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Is a Cultural Ethology Possible?[1]

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The possibility, desirability, and potential outcomes of applying ethological methods to the study of culture-specific human behaviors are investigated. Ethology and culture are explored. A new term, "instruction," and its use in cultural ethology are proposed. Genetics and survival value are related to cultural ethology. A cultural ethology is given a possible theoretical foundation, and current attempts at a cultural ethology are appraised. A research program in cultural ethology and related fields is proposed.

KEY WORDS: ethology; culture; behavior; natural selection.

INTRODUCTION

Ethology, the study of species-specific animal behaviors under more or less natural conditions, has recently enjoyed a spurt in productivity and in popularity among laymen. Some authors have tried to apply findings derived from the study of other animals to humans; others have undertaken classical ethological studies of humans in order to describe and explain human behaviors which are species-specific, panhuman, and presumably genetically controlled. Here we investigate the possibility, desirability, and potential outcomes of applying ethological methods to the study of culture-specific human behaviors.

What is meant by "cultural ethology"? To answer this question, I begin by noting some characteristics of ethology and of culture.

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ETHOLOGY

In the broadest sense, ethology is the study of animal behavior in the wild, under simulated natural conditions, or at least under laboratory conditions resembling natural or wild ones.[3] This emphasis on field studies or surrogates thereof derives from an interest in the *adaptiveness* of behavior. Really to understand a behavior, one has to know its survival value, its function (Lorenz, 1965). The only way to do this is by studying the behavior as a response to naturally occurring stimuli to which the phylum has become adapted over generations, and by observing the effect of the behavior in the animal's natural environment.

Another characteristic of ethology, and one which is linked to naturalism and functionalism, is a concern with species-specific behavior. An ethologist is usually an expert on the behavior of a particular phyletic group, and theoretical work tends to be comparative. When constructing an "ethogram" of his animal, the ethologist tries to describe each behavior as exactly as possible. Such a description includes details of both what the animal does and the exact circumstances in which it does it. Great effort is often devoted to identification and description of the environmental cue that "releases" a given behavior. For example, it was observed that black-headed gulls remove broken eggshells from their nests. Tinbergen *et al.* (1962) were able to show, by a series of 22 field experiments, precisely what features of the broken eggshells released that behavior.

The ethologist tries also to specify in detail what the animal does when confronted with the particular environmental cue. The gull, for instance, flies or walks away from the nest, carrying the broken eggshell in its bill, and drops it. Finally, as a sort of final confirmation that he has indeed described a natural behavioral unit, the ethologist tries to demonstrate the survival value, or function, of the behavior. In the example, it was shown that predators do indeed use broken eggshells as a beacon for homing in on unhatched eggs and newly hatched gull chicks.

In short, the ethologist (1) chooses for study behavior that is somewhat stereotyped and (2) describes that behavior in full detail.

Yet, in much of the ethological literature one is struck by what appears to be a terminological ambiguity, and perhaps even a conceptual one, in the use of the term "a behavior" and of terms denoting specific behaviors. Insofar as behaviors are supposed to be the very units of ethological observation and analysis, I think this ambiguity has seriously retarded the development of ethological thought. Sometimes the term(s) denoting behavior seems to be used just for what an animal does. ("What cue releases behavior X?") But at other times it

[3] My usage of "ethology" here is very similar to that of G. P. Baerends (1958). Others tend to view ethology as a coming together of this kind of ethology and other ways of studying behavior to produce what I would call an "ecology of behavior," an overall understanding of a complete behavioral system in context.

seems to be used for something encompassing both the cue *and* what the animal does. ("What is the survival value of behavior X?")

In the first usage, the behavior term denotes a regularly observed change in relationships of material structures. In the second usage, on the other hand, the behavior term implies an unobserved, enduring structure or set of related structures *internal* to the animal. Linking one or more environmental stimuli or cues to a behavior (usage 1), this internal structure or set bears a causal relationship to that behavior (usage 1), and this compounds the ambiguity.

To resolve the ambiguity, I propose to continue using "a behavior" and specific behavior terms just for what the animal does, and to use the term "instruction" to refer to the unobserved enduring structure or set internal to the animal. That an animal has or *carries* a certain instruction is inferred from the fact that it exhibits a certain behavior, but only under certain environmental conditions. One of these conditions - the cue - seems unique to that behavior. The cue and the instruction are thus both necessary causal conditions of the behavior. A given instruction can be depicted by a brief description of its cue and its behavior.

In the strict sense, an instruction is *any* material structure that regularly exhibits a characteristic behavior in response to a characteristic cue. But here we are interested in *interneural* instructions, which establish unique causal links between sensory mechanisms and motor mechanisms in animals that have nervous systems (*cf.* Klopfer and Hailman, 1967: 198).

Now, the sensory and motor mechanisms are, of course, themselves instructions; so, to speak strictly, an observable behavior is the product of *three* instructions behaving serially: (1) a sensory instruction: cue, an observable event in the animal's environment; behavior, the dumping of neurotransmitter substance (NTS) into certain synapses; (2) an interneural instruction: cue, the receipt of NTS from the sensory instruction; behavior, again a release of NTS; (3) a motor instruction: cue, receipt of NTS from the interneural instruction; behavior, observable change in relationships of material structures of animal and/or environment. For the sake of brevity, we will hereafter depict any interneural instruction by brief descriptions of (1) the cue of the sensory instruction that cues it and (2) the behavior of the motor instruction it cues. So, in the black-headed gull example, the interneural instruction might be depicted as: "cue, small white object in nest; behavior, removal of object from nest." And the term "instruction" will henceforth refer to an interneural instruction unless otherwise indicated.

Compound-Complex Instructions and the Hierarchy of Instructions

The black-headed gull (BG) instruction is *a compound-complex* instruction. It is complex because in all probability it involves a hierarchy of simple interneural instructions and control systems (Cloak, 1974). The behavior of the simple instruction highest in the hierarchy, BG#1 (depicted exactly as the compound-complex instruction itself), is to *address* or "motivate" other instructions and control systems (Powers, 1973; *cf* Fisher, 1964/1966) - a control system, BG#2, controlling the perception "small object (retained) in bill"; an instruction, BG#3, "cue, small object in bill; behavior, move(ing) away from nest"; and many more.

The gull instruction is compound because its elements operate in *series* (*cf* Marler and Hamilton, 1966: 251-253). Although addressed (motivated) by BG#l, instruction BG#3 behaves only when cued by the observable behavioral result of control system BG#2; that is, only when BG#2 has got the eggshell in the animal's bill.

It seems hardly necessary to remark that component instructions like BG#2 and BG#3 may themselves very well be compound and/or complex (*cf* Marler and Hamilton, 1966: 203-225). Only simple instructions and control systems at the bottom of a hierarchy actually address motor mechanisms.

The *motivation state* established by a high-up instruction like BG#1 may simply decay after a time, or it may be scavenged by a special instruction cued by the behavioral result of an instruction late in the series (e.g., Marler and Hamilton, 1966: 124-131).

Analysis of an instruction into its components, by observation and experiment, is characteristic of ethological work (*cf.* Eibl-Eibesfeldt, 1970: 45; Dilger, 1962/1966). But these components are surely themselves compound-complex instructions which could be analyzed further to show that they are actually sets of simple instructions, ordinarily executed together in hierarchical and series arrangements. Experiments in electrical stimulation of the brain (*cf.* von Holst and von St. Paul, 1962/1966) sometimes serve to pick out bits and pieces of a hierarchy; they may also reveal interneural instructions by cuing them "directly," i.e., without the usually necessary execution of a sensory instruction.

Continuity and Discontinuity in Behavior

Variation in the environment of a compound-complex instruction may cause variation in the presence, cuing, and/or sequencing of some component simple instructions. Simple instructions are quite rigid; observed in isolation, the expression of a single simple instruction is a model of stereotypy. The reason much complex gross behavior appears to flow smoothly and to vary continuously (both from animal to animal and from time to time in the same animal) lies not in "elasticity" or "plasticity" of the component simple instructions, but rather in their numbers, the relatively small contribution each makes to the total movement, and the physical inertia of the animal and its parts. So the fact that the compound-complex instructions selected by ethologists for study often have a

stereotyped, fixed-action quality does not mean that they are seriously atypical. They simply reveal more clearly the fundamentally discontinuous nature of behavior, just as the traits Mendel selected for study revealed more clearly the fundamentally discontinuous nature of heredity.

Acquisition of Instructions: The Genetic Bias of Ethology

The basic structures of a nervous system are, of course, shaped by the animal's genetic system during its ontogeny. The uniquely defining characteristics of each interneural instruction, the "fine tuning" or *programming* of the nervous system, are determined in some cases by gene action and in other cases by specific learning mechanisms, i.e., by mechanisms which respond to specific kinds of environmental events. Such programming is often called "acquisition" of behavior (i.e., of instructions).

A characteristic of ethology is that it recognizes and studies several modes of acquisition rather than specializing in the study of only one mode. I think it may be fairly said, however, that most ethologists regard genetically programmed instructions as somehow fundamental or basic (Tinbergen, 1969; Crook, 1970; Hinde, 1966). In other words, the idea of instructions seems implicit in their work, but only when they are discussing genetically programmed mechanisms. They seem to see learning or other forms of experience in the organism's environment as somehow "modifying" the genetically programmed repertory or even as merely influencing its expression; they do not speak of environmental or cultural *programming.* This bias is understandable, arising from the emphasis on species specificity of behavior, but I believe it is incorrect.

Take a computer analogy: A general-purpose digital computer leaves the factory with a few instructions wired in - for addition and subtraction and counting, and for receiving, storing, and executing simple instructions transmitted to it by a tape reader in the form of electrical impulses. At the user facility, the computer is fed a tape. The first few instructions on that tape, once stored, direct the computer to process and store the next few instructions, etc. Hence the computer is wired not just to "learn" but rather to "learn" to "learn" to "learn," etc. By the time the computer is ready for the user's program, a sizable portion of its "memory" is filled with acquired instructions without which it could not even receive and store the user's input, let alone do anything with it.

Now, to say that those acquired instructions merely modified the basic wired-in instructions or merely influenced their expression would be highly inaccurate. Each instruction is a complete behaving unit; once it is there, its effect on the overall behavior of the machine is unrelated to its source or its age. Whether it was programmed in just recently or whether it appeared in the memory the instant the machine was plugged in has no bearing on its durability or its effect.

Now apply this analogy to an ethological example. Every baby chick, baby monkey, or other baby omnivore carries instructions (1) to put any object it sees, within a certain range of sizes, into its mouth, and (2) when such an object contains sugar, to program itself with another instruction, namely (3) to approach and eat such objects on sight. Instructions (1) and (2) are presumably programmed into the nervous system by genes during the normal ontogeny of the animal. Instruction (3) is acquired through interaction with the organism's environment, and its acquisition is utterly dependent on the presence, and behavior, of instructions (1) and (2). So, to generalize, that which an organism learns is highly species-specific. Yet, once programmed in, instruction (3) has exactly the same status and, I think, the same general material structure as instructions (1) and (2). Indeed, the food preferences of adult omnivores are highly stereotyped, so it appears that instruction (1), at least, is somehow deactivated or disconnected by later ontogenic processes.

So, to generalize again, the role and relative importance of an instruction are independent of its mode of acquisition. If this generalization holds, it follows that controversies about "nature vs. nurture," and about the innate nature of the human species, are founded on false premises - not because the nervous system is infinitely "plastic" but because the infant nervous system, constructed and programmed by the genetic system, requires, demands, and seeks a great many specific kinds of environmental inputs in order to acquire the neural instructions which make it a normal adult nervous system. So, functionally speaking, learning is part of the *ontogeny* of an individual animal. Just as it needs the right enzymes, etc., produced inside its skin and, therefore, the right nutrients available outside it, so also it needs a set of instructions normal for its kind, and, therefore, the right species-specific sensory and motor experiences. The latter may be provided by the behaviors of its conspecifics or by other environmental features.[4] Once these instructions have been acquired, however, they are as much a part of the animal as its cells, tissues, and organs, and, of course, its genetically programmed instructions.

To summarize our characterization of ethology: Ethology is distinguished from other studies of behavior by its insistence on close observation of the details of animal activity, its emphasis on the analysis of each instruction into its components, and its attention to the evolutionary function, or survival value, of each instruction in its environmental and instructional context. Concerned with species-specific instructions, it tends - erroneously, I think - to consider genetically programmed instructions as fixed and discontinuous, and environmentally acquired behavior as plastic, continuously variable, and somehow less fundamental than genetically programmed instructions.

[4] This is Bowlby's "environment of evolutionary adaptedness" (Barkow, 1973: 375-376).

CULTURE

There are several related processes by which instructions, once stored, may program the central nervous system with further instructions. For humans, these processes include habituation, imprinting, classical (Pavlovian) conditioning, operant (Skinnerian) conditioning, observational learning, and tuition.[5]

Cultural Instructions

By "tuition" I mean the programming of an instruction upon one's hearing a linguistic analogue of that instruction uttered by a conspecific. ("When the streetlights come on, you come straight home.") Tuition, therefore, is almost surely unique to humans. But more important to humans is observational learning, which we carry on in a highly refined form. Here I mean the programming of an instruction upon observing the execution (cue and behavior) of that instruction by a conspecific. I think we have and often use the ability to acquire by observation each simple component instruction of an elaborate compound-complex instruction, and thus to copy and store with great fidelity the instructions carried by a conspecific (Bandura and Walters, 1963: 106). Since the observer/acquirer is usually younger than the demonstrator, and since they are usually members of the same small social unit, instructions acquired and passed on by this process are, collectively, the behavioral tradition of that unit, i.e., *cultural instructions.*[6]

On the basis of various natural experiments and observations, I believe that culture is acquired in tiny, unrelated snippets, which are specific interneural in-

[5] The last three processes mentioned appear to be the primary modes for E. T. Hall's (1961: 61-72) "formal," "informal," and "technical" levels of culture-learning, respectively (*cf.* Scribner and Cole, 1973).

[6] Bandura's (1971) recent thinking seems to diminish the distinction between tuition and observational learning. One could argue that tuition is a phylogenetic acquisition of the human species alone; it utilizes the internal mechanisms of observational learning, copying neural images evoked not by actual observations but by linguistic utterances. The tuition function of language, however, should be distinguished from its cuing function. The linguistic utterances of others are features of our environment, and thus may serve to cue gross-behavior instructions carried by us. Gross behaviors change our environment, or our relationships with it, and therefore have direct survival value. Language also participates in the programming process when the central nervous system (CNS) being programmed already carries instructions to be "rewarded" or "punished" by certain utterances; then the utterances can be used for operant conditioning. Generally, however, I think the cuing function is by far the most important function of language. Specific utterances, vocabulary, etc., are presumably acquired mainly through observational learning (Hall, 1961: 102). As for the acquisition of instructions for using and interpreting language, whether that involves another separate neurogenetically based process or whether it is accomplished entirely through the last two or three processes mentioned appears to be a completely open question.

structions culturally transmitted from generation to generation. These "corpuscles of culture" are transmitted and acquired with fidelity and ease because the organisms in question are phylogenetically adapted for transmitting and acquiring cultural corpuscles, an adaptation that has required at least 2 million years, and perhaps 40 million, of intense "selection pressure."

In most social science discourse, "culture," like "behavior," is used in two senses bearing causal relationships to each other. In the case of culture, the causal relationships seem to be more complicated and profound than do those between an instruction and its behavior. If we investigate this ambiguous usage, we find that what can be called the *i-culture* of a people is the set of cultural instructions they carry in their central nervous systems. The *m-culture* of the people encompasses the material structures, relationships among material structures, and changes in these relationships which are actually brought about or maintained by behaviors of those cultural instructions. Features of a people's m-culture thus include features of their behavior, their technology, and their social organization (and their ideology when considered as a set of verbal behaviors).

It is curious that while the elements of i-culture are tiny, unrelated snippets, acquired and stored in a rather helter-skelter fashion like a genotype, the behavioral outcomes of those elements, the features of m-culture, often exhibit a high level of orderliness, pattern, functional integration, etc., like a phenotype. How does this orderliness come about? We shall deal with this question shortly.

To illustrate the distinction, and the causal relationships, between i-culture and m-culture, let us look at them in the context of an individual culture carrier, Joe. First, i-culture affects m-culture. When any of Joe's cultural instructions are executed, he usually modifies his surroundings, thus contributing to the building and/or maintenance of the m-culture he lives in. He may greet a friend, participate in a ritual, get married, help build a house, etc.

Second, i-culture affects itself. This occurs when the process of observational learning operates and one of Joe's cultural instructions thereby replicates itself in the nervous system of an observing organism, for example, his child.

Third, m-culture affects itself. If an m-culture feature cues one of Joe's cultural instructions, it is a cause of his behavior, an m-culture feature. Or an m-culture feature may facilitate or prevent the occurrence of another m-culture feature, partially or completely, by interfering somewhere in the sequence of outcomes of behavior that result from one of Joe's cultural instructions.

Fourth, m-culture affects i-culture. For instance, some of Joe's m-culture behavior may expose Joe differentially to potential demonstrators - sources of observational learning - thus determining what additional cultural instructions he acquires. Other m-culture features may reward certain trial responses he makes and punish others, thus helping to determine what interneural instructions he acquires through conditioning. Further, certain of Joe's m-culture behaviors may put him in a better position to *be* a demonstrator. In other words, features of m-culture, like other environmental features, help determine the *occurrence* of cultural instructions in certain *locations,* either by helping to put a human nervous system into a location or by helping to put a cultural instruction into a nervous system already in a location. Thus m-culture features are causal in the process of *natural selection* of cultural instructions.[1]

The Natural Selection of Cultural Things

I have given a fairly thorough theoretical treatment of natural selection of instructions elsewhere (Cloak, 1973). Here it suffices to say that a cultural instruction whose behavior, an m-culture feature, helps determine its subsequent occurrence, replication, or endurance in many locations will thereby survive and propagate, and thus its behavior will frequently recur, and so on. But, as a rule, cultural instructions, even compound-complex ones, do not operate alone on the world. Rather, a number of cultural instructions cooperate. That is, their immediate behaviors interact with each other to produce an m-culture feature that, in turn, helps determine the occurrence of all such instructions in certain locations.

Such a set of cooperating cultural instructions is *a system.* As a system of instructions proliferates in a given environmental subregion, its several instantiations come into "constructive" *competition* with each other. Any instantiation of the system which is fortuitously modified - usually by the acquisition of a novel component instruction - so that the m-culture feature it produces is better able to help determine the occurrence of the whole set in certain locations will often thereby exclude the other instantiations from surviving or propagating in those locations. Then it is only a matter of time before the modified instantiation becomes typical of the system.

As this competition process is repeated, of course, the system becomes more complex and, as a rule, the m-culture feature becomes more elaborate and more "powerful" in terms of its particular environmental effects. There are certainly limits to elaboration. Beyond these limits, further elaboration either makes the m-culture feature less powerful and directly noncompetitive or else has indirect deleterious effects; i.e., its activity may begin to interfere with the sur-

[1]That the cultural environment (m-culture) has helped control the evolution of the human *genetic* repertory is now thoroughly accepted in anthropology through the writings of T. Dobzhansky and many others. That the cultural repertories (i-culture) of different human groups have evolved through natural selection has been acknowledged sporadically, but discussion has invariably been in terms of cultural adaptation to the *natural* environment. I find it curious that the idea that the *cultural* environment helps control the evolution of a *cultural* repertory, and that this process might be the principal one in the general evolution of culture, has not been accepted - if, indeed, it has ever been put forth. It is perhaps implied by Ruyle (1973). This idea is, cybernetically speaking, one of positive feedback; it applies *mutatis mutandis* to genetically controlled environments and genetic repertories (Bajema, 1973; Crook, 1970).

vival/propagation of some larger instructional system of which it is a component subsystem.

Often, however, better *organization* enables an m-culture feature to become more powerful without exceeding the limits of elaboration. Thus cultural instructions that help to make a better-organized version of an m-culture feature will often be adopted into a system by the above competition process. This explains the curious fact, already noted, that m-culture features often appear remarkably well organized or integrated. It also explains how an m-culture feature becomes shaped for the *functions* it performs - to do the particular things it does to help determine the occurrence of cultural instructions in certain locations in a certain environment.

The outcomes of the i-culture-m-culture interactions can be summarized thus: An i-culture builds and operates m-culture features whose *ultimate function is* to provide for the maintenance and propagation of the i-culture in a certain environment. And the m-culture features, in turn, environmentally affect the composition of the i-culture so as to maintain or increase their own capabilities for performing that function. As a result, each m-culture feature is shaped for its *particular* functions in that environment.

After certain specific kinds of m-culture features become common through repeated performance of their functions, we can begin correctly to say that in-culture features of that kind have their specific shape *in order to* perform their particular functions, and that they perform them *in order to* accomplish their universal ultimate function. They are *teleonomic* structures (Monod, 1971).

We can assign the term "ultimate" function to the maintenance and propagation of the i-culture because cultural instructions, through their behaviors, determine the specific *shape* or fine structure both of m-culture features and of their own replicas.

In other words, even if all the m-culture features of a certain kind were wiped out, a single set of the appropriate cultural instructions could reconstruct and repropagate them. But, if all those sets of cultural instructions were wiped out, the m-culture features could not ordinarily reconstruct them or replace themselves, and would also become extinct. So the ultimate function of both an i-culture *and* an m-culture is the maintenance and propagation of the i-culture. The particular function of a set of cultural instructions is thus to build a specific m-culture feature. A commonly occurring set is then correctly called teleonomic; it has its particular inventory *in order to* build a certain m-culture feature which will then provide for its maintenance or propagation.

A CULTURAL ETHOLOGY

From the above remarks, it would seem to follow that a cultural ethology would be grounded in the study of cultural instructions, or i-culture. A cultural ethologist would observe human behaviors very closely, try to depict the instruction involved in each by its cue and its behavior, and then analyze each such instruction into its hierarchy of component simple instructions. He would be especially interested in the mechanisms of acquisition of cultural instructions, and in the mechanisms of their execution or expression - such as those governing motivation states and those integrating the responses of simple instructions into compound-complex ones.

Besides such general study, a cultural ethologist would, like the bio-ethologist and the anthropologist, be interested in comparative work, observing and accounting for variations in cultural instruction repertories both within and between human groups, large and small.

Finally, he would conduct field and field-laboratory studies, probably in cooperation with anthropologists, sociologists, social psychologists, human ecologists, and/or other "social" scientists, to study m-culture features and their effects, and thereby to elucidate the *survival values* of specific cultural instructions, and systems thereof, in specific total environments.

On Survival Value: Cultural instructions, m-Culture Features, and Organisms

The term "survival value" probably comes from the Darwinian phrase "survival of the fittest." Generally applied to a phenotypic feature of an organism, which may be a behavioral one, it is often used rather loosely. We have repeatedly used the term as synonymous with "function" above. Now that we have defined "function," we should try to justify that usage.

To say that feature X has survival value means that X has positive value *for* the survival of *something.* As a first naive approximation, that something would be the organism possessing feature X. A more sophisticated interpretation would be that X has value for the survival and *especially the reproduction* of the carrying organism *and/or its conspecifzcs* (according to Williams, 1966, only those closely related).

But if "survival value of X" is to be equated with "function of X," then, according to our definition of the latter, it must refer to the value of X for the survival and especially replication, not of carrying organisms and conspecifics, necessarily, but rather of the *instructions* that produced X and/or their replicas or homologues.

In the case of genetic instructions, this correction may not be of immediately obvious importance. A gene cannot replicate itself interorganismically until the organism carrying it has survived through the lengthy, elaborate processes of ontogeny and reproduction. Thus it is not surprising that most, if not all, genetically constructed features perform their ultimate function - have their survival value - through a particular function of producing, maintaining, or reproducing an organism.

A cultural instruction, on the other hand, although carried by an organism, can replicate itself interorganismically without waiting through the organism's life cycle. Indeed, it may replicate itself within minutes of being stored. So it should not be surprising to find m-culture features that perform particular functions which are irrelevant, or even destructive, to the organisms whose organs help to make or do them.

Just as the survival value of an m-culture feature is the same as its function, so the survival value of a cultural instruction is the same as its function; it is its value for the survival/replication of itself or its replica(s), irrespective of its value for the survival/reproduction of the organism which carries it or the organism's conspecifics. In a human carrier, then, a cultural instruction is more analogous to a viral or bacterial gene than to a gene of the carrier's own genome. It is like an active parasite that controls some behavior of its host. It may be in complete mutual symbiosis with the human host, in which case the behavior it produces has survival value for itself through the value it has for the survival/reproduction of the host. On the other hand, it may be like the gene of a flu or "cold" virus; when the virus makes the host behave, e.g., sneeze, that behavior results in extraorganismic self-replication of the virus gene but not in survival or reproduction of the host or his conspecific. From the organism's point of view, the best that can always be said for cultural instructions, as for parasites of any sort, is that they can't destroy their hosts more quickly than they can propagate. In short, "our" cultural instructions don't work for us organisms; we work for them. At best, we are in symbiosis with them, as we are with our genes. At worst, we are their slaves (Henry, 1963; Sapir, 1924).

SOME APPROXIMATIONS TO A CULTURAL ETHOLOGY, AND HOW THEY FALL SHORT

I think that the idea of a cultural ethology can be clarified by showing in what way the approach of each of several disciplines falls short of being a true cultural ethology.

Human Ethology

A number of bioethologists have studied human behavior, especially that of young children. Characteristically, they have concentrated their attention on *species-specific* and genetically programmed instructions. Some have isolated rather complex panspecific genetically programmed instructions and control systems which are in turn components of more complex instructional hierarchies, wherein they are addressed (motivated) by cultural instructions. Thus, for

example, Eibl-Eibesfeldt and Hass (1967) demonstrate certain fixed-action patterns of facial expression which appear to be components of female flirtation-instructions all over the world, although the context and sequence in which they are released are in part culturally determined.

Information about such relatively complex genetically programmed instructions is valuable, indeed essential, for the development of a cultural ethology (as well as for a "biosocial psychological anthropology"; Barkow, 1973), in part because they are a putatively universal part of the *environment* in which cultural instructions succeed or fail to establish places for themselves. In this respect they are precisely analogous to any other biological features of the humans species - anatomical, physiological, or behavioral (e.g., "biogenetic structures" of human cognition; Laughlin and d'Aquili, 1974).

When human ethologists turn to more complex sequences, e.g., to cultural instructions, they tend to fall back on an implicit "plasticity" of behavior. They make some reference to cultural determination of behavior, but I gather that their idea of culture is limited to what we have dubbed "m-culture" or culture as *environment.*

Thus, for them, the behavior of a developing child is operantly shaped by the behavioral responses it elicits from adults, but these responses are apparently "just there." The fact that the child is simultaneously acquiring those "adult" instructions directly through observational learning, and that this is the way the adults acquired them, is scarcely recognized. A cultural anthropologist would say that the human ethologists are overlooking the historical nature of culture; they don't seem to understand that a people's culture is their behavioral tradition as well as part of their environment. (Some anthropologists seem also to ignore this fact; *cf.* Callan, 1970; Konner, 1972.)

For example, the best that Blurton Jones can do in summarizing a section entitled "Class and Cultural Differences" is to say that "Describing the species-specific behavior of *Homo sapiens is* not a matter of peeling off the cultural crust but either seeking out the ranges of variation of behavior, or finding the general rules which underlie the variation of behavior within this range" (Blurton Jones, 1972: 28).[$] The cultural ethologist might reply, "Yes, and among these general rules is one that states that each human group carries a set of unique invariant cultural instructions which is acquired, and which has evolved, through a two-way causal interaction with its environment - including its cultural one - by processes analogous to those by which genetically programmed instructions are acquired and evolve."

° This quotation effectively serves as the ethological answer to the efforts of a few ethologists and publicists to attribute to modem people certain global, genetically based natures - permanent motivation states - based on the early evolutionary history of the human species.

Anthropology

Cultural anthropologists might be doing cultural ethology. Like the ethologist, the anthropologist studies his animal under natural conditions; he is expected to "know his people" (or culture) in the sense that the ethologist is expected to "know his animal." And cultural ecologists, at least, seem to be interested in the survival value of the cultural features they study - although they generally ignore the m-culture-i-culture causal relationship, i.e., the fact that the function of a cultural feature (an instruction) may be related to some aspect of the *cultural* rather than the "natural" environment.

But cultural anthropologists do not do cultural ethology - they do not actually study behavior. This may be due to the ethnological/ethnographical tradition of trying to get a whole culture under intellectual control, or to the comparative tradition which requires the use of large labeled categories, or both. Or, it may be due to panhuman instructions to view our conspecifics empathetically. We unconsciously concentrate not on what another human being is doing but rather on what he is "trying" to do.' So the usual methods and techniques of cultural anthropologists do not include the fine-grained observation and analysis of behavior - the search for simple instructions - that characterizes ethological studies.

Beyond that, however, many if not most cultural anthropologists, like many human psychologists, are frankly phenomenologists rather than behaviorists (Wann, 1964), or, in Harris's terms, emicists rather than eticists (Harris, 1968). They seem to believe that a people's i-culture consists of a limited number of such *mental* things as ethos, norms, values, ideas, themes, postulates, rules, symbols, and so forth. Apparently from these axioms, although I have nowhere seen this spelled out, an individual infers the appropriate, correct, or

[9] If he is making something, we look at the object and hear (or infer) his intent; we don't watch his hands. If he is signaling with his face, we "look through the surface" of his face and infer "what is going on behind it." We aren't aware of the muscular pattern at all, any more than we are aware of phonology when we listen to speech. These instructions have obvious survival value in one's own natural habitat (m-culture). But in the field they may actually prevent the recording of primary ethological data and cause, instead, the recording *as* data of culture-bound interpretations, and misinterpretations, of primary data that never come into awareness. Sweeping statements like these have, of course, their important exceptions. Exceptions to these statements about cultural anthropologists include Birdwhistell (1952), Chapple (1970), Hall (1963, 1966), and Harris (1964), who by precept and by example have tried to get anthropologists to study behavior. But consider the following very recent statement: "When an anthropologist gathers data in the field, he observes people doing such things as building canoes, tilling their fields, dancing in ceremonies, marrying, rearing children, divorcing, avoiding their mothers-in-law, and so on. But when he comes to writing up these observations, to describing the way the society functions, he employs a whole array of concepts and constructs not given by direct observation" (Kaplan and Manners, 1972: 89). To "an anthropologist," then, building a canoe, etc., is a datum given by direct observation!

actual behavior for a specific life situation by some sort of deductive process (Kaplan and Manners, 1972: 118-121). This process is meant to explain why human cultural behavior is ordered, integrated, consistent, predictable, nonchaotic; it is so because it obeys a limited set of rules. A social scientist can infer the rules, for each people in turn, by induction from their behavior and from their verbal statements of the rules. The rules, etc., are the reality of i-culture and the behaviors are mere derivations from them. Simple cultural instructions have no place in this scheme of things. While it is easy to imagine how the rules, etc., are acquired by the individual, it is much more difficult to imagine how they could evolve or change (be lost or adopted) without utterly disrupting the people's accommodation with their environment.

In any event, phenomenological notions seem to me quite incompatible with an ethological outlook.[10] The notion of an evolving set of simple cultural instructions implies that there is no more *intrinsic* order among those instructions than there is among the genes of a chromosome. The symbolizing which people allegedly do and the apparent deduction of appropriate behavior from general principles are merely epiphenomenal mirages, resulting from the fact that behaviors, like the other phenotypic features of any living organism or population, have a certain amount of functional integration - with each other and with their environment. It may be that, observing behaviors *ecologically* integrated, one is misled into concluding that the behaviors *are logically* or aesthetically integrated, or that a particular environmental feature which happens to elicit various kinds of behaviors under various circumstances is *a symbol* and not merely a cue for several instructions.

I believe that such notions could not survive a radical and thoroughgoing refusal to accept introspection and empathy as methods of validating scientific knowledge of human behavior. Believing (erroneously) that we base our behavior on rules and symbols, we attribute similar processes to those we study. An instruction to state a certain rule may serve as a mnemonic or tuitional device for an analogous gross-behavior instruction. But it is just as likely to have the converse function, to provide a verbal "cover" for counteranalogous behavior.

There is another school of thought in anthropology which, like the phenomenologist school (although materialist in intent), uses concepts that are too abstract to permit an ethological method of investigation. This is the school I call "macrodeterminist." Like many political scientists, economists, and sociologists, anthropologists of this school seem to see institutions and social structures as acting directly upon each other and upon individuals, moving the latter about like so many chessmen. These anthropologists ignore or even reject the fact that institutions and social structures, like other "man-made" structures, are simply

[10] They could be less so if it were agreed that each simple cultural instruction is a rule, but that would entail hundreds of thousands, perhaps millions, of rules; I don't think that this conception of "rule" would be acceptable to the phenomenologist.

the products of cultural instructions carried by, and executed through, individual human organisms.

They also ignore another fact: that individuals *respond* to features of these structures; i.e., if the structures "control" people they usually do so by cuing cultural instructions carried by those people, not by literally pushing them around. For example, some social anthropologists have used macrodeterminist ideas to explain the fact that a relatively large proportion of households in the Caribbean (up to 28%) are headed by females (Otterbein, 1965). They argue that the low sex ratio in the islands is due to the economic structure of the region - men leave in order to find wage labor or to fish - and that the high proportion of "matrifocal" households is due, in turn, to the low sex ratio.

What these anthropologists seem to forget, however, is that, first, the men would not respond to those "opportunities" in that way if they did not carry instructions to do so; and, second, that the women do not *have* to become heads of households when the men leave. The women could simply go on as they had, or give up, or themselves emigrate. The reason they do none of these things is that they have acquired, somehow, the cultural instructions that set up and operate a viable female-headed household (*cf.* Valentine, 1968). So from an ethological standpoint it is nonsense to make the macrodeterminist statement that the "regional economic structure" materially determines the "household composition" of the islands, unless that statement is clearly recognized as an abbreviation for a microdeterministic formulation such as the following:

1. The i-culture of a number of people builds an "economic structure" in the Caribbean.
2. This economic structure is part of the m-culture of the inhabitants of island X.
3. The males of island X carry instructions, cued by features of the economic structure (e.g., shortage of cash and goods plus blandishments of recruiters), the functioning of which absents the males from the island for long periods of time.
4. The absence of men is thus a feature of the m-culture (*ergo* of the environment) of the adult women of island X.
5. The adult women carry instructions which are adapted to that feature of m-culture through their function (survival value) of setting up household units that can endure in that man-short environment through a whole human generation and "reproduce" themselves.
6. About 25% of the households of island X are of this kind.

A formulation like that is not only closer to observable reality than a macrodeterminist statement. It has the additional advantage of being the outline of a hypothesis (or set of hypotheses) which can direct field research which, in turn, can fill in and correct, or even reject, the outline. What is the nature of those instructions in (5) above? How are they acquired by individual women?

How were they acquired by the society? What is their history? Do they have a function in the regional economic structure as well as in the island environment? Is the function to provide a source of migrant laborers? Does this help us to understand how one cultural system domesticates and exploits another? And so forth. Answers to such questions can be forthcoming through field research.

Experimental Psychology

While behavioristic methods are necessary for a cultural ethology, they are hardly sufficient. The experimental psychologists who write about human behavior seem to limit themselves to the *acquisition* of behavior (instructions) and, as a rule, to one or two modes of acquisition at that - operant and perhaps classical conditioning - which they see as universal processes." Because of this, they, like the human ethologists, ignore i-culture and the culture-specific historical or traditional nature of human behavior in its natural environment. Unlike the human ethologists, however, they ignore the *content* of instructions as well, perhaps because a laboratory-learned instruction usually has no apparent natural function. Thus they tend not to be interested in the survival value of particular instructions other than those for operant learning.[12] More seriously, however, I think their lack of interest in content and survival value has made it hard for them to see the real-life importance of modes of acquisition other than conditioning. Thus they, who are uniquely prepared to study it, have devoted very little research to observational learning. (Bandura is a notable exception: see Bandura, 1965, 1971; Bandura and Walters, 1963.)

A RESEARCH PROGRAM IN CULTURAL ETHOLOGY

The considerations developed so far indicate, I believe, that a cultural ethology is indeed possible and highly desirable but that developing one will not be easy. It will require a strategy of research that coordinates advancement on a

"The question of phylogenetically acquired, species-specific learning mechanisms vs. a universal, species-general propensity to learn is an interesting one. Although I believe it has been satisfactorily resolved in favor of the former (Beach, 1960; Lorenz, 1965; Lenneberg, 1964/1968: 245), it still provokes lively controversy (e.g., *Animal Behaviour,* 1973).

[12] Campbell (1960) has pointed out that all "knowledge processes" including operant learning are *special* mechanisms for *blind variation and selective retention,* achieved through the *general* mechanism for blind variation and selective retention, Darwin's radiant variation and natural selection. This is quite correct, if natural selection is seen as an interaction between an organism or population and its environment. If, however, it is an interaction between an *instruction* and its environment, including its carrying organism, then we can go further and say that operant learning is simply a kind of natural selection. Like most events of natural selection (Cloak, 1973), an event of operant learning takes place in an environment produced, in part, through previous events of natural selection.

number of fronts, maintaining a balance between empirical studies and the construction of theory. Accordingly, in the following paragraphs I want to suggest what a program of research in cultural ethology might look like.

The Necessity of "Instruction-Thinking"

While special field methods and special methods of first-order analysis are necessary for the foundation of a cultural ethology, they are by no means sufficient. Even as we build a data base and derive insights from it, we must develop some rather high-level theory, test it logically, and derive hypotheses from it, and so build "down" as well as "up."

The theoretical framework I propose to begin A ith can be called "instruction theory" or "general theory of natural selection" or "theory of continuing evolution." I have outlined this framework elsewhere (Cloak, 1973). Its development requires cooperative effort from many thinkers, which in turn requires provisional acceptance of what I suppose could be called "instruction-thinking," exemplified above in the discussion of survival value and elsewhere. A shift to instruction-thinking is, I think, a logical extension of the shift from eidetic thinking to population thinking which Mayr (1959: 2) credits to Charles Darwin. And it appears so far to be just as hard to bring about, perhaps because it seems to reduce further the importance and efficacy of the individual human personality in our view of the universe. (But if we *are* the slaves of some of "our" cultural traits, isn't it time we knew it?)

Theoretical Problems

Besides the pseudoproblem of gaining acceptance, instruction theory presents some genuine scientific problems of logic and definition. Terms such as "cooperation," "competition," "exploitation," "domestication," "system," "function," "environment," "niche," and even terms such as "evolution," "natural selection," and "organism" must be examined, and perhaps reinterpreted, for incorporation into the theoretical frame. To give just one example: In the paper cited above (Cloak, 1973), I have defined an "event of cooperation" in a certain way, and then characterized a system of elementary self-replicating instructions as a set of instructions which regularly engage in events of cooperation in an environmental subregion. This characterization could well apply to a whole ecosystem. But within that ecosystem we will find *subsystems* of instructions cooperating, competing, exploiting each other, and so forth.

Problem: how can we define the boundaries of a subsystem? We cannot do *so a priori* on an organismic basis or a species basis. This is how the bioethologist does it, but, in my view, he should not. The boundaries of a subsystem are maintained by the instructions in that subsystem. The boundaries of cultural subsystems are clearly semipermeable; instructions can be acquired from other subsystems (borrowing, diffusion, acculturation). And the behaviors of each subsystem are, of course, environmental to every other subsystem as well. So, to repeat, with all these complex interrelationships going on among the subsystems of an ecosystem, how can we define the boundaries of the subsystem so that we can study those interrelationships? It certainly would seem that considerable theoretical work, using insights from a number of disciplinary specialties, is the *sine qua non* of a program in cultural ethology.

Data Gathering

In this area, it appears that the first order of business is the identification and description of cultural instructions. Probably, looking at human behavior in its natural habitat, we will infer compound-complex cultural instructions first. Close observation of several executions of a compound-complex instruction, by the same person and by different people, may reveal differences. Such differences point to differences in composition and thus to the component instructions, which can then be identified and described. This process may be repeated to reveal components of components, etc.

At some point, it will become desirable to move the focus of observation from the pure field situation into a laboratory of some kind. I think that the ideal for this may be a sort of field laboratory in which controlled surrogates of naturally occurring cues can be presented to a series of individuals in order (1) to test hypotheses about cues derived in fieldwork and (2) to reveal variation in instructional repertories. The film records of this experimental work should perhaps be printed on paper strips for simultaneous frame-by-frame comparison of several subjects' responses."

The art of producing surrogate cues will be practically a field in itself. If films are used, it may be desirable to project them on a "wraparound" screen for maximum stimulus value. They may be short, but would be better long; they may be made by an ethnographic filmmaker, with the guidance of a member of the host population or *vice versa.* Other sorts of cues should be considered and tried as supplements to, or substitutes for, film cues.

Methods other than filming should be considered for the recording of responses. Perhaps the recording polygraph will prove a useful tool for that. Perhaps also we can use electromyographic techniques to record from a chair-bound artificially cued subject "intention movements" for real-life large-muscle behavior. And, too, we will want to elicit and record linguistic and other vocal responses.

[13] I have such data from a pilot series of ten experiments, each on 12 subjects, carried out in 1969. Analysis of data from one experiment has been completed, and it eloquently illustrates the hierarchy of instructions and control systems discussed above (Cloak, 1974).

Data Interpretation

Linguistic responses can be elicited in environments ranging in authenticity from a practical total surrogate to the sort of structured formal interview situation that political pollsters use. Even the latter extreme may be useful provided that, in analyzing the subjects' responses, we never forget that we are dealing with *data* that must be interpreted, and not with "instant theory," a direct pipeline to "what's really going on" among a given people. For example, a response to a question like "What do you do when ... ?" is not only a response to the hypothetical situation posed in the question; it is also a response to the questioner and to his whole question and to everything else in the present and recent experience of the subject. Yet it *is* a response determined by cultural instructions. While the content of the responses may not be significent, the distribution of the responses over a number of subjects, if it proves to be systematically ordered, may be highly significant for the isolation of instructions (Goldberg, 1973) and/or for the study of acculturation/diffusion and microevolution (Cloak, 1966). The determination of whether a response distribution is systematically ordered requires some serious efforts in applied probability theory and the development of some sophisticated statistical and computer techniques not available at present (Cloak, 1966).

Connections with Other Disciplines

Finally, besides moving from the top down, and from the bottom up, a cultural ethology program must move "sideways" into such areas as ethology, anthropology, human ecology, philosophy and logic of science, computer science, statistics, experimental psychology, and neurophysiology. We must not only learn what is now available from other fields, we must also stimulate research in those fields along lines whose results will be useful, even necessary, to cultural ethology.

Perhaps the most important of these fields is neurophysiology. If we can persuade neurophysiologists of the value of the idea of an instruction, we may encourage research leading to an understanding of precisely how instructions, including cultural ones, are programmed into the nervous system. Such an understanding will be extremely useful for all of ethology, especially cultural ethology, and is a practical necessity for field and laboratory research on cultural acquisition, whether it is carried on by experimental psychologists or by people calling themselves cultural ethologists.

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