follows:

(1) There is a phylogenetic scale, a sort of linear arrangement from simple to complex, from unintelligent to intelligent, from amoeba to man.

(4) Because of the scale, animals lower in the scale are increasingly simpler but not different in kind. A white rat is a simple version of a human.

(5) Learning is the key to animal behavior because most behavior is acquired. Hunger, thirst, sex, respiration, and a thing or two more may be built in as initial tendencies, but these few things are merely the unconditioned responses on which behavior is built.

(6) Because so little is built into animals, genetics and evolution are irrelevant to psychology.

(7) Most animals are pretty much alike. Species differences are few and are probably accounted for by sensory differences and different experiences.

(9) Animal behavior can be studied best in the laboratory because of the controlled conditions. Laboratory conditions simplify behavior.

Aggression and agonistic behavior in nonhuman species. As for the frequency or ubiquity of aggression among animals, overt aggression (as distinguished from agonistic behavior defined below) is apparently relatively uncommon among vertebrate species (Scott, 1969; Gottier, 1972), including primates (Pilbeam, 1973; Montagu, 1973b). Usually, attack and threat are "ritualized" (Scott, 1969; Lorenz, 1966; Gottier, 1972), so that overt fighting generally plays little part in animal social life, although this varies by species (Scott, 1969). As for invertebrates, which comprise 96% of all animals (Lockard, 1971), "ant wars" and fighting among crustaceans have been noted, but "there are thousands of species of arthropods in which fighting has never been reported" (Scott, 1969: 124). Contrary to popular belief, humans are not the only species which kills greatly in excess of predatory needs. Kruuk (1972) describes a number of incidents in which different predatory animals killed far more of their prey than they or their fellows could eat. However, such incidents are quite rare and are thought to be explained ethologically by either the overabundance of prey available at the time or their panic, providing an excess of stimuli that release predatory behavior.

Animal behavior does not provide good analogies to human social conflict because the great bulk of the overt fighting that does take place is between individuals rather than groups, the latter being relatively rare in most species and perhaps nonexistent in "man's closest biological relatives, the anthropoid apes [chimpanzees and gorillas]" (Scott, 1969: 128). Regarding comparisons to the quintessential human conflict, Scott says: "Of the cases [of organized group fighting] that do exist . . . there is none which provides a good analogue of human warfare, and certainly none which can be considered a homologue."

Although overt aggression seems to be rare, the broader category of "agonistic behavior" is not. This is "behavior which arises from a situation of conflict between two members of the same species" (Scott, 1969: 121), and includes threats, bluffing, displays or postures of dominance, submission, freezing, and escape. Such behavior apparently is intimately connected with the establishment and maintenance of dominance hierarchies (Gottier, 1972; Scott, 1969).

Gottier (1972) has reviewed a great deal of the literature dealing with factors affecting agonistic behavior in nonhuman species, including both biological variables and learning or experiential factors. The biological determinants considered are heritability, sex, and hormonal balance.

(a) *Heritability*: There is evidence for interbreed and even intrabreed differences in agonistic behavior by means of selective breeding, although the evidence for the latter is somewhat contradictory. However, the generality of these findings is uncertain, for while positive results have been obtained by "artificial" selection in experiments, it is not clear that in natural surroundings the most dominant males and the most dominant females would consistently choose each other for mating.

(b) *Sex*: The expectation that males are more aggressive than females and always dominate them in the status order has received only partial and highly qualified support. Dominance has been found to be unrelated to sex in several species (e.g., cats, rats, monkeys), while in others (e.g., the domestic chicken) there are separate dominance hierarchies for males and females, with little hierarchy-relevant agonistic interaction between them. For the many vertebrate species in which males are more aggressive than females, two determinants have been suggested (Scott, 1958): greater amounts of androgen in males (see below), and superior size, which "may operate as much as a perceptual learning factor as it does in giving an advantage of strength" (Gottier, 1972: 185).

(c) *Hormonal influences*: Hormonal modification, principally androgen, appears to enhance aggressiveness and position in the dominance hierarchy, but there are important qualifications. The injections administered have been artificially large; the effects are considerably larger among strangers than in already established orders; and the effects have been either absent or considerably smaller among "higher" species—presumably, where learned response patterns are more important.

While Gottier does not deny the importance of these biological variables for agonistic behavior, he feels that the evidence shows that learning or the past experience of the organism "stands out above the others in its basic importance" (Gottier, 1972: 206). He reaches this conclusion after reviewing supporting evidence from a number of areas, which include the following as factors which influence agonistic behavior:

- (a) evidence for learning (early experience, increased experience at competition, "social inertia" of the dominance order even in the face of factors which normally would modify the order, and avoidance and operant conditioning);
- (b) perceptual factors (appearance of the organism, size as a "learned perceptual factor," and morphological modification of the organism);¹

1. While Gottier interprets these perceptual factors in learning or experiential terms, several of those discussed in the studies cited are also open to a biological interpretation insofar as they may function as "sign stimuli" or "releasers" (see definitions under "Ethology").

- (c) social factors (group stability or social disorganization, the status of newcomers); and
- (d) ecological factors (availability of food, and territoriality, the popular assumptions of which Gottier disputes).

He concludes that "the European emphasis on an innate aggressive drive is not nearly as important as the past experience of the organism" (Gottier, 1972: 211). However, his conclusions are certainly not shared by all investigators (Lockard, 1971; Eibl-Eibesfeldt and Wickler, 1968), and portions of the evidence he cites are vulnerable to ethological interpretations. Further, he deals with only a few selected species among vertebrates, and many of the studies cited were conducted under laboratory rather than natural conditions. As a result, all his conclusions cannot be accepted uncritically.

Criticisms of comparative psychology. Comparative psychology and its assumptions, cited earlier, have come under severe attack in the past two decades, sometimes from within its own ranks and sometimes from ethologists. The basic charge has been that its biological assumptions have been uninformed and fundamentally wrong. Lockard (1971: 173-175) has summarized the modern positions rebutting each of the previously cited assumptions:

(1) There is no phylogenetic scale. . . . The living species of today form no scale at all, except for partial reconstructions made with great caution and competence within a related group. . . .

(4) White rats are not simple versions of humans. . . . The place of rodents in psychology is for the study of rodent behavior, not human behavior. Animal behavior research can illuminate human behavior in the context of the comparative method or the ecological method; but research on animals unrelated to humans by one of these two methods has no scientific relevance in its results.

(5) Learning is not the key to animal behavior because most behavior is not acquired. . . . The old concept of an animal as having some degree of intelligence and thus able to learn nearly anything in accord with its endowment is giving way to the view that natural selection has probably produced rather specific learning mechanisms that correspond to ecological demands. . . . In short, learning abilities may evolve as discrete entities related to environmental particulars where they convey a reproductive advantage.

(6) Because so much is built into animals, genetics and evolution are of the utmost importance to any science of behavior. . . .

(7) Animals are quite different from species to species; each has its own autecology and its own set of adaptations. . . . The old view that the same hidden but lawful processes resided in all animals is untenable both in the face of empirical findings and because of the independent origin of behaviors. . . .

(9) The laboratory has limited usefulness and subtle disadvantages in the study of animal behavior; a new role for the laboratory is emerging. . . . sometimes the problem can be analyzed only so far [in the field], and it must be moved into the laboratory. . . . Much of the history of comparative psychology consisted of inventing clever procedures and imposing them upon animals; today, the interest is not in what an animal can be made to do, but in how it normally functions.²

Lockard feels that, because comparative psychology has been unable to fulfill its promise for animal behavior research to illuminate human behavior, it has been virtually supplanted by ethology and other more biologically sophisticated specialties. This is an exaggeration, but many of the older assumptions, at least in their simpler forms, appear to have been discredited.

EVIDENCE FROM ETHOLOGY

Basic assumptions and methods. To those unfamiliar with modern developments in the biological sciences, it may have seemed that ethology, widely thought to have "a short history and a shaky foundation" (Lockard, 1971: 170), had crept quietly onto the scientific scene until the Ardrey and Lorenz books cast the harsh glare of publicity on it. In fact, however, it is one of the more recent developments in a trend with deep and continuous roots reaching back to Darwin's *Origin of Species*, a movement that has seen disciplinary boundaries between formerly separate specialties collapse, uniting them and a series of newer, interdisciplinary branches into "the modern synthesis," with updated evolutionary concepts at the heart of the synthesis (Lockard, 1971). Ethology's contribution has been to provide a rationale for bringing the study of behavior into that synthesis. And if social scientists have been harboring any residual doubts about whether ethology has "arrived," the recent awarding of the Nobel Prize in Physiology or Medicine to Konrad Lorenz, Niko Tinbergen, and Karl von Frisch, the fathers of ethology, should dispel them.

There seem to be various, not entirely consistent definitions of ethology. The one most frequently encountered recently is "the biology of behavior," or "behavioral biology," although some writers (e.g., Barnett, 1973) refer to it as "the science of animal behavior." Willhoite (1971: 619) summarizes its attraction: "The main promise that ethology seems to hold out to students of society and politics is the possibility of developing a scientifically defensible conception of man's nature." The expectation is premature, as we shall see, but ethology has captured the imagination of a

2. Boice (1973: 6) states: "The complex learning processes evidenced by animals in a laboratory are probably not primarily the result of Darwinian evolution. They are, rather, the result of captivity, boredom, playfulness, and domestication."

number of behavioral scientists and seems to be finding increasing application to problems of human behavior.

The basic assumption of ethology is that all organisms, including humans, are products of evolution in which "fitness" for survival in an ecological niche is determined by an interaction of mutation and natural selection. They believe that, in addition to physical characteristics, genetically transmitted behavior patterns have been fashioned by natural selection. Hunt (1973: 29) perhaps oversimplifies when he says: "The basic tenet of ethology is that by far the largest part of what animals—including man—do is instinctive," for different ethologists would agree or disagree to varying degrees with different portions of his statement.

Nevertheless, the primary focus is on the biological bases of behavior. As such, the basic units of behavior which interest ethologists are what they call "fixed action patterns." These are complex behaviors, or series of behaviors, which appear to be innately "programmed"; that is, they are rigid, stereotyped, goal-oriented series of behaviors which are run off in an inflexible order. Evidence for innateness comes from the fact that such behaviors are produced even by animals reared in isolation from their conspecifics, upon the first opportunity to do so (i.e., being presented with the object or situation in question). For example, when first presented with a nut in a laboratory situation, young squirrels reared in isolation run off a sequence of nut-burying behaviors, including trying to cover the nut with nonexistent dirt and tamping it down—apparently oblivious to the fact that, at the end, the nut still sits uncovered on the laboratory table (Brown, 1965).

In order to facilitate the discussion of Lorenz' theory of aggression, it may be helpful to present an oversimplified version of a model of behavior used by ethologists. Behavior is often thought of as having both an external and an internal determinant. The external one is the "sign stimulus" or "releaser"—that is, a particular feature of the environment that consistently elicits a fixed action pattern. The internal determinant, and one crucial to Lorenz' concept of the "spontaneous" nature of aggression, is described by Brown (1965: 29):

[The] internal determinant is a state of drive or a charge of energy specific to the fixed action pattern and indeed produced by the mechanism of that pattern. It was Lorenz . . . who first proposed the notion of 'action-specific energy.' It is not at all clear how to translate these 'energies' into serious neurophysiology and some ethologists (Hinde, 1959) prefer the term 'specific action potential.' The idea is that there must be distinct forces pressing for the performance of distinct reaction patterns. Some of these forces may be rather continuously generated and some are probably seasonal. These forces intensify

with time and their strength determines the ease of firing the action pattern. Performance of the action reduces the strength of the internal force.

In addition to these two determinants, ethologists posit two internal mediating mechanisms: (a) "specific inhibitory blocks" which normally prevent discharge of action-specific energy and therefore the behavior in question; and (b) "releasing mechanisms" which remove the inhibitory blocks when activated by a releaser stimulus, thereby releasing blocked energy and producing the behavior (Brown, 1965: 29). The releaser mechanisms may be either wholly innate (genetically transmitted) or partly learned (Eibl-Eibesfeldt and Wickler, 1968).

According to Tinbergen (1968), there are four questions that are central to ethological investigation.

- (a) Survival value: "In what ways does this phenomenon (behavior) influence the survival, the success of the animal?"
- (b) Immediate instance causation: "What makes behavior happen at any given moment? How does its 'machinery' [i.e., its biology] work?"
- (c) Developmental causation: "How does the behavior machinery develop as the individual grows up?"
- (d) Evolutionary causation: "How have the behavior systems of each species evolved until they became what they are now?" (Tinbergen, 1968: 1412).

Despite Tinbergen's (1968) insistence that it is ethology's methods rather than its other-species results that should be applied to humans, many ethologists continue to be interested in cross-species generalizations, especially as they may apply to humans. While the "principle of independent evolution of behaviors" would seem to preclude cross-species generalizations, two principles which flow from evolution have "saved" this possibility. (a) Phylogenetic relatedness: "Behavioral homologies [i.e., "truly similar because of common ancestry"] increase in frequency and detail among different animal species as proximity to a common ancestral species increases" (Lockard, 1971: 172). This is the basis for the comparative method. (b) Ecological convergence: "Similar behaviors among unrelated forms result from similar selection pressures" (1971: 172). This is the basis for the ecological method.

Aggression from the ethological viewpoint. Ethologists, like the individuals in any other discipline, are not of a single mind on the subject of aggression. However, if there is anyone among them who has come to be associated with this topic, it is Konrad Lorenz. His theory of aggression (Lorenz, 1966) deserves coverage here not because it is representative of the field—it is not (although when given the opportunity in print, several prominent ethologists have declined to offer substantive criticism)—but for

a number of other reasons. He is considered one of the founders of ethology—if not *the* founder—and is thought by many nonethologists to be its spokesman. Further, his account is probably the most extreme offered by a responsible scientist and is certainly the best known. For these reasons, it is the one which anyone writing in the area of aggression has to deal with sooner or later.

Very simply, Lorenz believes that aggression in human beings stems from an “instinct for aggression” that humans share with most other species. Strictly speaking this is *intraspecies* aggression which has evolved because of its alleged survival value for most species. Contrary to what one might expect, intraspecies aggression is not dangerous or maladaptive for the species, at least among nonhumans, because together with this aggressive instinct have evolved “instinctive” inhibitory mechanisms including the ritualization of aggression (enabling the receiver to instinctively recognize the aggressor’s behavior as being different from a real mortal attack) and gestures of appeasement or submission which cause the aggressor to instinctively terminate the attack.

Lorenz accounts for the impressive degree of aggression among humans by suggesting that they have no such instinctive inhibitions to aggression. This is so, he says, because early in man’s evolution, such inhibitions were not necessary. Man’s physiology contained no terribly dangerous weapons on the order of the wolf’s teeth and jaws or the lion’s claws, with which to kill his conspecifics with dispatch; consequently, no (or only weak) compensating inhibitory mechanisms were evolved. However, according to Lorenz, the invention of “artificial weapons” upset the natural balance between instinctual aggression and killing potential on the one hand, and the weak inhibitions against aggression on the other. Over time, things got out of hand because of the weapons’ increasing effectiveness for killing as against the still-feeble innate inhibitions against killing. In more modern days, this imbalance has been further exacerbated by the lack of immediacy or direct experience with the victim’s suffering, a condition afforded by increasingly effective long-range weapons.

One of Lorenz’ fundamental assumptions is that “human behavior is fueled and motivated, directly and indirectly, by ‘spontaneous’ instincts physiologically identical with those of animals lower on the evolutionary scale” (Willhoite, 1971: 621). The reader may now more fully appreciate the earlier discussion of “action-specific energy,” for Lorenz sees the aggressive instinct as subject to these principles. Energy specific for the performance of aggression accumulates in the central nervous system, generating pressure for its release by appropriate stimuli. If the appropriate stimuli are not present, the organism may seek them out for release, a

process called "appetitive behavior" (Eibl-Eibesfeldt and Wickler, 1968). Or if none are found and energy accumulates beyond a certain point, the behavior may "explode" in the absence of any relevant external stimuli. However, once sufficient behavior has been enacted to exhaust the previously accumulated energy, normal releasing stimuli are ineffective in getting the organism to respond until sufficient energy can accumulate again in the "instinct center."

Lorenz also conceives of the aggressive instinct as a general, unitary drive capable of powering a wide range of other behaviors as well. He speculates that social, affectional bonds may be derived from diverted aggressive energy, and that smiles of greeting may be a result of ritualized, redirected aggression.

Because Lorenz conceives of aggression as an "ineradicable instinct," his prescriptions for dealing with it understandably concern ways of rechanneling it rather than trying to eliminate it entirely. Because he feels that the society makes impossible demands upon the individual for suppressing his "natural" instincts, what humans need is "ritualized" release: their aggression needs to be redirected into socially beneficial or at least harmless channels such as space ventures or international sports.

Criticisms of Lorenz' theory. Lorenz' theory of aggression, of course, has been highly controversial and has prompted a number of serious criticisms varying widely in quality. Leaving aside for the moment such ascientific questions as the societal effects of Lorenz' view of human nature, he has been severely attacked for the inadequacy of both his data and his logic. I can only begin here to sketch out some of the more telling counterarguments in their briefest form, and the reader is urged to consult the cited sources for more detail. There are two critical questions in evaluating Lorenz' theory. First, how valid is his theory for animal (i.e., nonhuman) behavior? How accurate, complete, and generalizable is it? And second, how well does the theory apply to human behavior? These questions will be considered in this order, with an intervening section dealing with cross-species comparisons in the theory.

(1) Applicability to animals: Aside from near-hysterical outbursts from literati, Lorenz' most severe critics have been his fellow professionals working in animal behavior and related fields. It is interesting that many reviewers who do not specialize in any such related fields have been willing to assume the validity of Lorenz' animal evidence but feel he has erred in applying these results to humans, whereas many specialists in these areas feel that his conclusions about animals are fundamentally wrong. Barnett (1973: 76) goes so far as to say that Lorenz' book "does not in fact represent the methods or opinions current in ethology." Scott (1973),

Crook (1973a), Gorer (1973), and Berkowitz (1969) all charge in one form or another that Lorenz' notions are outdated, that he has ignored the bulk of experimental literature in the field, and that his expertise is limited to only a few species of animals.

Some writers have charged that the data from which Lorenz generalizes and with which he is intimately familiar are either artifacts of the setting or specific only to a few selected species. Carrighar (1973) alleges that Lorenz' use of "tamed" animals in captivity (e.g., geese and fish) has resulted in abnormally heightened aggressiveness. Meyer-Holzapfel (1968), Pilbeam (1973), and Zuckerman (1932) all provide evidence that captivity may in fact enhance social conflict and aggression. Also, irrespective of captivity, crowding may have had the same effect (Schneirla, 1973; Carrighar, 1973). Also, Carrighar (1973) suggests that Lorenz has used only animals who demand "individual distance" (e.g., birds and fish) and dislike close association with their conspecifics. By contrast, mammals—to which humans belong—are "contact" species—that is, they seek and enjoy the touch of their own kind" (Carrighar, 1973: 129). However, there is some inconsistency on this point, for Crook (1973a) states that there are "contact" and "distance" species among both birds and mammals. Lorenz also shows weakness in his knowledge of primates, in one place referring to them as "irascible," whereas the consensus generally is that, except for the usual kinds of ritualized "aggression" (Washburn, in de Reuck and Knight, 1966), primates and especially man's nearest relatives, the anthropoid apes, are quite "amiable" (Montagu, 1973b) in their natural environment.

Lorenz' assumption that aggression is a general, unitary drive has come under attack from several writers, including Berkowitz (1969), who cites Moyer's work (summarized in Moyer, 1971) which suggests that "there are several kinds of aggression, each of which has a particular neural and endocrine basis" (Berkowitz, 1969: 381). Further, Lorenz' assertion that social, affective bonds are derived from the aggressive instinct is contradicted by Harlow's famous monkey studies, which showed that affection developed first, followed by fear, and then aggression.

A number of writers have found fault with Lorenz' notion of the "spontaneous" nature of aggression. Berkowitz (1969) cites two nonconfirming sources: Hinde's suggestions that stimulus satiation may account for reduced elicibility of response rather than an "exhaustion" of "action-specific energy;" and explanations by Hinde and Ziegler separately "that many apparent demonstrations of internally-driven spontaneity can be traced to external stimuli and the operation of associative factors" (Berkowitz, 1969: 380). Other writers have questioned whether the spontaneous discharge of aggressive energy, which would unnecessarily

thrust the organism into danger, could have evolved at all, being contrary to natural selection (Bartlett and Bartlett, 1971; Scott, 1973). Summarizing his critique of Lorenz' notions of spontaneity, Crook (1973a: 193) states: "In any case, the whole structure of motivation theory in ethology has undergone a major conceptual revision in recent years and even if aggression could be classified descriptively as an 'appetite,' the simple Lorenz-Ardrey account of its causation would have to be severely modernized."

Often forgotten in the attention to aggression is the fact that animals have a second major, and probably more frequently used, response to threat, namely flight (Tinbergen, 1968; Fromm, 1973). For most species under such conditions, fighting is only a last resort when flight is not possible. Fromm (1973: 36) comments on Lorenz' downplaying of this response:

In no manner is aggression more 'natural' than flight. Why, then, do instinctivists talk exclusively about the intensity of the innate impulses of aggression, rather than to speak with the same emphasis about the innate impulse for flight? . . . A theory centered around man's 'uncontrollable flight instinct' may sound funny, but it is neurophysiologically as sound as that of 'uncontrollable aggression.' In fact, from a biological standpoint it would seem that flight serves self-preservation better than fight. . . . The speculations [omitted here] are only intended to point to the ethological bias in favor of the concept of *homo aggressivus*.

Barnett (1973) criticizes Lorenz for confusing heredity and development and for failing to take account of how behavior develops throughout the organism's life. It is mistaken to assume that mere uniformity of behavior within a species is evidence of innate factors. Hunt (1973) describes cases in which some ethologists, formerly committed to traditional notions of "fixed action patterns," upon closer examination discovered that some of these behavior sequences were actually molded to a great extent by particular environmental opportunities and learning experiences.

And finally, Scott (1973) claims that Lorenz simply leaves out the other major causes of aggression, such as differential heredity, training, and social disorganization.

(2) Lorenz' cross-species comparisons: Under any circumstances, it is risky to make cross-species generalizations of physiological commonalities assumed to underlie behaviors that appear similar. Lehrman (1953) states that "it is not very judicious, and actually is rash . . . to assume that the mechanisms underlying two similar response characteristics are in any way identical, homologous, or even similar" (quoted in Berkowitz, 1969: 376),

simply on the basis of apparent similarity of behaviors across species—similarity which, Berkowitz adds, may be only in the eye of the beholder. However, Lorenz is particularly loose in making such assumptions. As Alland (1969: 553) puts it, “Lorenz has a tendency to describe animal behavior anthropomorphically, and to reverse his field and attribute animal-like responses to man when it suits his arguments.” Both Eisenberg (1972) and Berkowitz (1969) accuse Lorenz of acting as if he has “explained” such behaviors simply by attaching the same labels to them, thereby “misrepresenting analogy as homology” (Eisenberg, 1972: 125).

(3) Applicability to humans: Erich Fromm (1973: 35) distinguishes two quite different kinds of aggression among humans: benign and malignant.

The first, which man shares with all animals, is a phylogenetically programed impulse to attack (or to flee) when vital interests are threatened. This defensive, *benign* aggression is in the service of the survival of the individual and the species; it is biologically adaptive and ceases when the threat has ceased to exist. The other type, malignant aggression, i.e., destructiveness and cruelty, is specific for the human species and is virtually absent in most mammals; it is not phylogenetically programed and not biologically adaptive.

Only the second type of aggression, says Fromm, is “evil,” and it is not instinctive at all but rather distinctly human and part of man’s own doing. Fromm’s distinction is a good one, but in real life it is hard to preserve in such clear-cut form.

One of the most striking characteristics of human beings is their impressive diversity of culture. Practices may vary enormously from culture to culture. The same behavior may be praised or taken for granted in one culture and may be pilloried and condemned in another. Eisenberg (1972: 126) describes the difficulties that such diversity poses for the theory of instinctive aggression:

If we explain the murderous raids of Brazilian Indians on the basis of an innate aggressive instinct, we shall have to invent an involved theory of repression, reaction formation, and sublimation to account for the peacefulness of the Eskimo. Would it not be far more parsimonious to begin with the assumption that men are by nature neither aggressive nor peaceful, but rather are fashioned into one or another as the result of a complex interaction between a widely, but not infinitely, modifiable set of biological givens and the shaping influences of the biological environment, the cultural envelope, and individual experience?

Lorenz’ suggestions for dealing with aggressiveness in humans have also come under attack. Drawing on his own extensive experimental research, Berkowitz (1969) concludes that aggression excites rather than deflates aggressive urges. This point is supported by Moyer (1973), who cites

soccer riots in several countries as well as research by Goldstein and Arms (1971), who found much greater hostility and irritability in fans who had watched a football game than among fans who had attended a gymnastics match. There is enough research to conclude, says Berkowitz, that aggression is likely to lead to even more, not less, aggression. And even Desmond Morris (1968) disagrees with Lorenz, preferring "massive de-population" to "boisterous international football" as the key to lowering the level of human aggression (quoted in Berkowitz, 1969).

In addition to these rather specific criticisms, it should be noted that Lorenz' theory ignores the role of three major classes of determinants of aggression in humans: learning, structural causes, and "semi-autonomous" psychological causes. (a) Learning: Learning may modify, subordinate, or replace physiologically determined aggressive impulses. Learning mediates the acquisition of aggressive behavior in many different ways. Aggressive behavior is learned by observation of others (Bandura, 1973) or by direct experience. Children learn that aggression is often highly instrumental, useful, effective behavior and that it is often reinforced. They also learn that the successful use of aggression often is a function of one's power over others. They are surrounded with examples of aggression and may become desensitized to media displays of it (Cline et al., 1973). Eisenberg (1972: 127) summarizes the point well:

The very ubiquity of violence in Western society, however we explain its genesis historically, guarantees that children are surfeited with opportunities to learn violent behavior. The child sees that violence pays off; he is provided with adult models of violent behavior with whom to identify (television pales beside real life). Violence as an appropriate response to the resolution of intergroup conflict is sanctioned by national leaders. . . . When violence is sanctioned, it will increase. It can be expected to generalize to situations not 'intended' to come within official pardon. Learning may not account completely for human aggression, but the social forces in contemporary society that encourage its development are so evident that preoccupation with hypothesized biological factors is almost quixotic.

However, most prolearning writers do not discuss the very real possibility that humans may be biologically predisposed to easily learn aggressiveness, through specific mechanisms discussed later.

(b) Structural causes: Also ignored by Lorenz are the many structural forces which promote conflict and aggression in humans. Scott (1969) and Fromm (1973) suggest that just as social disorganization in animal societies leads to greater overt aggression, its analogues in human societies—poor international organization, and normlessness and lack of community—also predispose humans toward aggression and violence. Further, a great deal of human conflict stems from the fact that human

groups often have incompatible goals and are engaged in competition over scarce resources. Such conflicts and much behavior in them are “realistic” (Campbell, 1965; Levine and Campbell, 1972), rather than instinctual in origin. If the vast number of Prisoner’s Dilemma studies which have filled this journal’s pages have shown anything at all, it is that, in a wide range of situations characterized by particular strategic or motivational patterns, what appears to be “hostile,” “destructive,” or “selfish” behavior is not at all the result of “innate aggression” or the characteristics of the actors, but instead are conventionally (i.e., individualistically) rational decisions in the context of a very difficult situation. Such problems are structural, not biological in nature. And finally, the Bartletts (1971) argue that Lorenzian notions of aggression—especially when misused as by Ardrey (1966, 1961)—both ignore and detract attention from exploitation:

This ‘science’ precludes criticism of social structures and thwarts genuine inquiry into the human practice of exploitation. The convulsive current thrust of the exploiting classes to keep in their grasp the earth and its people is accepted by these authors as the action of the instincts. The question: Who gains by the power to exploit? is put outside the realm of science [Bartlett and Bartlett, 1971: 218].

Similarly, blaming “technology” for the human abuses of technology and of other humans masks questions of what groups of people have created it, own it, and use it, and for what purposes.

(c) Psychological factors: The proponents of biological explanations of human conflict fail to appreciate adequately the role that symbolic issues often play in causing conflict or even aggression. (The most immediate example is the very controversy under review here, which is at least in part a conflict over an “image of man.”) Whereas animals’ aggressive drives lead them to attack only actual enemies who present an actual threat, “man . . . by virtue of his ability to manipulate symbols, attaches the label ‘enemy’ to entire categories of things: other animals, other people—even inanimate objects and ideas. Accordingly aggression ceases to be ruled by the situation” (Rapoport, 1965: 118)—or, one might add, by one’s physiology. Fromm (1973) and Erikson (1968) even suggest that it may not be accurate to refer to some human aggression as intraspecific, since the victims of much violence are not even thought of as fellow humans. Another psychological dimension thought to lead to human aggression—although it may have immediate social-structural roots—is the feeling of powerlessness or helplessness. More specifically, in several well-documented cases of urban disorders, the operative factor was found to be a combination of the lack of political efficacy together with a high sense

of personal efficacy (Caplan, 1971; for a more comprehensive treatment of structural and psychological inputs to conflict, see Nelson, 1971).

Thus far under the heading of ethology, I have considered only Lorenz' theory of aggression. Aggression is also dealt with indirectly in a number of other ethological topics, and some coverage of a few of them is appropriate here.

Dominance hierarchies. The central question here concerns the relative role that aggression, either overt or ritualized, plays vis-à-vis other possible determinants of the dominance hierarchy. Aggressiveness is a factor in dominance hierarchies in at least two different ways. As we have seen, in either overt or ritualized form it may serve as a means of deciding positions within the order. However, the level of aggression is also controlled by the dominance hierarchy—the hierarchy has the function (but not the “purpose,” as some authors carelessly imply)³ of containing and channeling aggressiveness. I am concerned only with the former relation here.

Earlier we saw that aggression, in either overt or ritualized form (a distinction which varies with species), may play a central role in establishing position in a dominance hierarchy in certain species, particularly those discussed by Scott (1969) and Gottier (1972). However, in certain other species, aggression in any form may be relatively unrelated to position and dominance. Reviewing recent work on primates, Pilbeam (1973: 116) says, “What is particularly interesting in the newer animal studies is the extent to which aggression, priority of access, and leadership are divorced from each other.” Klopfer (1969) also notes that in some herd animals (e.g., ungulates) aggressiveness appears unrelated to leadership in matters of when and where the herd moves, and that this may be a matter of greater responsiveness to the environment and reduced dependence upon peers. Gottier (1972) reviews studies of chickens in which morphological modifications of structures functional for aggression (e.g., removal of the bird's beak) had little effect upon position in the hierarchy, whereas modifications of more symbolic than functional significance (e.g., removal of the comb and wattle) had a much greater effect. And finally, Mazur (1973: 514) notes that the determinants of status orders among humans typically include neither overt aggression nor “overt gestures of

3. “In order to contain male aggressiveness and subordinate it to the needs of the group, males tend to form fairly stable dominance-submission hierarchies” (Corning, 1971: 342). This statement is altogether too anthropomorphically purposive for meaningful discourse, for it implies intention or design on the part of the male animals in question. Most writers, including Corning, warn against just such practice.

threat or submission," although his data suggest biological bases of some unspecified kind.

Furthermore, it is clear that a number of nonbiological factors also have important influences upon dominance hierarchies and position within them. Gottier (1972: 211) emphasizes the role of learning via a process of distinguishing "cues relevant to winning" from "cues relevant to submitting" through discrimination learning. In explaining the process by which a young macaque's position in the hierarchy is largely determined by its mother's relative rank, Lancaster (1973: 34) concludes: "From these experiences, a young monkey learns both general social attitudes and the specific treatment due others in the group." Crook (1973a: 212) also emphasizes the often overlooked importance of "social learning in the structuring of groups." In a passage reminiscent of Bandura's studies of modeling, Crook states: "Hall . . . has stressed the importance of observational learning in monkey groups; the experience of one animal is witnessed by others and the information obtained utilised by them on later occasions" (Crook, 1973a: 212). Washburn (1972: 34) notes that even the *style* of dominance in some primates may be affected by prior experience—he contrasts a "very relaxed and friendly" leader monkey who exerted authority only when necessary with another monkey "who had occupied a very insecure position before he came into control of a group" and who "continually asserted his dominance over the other animals."

Finally, Pilbeam (1973) calls into question much of the literature dealing with primate dominance hierarchies and aggression. Earlier studies of baboon behavior, stressing rigid dominance hierarchies and their relation to aggression, male supremacy, and female subservience are misleading, Pilbeam suggests, because they were carried out not in the animals' natural habitats but in game parks. There, he speculates, they live under greater than normal tension due to the increased presence of predators, especially humans. He claims that recent studies in more natural environments have demonstrated more fluid group composition, hardly discernible dominance hierarchies, infrequent aggression, and periodic leadership by females. "In undisturbed species in the wild, dominance hierarchies are hard to discern, if they are present at all; yet workers still persist in trying to find them" (Pilbeam, 1973: 115). Pilbeam suggests that the data from the earlier studies are artifactual results of an unnatural environment: "The high degree of aggression, the hierarchies, the rigid sex-role differences, were in a sense abnormalities." Experimental manipulation of the same species' environment has yielded corroborative findings, according to Pilbeam: stress-inducing confinement results in "more aggression, more fighting, and the emergence of marked dominance

hierarchies." He concludes: "If dominance can come and go with varying intensities of certain environmental pressures, then it is clearly not innately inevitable, even in baboons. Rigid dominance hierarchies, then, seem to be largely artifacts of abnormal environments" (Pilbeam, 1973: 116).

Territoriality. While territoriality is a legitimate subject of study among serious ethologists and is thought by some to apply to humans (Tinbergen, 1968), its best-known treatment is the highly unscientific book by Robert Ardrey (1966). Once again, I must discuss the atypical theory in order to correct a large number of popular misconceptions. Ardrey believes that humans, like other animals, have an "instinct" of territorial aggression, by which one defends territory with which one has identified against strangers of one's own species. Both the identification with the territory and aggression defending it are thought to be instinctive. Ardrey's dramatic flair and propensity for fast and loose thinking at the human level make even Lorenz uneasy (Lorenz, 1970). The following example from Ardrey (1966: 213) may suffice:

The continuity of human evolution from the world of the animal to the world of man ensures that a human group in possession of a territory will behave according to the universal laws of the territorial principle. What we call patriotism, in other words, is a calculable force which, released by a predictable situation, will animate man in a manner no different from other territorial species.

Territoriality, however, is not the simple "universal law" that Ardrey would have us believe, even among nonhuman species. To begin with, there are a great many species that do not demonstrate territoriality at all. Montagu (1973b) cites Bourlière (1954) to the effect that territoriality is nowhere near as important in mammals as it is in birds, where the study originated. Second, rather than causing aggression, territoriality may actually have the effect of *reducing* agonistic behavior (Gottier, 1972) by spacing the animals in such a way as to provide adequate food, breeding opportunities, nesting space, and so forth. In so doing, territoriality also serves as a mechanism for regulating population within a given area (Wynne-Edwards, 1962). For more thorough discussion of the inadequacies of Ardrey's speculations for animal behavior, the reader is urged to see Crook (1973a), Klopfer (1973), Gorer (1973), and Holloway (1973).

Furthermore, it is doubtful that territoriality—at least as Ardrey conceives of it—applies to humans. The carelessness of his multilevel analogizing is revealed in the fact that group-level "territoriality" is inconsistent with "instinctual" individual-level territoriality (Campbell, 1972: 24):

Even though efforts to mobilize human ethnocentrism often make reference to protecting home and family, group-level territoriality has always required that the soldier abandon for extensive periods the protecting of his own wife, children, and home. Individual territoriality and aggression means *intra*-group conflict, and is regularly suppressed in the service of *inter*-group conflict.

In addition, there is enormous variation among cultures in the importance of territoriality and private property. In those cultures where concern for territory and private property is greatest, it is more likely that they have their basis in their instrumental or even symbolic importance than in any biological urges. This is illustrated most vividly by two observations made by Bartlett and Bartlett (1971: 216-217). "First, what is owned often does not lie within the territory or the nation of the owners. [It] is often a piece of someone else's 'territory.'" Second, the largest holders of property (e.g., multinational corporations) are increasingly "extraterritorial." It is far more likely that the great concern for territory and property among humans, at least in Western cultures, is predominantly an acquired value, although there is no conclusive evidence that some form of territoriality might not have a biological basis in humans (Scott, 1973).

"*Crowding*" and aggression. Another popular belief is that crowding has the effect of increasing the level of aggression, both in human and nonhuman species. Freedman et al. (1972: 529-530) have concisely summarized the basic findings:

A large number of studies on nonhumans . . . have found that under a very high population density normal social behavior tends to break down and a sharp drop in the population occurs. A similar phenomenon has been observed in natural settings . . . and has been noted in a wide variety of animals. On the basis of these observations, several authors, notably Calhoun and Lorenz, have concluded that high population density always leads to an increase in aggressiveness and that this also occurs in humans. . . . But . . . even the animal research has not always produced consistent evidence for this proposition.

Among humans, both correlational and experimental studies have produced inconsistent results. Freedman and his colleagues have conducted experiments showing generally negative results, but such results may be largely artifactual and specific to features of the experiments. Some areas of the world (e.g., Hong Kong, Holland) are *prima facie* evidence that density alone does not lead to greater aggression. It is routinely pointed out that there may be no necessary one-to-one relationship between population density (the objective description of a physical condition) and the subjective experience of not having enough space. Fromm (1973: 37) believes that it is not population density as such but the "lack of social structure, genuine common bonds and interest in life" that causes

increased human aggression, but he does not mention the possibility that an increase in sheer numbers may facilitate the disintegration of social structure and common bonds. Fromm mentions a second closely related condition which may increase the level of human aggressiveness: an imbalance between the size of a population and the economic base for supporting that population. Fromm (1973: 38) concludes:

It follows from these considerations that all analogies from animal to human crowding are of little value. The animal has an instinctive 'knowledge' of the space and the social organization it needs. It reacts instinctively with aggression in order to remedy a disturbance of its space and social structure. It has no other way to respond to threats to its vital interests in these respects. But man has many other ways. He can change the social structure, he can develop bonds of solidarity and of common values beyond what is instinctively given. The animal's solution to crowding is a biological instinctive one; man's solution is social and political.

Fromm does not comment on whether humans might not also have "instinctive knowledge" of how much space or the degree and kind of social organization they need.

Closely related to "crowding" and perhaps territoriality as well is the study of "personal space," which has been receiving extensive but not uniformly rigorous investigation (Evans and Howard, 1973). The concept is well established in ethology and denotes "the physical space surrounding an individual within which approach by another animal (generally a conspecific) will elicit aggressive or defensive behavior" (Barash, 1973: 68), uneasiness, or retreat. Personal space is thought to have a biological basis even in humans, but it is mediated by psychological factors, such as expectations of the per capita space available (Barash, 1973). It has been found to vary cross-culturally, between subcultures, by sex, and over time by a developmental sequence. It is also studied under the rubric of "proxemic behavior."

Biologically based sex differences in humans. There is general acknowledgement that, despite a great deal of overlap in the distributions of the two sexes in terms of aggression, there are innate biological differences between them in "aggressiveness" in the more general sense (Brown, 1965; Bardwick, 1971), although Bardwick notes the ambiguity and indeterminacy of such terms as "aggressive," "active," and "passive." Here again, however, cultural beliefs and practices interact strongly with biological predispositions, so that disentangling the two sets of factors in order to assess their relative contributions may be next to impossible (see Crook, 1973b, for an excellent summary of these points).

It is difficult to discuss sex differences in aggressiveness without commenting on the embarrassingly sexist tone that creeps into some biologically oriented discussions of sex differences in general.⁴ The Bartletts' (1971: 211) comment on the recent popularized books applies as well to much other work: "The role of the female, not only in human culture, but among other animals, has been glossed over and falsified." Nowhere is the sexist attitude more conscious and explicit than in Tiger (1969), whose theory of biological sex differences justifies males monopolizing leadership positions in human culture on the basis of alleged superiority of "male bonding" compared to the alleged inability of females to form such "bonds." According to Tiger, society looks to the male for stability and leadership and to the female for child care, this division of labor having a biological, evolutionary basis. At one point Tiger (1969: 60) refers to "male bonding" as "the spinal column of a community."

Much of Tiger's evidence is taken from primate studies, so it is appropriate to mention that recent primate studies tend to undermine his thesis. Recall Pilbeam's (1973) account of the recent studies, conducted in more natural settings, showing less rigid sexual division of labor, with females sharing leadership tasks. Lancaster (1973) also cites recent studies showing that, among many primates, it is the older females who ensure the stability of the group. Furthermore, because the males may often become adventuresome wanderers and leave the troop (which we might call "male dis-banding"), adult females assume a large share of group leadership, due to "the stability of their social relations, their attachment to their home ranges, and their knowledge about the resources of their environment" (1973: 36). Criticizing the "male-centered bias" in earlier studies, Lancaster regards Tiger's alleged "psychobiological" differences regarding a sexual division of labor as unnecessary. Some division of labor was necessary, because of the lengthy dependence of human children. However,

There is no need to say that evolution has brought about major psychological differences between males and females to explain role differences. The only difference we need to establish is that females lactate and males do not—all the rest will follow through quite simple and obvious processes of social dynamics reinforced by socialization [Lancaster, 1973: 99].

Kurtén (1972: viii) also believes that sex differences have been over-emphasized in the study of early humans: "There is reason to suspect that

4. The reader will hopefully appreciate the difficulty of trying to distinguish humans from other species without using the word "man" to refer to the human species. I have tried to avoid such use generally but have occasionally deviated in order to avoid inordinately clumsy exposition.

there was no absolute division between foraging-hunting-fighting males, on the one hand, and stay-at-home, baby-sitting females, on the other." For a particularly penetrating review of Tiger's thesis, the reader should consult Fried (1969). In the rest of the biologically oriented literature, the reader should be alert for the frequent tendency to uncritically interpret data in ways consistent with long-standing cultural stereotypes. Like many stereotypes, these may have some basis in fact, but they are prone to exaggeration and selective interpretation.

The validity of the innate-acquired distinction. The distinction between "innate" and "acquired" is far more complex and hazy than it might appear at first. I will try to outline some of those complexities and discuss some of the more carefully delineated relationships that are being investigated.

At one extreme is the concept of instinct. Once common in the early days of social science, the concept was discredited by early behaviorists in psychology and fell into disrepute among social scientists generally. However, instinct regained a cautious respectability within ethology and has been reintroduced to the behavioral science parlance, although its status has been diminished somewhat by Lorenz' (1966) and Ardrey's (1966, 1961) grossly imprecise use. However, the use of the concept of instinct still encounters hostile resistance in many quarters. Montagu (1973b) concedes that animals may have instincts, but insists dogmatically that humans have none. And prior to his conversion to the "instinctivist" position, Bolles (1967: 106) made a statement which still commands much support but which he probably now regrets: "We may begin to wonder whether instincts (or drives) are invoked only when not enough is known about some behavior to explain it structurally."

In spite of now-obligatory but still grudging admissions on both sides that innate and environmental factors "interact" in vague, undefined ways to produce much of behavior, there is still a great deal of all-or-none thinking when it comes to the innate-acquired distinction. Here is an example from a supposedly definitive recent description of ethology:

The appearance of a particular fixed action pattern in animals isolated from their own species is clear evidence of genetic fixity. It is a constant characteristic of the species concerned and is based upon a specific central nervous mechanism that is inherited just as are morphological and physiological characteristics [Eibl-Eibesfeldt and Wickler, 1968: 188].

This passage shows no awareness that many behaviors are shaped in ways that do not involve "learning" but are the results of interactions (perhaps even prenatal) between the growing organism and its species-specific environment.

More specifically, Schneirla rejects Lorenz' dichotomizing of behavior into innate and acquired or learned and insists that developmental processes of behavior must be considered throughout the entire life cycle. Tinbergen (1968: 1416) parts company with his old friend Lorenz on this score.

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. . . . 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.

But, Tinbergen continues, this recognition has further implications:

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.

Seen in this light, Lorenz' notion of "innate" is so "only in the sense of 'nonlearned,' not in that of 'having grown without interaction in the environment'" (Tinbergen, 1968: 1416). Besides Schneirla and Tinbergen, Lehrman (1962), Crook (1973b), and others have also concluded, by somewhat similar reasoning, that heredity and learning cannot be meaningfully distinguished; Verplanck (1955) arrives at the same conclusion by a slightly different route.

Most recent writers on the subject agree that even apparently "instinctual," nonlearned behaviors are shaped by the organism's environment to a degree not fully appreciated previously. Hunt (1973: 33) quotes William Tavolga as saying: "At every level of organization from amoeba to man, behavior develops out of the interaction between the cytoplasm and the environment. You cannot go directly from the DNA molecule to a specific piece of behavior, and there is no special or separate category of behavior that can be called instinctive."⁵

5. "No serious primatologist today accepts the notion of pre-programmed primate instincts that emerge full blown. Biological tendencies mature and are modified through interaction with the social-cultural environment" (Mazur, 1973: 527).

The question in Verplanck's (1955) title is apt: "Since learned behavior is innate, and vice versa, what now?" If there is increasing consensus that the old, easy distinctions are no longer meaningful and that most behavior involves an "interaction" of some sort between biological and environmental forces, just how far does that take us? Not very far at all, unless we can begin to specify the nature of those interactions. Some beginnings have been made, and below I briefly mention five somewhat more specific areas in which work is taking place.

- (1) Interaction between biological factors and the physical or physiological environment: This refers to the kinds of interactions discussed above, whereby innate predispositions are shaped by the environment, in ways not involving learning, into fully operative behavior patterns. No further discussion is required here.
- (2) The influence of biological factors upon learning: It is commonly observed that one's *capacity* for learning is to some extent genetically determined (Verplanck, 1955; Alland, 1972; Emlen, 1967; Somit, 1968). It is perhaps less well known that ethologists believe that biological factors may also determine the directionality and types of learning which are easily acquired. While Montagu believes that learning in humans is generalized and directionally neutral, many ethologists insist that "there is bias built into the human genotype, a bias toward learning more readily that which, in the evolutionary history of the species, has tended to contribute most to individual and group survival" (Willhoite, 1971: 624). Theoretically, such learning is guided by internal "*Sollwerte*" or "templates for proper feedback" (Tinbergen, 1968: 1417), or as Lorenz calls them "innate teaching mechanisms." Washburn (1972) is one of the main proponents of the idea that humans are predisposed by evolution to easily learn aggressiveness, thus skirting the issue of "instinctive aggression."
- (3) Interaction between biological factors and learning: Here learning modifies, or is added to, the biological component, such that the behavioral outcome is different. In animals, "there are . . . behavior patterns which do appear in the inexperienced animal, but in an incomplete form, and which require additional development through learning" (Tinbergen, 1968: 1416)—for example, the calls of several bird species. In humans, Alland (1972: 151) argues, while "genes are responsible for man's capacity to acquire culture," the actual expression of culture requires experience and learning in the environment.⁶ Tinbergen (1968) describes a complex interaction of biological givens and learning in the environment, which characterizes childhood intellectual development, whereby learning at various stages is guided by different *Sollwerte*. If this is reminiscent of Piaget, it should be remembered that Piaget's original training was in zoology.

6. "But these distinctly human characteristics must not be taken as signs that we have 'escaped nature' or otherwise avoided our evolutionary history. On the contrary, our cultural mode of adaptation, which is completely linked to our physiological capacity for the acquisition of linguistic competence, is our species-specific evolutionary heritage" (Bastian and Bermant, 1973: 357).

- (4) The influence of biological factors upon culture or social structure: Hall (1968) and Emlen (1967) conceive of culture as an extension of basic biological processes. Corning (1971: 340) also outlines several ways in which "our biological needs impose [numerous imperatives] upon our social organization." Obviously such an approach can be prone to mis- or overinterpretation and has met with criticism (Crook, 1973a, 1973b).
- (5) Interaction between biological factors and culture: Here culture modifies biological factors, such that the outcome is a product of the interaction of the two. This is an oft-recurring theme, both for behavior in general (Corning, 1971; Willhoite, 1971; Alland, 1969) and for aggression in particular (Crook, 1973a; Dubos, 1973; Corning, 1971).

An assessment of ethology. An overall evaluation of ethology is difficult because of the widely varying quality of its offerings. However, a few rough generalizations can be made. On their original home ground, animal behavior, ethologists generally possess an enormous wealth of information and are biologically very sophisticated. However, many of them—but by no means all—still appear to be prone to reach too readily for purely biological interpretations of complex phenomena while underestimating the importance of experience with the environment. Lockard's (1971) claims to the contrary, comparative psychology is at least alive, if not well, and continues to perform a valuable service as a check upon the data, logic, and interpretations of ethology. Furthermore, despite their criticism of laboratory experiments, ethologists in some cases have been forced by methodological considerations to use more tightly controlled experiments in what they call the "semi-wild."

In the realm of human behavior, serious and careful ethological work has been done (for example, Jones, 1972; Tinbergen, 1972), and the approach appears to be gaining adherents. However, on the subject under discussion in this review, aggression, ethologists tend to break into at least two groups on the issue of whether aggression is an ineradicable part of human nature. (I am ignoring for the time being the fact that definitional differences may account for some of the discrepancies, as explained in the introduction, and continue to use the more narrowly restricted definition of aggression.) Some appear to hold an "image of man" (nonscientifically derived, I think) as intrinsically malevolent and destructive, which they are at great pains to uphold at all costs. (Lorenz does not fit entirely into this camp.) Others apparently see aggression as only a specific example of the more general belief that there are definite limits—genetically determined, they believe—to human beings' plasticity and malleability. Even within the latter group, however, there are often overtones of gloom and despair (see, for example, the remarkable final two paragraphs of Tinbergen, 1968; also see Lorenz' views, Newsweek, 1973).

Unfortunately, the real substantive contributions of ethology in this area have, if anything, been delayed or subverted by the generally inept popularized accounts. Whatever the real motives of those writers may have been, their books give the appearance of trying an end run on the scientific community and (to mix metaphors badly) playing to the crowd (and in the case of the repeaters and later arrivals on the best seller lists, simply cashing in on a good thing). While their notions may have temporarily become intellectually chic and made for exciting cocktail-party patter, they were sitting ducks for serious scientists with more adequate data and superior logic. As Crook (1973a: 216) puts it, "The promulgation of one-sided and misleadingly simplified doctrine is of no assistance to those concerned and could lead them to neglect those highly relevant contributions that modern ethological theory and experimental method can legitimately supply."

Finally, the general theoretical approach of ethology deserves comment. First, a feature of many ethologists' arguments which is both obfuscatory and yet perhaps more appealing to nonscientists is their proclivity to "explain knowns with unknowns." Two prominent ethologists say about their field, "Pronouncements about inaccessible psychic phenomena are avoided" (Eibl-Eibesfeldt and Wickler, 1968: 187). Yet, many ethologists are not reluctant to make pronouncements about inaccessible, equally mystical structures or mechanisms which are alleged to exist (e.g., Lorenz' "innate teaching mechanism"). This seems odd for a discipline that takes great pride in hard-headed objectivity.

Second, if we are to take ethologists at their word—and often, their deeds—many have opted for as artificially narrowly constricted a view of behavior as social scientists have traditionally held by not considering biological factors at all. The field is often defined as "the biology of behavior," or, even more extreme, in Lorenz' words, "the study of innate behavior; the study of species-specific drive activities" (Hunt, 1973: 29). Similarly, Eibl-Eibesfeldt and Wickler (1968: 187) say: "The aim of ethology is to explain both phylogenetically and physiologically the functional relationships of all factors involved in behavior." By defining their field in this way, ethologists have ruled out the study of any causal factors other than biological ones. Rather than seeking in an open-minded fashion for all possible causes of phenomena, they instead have a particular case to argue. They seek not so much to explain dependent variables as to push the limits of a small set of particular independent variables. (The sharp distinction between "innate" and "learned" was apparently drawn by the ethologists themselves—see Berkowitz, 1969; Verplanck, 1955; Crook, 1973b.) This process, in which one assumes at the outset that one's

hypothesis is not only valid but causally prepotent to any other type of factor, may use scientific procedures for its research, and may be one way in which the store of scientific knowledge as a whole advances. But it is not the model held up as the ideal method for scientific discovery. And it runs a greater risk of leading to errors of fact, of logic, and of utilization of its ideas in the real world. This is, of course, not to say that other fields have not followed the same course—for example, behaviorism and “overly socialized” views of man (Wrong, 1963).

THE RELEVANCE OF ANIMAL BEHAVIOR FOR THE STUDY OF HUMAN BEHAVIOR

Can we really learn anything valid about human behavior—in this case, aggression and conflict—from the results of animal behavior? The growing consensus, based upon increasingly sophisticated work in several fields, is a carefully qualified no. I will attempt to summarize the major reasons for this belief.

To paraphrase a principle used by Rae Carlson (1971), human beings are like *all* other species (or more broadly, all living systems), like *some* other species, and like *no* other species. The crucial question is which of the three comparisons is most likely to account for most of important human behavior. Humans share with other animals, especially their nearest biological relatives, the primates, certain important behaviors and characteristics: a social nature, small-group attachments, mother-infant bonds, dominance relations (Washburn, 1972), and peer play (Rule, 1967). Hebb (1971) notes that other animals also demonstrate “altruism,” and other writers discuss the possession of a simple “protoculture” by several species (Berry, 1973), characterized by a few of the many elements which comprise human culture. However, humans also possess a large number of key, unique characteristics: greater and more generalized cognitive-intellectual capacity (Bermant, 1973); symbolic language with a generative grammar; greater control over emotions, especially rage (Washburn, 1972); greater skill in tool use; highly developed culture, as compared to the “protoculture” of nonhumans; self-awareness; and, because of many of the foregoing, lesser dependence on or vulnerability to any particular environmental arrangement.

The points quoted earlier from Lockard (1971) also underline the error of making easy translations of animal behavior results to human behavior. To repeat briefly: (a) There is no phylogenetic scale among living species (Hodos and Campbell, 1969). This means, for example, that, in extrapolating from primate behavior to humans, “we are really making two leaps in the dark, one from the monkey back to man’s common ancestor, and again from this ancestor to man” (Boulding, 1973: 170). And, as we have

seen, any one such leap is fraught with difficulties. (b) The learning abilities of humans are qualitatively, not merely quantitatively, different from those of animals. Humans possess highly generalized (although perhaps not always directionally neutral) learning abilities, while the consensus among animal behaviorists is that animals possess "more-or-less specific complement[s] of learning capabilities" (Bermant, 1973: 7). (c) Different species are quite different from each other and can be compared only under certain special conditions, by use of the comparative method or the ecological method.

We can now see the relevance of animals in the comparative method and in the ecological method; the animals that are behaviorally relevant are those related to man by common ancestry, or those with similarities due to similar selection pressures. Apes are relevant by relatedness, wolves for ecological reasons. . . . [However,] there are too few ape species, each too specialized, for easy use of the comparative method; and too few ecologically analogous species for easy use of the ecological method [Lockard, 1971: 177].

If all this is so, then what do we make of apparent similarities in behavior in different species? Lockard (1971: 173-174) lists four alternative interpretations for such alleged similarities:

(a) Some truly homologous property, perhaps widespread [across species], was demonstrated. (b) Unknown ecological factors happened to produce analogous properties in these two—and perhaps no more—[species] which have superficial resemblances, but which differ in fundamental ways not explored. (c) Coarse analyses often produce results that fail to discriminate between things that are actually quite different. (d) The general climate of opinion and the eagerness to find similarities have led to casual interpretations and an easy task of persuasion of a credulous audience. One currently has no way of knowing which interpretation to give a study.

Bermant (1973) has examined four different types of potential "discontinuities" between human and nonhuman species to see whether claims of a discontinuity or qualitative difference are valid. These dimensions include species separation, biologically historical discontinuity, technical (or manipulative) discontinuity, and moral discontinuity. He concludes:

In each of the four areas discussed . . . the conclusion has been the same: there is no good reason to suspect that data or theories based on the behavior of nonhuman animals will give an adequate account of human conduct. Future generations of behavioral scientists will, I suspect, view the opposite claim as one of several curiosities coming out of a rather bizarre period in the history of our discipline [Bermant, 1973: 14].

There is a growing scientifically based consensus that, in order to understand human behavior adequately, both from biological and non-biological viewpoints, we must study humans as humans, rather than

relying upon results from animals, except under the special conditions noted earlier (see especially Tinbergen, 1968; Scott, 1969; Lockard, 1971). In the remainder of this review, I will consider approaches which to greater or lesser degrees adhere to this premise.

EVOLUTIONARY APPROACHES TO HUMAN BEHAVIOR

There was an Ape in the days that were earlier;
Centuries passed and his hair became curlier;
Centuries more gave a thumb to his wrist,—
Then he was a Man,—and a Positivist.

—Mortimer Collins
The British Birds

The separation of evolutionary approaches from the other areas considered in this review is to some extent artificial. However, insofar as scientists have employed evolutionary ideas to study human beings as such rather than inferring their characteristics primarily from other species, the approach warrants separate coverage. Earlier I stated that evolutionary theory is the very heart of modern biology generally (encompassing a wide range of previously unrelated specialties), and, more specifically, of the biology of behavior. More recently, these ideas have been seriously applied to new areas of human behavior. "This interest has opened an exciting field for theory and research specifically because it operates without the assumption that, since men are animals, they must behave like other animals" (Alland, 1972: 2).

The social sciences, except for anthropology, have largely ignored or consciously avoided evolutionary theory, perhaps as much for social and political as for purely intellectual reasons—namely, the exploitative abuses to which bastardized versions of the theory were put in the late nineteenth and early twentieth centuries. This calculated ignorance or avoidance is no longer tenable, for the approach is finding increasing application in the social sciences. (For a good introduction to such applications, see Corning, 1971, a piece which is occasionally brilliant but highly uneven, and Alland, 1969, an excellent and concise summary of "strong"—i.e., excessively deterministic—and "weak" theories of biological causes of human behavior.)

This is not to say, however, that evolutionary theory is infallible. Macbeth (1971), for one, has done a fine and eminently readable job of exposing all the warts on the face of at least classical Darwinian theory, as well as many of the concepts retained in the "modern syntheses." His position is that, while there is no doubt that evolution—or something like

it—has occurred, the theories purporting to explain the process are manifestly inadequate, sometimes embarrassingly so in light of the often grandiose claims made for them.

While the evolution of physical characteristics has long been accepted, the evolution of behavior patterns has not yet won widespread support, for there is no “fossil behavior” (Carrighar, 1973: 123). Lockard (1971) provides a good short overview of the historical development of modern biology up to the point of incorporating behavior into the theory. At that point, he says,

Scientifically speaking, only two pieces of information were needed to bring behavior into the modern synthesis of the new biology: the fact that behavior has a genetic basis, thus making it heritable and therefore subject to natural selection; and the fact that behavior, or rather, particular behaviors, are adaptive. . . . The genetic basis of hundreds and hundreds of particular behaviors has been demonstrated beyond doubt, and the adaptive significance of particular behaviors has been demonstrated in hundreds of cases [1971: 171].

Obviously, these two requirements are considerably easier to meet for animal than for human behavior. The reader is advised to keep these requirements in mind for the remainder of this discussion, for they are often lost sight of in the excitement of theoretical speculation. Space does not permit discussion of the process of evolution of behavior, but the method is analogous in major respects to selection of physical structures (Corning, 1971; Lockard, 1971).

AGGRESSION IN EARLY HUMANS

A great deal of controversy surrounds the nature of early human beings and the relative role that aggression did or did not play in their evolution. It is frequently assumed that the aggression displayed by present-day humans evolved long ago because of its adaptiveness, but has recently become increasingly dysfunctional in the modern world (Washburn, 1972; Corning, 1971; Somit, 1968). However, opinion is by no means unanimous that early man was very aggression-prone. I have already discussed Lorenz' theory and critiques thereof, so I will briefly summarize a few propositions of other writers.

Most spectacular, of course, is Robert Ardrey's (1961) thesis that humans evolved from “killer” prehomínids, *Australopithecus africanus*, with the further implication than man was born a killer. There has been controversy surrounding whether *Australopithecus* used weapons for predatory purposes and possibly even for intraspecies aggression, and

further controversy concerning whether the species was even directly in the line of human evolution. The latter question may now have been resolved in the negative with the recent discovery by Richard Leakey's team of a skull far more similar to modern man's (in terms of cranial capacity) which antedates the earliest known *Australopithecus* species by at least one million years. This suggests that Ardrey's "killer apes" may not be man's ancestors at all, but rather an unsuccessful side branch in the line of human evolution.

Fromm (1973: 41) criticizes S. L. Washburn's theory that early hunting man had a "carnivorous psychology" and "a drive for and pleasure in killing," although Fromm's interpretation that the pleasure was not in the killing but in the development of hunting skills is no more compelling than Washburn's. A number of theorists have tried to explain humans' aggression and cruelty in terms of man's "carnivorous and cannibalistic origins" (Rapoport, 1965: 115). However, as Rapoport (1965: 115-116) observes,

If we confine ourselves to man's 'carnivorous psychology,' we do not really have an adequate explanation of man's cruelty. When one speaks of man's inhumanity to man, one usually refers to intraspecific cruelty. There is no evidence, however, that carnivores exhibit more intense intraspecific aggression than other mammals. Indeed, among the carnivores intraspecific fighting is often highly ritualized and hardly ever lethal.

Further, Helmuth (1973) shows that the bases of cannibalism are most often not aggressive, although they may be in some cases. They usually are tied to religious or cultural beliefs and are often ceremonial. Also, although his data are only of the most tentative sort, Helmuth shows that exocannibalism may have developed out of endocannibalism, hardly evidence for "aggressive" origins of the practice.

HUMAN AGGRESSION AND CONFLICT FROM THE EVOLUTIONARY GENETICS VIEWPOINT

Here the concern is whether particular behavioral tendencies which are assumed to have a genetic basis will be selected for or against in the process of evolution. Aggression itself will be considered first, followed by other characteristics which may have implications for aggression and conflict.

Aggression. A number of authors believe that aggression would have been of great adaptive value in human evolution (e.g., Corning, 1971; Washburn, 1972). Part of the problem of evaluating such hypotheses stems from definitions. If one expands the definition of aggression to be virtually synonymous with assertiveness, as some authors do, then it is much easier

to agree that "aggression" may have been adaptive. If, however, we use the narrower definition (an act done with the intention of injuring persons or damaging property), then the answer is not so clear. As Halleck (1971: 231) says, "It is relatively easy to make a case for man's biological need for initiative or assertiveness, but whether the tendency to hurt others is instinctual is debatable." Both Corning and Washburn, I believe, would still insist that even the narrower form of aggression was adaptive. Other authors (e.g., Klopfer, 1969), however, insist that aggression would have been of little or no selective advantage, and that "it is at least equally plausible that early man showed very little 'aggression'" (Klopfer, 1969: 7). These positions may not be as diametrically opposed as they appear: it is at least conceivable that the capacity for aggression could have been selected for, but that its display was either infrequent or closely determined by objective necessity only.

Small-group attachments. It is often pointed out that humans are social animals, and that they are so by reason of biological predisposition (Corning, 1971; Willhoite, 1971). However, others feel that this social nature may have limitations. Washburn (1972) suggests that humans share with other primates a biological predisposition toward attachments to small rather than large groups, pointing out how orators use the rhetoric of the family to encourage people to come to the aid of much larger groups. Campbell (1972: 26-27) qualifies his main thesis (summarized in the next section) by saying:

The kind of 'selfishness' selected needs to be spelled out in more detail. . . . Thus familial solidarity [i.e., sacrifice on behalf of one's immediate family] is selected for, but group solidarity on larger than family lines that involves much risk or sacrifice on the part of the cooperator is in general selected against. . . . The degree of vertebrate sociality thus produced probably reaches its limit in that found within packs of wolves and chimpanzees which include several families, that is, a very limited degree of social interdependence.

Altruism versus "selfishness." The conventional wisdom used to be that humans are innately selfish and that social and cultural mechanisms are responsible for whatever cooperation or altruism exists. Then came the "enlightened," social-science-based view that humans are inherently neither selfish nor cooperative but that they are shaped in either direction (or both) by social forces. This was followed by the evolutionary-geneticist view that humans are genetically predisposed toward sociability, cooperation, and altruism. Now Campbell (1972) summarizes a more recent evolutionary-geneticist argument that suggests that the conventional wisdom was right all along but without knowing precisely why: namely,

that humans are necessarily genetically predisposed toward "self-saving cowardice" as opposed to "self-sacrificing altruism," and that the elaborate social and cultural mechanisms which are needed to overcome this tendency are responsible for humans' extraordinary degree of social interdependence. This predisposition occurs because, where there is "genetic competition" (involving genes for "altruism" and those for "selfishness") among cooperators, "there are stringent restraints against genetic selection for self-sacrificial altruism" (1972: 26). Selection, then, favors "self-saving cowardice," provided that there is "a self-sacrificial component to the bravery" (1972: 25). The only other species whose degree of social interdependence even approaches that of humans are the social insects, and this occurs because they are genetically predisposed toward self-sacrificing altruism due to the lack of genetic competition among cooperators (who are sterile). This is one of the rare cases in nature which escapes the process of natural selection, since selective elimination occurs to individuals different from those who reproduce.

Fear of novel stimuli. Dubos (1973) has suggested that mistrust and fear of the foreigner or the stranger may have biological origins. A similar but even broader idea is suggested in this quote from Bolles (1970: 33):

What keeps animals alive in the world is that they have very effective *innate* defensive reactions which occur when they encounter any kind of new or sudden stimulus. These defensive reactions vary somewhat from species to species, but they generally take one of three forms: Animals generally run or fly away, freeze, or adopt some type of threat, that is pseudo-aggressive behavior. . . . The animal which survives is the one which comes into its environment with defensive reactions already a prominent part of its repertoire.

A caveat. It must be remembered that, while selection acts upon phenotypes, including behavior, transmission of characteristics involves only genotypes. That is, behaviors may be selectively eliminated or "encouraged" by the environment, but only genetic material is transmitted. Thus, particular behaviors are "evolved" in the biologically evolutionary sense only if they have a genetic basis. If they do not, then their "selection" is, evolutionarily speaking, irrelevant. The theorizing about human behavior in the preceding sections is dependent on the assumption that each behavior in question has some genetic basis. Most of these assumptions do not yet have a solid evidential foundation. Before the authors' hypotheses can be accepted, they must demonstrate, for each behavior they discuss, that the behavior in question does indeed have some genetic basis. It will not do to show that *some* behaviors have a genetic

basis, or that behavior in general can, in principle, have a genetic basis. There is a separate burden of proof for each behavior in question. Until this can be convincingly demonstrated, the hypotheses must be regarded as unverified. (A few theorists mention an apparent exception to my strictures. The saving factor—or rationalization, depending upon one's viewpoint—is the so-called “Baldwin effect,” by which “natural selection . . . favors genetic fixation of traits which a given environment induces in the phenotype” [Dobzhansky, 1962: 289]. The evolutionists assure us that this is not the same as Lysenkoism or Lamarckism [Corning, 1971].)

CONFLICT OVER SCARCE ESSENTIAL RESOURCES

There is at least one other, somewhat more indirect, way in which biological factors can be said to underlie human conflict. In the course of outlining a grand evolutionary theory of “all human life” Corning (1971: 366) reminds us that the overriding goal of all organisms, including human beings, is still survival. It may seem elementary to point out that survival depends upon the satisfaction of biological needs, but in the context of present and future world conditions—burgeoning populations, an overall inadequate food supply, depletion of many natural resources, and stagnating national economies—one can begin to see another way in which biological factors may underlie conflicts. Such conditions will increasingly lead to conflicts of interest over diminishing resources needed to satisfy basic bodily needs, such as food and, in less temperate regions, fuel or energy for warmth. These conflicts will occur between nations, between classes, and even between individuals or families within classes. (In the recent beef shortage, there was a race to the supermarket in order to hoard. The slow got left out; the poor had no chance anyway.) The fact—and the psychology—of scarcity can be expected to promote conflict.

HUMAN PHYSIOLOGY AND AGGRESSION

The past two decades have seen remarkable progress in human physiology, much of which is relevant to the study of aggression. A great deal of evidence indicates that the level of aggression displayed by individuals, or their readiness to show aggression at all, depends in part on the state of their nervous and endocrine systems. Once again, I can mention only a few areas and those only in passing, but it should be enough to give the reader a feel for some of the areas under study. I will discuss work in three categories mentioned by Moyer (1973), plus a fourth category suggested by Davies (1970).

There are no data for humans comparable to those for animals showing that aggression can be selectively bred for, but the belief persists that differential aggressiveness in humans is to some unknown extent inherited. On the genetics side, there have been claims, based largely on clinical impressions, that XYY males are prone to extreme aggression and, as a result, are overrepresented among criminals. However, Owen's (1972: 224) careful review of the evidence casts serious doubt on the validity of "the 'aggressive XYY' stereotype." One study (Price and Whatmore, 1967b) even found that XYY's had *fewer* crimes of violence than a suitably matched control group (Owen, 1972: 255). Another study by the same authors (Price and Whatmore, 1967a) found no differences in property crime rates, but large differences in crimes against persons: although there were no differences for murder rates, XY males greatly exceeded XYYs in rates of assault and sexual assault. These findings illustrate the necessity for careful and thorough research before public policy recommendations are made (Somit, 1968).

NEURAL SYSTEMS AND THE BRAIN

Work seems to be proceeding rapidly on the mapping of brain-behavior relationships. This is a highly complex area which I am not competent to review adequately, and I refer the reader to Moyer (1971) and Delgado (1969). The evidence is sufficient to lay to rest the notion that aggression is wholly learned. Particularly impressive is Moyer's (1971) delineation of seven different types of aggression, each characterized by a particular neural and endocrine pattern.

Another topic, indirectly related to aggressiveness, which has received a great deal of recent publicity, is the phenomenon of the "minimal brain dysfunction" (MBD) child. The unusual thing about this disorder is that it is diagnosed behaviorally, on the basis of "hyperactivity," rather than biologically, since direct evidence of organic disorder has not been found. Large numbers of children have been diagnosed as having this syndrome and are being given daily drug treatment of stimulants (which have the paradoxical effect of calming such children). This practice has come in for harsh criticism, partly because so little of a physiological or biochemical nature is actually known about the syndrome, and partly because diagnosis on the basis of behavior may be highly subjective, and may in some cases simply be an expedient way of controlling unruly children who may or may not have a physiological problem (Chorover, 1973).

Research on the relationship between the brain and aggression is becoming increasingly controversial because of its implication for "con-

trolling" aggression. Writers in this area often slide easily from discussions of cause-and-effect relationships to recommendations for methods of control, with only the most perfunctory or bland references to ethical considerations (Moyer is an exception to this pattern, but he still comes down on the side of control). Frequently proposed techniques for controlling aggression include (a) psychosurgery (for opposing views on this volatile issue, see Mark and Ervin, 1970, for the pro-surgery side; and for the anti-surgery arguments, see Chorover, 1973; Trotter, 1973a; and almost anything by Peter Breggin); and (b) electrical stimulation of aggression-inhibiting neural systems in the brain (perhaps by means of an already-operational radio receiver-transmitter which can be implanted in the brain for the purpose of monitoring a person's whereabouts and delivering shocks if he shows signs of becoming hostile or aggressive; see Ingraham and Smith, 1972). The so-called "side effects"⁷ of these techniques often do not receive as much attention as is warranted.

BLOOD CHEMISTRY AND ENDOCRINE SYSTEMS

The field of psychopharmacology is obviously highly relevant to the study of aggression. Once again I will not attempt to summarize this area and will instead refer the reader to other sources: Somit (1968), for a good layman's introduction; Kumar et al. (1970), for a more technical, thorough treatment; Crane (1973), for a discussion of some of the field's shortcomings and unintended consequences; and Warren (1973, 1972), for an account of public and professional reaction to the proposal by a recent president of the American Psychological Association that world leaders be given pacifying drugs in order to save humanity from certain destruction.

Also indicative of work in this general area are two other studies. Persky et al. (1971) discovered a close relationship in young men between testosterone levels and psychological measures of aggression and hostility. And ethnologist Ralph Bolton has found at least preliminary evidence among the Qolla (an Andean subculture in Peru and Bolivia) that their extreme aggression may be related to chronic hypoglycemia (abnormally low glucose level in the blood; Trotter, 1973b).

DIFFUSE BIOLOGICALLY BASED NEEDS

In this residual category belong a number of supposedly universal and inherent needs. Davies (1970: 617) includes among them the "substan-

7. "When we think in terms of systems, we see that a fundamental misconception is embedded in the popular term 'side-effects.' . . . This phrase means roughly 'effects which I hadn't foreseen, or don't want to think about.' As concerns the basic mechanism, side-effects no more deserve the adjective 'side' than does the 'principal' effect. It is hard to think in terms of systems, and we eagerly warp our language to protect ourselves from the necessity of doing so" (Hardin, 1969: 291).

tive” needs of the familiar Maslow hierarchy (“the physical needs, the social-affective or love needs, the self-esteem or dignity needs, and the self-actualization needs”) as well as three “implemental” needs: “the needs for knowledge, security, and for control or power.” What is innate, says Davies, are these basic needs plus the tendency to fulfill them, *not* the tendency to fulfill them violently. His basic model is a version of the “frustration-aggression” theory: “The basic needs that all human beings have in common are fundamental instigators to an action sequence which, when severely frustrated, is likely to produce aggression, including political violence when government is blamed for the frustration” (1970: 618). Unfortunately, he says, we do not yet know enough “to establish either the loci, the circuitry, or the glands in the nervous and endocrine systems where the basic needs of man generate and function” (1970: 622).

BIOLOGICAL FACTORS AND WAR

It would be a great oversight for this review not to give some special attention to the problem of war, one of the central concerns of this journal. It has already been established that research on animal behavior has little or no relevance to human warfare, first because there are no adequate homologues or even analogues to it among nonhuman species (Scott, 1969), and second because of the inadmissibility, even if such similarities did exist, of direct extrapolations from one species to another.

Even at the human level, individual aggression is not even a necessary, let alone sufficient, cause of warfare. War does not result from the outpouring of individual aggression, whether instinctual or not. It is planned and coordinated rather than expressive and is intended, at least, to be highly instrumental, even when fighting is over symbols rather than material interests. This orientation is epitomized by the Clausewitzian dictum that war is an extension of diplomacy by other means (see Rapoport, 1966). Wars are not products of personal aggression, although individual acts in the course of a war may be triggered by certain immediate conditions tapping into a possibly “biologically programmed” source of aggressive behavior. It is well known that it is even possible for large aggregations of individuals to carry out acts of violence and destruction without any feelings of aggressiveness or hostility (Kelman, 1973; Sanford and Comstock, 1971). Indeed, to some who think of aggression in the broad sense of initiative and assertiveness, it may be a force for life rather than death.

The real responsibility for war lies in the tendency to follow society's demands quietly, through repetitive roles, following the line of least resistance in the service of some ideal of nation, race, class, etc. The army recruit is following the line of least social resistance. . . . However, the young man who refuses to fight is actually the most aggressive in this situation. He will encounter more social tension, standing as a self against society [Agel, 1972: 118].

Rapoport (1965: 116) summarizes the viewpoint that war is not the result of individual aggression.

We see how the problem of war can confuse analysis, for example by making 'fighting' and 'warmaking' appear practically synonymous in spite of the fact that there may be only a remote connection between them. We say that boys fight and also that nations fight. Having identified the two manifestations of aggression, we seek mechanisms common to both types of fighting. There may or may not be such common mechanisms. Or there may once have been common mechanisms underlying both individual aggression and warmaking—mechanisms that no longer exist. Certainly a personal predilection for overt aggression is no longer the mark of the warrior. Indeed, the warrior himself is about to disappear as a component of a nation's warmaking apparatus. He seems to have been replaced by the strategist, the scientist and the technician. It is not obvious that people in these roles have greater propensities for fighting than other people. Yet when and if the time comes, their activities will result in more bloodshed than those of the combined hordes of Attila, Genghis Khan and Adolf Hitler.

Wars occur because they are thought to be useful in attaining certain goals (Scott, 1973) such as material interests and sovereignty, or other symbolic goals such as saving face, maintaining a particular image in the world, or advancing an ideology. In the latter connection, Kenneth Boulding has said, with characteristic hyperbole, "The only religion that still demands human sacrifice is nationalism."⁸ Recall that while some evolutionary biologists have proposed that humans are biologically predisposed to strong attachment to ethical and ideological beliefs (Waddington, 1960), others have come to believe that selection does not favor tendencies toward sacrifice on behalf of such beliefs (Campbell, 1972). This may be cause for qualified optimism:

Accepting the conclusion that man's termite- and ant-like capacity for military heroism is in culturally transmitted dispositions, not genetic ones, makes me more optimistic about the possibilities of social inventions eliminating war, for such developments will have the temptations of biological selfishness on their side. However resistant [sic] culture is to change, it is probably less so than the gene pool. . . . These optimistic observations do not of course imply optimism about the organizational future of those societies which are first to

8. In a lecture at the University of Michigan, January 28, 1969.

lose the archaic capacity to fight wars, for until all nations have achieved this state of intelligent cultural decay, those that achieve it first will be at a decided disadvantage in international competition [Campbell, 1972: 34-35].

CONCLUSIONS

THE APPLICABILITY OF BIOLOGICAL APPROACHES TO HUMAN CONFLICT

It is difficult to generalize about approaches which vary as widely as those presented here. (Remember also that "biological" does not necessarily mean "innate" or "immutable"—biological factors may be the result of experience or interaction with the environment.) However, I think it can be said that a valid case has been begun for the applicability of biological approaches to conflict, or at least aggression. But the question is not really *whether*—that answer has to be in the affirmative—but rather *how much*. How relatively important are biological factors compared to others discussed in this review? Given the current state of still-limited knowledge, one's answer is likely to be highly subjective, not unlike an assessment of whether a partial glass of water is half full or half empty. In addition, such an evaluation depends heavily on the type of aggression being considered—in the case of war, biological factors probably have no causal importance, whereas in cases of, say, wife-beating or child abuse, their influence may be considerable. However, despite these difficulties, we do know enough to at least put the problem in some kind of perspective.

What are the criteria by which we can evaluate a theory? At a minimum, we can list the following: (a) Other things being equal, are the propositions true with regard to the phenomena chosen for explanation? (b) Other things *not* being equal, how "powerful" are the effects hypothesized? Are they "robust" or, instead, more subtle and "overrideable" by other effects? If they are true, are they the whole truth? (c) What is the scope of application? How wide a range of phenomena can the propositions accurately account for? How ubiquitous are the hypothesized effects—are they all-pervasive, or do they occur only under certain conditions? (Some might include parsimony as a criterion, but in many cases this may be more an epistemological, stylistic preference than an essential requirement for adequately understanding a phenomenon.)

With these criteria in mind, let us try to put the biologically oriented approaches to behavior into perspective. Table 1, adapted from Scott (1969) with only slight revisions for applicability to human as well as

TABLE 1
THEORETICAL FACTORS AFFECTING BEHAVIOR

<i>Level of Organization</i>	<i>Scientific Discipline</i>	<i>Examples of Phenomena Studied</i>
Ecological	Ecology	Population organization; supply of natural resources and food
Societal	Sociology, anthropology, political science	Social organization; cultural and political phenomena
Organismic	Psychology, animal behavior	Behavior; mental processes; learning
Physiological	Physiology, biochemistry	Physiology of behavior; emotions
Genetic	Genetics	Inheritance of behavioral capacities

SOURCE : Scott (1969), with minor revisions.

animal behavior, shows the range of factors that must be considered in trying to account for behavior. Behavior is affected by factors at each level, but, in addition, "there are influences that are not apparent from this scheme and which act at every level of organization—the evolutionary history of the species and the developmental history of the individual" (Scott, 1969: 134).

In this larger context and in light of the theoretical criteria listed above, it should be fairly clear that purely biological approaches to the study of human conflict will in general have some, but quite limited, applicability. Their contribution to knowledge in this area is probably greater than most social scientists would care to admit, but considerably less than is claimed by many of their advocates. Much of their knowledge is sound, but taken apart from knowledge in a great number of other areas, it paints at best incomplete and at worst fatally misleading pictures of the nature of human conflict. The only sort of biologically oriented approach that has any hopes of even coming close to adequately accounting for human conflict might be something of the order of Corning's (1971) ambitious evolutionary-based theory, but even it has a number of blind spots.

Any biologically based approach that aspires to an adequate explanation of human conflict must of necessity be an amalgam of social, psychological, and biological factors (probably with the biological factors as mediating variables or mechanisms for social and psychological stimuli). This is so because there are a priori logical reasons why purely biological approaches are doomed to limited applicability. First, they cannot adequately account even for the behavior of individuals, as we have seen in

this review. And furthermore, even if they could, social relationships and processes such as conflict cannot be explained in terms of individual characteristics or properties. For example, commenting on the relevance of several earlier psychological works on personal aggression for the study of international conflict, Daniel Katz (1961: 70-71) states:

Interesting as these contributions are, I believe they are not central to an explanation of violent conflict between nations. They help in our understanding of why violence is part of the behavioral repertoire of human beings but they have nothing to say about particular forms of group action which involve the use of violence. Conflict between nations cannot be equated to conflict between individuals. Too many other variables are involved in international conflict to consider it as the sum of the aggressive tendencies of individual citizens. The properties of a social system are not found by the simple addition of the properties of individual component members. These properties are the result of the complex interaction of people engaged in the many role relationships of national systems and sub-systems. In other words, our actions in the roles we play in a social system are limited and determined by the role requirements of the system and are not the direct reflection of our own personality needs and desires.⁹

I mention these issues because the momentum which the biological approaches are picking up is tempered only slightly by any professed recognition of where they actually fit in the larger picture. (There are laudable exceptions—e.g., Alland, Scott, and Corning, to mention just a few.) There seems to be a pattern within many disciplines that, when a prevailing one-sided or incomplete paradigm is overtaken by a challenging school of thought, the new orthodoxy is often as extreme in the opposite direction as was the original. “One mark of the profoundly influential social theory seems to be its strong biases. To reorder our thinking, to shake loose from customary strictures, large segments of reality must be sacrificed” (Gergen, 1973: 306). One might say that, following every paradigm, there is an equal and opposite counter-paradigm. If one is to believe Davies (1970), a biologically naive environmentalism flowing out of Locke and Marx has been the prevailing orthodoxy in the social sciences until recently (psychology has been an exception, at least with regard to its view of real-world problems; see Caplan and Nelson, 1973). If

9. Similarly, Feibleman (1973: 8) says: “Recent studies of the integrative levels show that the cumulative upward accumulation of properties, including qualities, cannot be reversed. The higher structures are built on the lower, but they cannot be reduced to the lower. Molecules contain atoms, cells contain molecules, organisms contain cells, but molecules cannot be reduced to atoms any more than cells can be reduced to molecules or organisms reduced to cells. The qualities which emerge at each of these levels is [sic] also irreducible.”

enthusiasm rather than sagacity carries the day with regard to the biological approaches, we may simply be in the process of "[replacing] one form of determinism with another" (Somit, 1968: 561). Severin (1971) postulates a predictable sequence which "models of man" undergo in the course of psychological research. They start out, he says, as guides for certain areas of research; before long they are considered an adequate description of human beings; and finally they become "dogmas about what people are really like" (1971: 1). However, responsible contributions, both to theory and to society, necessitate rejecting the Law of the Instrument (Kaplan, 1964; Caplan and Nelson, 1973) as an operating principle, and becoming "interdisciplinary" in ways which many of us had not even considered until now and which make earlier uses of that term now seem ironically truncated.

CONTRIBUTIONS OF BIOLOGICAL APPROACHES TO HUMAN BEHAVIOR

Some comments are in order regarding the positive contributions of the biologically oriented approaches to human behavior as they are presently formulated.

(1) They make up a fast-developing set of interrelated fields capable of drawing upon each other and which show great promise for shedding more light on the biological inputs to a wide range of human behavior.

(2) By reminding us all of our basically biological nature, they destroyed the, at times, smug complacency of the "oversocialized" conceptions of humans (Wrong, 1963). Although it may seem "logical to say that social facts can be accounted for only by antecedent social facts" (Washburn, 1972: 35), this can no longer be automatically assumed. Social scientists can no longer ignore or avoid biologically oriented approaches to human behavior, for the battle has been carried to our domain (territory?), and the ranks of these approaches are swelling. They are at the parapets, and we had better decide soon whether to join them, fight them, or, perhaps better, establish diplomatic negotiations that will be critically constructive to both sides.

(3) In a closely related vein, these approaches have demonstrated the necessity for social scientists to "tool-up" on biological matters, and for some of the biological advocates to "rigor-ize" their logic and to become more aware of social factors. It is difficult to say which side's needs are greater. The dialogue between the two camps now suffers from a tendency toward bifurcation: many of those who are trained in biological areas are inclined to be less critical than they should be toward application of

biological concepts to other areas, while many of those who are predisposed to be skeptical about such applications do not have the necessary skills and knowledge to adequately criticize them. There are an encouraging number of individuals in the middle ground, but there should be more. In short, we need more technical competence in biological matters by social scientists, and a less evangelistic, more self-critical approach by those already competent.

(4) The relationships between “innate” and “acquired” factors have been clarified and sharpened somewhat, thereby demonstrating that neither is quite as pure a concept as its respective proponents had thought.

(5) The biological approaches have sparked great new public interest in the study of human origins. The challenge remains, however, as to whether the facts can be responsibly communicated or whether the public will be pandered to with myths of “killer ape” ancestors and such.

(6) These approaches have clarified relationships between species and have shown the true relevance of animal behavior studies for human behavior: namely, for hypothesis-generating and method-sharpening (Lockard, 1971), rather than direct extrapolation of results.

(7) Finally, the biological approaches have served to remind us of our place in nature: we are in a very rough continuum of animal life, significantly but not wholly different from other animals, an intrinsic part and product of nature. As Julian Huxley optimistically put it:

Only if we know and face the truth about the world, whether the world of physics and chemistry, or of geology and biology, or of mind and behavior, shall we be able to see what is our own place in that world. Only as we discover and assimilate the truth about nature shall we be able to undertake the apparently contradictory task of re-establishing our unity with nature while at the same time maintaining our transcendence over nature [quoted in Somit, 1968: 553].

Embeddedness in and transcendence above nature—yet another human dilemma, and a peculiarly bittersweet one, as Shepard and McKinley (1969: 13-14) remind us.

Men hail the freedom which raises them above animals. Our consciousness is deeper, the bondage to instinct is loosened, the limitations to place are transcended. And then there is the dream of ultimate freedom from nature itself which would be the next logical step, for the physical world seems to exert a kind of tyranny.

But this idea of tyranny misuses both ideology and nature. The uniqueness of the human species is not a great political triumph, but is itself nature. It has a

biological basis and an evolutionary background. To understand human freedom is to discover its limitations. . . .

If freedom for us be choice of action guided by knowledge, we are in a strange paradoxical situation. For our knowledge of the intricacy of life and of our ability to destroy it links us by a thousand new ecological strands to the most minute as well as the grandest aspects of our world. In our freedom is a burden if not a bondage that no monkey was ever asked to bear.

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