IS A CULTURAL ETHOLOGY POSSIBLE?

by

F. T. CLOAK, JR.

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IS A CULTURAL ETHOLOGY POSSIBLE?*

Ethology, the study of animal behavior, under more or less natural conditions, has recently enjoyed a spurt in productivity and in popularity among laymen. Some rather unfortunate attempts have been made to apply ethological findings to human beings. Unlike those attempts, this paper is an inquiry into the possibility of applying some of the methods of ethology to the study of human beings under more or less natural conditions.

PART 1

In the broadest sense, ethology is the study of animal behavior, as differentiated from anatomy, the study of animal structure, and physiology, the study of animal substance. More narrowly, ethology is the study of animal behavior in the wild or under simulated natural conditions or at least under laboratory conditions based on natural or wild conditions. This emphasis on field studies or surrogates thereof rather than on purely experimental studies, derives from an interest in the adaptiveness of behavior. To really understand a behavior, says the ethologist, you have to know its survival value, its function.[1] The only way you can know that, of course, is by studying the behavior as a response to naturally occurring stimuli--stimuli to which the phylum has become adapted over the generations. Another idea linked to these of naturalism and functionalism is that of species-specificity of behavior. An ethologist is usually an expert on the behavior of a particular phyletic group, and theoretical work tends to be of a comparative type. The contrast here is with the psychologist's approach to the study of animal behavior, which consciously tries to ignore species differences and talks about behavior-in-general. This approach leads the psychologist into two serious errors: (1) the error of believing that all significant behavior is learned, and (2) the error of forgetting that learning, too, always takes place in a particular kind of animal, which has phylogenetically acquired the capability to accomplish that particular kind of learning. The learning animal is not just an undifferentiated hunk of protoplasm; rather, the ability to learn has been acquired through evolutionary adaptation by natural selection.[2]

Ethologists, like anthropologists (of all sorts), are concerned with variation among groups in space and time. The ethologist's motto "know your animal" is precisely analogous to the anthropologist's unspoken maxim "know your culture." While that maxim may be unspoken, I don't know how many times I've heard an anthropologist criticize another social scientist by saying that he didn't know his people, that he made an incorrect interpretation of certain behavior because he didn't know the culture.

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*Dr. Cloak is Assistant Professor, Department of Anthropology, and Research Associate, Institute for Research in Social Science, University of North Carolina at Chapel Hill. This paper was delivered at the annual meeting of the Southern Anthropological Society, Gainesville, Florida, February 1968.

At this point it may appear that my question--is a cultural ethology possible?--is a moot one. Cultural ethology might be just another name for cultural anthropology. I think, however, it would be wise to look a little more closely at ethology before we come to that conclusion. Defined still more narrowly, ethology can be said to be the study of propensities to behave and of the modes of acquisition of these propensities.

To say that an animal has a certain propensity to behave is to say that in the presence of a certain stimulus--releaser, condition, situation, object, etc.--the animal will exhibit a certain response. Alternative expressions for "propensity" could be "internal instruction" or "disposition." The existence and nature of the propensity are inferred from observations of regularities in repeated behaviors, so it is important that the behavior be described as exactly as possible. If the stimulus and the response can be described in simple, concrete terms, the presence of the propensity in the organism can be verified much more easily and surely, by repeated observations.

The ethologist frequently spends a great deal of time identifying and describing the stimulus, releaser, object, etc., of a given behavior. For example, it was observed that herring gulls remove broken eggshells from their nests. Tinbergen[3] was able to show by a series of experiments that it was the whiteness of the inside of the eggshell that triggered this behavior, and that nothing else would trigger it. He experimented by putting ping-gong balls in the nest and, sure enough, the balls were removed. Ping-pong balls and eggshells were not removed, however, if they were painted to match the outside of an egg. Egg-sized cubes were removed if and only if they were white. The stimulus value of the black baby baboon for the adult male is an example closer to our own species.[4]

Another subject of interest is, of course, the particular response. The ethologist wants to specify exactly what the animal does under the particular stimulus. The gull, for instance, carries the broken eggshells about four feet away in its bill. Finally, as a sort of final confirmation that he has indeed described a natural behavioral unit, the ethologist wants to demonstrate the survival value of the behavioral propensity. In the example we have been using, it was shown that predators do indeed use the white insides of eggshells as a beacon for homing-in on newly hatched baby gulls. The survival value of the shell-displacing propensity is clear, in straight natural-selection terms.

The critical difference in methodology, then, between ethological studies and those usually done by anthropologists, lies in the high specificity of the behaviors described and interpreted. To give one more example from Tinbergen: A red spot on the adult gull's bill elicits pecking by the infant gull. If the spot is painted out, the infant doesn't peck. If a bigger than normal red spot is presented, the baby gull pecks harder and more rapidly than normal. The behavior chosen for study is either on an all-or-none basis or else its intensity is easily measured. The selective value of the propensity to peck at a red spot lies in the fact that the pecking, in turn, is the stimulus which releases a feeding-behavior propensity in the parent.[5]

Turning now from the propensities as such, we take up the question of modes of acquisition of behavioral propensities. Konrad Lorenz divides these modes into two main categories: phylogenetically acquired and acquired by the individual through interaction with the environment.[6] To use a computer analogy: Some instructions are wired-in at the factory, other instructions are

programmed into the individual computer at the using facility. It is all right to refer to behavioral propensities as "innate" and "learned," respectively, provided one is careful; for instance, one must remember that a propensity can be innate even though it appears only in the adult, after a period of maturation. One must also keep in mind that learned propensities are always based on innate structures of greater or lesser specificity, and that while these innate structures are often anatomical and/or physiological, they may also be behavioral. For example, baby chicks and baby monkeys and other baby omnivores learn to recognize what is good to eat through trial and error because they have an innate propensity to try all possible objects in their mouths and because they have an innate selectivity for taste and texture. In other words, an adult omnivore has certain learned food preferences—learned in the classical sense--but these learned preferences were acquired only because of certain innate propensities: a propensity to try different potential foods and a propensity to judge tried foods to be rewarding or otherwise. Without those innate propensities, the learning could never have occurred.[7]

Similarly, certain primates have an innate propensity to imitate under certain circumstances,[8] and human beings have an innate propensity to acquire language at a certain point in their ontogenies.[9]

Finally, it must be kept in mind that learning includes for our purposes not only classical conditioning, or trial and error learning, but also insight learning, imprinting, and imitation. In man, it includes the receipt of instructions through linguistic utterances in the conditional mode; that is to say, through parents' or other enculturators' saying, "Whenever this happens, do that."

My point is that there are several different modes of acquisition of behavioral propensities which can be classified together under the broad heading "learning," or "acquisition by the individual through interaction with the environment." Most, if not all, of these modes play a part in the individual's acquisition of those behavioral propensities which we call culture.

PART 2

Some animal ethologists have recently turned their attention to human beings, calling their studies "human ethology." Their work has not been exactly welcomed by social scientists, partly because of the outworn social science dogma that except for a few basic drives, all human behavior is learned, but mainly because the human ethologists have confined their discussions exclusively to innate behavioral propensities, seeming to ignore learned propensities or even to insist that certain propensities are innate when our studies have shown that they are mainly learned. Besides the obvious examples of Ardrey, Lorenz in On Aggression and Desmond Morris' Book-of-the-Month we have Eibl-Eibesfeldt and Hass's article in a recent issue of Current Anthropolocv.[10] In this article the authors assert that flirting behavior in the human female is innate; yet examination of the rapid-motion photographs suggests, rather, that while some of the components of flirting behavior nay be innate, the arrangement of these components varies from example to example. When I say components, I refer to the smile, the enlarging of the eyes, the wrinkling of the skin at the bridge of the nose, and so on. We might assume that these

individual components are each wired-in by evolution, at least in their motor aspects, i.e., as responses. There is such a thing as a human smile.[11]

Assuming, then, that the components are wired-in, we can go on to ask whether the sequence of components is wired-in or acquired through cultural learning. Even if we must come to the latter conclusion, we should not abandon an ethological approach to human female flirtation behavior. We should, rather, use the ethologists' investigative techniques to describe flirtation behavior among various peoples and to determine the range of variation within and between societies, including variation in eliciting stimuli as well as variation in flirtatious response. We should study highly specific behaviors in man, and plot their distributions and relationships through space and time, but we need not concern ourselves too much with determining immediately whether they are innate or learned; that last question should resolve itself, in many cases, when sufficient data have been collected.

The ethologists, then have left the field of learned human behavioral propensities to other students. It seems to me that these other students can be divided on methodological grounds into three groups: The first group, including many social scientists, psychiatrists, and clinical psychologists, have tried to explain human behavior in anthropocentric and even ethnocentric terms--in terms of "human" wants, needs, desires, purposes, cognitions, etc. They suffer from the unfortunate misapprehension that one can find out why people do what they do by asking them or by empathizing with them; in short, they believe there is a royal road to the understanding of human behavior, a road which is not open to the student of the behavior of other animal species. They don't understand that an informant's statement is never prima facie an explanatory statement, or even a descriptive statement, but is rather a behavioral datum which must be described and interpreted (explained) like any other behavioral datum, human or animal.

The second group of students are the behaviorists in psychology. These have emphasized classical learning or conditioning at the expense of other forms of learning, mainly because of their aforementioned unwillingness to recognize the existence of different innate propensities to learn different things in different ways. They should realize that evolution has produced many marvelous structures, behavioral as well as anatomical and physiological. Surely evolution could produce a propensity to learn by imitation, or even a propensity for inspired insight, if such a propensity were of selective advantage to the species in question in its particular adaptive zone. Man, in particular, is wired-up to learn all kinds of things in all kinds of ways, and Skinner's Verbal Behavior is a most striking example of the kind of trouble one gets into when trying to explain all of human behavior in terms of one mode of acquisition.[12]

The third group of students seem to forget that man is even an animal; they treat him more as a mere object that doesn't respond to stimuli but is just pushed around by impersonal forces or by more powerful human beings. Many economists and political scientists and quite a few sociologists and social anthropologists seem to fall into this third group. As an example, I suggest the writings of several social anthropologists who have discussed the problem of household composition in the Caribbean.[13] These writers have shown very convincingly that the presence of a large proportion of households headed by females is a direct consequence of a scarcity of males in the society as a

whole. They don't seem to realize, however, that the real interest lies not in the extracultural cause of a particular trend in family composition, but rather in the culturally-acquired propensities for behaviors which constitute an adaptation to this enforced household composition.

To summarize: The human ethologist ignores culture because he deals only with innate propensities, while culture is learned. The first kind of social scientist, the phenomenologist, misuses culture (as I understand the term) by assuming he can get at it directly through introspection or through others' statements of norms, motivations, etc. (Marvin Harris says that this kind of student accepts emic statements as descriptions and explanations of the etic reality, in spite of the fact that it is hopeless to predict behavior on the basis of such statements.)[14] The second kind of social scientist, the behaviorist, ignores culture because he doesn't recognize that humans and other primates have an innate propensity to acquire culture without conditioning; thus he forgets that human behavioral propensities are largely traditional, and hence more variable between societies than within societies. The third kind of social scientist, the structuralist, ignores culture because he sees people simply as passive pawns of social forces, unable to respond, let alone to act--in short, he believes it doesn't matter what their traditions are.

PART 3

What a proper cultural ethologist will do, in contrast to the above, is look for, identify, describe, interpret, and ultimately explain highly specific behavioral propensities which are acquired by the individual culturally; that is, he will study those behavioral propensities which are acquired by imitating other members of the species (and, in the case of humans, by receipt of verbal instructions), and thus are the observable output of cultural traditions. Studying the distributions of such propensities within and between groups will broaden our understanding, not only of the histories of particular cultures but of the mechanisms which control culture change. (I made a start toward such studies when I discovered an apparent natural order of cultural adoption and loss in a corpus of 3,666 behavioral propensities of 28 immigrant families in a village in Trinidad.)[15]

Ray Birdwhistell and Edward T. Hall have done some pioneer thinking in this area; I'm thinking of Birdwhistell's studies of kinesics,[16] especially of "dialects" of facial expression studied through high school yearbooks; [17] and, of course, of Hall's studies of comparative proxemics.[18] Neither seems to have described the precise mode of acquisition of the propensities they discuss, but the behaviors in question seem to be quite specific; presumably imitation is involved in some way. Anyway, their work gives me confidence that a cultural ethology is possible.

I gain further confidence from certain other sources. The idea is gradually taking hold that not only culture-in-general, or the capacity therefor, is adaptive, but specific cultural features, peculiar to particular groups, are adaptive--in the strict, natural-selection sense of the term--for the environment which those groups inhabit.[19] Let me put that another way, and

attempt to explain it. In the study of imitation, or cultural learning, what is learned is of crucial importance to the population or species. This is not so in the case of classical learning because, while the propensity to learn itself has survival value to the population, propensities learned by conditioning have value only to the individual learner. When something is acquired by imitation or cultural learning, however, natural selection can operate on it; it has positive or negative selective value for the population and it can be studied in that light, just like an innate propensity. This is true because propensities to behave acquired by imitation are perforce transmitted from one individual to another, and yesterday's survivors are today's transmitters.

To give an example of what I mean: The sacred cattle complex of India is a very complex complex, or so it would seem, but I believe it can be boiled down to a small number of culturally-acquired propensities to behave, perhaps to only one: an expression of disgust at the idea of having anything to do with the slaughter of a cow.[20] If we follow Marvin Harris' compelling argument from that point, we have to conclude that one highly specific propensity to behave has had tremendous survival value in Hindu villages.

Similarly, Marshall Sahlins' analysis of Evans-Pritchard's data on the Nuer-Dinka relationship[21] suggests to me that the Nuer have one highly specific propensity which the Dinka lack. I call this propensity the Principle of the Expandable In-group. It could also be described as a belief that anyone who attacks my brother attacks me and that, in practically any fight I am likely to hear about, one of the participants is my brother. The principle is generally accompanied by strong propensities for behaviors redolent of righteous indignation, such as angry repetition of atrocity-stories.

The principle has tremendous survival value in a competitive situation--just look what the Nuers were doing to the Dinkas, according to Evans-Pritchard when he was there. Let me compare my interpretation of Sahlins' findings to his own interpretation. Sahlins explains the Nuer's success over the Dinkas by the fact that they have a segmentary lineage system. To me, Sahlins is making the structuralist's mistake of reifying an inferred social form and giving it causal efficacy. He implies that Nuer aggression is caused by their lineage system; the individual Nuer is simply a helpless pawn of this impersonal social machine, although the mechanism by which a social form can cause a man to raise a spear has never been adequately described. The cultural ethological explanation, on the other hand, would have it that those individual Nuers who carried the Expandable In-group propensity would be selected for, in fights at every territorial (and lineage) level. As this selection resulted in the propensity becoming typical of the Nuer, it would lead first, to the predatory expansion of the Nuer at the expense of the Dinka and second, to the appearance in the eyes of the visiting social anthropologist, of a segmentary lineage system. The expansion of territory and the social form are both results of the spread of the behavioral propensity; neither of them is the cause of the other.[22]

Still another source of my confidence in the possibility of a cultural ethology lies in my frequent casual observation, in the field and elsewhere, of highly specific, utterly arbitrary, and totally conventional verbal responses, usually to identifiable specific stimuli. The propensities toward these behaviors are clearly culturally acquired, yet each is unique, and can be

studied in its own right. In the Trinidad study already mentioned, my wife and I were administering an interview schedule containing about 300 open-ended questions, all of which had been selected because previous field work suggested that they were culturally relevant to the people being studied. Again and again, especially from certain individuals whom we came to refer to as "culture bearers," we got extremely stereotyped, automatic, responses. It was as if one word or phrase in the question turned on a word or a phrase or even a whole sentence. To take only one example of many: "What do you grow in your garden?" "Corn-peas-cassava." Period. Then the informant (usually female, in this case) would proceed actually to enumerate the things she was growing at the time, and it would often turn out that she didn't grow one or more of that vegetable trinity, corn-peas-cassava. It was just that that question automatically turned on that answer--not in everybody, but in a significant number of people.

The point is that there seems to be no general principle from which any of these specific responses could have been deduced. Each propensity seems to be logically independent of all the others, to have been learned separately, and to be released by a highly specific stimulus.

Another Trinidadian example is the relatively exact replication of hundreds of long Yoruba utterances in the songs of the well-known Shango cult. The meanings of the words have long since been forgotten; the words have been handed down intact for several generations; to the singers they are just so many nonsense syllables; each acting as a stimulus for the next. Yet just last summer a Yoruba field worker was able to translate the songs, verbatim, into modern Yoruba and into English.

The nonsense syllables of children's game-chants provide a more homely example, as does English spelling, where the specific spellings of hundreds of words have to be learned independently, by rote, because there is no rhyme or reason to them. In fact, language itself is characterized by its arbitrariness, by the lack of any logical relationship between sound and meaning.[23] Nouns, for example, are learned as separate, specific behavioral propensities.

I am rapidly coming to believe that much, if not most, of culture is acquired in tiny unrelated snippets, specific behavioral propensities culturally transmitted from one generation to another with remarkable fidelity. The fidelity and ease with which these "corpuscles of culture" are transmitted and acquired is possible only because the organisms in question are phylogenetically adapted for transmitting and acquiring cultural corpuscles.[24] This adaptation has required at least two million years, and perhaps 40 million years, of intense selection pressure.

One implication of the particulate notion of culture expressed here is that there may be no more intrinsic order among cultural particles than there is among the genes on a chromosome. The symbolizing which people allegedly do, and the logico-aesthetic integration supposedly characteristic of culture, may be merely epiphenomenal mirages, resulting from the fact that behavioral propensities, like the other phenotypic features of any living organism or population, have a certain amount of functional integration. It may be that, observing behaviors so integrated, one is misled into concluding that the behaviors are logically or aesthetically integrated,[25] or that a particular

object which happens to elicit various kinds of behaviors under various circumstances is a symbol and not merely a stimulus.[26] I believe that such notions could not survive a radical and thoroughgoing rejection of introspection and empathy as sources of knowledge of human behavior,

PART 4

To develop a cultural-ethological approach, then, we have to observe the behavior of people, alone and in groups, concentrating on trying to ascertain what specific cues or stimuli elicit what specific responses. In conducting these observations, we will be especially alert for variation between individuals. If we observe that two individuals exhibit different responses to what appears to be an identical stimulus, we may be able to learn something by attempting to explain the difference. What we are especially interested in, of course, are behavioral differences due to the two individuals' being part of different traditions, in other words, behavioral differences due to cultural differences.

Some observed behaviors are, of course, extremely complex, because they are the product of several propensities operating simultaneously or because of complicated environmental limitations on expression, and so forth. The cultural ethologist will select behaviors which seem to operate in a clear-cut fashion for his initial researches. There is nothing wrong in doing this. Mendel didn't try to explain multigenic traits his first time out; instead he saw that he could learn most from characters which were controlled by only one factor, which showed complete dominance and independent assortment, and which had no selection at work to upset the predicted ratios.

Washburn, Joy, and Lancaster have said, "The interplay between naturalistic observation and controlled experiment is the essential key to the understanding of behavior.[27] As ethological field studies progress, laboratory studies can begin. Selected individuals can be placed in a controlled environment and then exposed to simulations of stimuli which the field studies have suggested are salient for various behavioral propensities. The ethologist will be aided in this by the general human willingness to accept poor substitutes for the real thing. We can use films, audio and video tapes, verbal cues, even olfactory cues as stimulus-surrogates.

Responses will be recorded in many ways. Besides videotaping the subjects' facial expressions and gestures, we can use telemetry to record respiration, heartbeat, skin moisture, and, possibly, the slight movements, as in a checked response, which the ethologist calls "intention-movements." (I follow Birdwhistell in feeling that the response made involuntarily and out-of-awareness can often teach us more than can, say, complex verbal responses made after due reflection.)

To conclude, then, I think that cultural ethology is possible, and that it is desirable. I think further that the development of cultural ethology is absolutely essential if we are to make anthropology into the natural science of culture Kroeber always said it was.[28]

Footnotes

1 Konrad Lorenz, Evolution and Modification of Behavior (Chicago: University of Chicago Press, 1965).

2 Ibid., pp. 13, 17, 18.

3 Niko Tinbergen, "The Shell Menace: Behavioral Aspect of Camouflage is Demonstrated by Gulls," Natural History, 72 (August-September 1963), pp. 28-35.

4 K. R. L. Hall and Irven DeVore, "Baboon Social Behavior" in Irven DeVore (ed.), Primate Behavior, (New York: Holt, Rinehart & Winston, 1965).

5 Niko Tinbergen et al., Animal Behavior (New York: Time Inc., 1965), pp. 66, 67, 69.

6 Lorenz, op. cit., pp. 3-5.

7 Ibid., pp. 16-18.

8 Sherwood L. Washburn, Phyllis C. Jay, and Jane B. Lancaster, "Field Studies of Old World Monkeys and Apes," in Morton Fried (ed.), Readings in Anthropology (2d ed.; New York: Thomas Y. Crowell, 1968), Vol. 1, p. 289.

9 Charles F. Hockett and Robert Ascher, "The Human Revolution," in Morton Fried, ibid., p. 331.

10 Irenaus Eibl-Eibesfeldt and Hans Hass, "Film Studies in Human Ethology," Current Anthropology , 8 (December 1967), pp. 477-479.

11 The maturation of the smile has been studied (W. H. Thorpe, Learning and Instinct in Animals [Cambridge, Massachusetts: Harvard University Press, 1956], p. 129) and Eibl-Eibesfeldt and Hass (op. cit.) include a convincing photographic sequence of a smiling boy who has been deaf and blind from birth. I suppose one might cavil and say that the boy could still have learned to smile through conditioning. There is also, apparently, at least one innate propensity to behave which has the smile as the response-element. The stimulus-element in this propensity is a smile on the face of another human being. In other words, there is an innate propensity in all human beings to respond to a smile with a smile. This was brought home to me when, as an undergraduate, I had a professor who apparently suffered from some sort of nervous disorder which caused him to put on an exaggerated grin every so often, at times which were completely inappropriate. In the middle of a lecture, this man would suddenly thrust his face close to us in the front row and grin. In spite of the fact that there was nothing funny about the situation, in fact it was downright embarrassing, we students would find ourselves helplessly grinning back.

12 Noam Chomsky, Review of B. F. Skinner, Verbal Behavior, in Language, 35 (January-March 1959).

13 See, for example, Keith F.Otterbein, "Caribbean Family Organization: A Comparative Analysis," American Anthropologist, 67 (February 1965), pp. 66-79.

14 Marvin Harris, "Etics, Emics and the Ethnography Machine," Informal remarks at Duke University, February 23, 1968.

15 F. T. Cloak, Jr., A Natural Order of Cultural Adoption and Loss in Trinidad. Working Papers in Methodology, No. 1 (Chapel Hill: Institute for Research in Social Science, University of North Carolina, 1966).

16 Ray L. Birdwhistell, Introduction to Kinesics (Louisville, Kentucky: University of Louisville, 1952).

17 Ray L. Birdwhistell. Informal remarks at University of North Carolina at Chapel Hill, Department of Psychiatry, October 20, 1967.

18 Edward T. Hall, "A System for the Notation of Proxemic Behavior," American Anthropologist,, 65 (October 1963), pp. 1003-1026, and The Hidden Dimension (New York: Doubleday & Co., 1966).

19 Alexander Alland, Jr., "Medical Anthropology and the Study of Biological and Cultural Adaptation," American Anthropologist, 68 (February 1966), pp. 40-51.

20 Or perhaps to Ahimsa. "Ahimsa in the Hindu principle of unity of life, of which sacredness of cattle is principal sub-case and symbol" (Marvin Harris, "The Cultural Ecology of India's Sacred Cattle," Current Anthropology, 7 [February 1966], footnote 51).

21 Marshall D. Sahlins, "The Segmentary Lineage: An Organization of Predatory Expansion," American Anthropologist, 63 (April 1961), pp. 322-345.

22 I have a hunch that this principle is an old one among us Indo-Europeans, and that it helps explain our success against both Africans and American Indians in the past, and the popular support for our unfortunate presence in Viet Nam in the present. I don't think it explains why we are in Viet Nam; I think it explains why the administration can muster support among the American people, who are not really bloodthirsty, for the revolting things we're doing to the Vietnamese. (The fact that I think we are on the wrong side in Viet Nam simply illustrates my point; I, too, am a carrier of the propensity, Principle of the Expandable In-group; I agree that my brother is being attacked in Viet Nam; I just disagree about who my brother is.)

23 Charles F. Hockett, "The Origin of Speech," Scientific American, 203 (September 1960), pp. 89-96.

24 I have referred to these "corpuscles" in the past as "Units of Cultural Instruction" (F. T. Cloak, Jr., "Cultural Microevolution," Research Previews, 13 (November 1966], pp. 7-10), and as "d-cultural things" (Cloak, A Natural Order....) which are expressed phenotypically as "culture traits" (ibid., p. 23).

25 James W. Fernandez, "Symbolic Consensus in a Fang Reformative Cult," American Anthropologist, 67 (August 1965), pp. 913-917.

26 Leslie A, White, "The Symbol: The Origin and Basis of Human Behavior," Philosophy of _Science, 7 (1940), pp. 451-463, and "The Origin and Nature of Speech," in Morton Fried, op. cit. See also Fernandez, op cit., pp. 917-922.

27 Washburn, Jay, and Lancaster, op. cit., p. 291.

28 A. L. Kroeber, "So-Called Social Science," Journal of Social Philosophy, 1, (1936), pg. 317-340, and Anthropology (New York: Harcourt, Brace & Co., 1948), pp. 11, 12, 295, 296.