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Culture: A *Human* Domain[1]

by Ralph L. Holloway, Jr.

INTRODUCTION

An alternative title for this paper might be "The Myth of Animal Culture." My purpose is to examine the question of continuity-discontinuity (degree vs. kind) in the behavior of man and other primates and critically to assess current arguments that "culture" can no longer be regarded as a domain specific to man. I will argue that certain writings based on primate studies and early hominid evolution are needlessly depriving us of our proper domain. The reason for this blurring of domains is that the central issues of the psychological attributes of human existence are not being squarely faced. I will argue that it is possible to give the concept "culture" some force once again as something unique to man. Furthermore, I hope to show that consideration of the relative merits of a strict "critical-point" theory (Kroeber 1948: 71–72) and a gradation framework is a burdensome pseudoproblem that distracts us from central issues. Neither gradualism, critical points, learning (even if transgenerational), tool-use, nor language per se is the fundamental issue to focus upon in deciding whether or not man is unique. The critical issue is how man organizes his experience.

I am not interested in imposing upon the field yet another definition of culture. Instead, I will discuss two attributes of human existence that, if honestly faced, might give culture back to man, regardless of what the clever baboons, vultures, ants, macaques, or chimpanzees have done thus far. These attributes are *arbitrary form* and *imposition*.

Briefly, I suggest that culture, in addition to being ". . . that complex whole . . . shared by man as a member of society," is also the *imposition of arbitrary form upon the environment.*[2] These two attributes are specific and unique to human behavior, and they can be identified by the appearance of stone tools in the archaeological record. While these attributes are based upon behavior common to mammals and particularly developed in the primates, their adumbration in man is an emergent phenomenon, a difference in kind as well as degree.

While other writers on the subject of human origins have long maintained that it is man's use of symbols that sets him apart (Cassirer 1944, DeLaguna 1927, Kroeber 1948, Mead 1934, Revesz 1956, Sapir 1921, Sommerfelt 1954, White 1942, 1949), and several of these writers have contended that tool-making presupposes language, the relationship remains to be clearly demonstrated. The central question framed so succinctly by Hallowell (1959: 41) has received little attention:

> We must ask whether tool-making presupposes a higher order of psychological structuralization and functioning than tool-using; whether it implies a social system different from that of infrahuman primates; or a different system of communication.

The point of departure in this essay is to demonstrate that the attributes of arbitrary form and imposition can take us

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[1] Much of this paper has grown out of conversations with colleagues, students, and some unpublished manuscripts written several years ago. It is difficult after a few years have elapsed to remember exactly what specific contributions others have made to one's thoughts. I am particularly indebted to T. D. McCown, University of California, Berkeley, who supervised some of my early attempts in this direction. I wish to thank him for his encouragement, patience, and critical comments on the use of certain aspects of communication theory as applied to stone tools. I am also indebted to T. D. Lanagan, who brought to my attention much of the psychological literature on social perception, and who encouraged me to consider the social psychological aspects of stone tool-making. I am similarly grateful to Robert Murphy and Myron Cohen, Columbia University, and to Theron Nunez, University of Florida, whose critical comments have been quite helpful. Naturally, I am solely responsible for the views taken in this essay and for any errors of fact or interpretation.

[2] Geertz (1964: 39) phrases this somewhat differently: ". . . the imposition of an arbitrary framework of symbolic meaning upon reality . . . ." We are certainly saying the same thing, but I am trying to give *imposition* and *arbitrary* form some further conceptual force by applying these attributes to language and tool-making processes.

beyond the simple criterion of symbolization (or the symboling of White) to a more direct confrontation with the essential question framed by Hallowell above: how to ascertain the appearance of such psychological structuralization from the palaeontological record. To do this, I will argue that tool-making and language are similar, if not identical, cognitive processes, and will use a number of models for language behavior to describe tool-making processes.

Before I proceed with the critique and the framework, certain additional comments are necessary: (1) The framework presented herein does not claim closure on this difficult problem. Indeed, this essay is to serve as a prelude for other analyses in preparation which deal more exhaustively with the question of tool-making and language as similar psychological processes. I hope it will also serve to open an area of discussion with other colleagues. (2) I regard this essay as a presentation of a *bias*.[3] (3) This paper is not a full review of the enormous literature that bears on the problem. I have purposely selected but a few references for critique because they are representative and summative of the kinds of thinking which I view as leading us nowhere. A more exhaustive analysis might well be profitable, but is not given here. (4) Many of the statements I have made with respect to symbols and tool-making are not original, but rather views more or less widely shared, here restated in a language which I feel may better focus attention on critical problems.

CRITIQUE

In general, three problem areas are involved in the nature of hominoid-hominid transition and the appearance of culture: gradualism vs. critical-point development; discontinuity and continuity in primate behavior (e.g., tool-using and tool-making, learning, and generational transmission); symbolization as a psychological process. This section will examine briefly these three interconnected areas and show that the third problem, symbolization as a psychological process, is the only one for which some solution can be expected.

In a recent symposium on transcultural psychiatry, Hallowell (1965: 29) outlined his thoughts on the evolution of human behavior and culture (see also Hallowell 1956, 1959, 1961):

> The Australopithecines, although hominids, were not "men" in the sense of "human beings"; they did not possess a "human nature" however ambiguous that term may be; they were not "men" biologically, culturally, or psychologically. This makes it more apparent than ever before that, whatever characteristics may be selected for emphasis, a "human" status necessitates a definition with reference to a position on some scale of structural gradation, as well as on some differential level of behavioral and psychological functioning. In terms of zoological criteria alone, the species comprising the genus *Homo* would suggest the lowest common denominator of a "human" status, while those constituting *Australopithecus* fall below it.

Whether or not Hallowell is ultimately correct in denying "human" status to the australopithecines matters little.[4] What is of concern is that the basis for the decision is a logical one; it is not based on an analysis of fossil morphology or of other concrete phenomena such as stone tools, but results from the constraints of the logical schema. To bridge the gap, i.e., provide a behavioral domain for these twilight creatures so that they might be placed within the logical schema, Hallowell develops the idea of a "protocultural" stage (p. 33):

> The most important feature of the protocultural stage, exemplified by non-hominid primates and certainly by the earliest hominids, was the existence of social structures, or systems of social action varying in size and mating patterns but in which parents of both sexes were associated with their offspring. These structures were based on role differentiation which depended in part upon the socialization of individuals mediated by observational learning, some tutelage perhaps, and systems of communication both gestural and vocal. There was social transmission of some group habits, and perhaps ad hoc tool-using in some groups.[5]

Further on (p. 34):

> Whatever the terminology used, some concept of gradation is necessary in dealing with the sociological, cultural, and psychological dimensions of hominid evolution, to replace a saltatory hypothesis.

In transcending the protocultural stage, the cultural stage adds the design features (see Hockett 1960, and Hockett and Ascher 1964) of productivity, traditional transmission, and duality of patterning. Reading further, one finds other attributes such as self-objectification (p. 44), symbolic reference (p. 42), self-identification (p. 44), self-awareness (p. 46), and self-appraisal (p. 49) appearing as concomitants of this psychological reorganization in the cultural stage.

One might argue from Hallowell's framework that a critical point did occur after the protocultural stage; so wide-ranging and important are the changes that it gives the impression of a quantum jump into a new dimension of existence. One might ask, also, whether the systems attributed to the protocultural stage are found in any contemporary primate group studied thus far.

I wish to make it clear that I am not in disagreement with Hallowell's basic premise of gradation, nor with his remark (p. 37; see also Harris 1964: 176–77) that:

> Concepts of culture that lay primary emphasis on shared and socially transmitted behavior without qualification do not enable us to make a necessary distinction of degree between different levels of behavioral evolution in the hominids.

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[3] This does not mean that I haven't tried to question my own biases and to refute them. I have, but I remain unsatisfied that the arguments reject my position. I merely mean that I am aware of my biases and should record them for the sake of accuracy.

[4] "Australopithecine" is used loosely here to include the early hominids known from both South and East Africa. I am including *Homo habilis*, because I am not at all convinced that the generic separation is warranted either on the basis of dating, fragmentary pieces, or ecological arguments. I am not including any of the Pliocene fossils, such as *Ramapithecus* or the so-called *Kenyapithecus*. Whether *H. habilis* is an australopithecine or not does not affect the substance of my arguments. Furthermore, I am making the assumption, for the purpose of this essay, that the Oldowan pebble tools were made by members of the Australopithecine taxon.

[5] As far as I am aware, no ape yet described has this combination of features. Gibbons appear to be the only apes that have parents of both sexes associated with offspring. The social transmission of group habits is certainly known for monkeys (macaque) and probably chimpanzees, but the family aspect has not been described. Ad hoc tool-using seems to be known only for chimpanzees. Thus we have no evidence from primate field studies for "protoculture" as Hallowell describes it.

I do disagree, however, with two aspects of Hallowell's view of behavioral evolution: (1) The denial of cultural status to the australopithecines is based on the logical constraint of finding some group to represent the protocultural stage rather than the interrelation of morphological and behavioral concomitants of their adaptation. (2) I do not know how to differentiate *Australopithecus* from *Homo erectus* (or *sapiens*) or even from apes in terms of self-identification, awareness, evaluation, or reference on any basis other than logical necessity. Symbols (abstract and arbitrary, "significant" in the sense of Mead 1934) would appear essential for carrying off the total psychobiological reorganization which Hallowell has so aptly described.[6] While these are undoubtedly lacking in non-human primates, I can see no way to prove that an ape lacks self-this or that. Later, I shall suggest a way of distinguishing between man and ape, one that depends on analysis of actual artifacts, not on logical imperatives based on a gradational framework.

Harris (1964) shares the gradation framework with Hallowell, but differs from him in letting the logic of gradation determine his view of human and non-human behavior. Harris (p. 174) says:

> The tendency to make culture a strictly human preserve has resulted in the widely accepted view that culture is symbol-mediated behavior and that only humans can make and use symbols. (Surprisingly, Leslie White, whose uncompromising evolutionism is everywhere else conspicuous, stands on this point with the special creationists.) To insist that only people can symbol is, in effect, to deny the possibility of transitional hominoid types.

Further on (p. 175):

> But no matter upon which hominoid or hominid . . . the honor of first symbol user is bestowed, we must sooner or later grapple with the problem that the first bona fide symbol system could not have sprung full blown from the head of some primitive genius. Language, like every other part of culture, has had an evolutionary career.

(See Geertz 1962, 1964 for a similar position.) Just prior to these statements, Harris has suggested the existence of insect "culture" (p. 173):

> . . . it is entirely possible and richly rewarding to apply a similar set of logico-empirical operations to the behavior stream of infrahuman organisms. All animals from the amoeba up can probably be shown to have repertoires of actones, episodes, nodes, nodal chains, scenes and serials. All sexually-reproducing animals probably also have repertoires of multi-actor scenes and serials, hence possibly nomoclones and permaclones. . . . many of my colleagues will undoubtedly be shocked by the heresy of insect culture.

Perhaps Harris has been richly rewarded in discovering that motor actions (that is, all that can be observed) are serial and hierarchical in organization. (What guides these actions and how the "strategies" are organized experientially is never analyzed.) I do not feel similarly rewarded. Harris says that most anthropologists believe that insects are driven by instincts while human behavior is learned, and he points out that insects are capable of learning. Surely the learning that occurs in humans is something more than the learning of insects; but for Harris, the differences are only "a matter of degree and do not justify the Aristotelian either/or approach" (p. 174). Primate field studies are cited as a further example, since primates also learn.

"Learned" vs. "innate" is hardly the issue; nor is there any doubt that many aspects of human behavior are rooted in biological operations that we share with almost all animals, particularly the primates. The issue is *how experience is organized.* Harris' argument is single-factor reductionist in not facing the central issues regarding comparative cognitive structuring. Harris is guilty of Aristotelian thought himself in neglecting to consider the possibility that the rubrics he accepts, such as learning and instinct, encompass many different kinds of organized behavioral patterns based on widely different nervous systems, ecological factors, and motivations. Hall (1963) has surveyed the literature on animal tool-use and has demonstrated effectively that tool-using as a rubric covers many instances with different neural, motivational, learned, developmental, and innate properties. The problem of equivalence with respect to these rubrics is hardly a new one; the past and recent psychological literature abounds with appreciation of this problem (see Nissen 1951, Schneirla 1949).

Furthermore, it is a non sequitur that to claim that only people can symbol is to deny transitional hominoid types. Both Harris and White (1942, 1949) confuse spoken symbols with the very complex issue of how experience is recorded, stored, and organized within various nervous systems. Munn (1955) and Hallowell (1959, 1960) have tried to face this issue forthrightly by differentiating between *intrinsic* and *extrinsic* symbolization.[7] The kind of oversimplification that Harris is involved in creates a pseudoproblem, as does Bryan's (1963: 301) statement:

> If the process of symbolization of abstract thought does occur in a rudimentary form among other animals, we must conclude that mental capacity and the capacity for the construction of culture is also only a difference in degree.

Just what does "the capacity for the construction of culture" mean, referentially and comparatively? Such statements leave the problem begging.

It is a simple matter to speak of gradation in behavioral evolution, but on the basis of the fossil record, nothing can be said with any certainty about the rate of change. A critical-point argument such as Kroeber's (see also Geertz 1962, 1964) need not be interpreted as a literal overnight or single-generation propulsion of apehood to manhood. No one is necessarily denying evolution or gradation in emphasizing, as have Hockett and Ascher (1964), the revolutionary changes which came with symbolization. The entire Pliocene bears witness to gradation within Hallowell's protocultural stage, yet it is not a logical requisite that the australopithecines are

[6] While I am in agreement with almost all of Hallowell's writings on the psychobiological reorganization during the hominoid-hominid transition, I remain convinced that many of the attributes he lists cannot be investigated with the frameworks we have available at present.

[7] Extrinsic symbolization refers to observable symbol-use, such as language, whereas intrinsic symbolization refers to the internal organization of experience, available to the observer only by inference.

simply the end product of such a stage. Hallowell has us leap into full *Homo*, qua cultural *Homo*, immediately after the australopithecines. Isn't this a sort of critical-point transformation? Couldn't culture "be invented" by the australopithecines in 20,000 years (just to pick an arbitrary span of time within which we have no empirical evidence by which to decide for either gradualness or sudden explosion)? On the other hand, given what we consider to be the extremely positive selective value in social adaptation for language, would not a sudden explosion logically be expected? I think that the issue is less the matter of gradation vs. explosion than a clearer understanding of the areas of continuity and discontinuity. This is an enormous problem. How to demonstrate such points of similarity and dissimilarity (whatever they actually are) within the fossil record is an even greater problem.

The continuities between man and his primate relatives have been receiving considerable attention recently. Social structures and social systems are now described for both apes and monkeys, apparently on the basis that social interactions between different members of a group are systematic in the sense of some invariant patterning. If this is social structure, then it seems permissible to talk about the social structure of chickens, geese, wolves, mice, dogs, insects,[8] and atomic particles. Fortes' (1965: 57) comment about *rules* being a requisite for society is of interest here, for it is man alone who can generate abstract and arbitrary *rules* for patterning social relations and actions outside of any primary biological tie.

It seems hardly surprising that apes or monkeys are more complex in their social habits than was thought before. I submit that the relevance of many of the studies is not the direct relation to human evolution or behavior (DeVore 1964), but simply that the complexity is interesting and of ultimate value in forcing us to think about and analyze the similarities and dissimilarities.[9] As Zuckerman (1933) pointed out, such behavioral data can aid in understanding both the classification and evolutionary relationships between different Primate taxa, and demonstrate how the functional viewpoint (physiological, anatomical, and behavioral data) could elucidate such relationships in the Primate order.

On the one hand, the return to a more zoologically based concern with human behavior is commendable, as when Tiger and Fox (1966: 76) state:

> Thus sociological findings in this perspective, provide data for a more comprehensive, zoological approach to the evolution of man as a gregarious organism. In consequence the study of human social behavior becomes a sub-field of the comparative zoology of animal behavior and is broadly subject to the same kind of analysis and explanation. No special theory other than Darwinian is necessary to explain the development and persistence of more general features of human social organization.

We cannot be so certain, however, that the level of analysis and explanation of comparative zoology will satisfactorily deal with all the general features of human social organization, particularly since these are rooted in symbol behavior, egoism, self, and these are key contributors to human selective perception. Tiger and Fox (p. 80) go on to say:

> . . . man's social behavior could be compared directly with that of other species, and interpreted by the same Darwinian concepts. Fruitful areas of research comparable to those developed in comparative ethology might be, for example: territoriality, optimum population maintainance, agonistic behavior, dominance and hierarchy, bonding, epimeletic behavior, mating and consort behaviour, ritualized display, play, intergroup relations, communication systems, etc. This expansion of orientation should lead to a better understanding of the non-cultural aspects of human social systems and in consequence to a sharper appreciation of the role of culture in human adaptation.

While symbol systems and their biosocial underpinnings were selected for during evolution and can in that sense be explained by Darwinian concepts, man's use of significant symbols raises the question whether the human behavior that falls under these rubrics is really comparable to the behavior of other animals, either in its expression or in the genetic unfolding and epigenetic development of behavior patterns within particular social contexts (see also Freeman [1966: 334], who is in apparent agreement with the above authors). The question is, is the object of study behavior or words? This question becomes particularly important when considering such complex processes as territoriality, dominance, bonding, ritualized display, play, etc.

The various findings from primate field studies have prompted alternative speculative schemes, centered around ecology, but emphasizing either the ecological similarities between early man and baboons or the greater degree of biosocial relationship between man and the great apes. Reynolds (1966) is not convinced that the best comparison need be ecological equivalence, but Crook (1967: 131) takes this approach in suggesting a cercopithecoid model of social organization, and so does Fox (1967: 419) in insisting on "baboon type 2" as the best model. Fox goes on to declare (p. 417) that man's capacity for cumulative learned behavior is a matter of degree and not kind. He attempts to strengthen his argument (p. 420) by appealing to the fact that ". . . the overwhelming majority of anthropological opinion now sides with trooping, ground-dwelling monkeys as the best model for the proto-human horde." This might be called "consensus anthropology" in lieu of real analysis and proof. How are we ever to know which social organization to accept, ape or cercopithecoid, for early hominids, and perhaps more to the point, what really hinges on it? The evidence we have, and will continue to accumulate, consists of pieces of bone of early man, pieces of stone either made to definite patterns or distributed in peculiar context, and associated faunal remains that give us clues to hominid adaptations. We must face up to the fact that

[8] There is probably some level of discourse at which it is meaningful to talk about the social structures of animals of different species. At the level we are involved with here, one can reasonably question the equivalence of units.

[9] I do not wish to be misunderstood when I take issue with certain aspects of primate studies. I am not against primate studies, and share with most the hope that many more will be attempted, both in the field and laboratory, by anthropologists, psychologists, sociologists, zoologists, political scientists, novelists, artists, and raconteurs. All that is at issue is how the data are used. Their relation to anthropology needs clarification, but this is not my concern here. Their value seems to me to be primarily heuristic: observation and analysis of primate behavior may lead to hypotheses about human behavior or human evolution, but I fail to see how they can provide evidence. I wish to thank N. A. Drekopf for his stimulating discussions on such points of methodology.

the degree of continuity between pongids and man in terms of intelligence and emotional behavioral patterns (not to mention brain and remaining anatomy and physiology) may be of more importance for understanding hominoid-hominid transitions than ecological similarities.

Apes and monkeys, sea otters, certain finches, ants, insects, etc., (see Hall 1963 for review) and, it is now reported, vultures (Lawick-Goodall 1967) occasionally use "tools." To elaborate upon these continuities, which are not particularly surprising and which in their essential respects antedate modern descriptions, is to draw attention away from the essential discontinuities. Of course monkeys and apes do not have a language based on arbitrary symbols concatenated according to definite rules; but what lies behind this fact in terms of comparative cognitive functioning and the social patterning which results in such functioning? The problem is not to label the discontinuities *language*, *rules*, *symbols*, etc., but to understand the psychological dimensions behind them.[10] It makes no sense to jump from present observations on apes, and particularly monkeys, to the "social structure," "social system," or "system of social action" of early man. Surely our concern with hominid evolution and the evolution of behavior, which has long preceded primate field studies, has set our perceptual processes so that we look at primates from the point of view of human evolution rather than the other way around. We are not learning about human behavior or human evolution from primate studies; we are learning about primate behavior and adaptation from our concern with problems of comparative psychology and a long interest in human evolution.

After all is said and done, the fact remains that the stone tools are the singular repository of any clues to behavior of a discontinuous sort.[11] Whether or not the australopithecines are "protocultural" or "cultural" will depend, not upon their brain sizes, reconstructions of ecology, or logical imperatives from a constraining framework of stages (although these can be supporting arguments), but on the analysis of artifacts.

What has been said so far in no way negates the very real problem of what kind of animal (or type of biosocial adaptation) immediately preceded man as a cultural being. Obviously, if one asserts that at some time x the human type of conceptual interaction based on arbitrary symbols through arbitrary non-iconic tool types appeared, this still leaves the problem of what preceded it. My point is that without evidence concerning their behavior, we are lost (but see Leakey, cited below) and must rely almost exclusively on such educated speculations as those of Hockett and Ascher (1964), Reynolds (1966), and Fox (1967), which are based on current behavioral studies of monkeys and apes in the field and, in the case of Hockett and Ascher, on our knowledge of the fossil record. Obviously, each of these methods has its own epistemological shortcomings. The recent evidence published by Leakey (1968) on tool-use by late Miocene hominoids (Leakey claims they are hominids) is surely coherent with these various models, since the stones are not standardized tool-types, but show bashing marks from bone.

[10] The "symbols" versus "signs" controversy is a good example of what I mean here, in that there are many psychologists who do not regard the distinction as either real or useful.

[11] Perhaps this is too strongly put. It is conceivable that other kinds of evidence might arise which could lead to hypotheses about emergent behavior patterns, such as large brain sizes, or certain archaeological contexts, such as crude stone shelters, etc.

TOWARD A FRAMEWORK

Arbitrary Form and Imposition

I have suggested above that whatever culture may be, it includes "the imposition of arbitrary form upon the environment." This phrase has two components. One is a recognition that the relationship between the coding process and the phenomenon (be it a tool, social network, or abstract principle) is non-iconic.[12] The other is an idea of man as a creature who can make delusional systems work—who imposes his fantasies, his non-iconic constructs (and constructions), upon the environment. The altered environment shapes his perceptions, and these are again forced back upon the environment, are incorporated into the environment, and press for further adaptation. This process is one of positive feedback (the "second cybernetics" of Maruyama [1963], or the amplification of deviation), the basic outlines of which were well understood by Engels (1896) and applied by him to bipedalism and the freeing of the hand and the subsequent growth of the brain and cultural complexity. The "invention" of symbolization, or the capacity to structure the environment arbitrarily (non-iconically), is thus the initial-kick (Maruyama 1963) which starts the process moving in the mutual-causal interplay between cultural and biological sectors of human evolution, e.g., expansion of brain, tool complexity, manual dexterity, social structure based on cohesion, communication. This interaction between the propensity to structure the environment arbitrarily and the feedback from the environment to the organism is an emergent process, a process different in kind from anything that preceded it.[13] Capacities such as intelligence, the ability to place distance in time and space between the reception of a stimulus and a consequent reaction or action, motor skills and sensory acuity, memory (both in terms of complexity of content and long-term storage), affection, motivation toward exploration and learning, are different only in degree from those of other primates and, indeed, other mammals. It is when these are integrated with the unique attributes of arbitrary production (symbolization) and imposition that man qua cultural man appears.

The recognition that man has a species-specific pattern of adaptive behavior does not invalidate any ideas of evolution or transitional types. (Lenneberg [1967] has

[12] I recognize that I am talking about symbols, and that a symbol is by its very nature unlike its referent. Nevertheless, I think that the arbitrary aspect of symbols has not been stressed enough, and that it can be useful in the analysis of early artifacts and of such behavioral patterns as kinship configurations, myths, laws, taboos, etc. It should also be appreciated that not all of symbolization need be arbitrary in the sense defined here; see footnote 13.

[13] It is worth stressing, on the other hand, that not all of man's interactions with the environment are arbitrary.

recently argued for such an attitude in his discussion of the organization of man's brain and language behavior.) The abilities of the human brain go far beyond what is usually meant by symbolization, language, or naming. Man has species-specific patterns of neural interaction, organization, and maturation that amount to "reorganizations" (Holloway 1966, 1967*a*, 1968*a*, *b*). The existence of these patterns cautions us not to place too much emphasis on cranial capacities alone, and it creates a need for modes of analysis that integrate the social nexus of nourishment and growth of the brain with its primary abilities and that employ a synthetic framework rather than one of serially concatenated variables (see, for example, Holloway 1968*b*).

Imposition, as I am using it here, has the connotation of ego involvement—something almost synonymous with effrontery and delusion. The intellective or cognitive aspects of symbolization or the production of arbitrary forms are without significance unless wedded to these psychoemotional tendencies. I define imposition as any "statement" (speech, motor act, gesture, action) that acts to maintain a figure-ground relationship against the resistance implied by its non-iconic nature (i.e., the fact that there is nothing in the stimulus itself to suggest it). In another context (1968*b*), I suggest that symbol systems organize experience into such anchorages that facilitate social control through communication, and that power relations, for example, can be established and defined outside of any strictly biological variables.

Undoubtedly, there are bases for the capacity to impose arbitrary form on the environment in the behavior of apes and monkeys, but the examples of learned traditions among apes and monkeys—washing potatoes in the ocean, unwrapping caramels or having a penchant for candy, or eating wheat, making nests, stripping a branch of leaves to get termites, or making cups out of leaves (see Frisch 1959, Lawick-Goodall 1965, 1967)—do not represent this capacity. The relationships expressed in these activities are iconic, and there is no feedback from the environment to the animal. A far better case can be made for the inventiveness, explosion, revolution, or "critical-point" suggestions relative to arbitrary symbols than for the complex interrelationships of emotional and cognitive factors inherent in the process of imposition (though it is difficult even to separate these two aspects and only an artifact of analytic procedure to do so). The capacity for imposition of arbitrary form must have been long in the making. A sense of gradation toward the expression of these two aspects as an integrated whole is important and necessary to a systematic view of human evolution. The problem of human evolution need not be cast in dichotomous, oversimplified terms such as gradation vs. "macromutation." The ground for revolution (in the sense of Hockett and Ascher 1964) was prepared by natural selection acting upon non-human primate groups for constellations of motor and sensory excellence, intelligence, and psychoemotional factors. Once the ground had been prepared, however, it was arbitrary symbolization and imposition that produced the revolution. Whether this development was "macromutational" matters less than whether we can identify it in the record. Stone tools are our major hope, but these should not be divorced from considerations of bipedalism, canine tooth reduction, ecological variables, or peculiar archaeological contexts. As I have argued elsewhere, changes in the brain cannot be used reliably to pinpoint the appearance of the human revolution (Holloway 1966, 1968*a*). We only have one parameter of the brain from fossil man —cranial capacity—and it is almost useless, since it tells us nothing about internal reorganization. The subsequent growth of the brain can best be seen as *one* resultant of the shift of selection pressures emerging from the new interactions between the organism and its symbolically produced environment. The question remains, then, how can we empirically identify this emergent process, and how can we understand more fully its psychological attributes?

Tool-Making and Language

What do stone tools tell us about psychological processes? The older much-worn distinctions between conceptual vs. perceptual thought (Oakley 1954, 1957) leave much to be clarified. The process of conceptualizing an end product and then maintaining a set of motor actions and appraisals of progress until the end product matches or satisfies the original conception is a complex business. It is a process extraordinarily heightened in man but continuous in essential structure with the higher non-human primates. Chimpanzees have been reported to fit together sticks (Kohler 1927) and to use sticks to procure termites (Lawick-Goodall 1965). These examples may represent simply ad hoc tool-use, but the borderline between tool-using and tool-making is probably very thin.[14] These few instances, plus a wealth of examples of problem-solving in chimpanzees (see Munn 1955, Warren 1965), clearly indicate the presence of *thought* in these animals (see Osgood 1953 for a discussion, also Nissen 1951), even if the internal processes are not coded in extrinsic symbols. We are fairly certain that the chimpanzee is capable of conceiving invariant relations between his actions and the outside world. There is no way of knowing what a chimpanzee is thinking, but it does seem fairly clear that its thought processes differ from man's in the absence of arbitrary form.

The token experiments (Cowles 1937, Wolfe 1936) on chimpanzees are often considered examples of conceptualization, and one might be tempted to argue these as examples of an understanding of non-iconic relationships, in that different-colored poker chips mean different units of reward. Three points can be made: (1) conceptualization is not at issue, only the organization of the concepts, and how experience is coded; (2) the chimps didn't make the poker chips, the experimenter did; (3) this is not different from pure association learning—the color of the token and the amount of reward (a convention established by man and forced upon the chimpanzees). Humans do learn this way; but they also name the items, invent them, and manipulate them *productively* beyond the immediate stimulus conditions.

These experiments, and many others (see Munn 1955, Nissen 1951, and Osgood 1953 for extensive reviews), show that symbolic processes must be granted to other

[14] It should be remembered that tool-use is also a major part of human activity.

animals on the basis of their actions. Somehow, what is perceived by the animal is inwardly coded; the symbolism is implicit, and we have no access to it. Reversal learning, delayed reaction, double alteration, oddity problems, all require memory, and this must be a symbolic process. The symbolism, is, however, *organic*, and without social convention, and thus not arbitrary. Whether we call these mediating perceptual carriers signals, signs, or symbols is not really relevant.

The iconic, non-arbitrary nature of much animal behavior can be seen from the following examples. Kohler's experiments with ravens (see Thorpe 1966: 478) are particularly illustrative. The raven was taught to open the lids of boxes bearing a various number of dots on them. The raven opened the box with the same number of dots as there were objects on a card presented in front of the box. Any amount of randomizing the boxes and key cards (and there were 24 permutations) led to the same result. The raven unerringly picked the box lid with the same number of dots as objects on the key card. Similarly, Logler (Thorpe 1966: 479) was able to train a parrot to associate a certain number of light flashes with the same number of irregularly distributed baits from a row of food trays. The number of correct solutions remained the same when flute notes were substituted for the light flashes. Obviously, there is a single property to the various stimuli which the animal focused upon—that of number—and it could transfer this from one stimulus to another *as long as the transfer was iconic*, i.e., 1 for 1, 3 for 3, 7 for 7, etc. One supposes that with sufficient memory capacity, animals could transfer 2 for 1, 3 for 2, 4 for 3, etc., or perhaps even learn a code such as 1 for 2, 3 for 1, 2 for 3, and 4 for 4, i.e., non-iconic. But in the latter case, each item would have to be learned singly and memorized. Nothing new could be generated or generalized. Any new relation would have to be invented by the human experimenter.

In the preparation of a stick for termite-eating, the relation between product and raw material is iconic. In the making of a stone tool, in contrast, there is no necessary relation between the form of the final product and the original material. Obviously, there will be considerable variation in the degree to which the form of a stone tool depends on the initial condition. Some pebbles need more or less working than others. The essentials of the operation remain, however, since the end product itself tends to be invariant in form (or its essentials) regardless of the shape of the original object.[15] Not all stone tools, however, are handaxes, Levallois flakes, or Oldowan choppers. Many are flakes, and items such as the Clactonian or Soan are not exceptions to the hypothesis given above. The hominid did decide to use the amorphous-formed flake, and the form of the flake, while perhaps more dependent than a handaxe on the shape of the original cobble or larger flake, is still not iconic. Furthermore, the form and the concatenations of motor activities and perceptual selections that go into its production are internalized. It does not seem unlikely that imitation and observational learning could explain some of the standardization of arbitrary forms. Certainly, psychological continuity exists between man and apes at this level. The important question is whether or not other processes were also operating, such as *consensus*, or *explicit rules* about the forming processes. What is at issue is concatenated activity according to rules, i.e., grammar. Imitation and observational learning seem to me insufficient to explain the tremendous time depth and wide geographic extension of certain tool types in much of the Old World. It seems more likely that rules, consensus, syntax, did exist, and that a communication system using symbolic language existed at least by the time of handaxes, if not before. (I am not trying to suggest an either/or approach to the question of imitation and observational learning and other processes. Both could have operated in combination.) If this suggestion is correct, then one might speculate as to internalization of self as a producer and internalization of arbitrary norms, role differentiation in terms of instructor-learner, and so forth; that is, the kinds of patterns which Hallowell has so often discussed can probably be generated once the capacity for imposition of arbitrary form upon the environment has been demonstrated. I will return to this matter of social psychological process somewhat later.

Returning to matters of syntax, rules, and concatenated activity mentioned above, almost any model which describes a language process can also be used to describe tool-making. This is hardly surprising. Both activities are concatenated, both have rigid rules about the serialization of unit activities (the grammar, syntax), both are hierarchical systems of activity (as is any motor activity), and both produce arbitrary configurations which thence become part of the environment, either temporarily or permanently. As an illustration, let us look at some of the design features that Hockett (1960) considers unique to the human communication system: *duality of patterning*, *productivity*, and *traditional transmission*. (I would also include arbitrariness, but this requires further comment, and will be discussed separately.)

Traditional transmission is so obvious that a mention will suffice; we know of no other way in which language or tool traditions could move as they do through time and space. *Productivity* is the feature of a communication system which permits new constellations to be formed and understood. The productivity of language is responsible for the complexity of the environment and for the capacity for efficient adaptation, and *arbitrariness* and *duality of patterning* are necessary complements to it. Theoretically, at least, a tool could serve a number of functions, depending on the particular stimulus conditions and the past experience of the maker regarding its use(s). It is clear from more recent discussions of Palaeolithic industries, even at the earliest levels (see Howell 1966, for example), that there is a wide variety of tool types within any assemblage. We now know that a handaxe industry, for example the Chellean or Acheulean, is composed of much more than the standard stereotypical biface. At any one time level, the number of variations around some basic pattern is extremely limited,

[15] Another factor that should be considered is the relationship of the hand to the range of operations possible. Krantz (1960) has shown through experimentation that the role of the thumb is particularly important in making an Acheulean handaxe. Napier (1961) has differentiated between "power" and "precision" grips on the basis of such experimentation.

but productivity can be seen in the facts that basic types were probably used for multiple purposes, that tool industries tend to expand with time, and that a slight variation on the basic pattern may be made to meet some new functional requisite. *Elements of a basic "vocabulary" of motor operations—flake detachment, rotation, preparation of striking platform, etc.—are used in different combinations to produce dissimilar tools, with different forms, and supposedly, different uses.* One cannot expect the tool inventory to have the same richness of variety and degree of openness as does language, however, since physical factors such as mechanical forces and the structural properties of stone are serious limiting factors. (If one wishes to embrace all of technology, that is another matter.) There is only a small degree of openness or productivity, based on smaller arbitrary units, or analogues to symbols, in tool-making, but it is the process that is of importance rather than the richness per se.

Duality of patterning is more difficult to discuss as a property of both human communication and tool-making. According to Hockett (1960) this design feature is "a set of conventions in terms of smallest meaningful elements, and also a set of conventions in terms of minimum meaningless but differentiating ingredients." This feature, according to Hockett, is specific to human language alone. If I understand this design feature correctly, it means conventions relative to what could be termed as (roughly) phonemes and morphemes. I think the feature would be better termed "triality of patterning,"[16] in order to encompass syntax or grammar, i.e., a set of conventions about the organization of Hockett's original two conventions. I hope that what follows will not distort the sense of Hockett's design feature. The process of making a stone tool, such as an Acheulean handaxe, is a concatenated activity, hierarchically organized. The number of separate blows delivered to detach flakes is variable, and there is a finite number of ways in which these blows may be delivered and still produce the desired result. Other motor patterns—rotation of the cobble, turning the blank over to do the same operations on the obverse side—alternate with the detachment of flakes. If the model of using both a hammerstone and a wooden or horn baton is correct, another set of operations is performed on top of the original or more crude blanking-out techniques. These latter operations are also finite in terms of number of blows, force, and direction. Taking each motor event alone, no one action is complete; each action depends on the prior one and requires a further one, and each is dependent in another way on the original plan. In other words, at each point of the action except the last, the piece is not "satisfactory" in structure. Each unit action is meaningless by itself in the sense of the use of the tool; it is meaningful only in the context of the whole completed set of actions culminating in the final product. This exactly parallels language. The organization of the entire activity is hierarchical and concatenated according to conventions of sequence. This organization is surely learned and transmitted through the generations and across space. We may reject immediately Pumphrey's (1951) declaration that such activities are the same as spiders' spinning webs, or birds' building nests. If this kind of analysis is not a distortion of Hockett's design feature, it can readily be appreciated that there is a sequence through time, an evolution, a gradation of complexity of organization of units as well as invention, productivity, and profusion of variations around basic themes.

Let us now turn to the question of *arbitrariness*. In his 1960 article, Hockett lists this design feature as one shared by man and apes. This has led some to insist that arbitrary productions are not a distinct attribute of the human cognitive process. The usual objection raised is that gibbon calls, for example, are arbitrary—the utterances (shouts, screams) bear no necessary relationship in their intrinsic structure to the phenomena they are connected to (see Marler 1961). Two objections: (1) The arbitrariness of a gibbon call is a judgment made by the human observer. Without access to gibbon experience, who can speak with authority about denotation and connotation in such calls, and gibbon perception and mood? (2) We are obviously dealing with differences at least in degree between the arbitrariness of a gibbon call and what man does. Perhaps there is a continuum of arbitrariness here; I do not know. We do not know to what extent gibbon calls are genetically determined, whereas for man we know that the arbitrariness is socially determined and the capacity for grammar and symbols genetically determined. Surely, the gibbon will not amount to much with its arbitrariness.[17] In any event, it is not the kind (?) of arbitrariness which is *imposed* on the environment by man. Finally, the arbitrariness in man is combined with other features which gives it great significance in terms of the generation of environment and the setting in motion of selection processes for complexity of structure and behavior (brain and social systems) to handle the imposed, arbitrary, environment.

In linguistics, a distinction is often made between *langue* and *parole*, *langue* being the language as a formal

[16] After this paper had been written, I discovered that Hockett (1966: 12) refers explicitly to the notion of "triality," which he credits to G. L. Trager and S. M. Lamb without citing any published account. Hockett sets aside this notion on the ground that any system with triality would have, a fortiori, duality. While I appreciate Hockett's point, I still think that the "triality" notion is useful, particularly with respect to cognitive functioning, since the third patterning attribute (grammar) appears as a species-specific operation of the human child's brain, and fits well with the analysis offered here for stone tools. In this same article, Hockett (pp. 12–13) adds a few more design features, including (1) prevarication, (2) reflexiveness, and (3) learnability. Prevarication means that linguistic messages can be false or meaningless. Reflexiveness means that a person can communicate about communication. Learnability means that a person can learn yet another language than the one he speaks. One is tempted to look for analogues in tool-making. Learnability seems the easiest, if one can grant that a tool-maker can learn techniques, traditions, or designs not common to his group. While this certainly might be done through observational learning and imitation, it is the pattern of unit activities that would be learned, and not each particular flake detachment. Reflexiveness and prevarication are more difficult. Reflexiveness suggests that the process of making a tool to make yet another kind of tool might be analogous. Prevarication presents more difficulties, since it suggests a motivational attribute, such as play, humor, or even perversity. Conceivably, a tool-maker might use the unit activities in an illogical manner, i.e., fashioning an object entirely outside of the range of variation of objects used customarily in his group, or mixing up activities to produce a nonsense tool. I prefer to ignore this particular design feature in my analysis.

[17] While the sounds that a gibbon may emit are arbitrary in the strict sense of that word, it would be interesting to know how they relate to degrees of intensity of the stimulus conditions. In any event, we are using arbitrary here in relation to *forms*, to the products of several streams of behavior.

system and *parole* the actual employment. The relation between a set of formal rules socially shared and the actual performance, potentially distinct in certain ways from the ideal or model, can readily be found in tool-making. Tool-making as a set of techniques, a set of idealized plans (*langue*), as against which each maker is involved with his own "ideo-making" ("ideolect," Hockett 1958), parallels the linguistic distinction. What I am suggesting here, and surely this is hardly original, is that any standardized outcome implies a prior set of communications involving an idealized set of operations or techniques. This does not mean that all standardized behavioral patterns presuppose language. Observational learning, for example, could take place through a process where the viewer internalizes (either by intrinsic or by extrinsic symbol processes) another's actions and uses these as a model for his own patterns. Social learning for animals other than man must operate something like this. The question is whether tool-making, given the great space and time distribution of particular patterns (e.g., Acheulean, Levalloisian), is not too complex to be explained wholly by observational learning without standardized symbolic codes of information-sharing. The tool-makers' activities, unlike those of apes stripping branches, are non-iconic patterns: these are concatenated one upon the other, and may be overlain by a new or different set of succeeding operations. It hardly seems debatable that a communication system based on symbols would facilitate learning, instruction, and techniques. The adaptiveness is too obvious to detail. To put it more concretely, tool-making involves a complex set of on-going perceptual events, each partly contingent on the past event and dependent upon the over-all plan or strategy involving the unit conceptualization of the final tool or form. The over-all plan or strategy is analogous to the *langue* and the total interaction between the idiosyncratic skills and the limiting factors (physical, mechanical) of the stone to the *parole*. The fact that standardization is obvious in the latter suggests an even tighter prior standardization of perceptual, cognitive, and motor processes. The observations of Brown and Lenneberg (1954) and Luria (1961) on how task performances are facilitated by the supplying of linguistic categories implies that the *langue* provides a set of anchorages or standardized frames of reference to facilitate the production of arbitrary form, e.g., a stone tool whose description bears no necessary relationship to the initial stone cobble. That is, naming acts as an "attribute filter" (see Brown 1958, Bruner *et al.* 1957).

Bernstein (1964: 56), in distinguishing between language and speech (roughly *langue* and *parole*), notes: "Between language . . . and speech is social structure." Working back from a finished tool, through the various motor and perceptual actions involved in its making, through the conceptualization, we will be led through social structural interaction which determined the norms of perceptual sets which operated to result in the final product.

Chomsky's (1956: 124) description of language is also interesting for purposes of analogy:

> . . . we picture a language as having a small, possibly finite kernel of basic sentences with phrase structure . . . along with a set of transformations which can be applied to kernel sentences or to earlier transforms to produce new and more complicated sentences from elementary components.

Again, possibly any behavioral act in its motor terms can be described accordingly, i.e., composed of basic muscle contraction patterns, organized into short sequences analogous to phrase structure, applied or added to other sequences to produce new and more complicated units of motor acts (e.g., locomotion, feeding, grooming). The success of description depends on the units of analysis. What interests us is the arbitrary nature of the patterning and the necessary imposition of schemas from the social surroundings to effect standardization. Stone tool types, from Oldowan through Chellean-Acheulean to Levalloisian, show a basic pattern (flake deflection) overlain by a set of "rules" about how many flakes (approximately) shall be deflected and where. As the tool becomes more complex, there is more interpositioning of basic units and other actions (e.g., rotation of tool, retouch, platform preparation, use of baton).

Greenberg (1967: 349–50) lists the following as common to language: (1) phonology, or sound system, made up of phonemes and sequences of phonemes; (2) grammar, or rules regarding the arrangement of elements that are meaningful; (3) semantics, or meaning. In tool-making, the "phonemic" level involves such units as striking a flake (two variants at least: percussion and anvil), revolution to expose obverse face or striking platform, detaching a flake by pressure, retouch, snapping, splitting, etc. (No effort is made here to provide more than a small set of examples.) The "grammar" is the concatenation of smaller unit operations that produces the tool. The "semantics" involves two levels: (*a*) the meaning represented by the use of the tool as finished product, and (*b*) the meaning of each unit action as an outcome of the preceding one and as preparation for the next, i.e., detachment of flake to provide a striking platform, or to provide part of a cutting edge, preparation of striking platform for detachment of a cutting flake as in the Levallois technique. As in language, the activity is made up of units concatenated non-randomly, there being contingencies both in language pattern and tool-making. In the case of tool-making, however, the alphabet, or analogue to the phonological system, is extremely limited, as are the grammar-like contingencies.

Morris (1946: 35–36) defines language in terms of the following criteria: (1) Language is composed of a plurality of signs. (2) Each sign has a signification common to a number of interpreters. (3) The signs must be com-signs, i.e., significant (in the sense of Mead 1934), producible by the members and having the same signification to the producers as to the interpreters (cf. Hockett's "interchangeability"). (4) Signs are plurisituational, i.e., signs with a relative constancy of signification in every situation in which a sign of the sign-family appears. (5) Signs must constitute a system of interconnection, combinable in some ways but not others.

Sign plurality can be seen in tool-making in two ways: (*a*) the production of different tools for different tasks, or (*b*) the preparation of portions of tools (a cutting edge, a

blunter edge for scraping or pounding, or a point for gouging) to serve specific tasks. At the more molecular level, that of actual tool-making, there is a stock of minimal units of activity, such as deflecting a flake, revolving the piece, preparing a platform, striking a final blow for a flake, snapping a blade, splitting a cobble in half or quarters, increasing the striking angle, retouch, baton work, which could be carefully defined and quantified (this work is in preparation at present). At the same time, these specificities satisfy the criterion of being plurisituational. Points (2) and (3) refer to the common signification aspect, and tool-making *and* tool-use are analogous both at the level of specific tools and tasks (or portions of the tool to a specific task and at the level of units of technique. The fifth point, that of non-random interconnection, means in tool-making analogy that the alphabet of chipping technique is not random either, and that each type of tool has its own specific program of construction from different minimal unit activities, where certain of these are contingent upon prior operations (e.g., Levallois technique).

On the matter of distinguishing animal sign-behavior from that of man, Morris (1946: 53–54) is quite specific:

> All such discussions usually culminate in the question as to whether language is unique to man. Here the issue is in part terminological, since if "language" is made synonymous with "communication" there is no doubt that animals have language; or if only some of the criteria which are incorporated in the preceding definition . . . are insisted upon . . . , then too there may be no doubt about the existence of animal language. But if the full proposed definition is accepted, I know of no convincing evidence that any animals other than men have either a signal or symbol language, though it is to be insisted that the problem is an empirical one and not to be dogmatically resolved.

In short, there are these analogies between language and tool-making in terms of the design features taken as unique to the human case. I am insisting that the cognitive processes involved are the same. The tools, made to standardized patterns, *do not (and cannot) prove* that their owners and producers had a language based on symbols, and I can see no good reason to claim that language must have followed tool-making. Tool-making and language are concordant. Selection favored the cognitive structures dependent on brain organization and social structure which resulted in both language and tool-making. The evidence of changes in the brain (in cranial capacity and the outside configurations of the cortical gyri and sulci) and in the anatomical configuration of larynx, nasal cavities, epiglottis, trachea, etc., need not be considered contradictory to this view; the first is simply useless information for the problem at hand, and the second bears only on the specific types of sounds produced and the effectiveness of their production.

At the same time, these features cannot totally be ignored. The increased relative brain size of certain members of the australopithecine taxon (Tobias 1965) suggests that some different timing of growth processes had occurred by this evolutionary stage, perhaps resulting in a somewhat more prolonged period of infant and child dependence on the mother, best served by a biosocial adaptation requiring enhanced affect-interplay between child and adults and an increase in co-operativeness between group or band members (see Holloway 1967*a*, *b*, 1968*b* for speculations). This may also have involved reorganizations in the central nervous system leading to the species-specific qualities associated with human language ability.

Similarly, the upright posture of *Australopithecus* surely must have meant a different range of economic activities (hunting, scavenging, gathering, sexual division of labor) than is known for other primates, and as upright posture could have meant a facilitation of the structures of sound production (Lieberman n.d.), the fossil evidence is concordant with the view that language was part of australopithecine behavior. The point is, there is no evidence from the anatomical patterns that rules out language behavior for these hominids. Orr and Cappannari's (1964) discussion of the neurophysiological evidence relating to language, the close proximity of the hand and tongue areas in the cerebral cortex, and the possible relevance of inhibitive interactions between cortex and subcortical systems is relevant to, and even concordant with, the views taken here about social behavior based less on competitiveness and more on co-operation, and the similar cognitive organizations associated with and underlying language and tool-making behavior.

Another argument for a correlation between tools and language is that of Leroi-Gourhan (1964, 1965), which came to my attention after the major part of this essay was completed. Indeed, he believes that the linguistic skills of early man can be gauged to some extent by the complexity of his tools (see particularly 1964: 163–64), resulting in a "paléontologie du langage." Crucial to this author's framework is the term "mémoire," which refers to a programming of chains of actions important to the adaptation of the group. In man, the *mémoire* depends on language rather than instinct, and only in man can the acts or chains of acts be "exteriorized," i.e., freed from strict biological dependence. This is done with a symbolic language.

Obviously, I am in agreement with Leroi-Gourhan that tools and language are correlated, but I hope I have shown somewhat more thoroughly how I arrived at this conclusion. I do not agree, however, that it is possible to go beyond asserting this correlation: I do not consider it feasible to gauge the complexity of language on the basis of tools, since these are but only a limited sector of early man's environment which he differentiated or carved up for his experience. Furthermore, my analysis deals with tool-making, not the tools themselves, and only where the patterns of tools are not only invariant (standardized) but also non-iconic.

There is no possible way in which either the lithic or fossil record can serve as direct evidence for language ability, but this does not render spurious Leroi-Gourhan's argument regarding the increasing complexity of language. The fossil and lithic record can only be used indirectly, as I have tried to show in this essay. Instead of relying on analogies between human and other animal behavior as does Leroi-Gourhan, I have relied on examining a number of models of language behavior to see if the conceptual descriptions will also work for tool-making. Here, I must emphasize tool-*making*, and not simply tools, since I believe that there is a spurious analogy between tools per se and language, i.e., that language serves as tools.

Thus far, the tool-making process has been approached from the viewpoint of understanding something of the individual cognitive processes involved. It has been argued that tool-making allows us to make some inference regarding conceptual processes imbedded within an arbitrary (non-iconic) framework. Such an approach is, of course, a speculative one, but there is a more important objection to leaving the matter there. Stone tool-making processes are both individual and social (group) activities. Unless tool-types are regarded as an outcome of some innate releasing mechanism, with the final product and its necessary motor and perceptual operations programmed in the genes and nervous system, it must be admitted that the definition of attributes of the tool, the frames of reference (the anchorages which aid to separate the figure from the ground), the perceptual sets, all were established in social groups.[18] In short, tool-making must be approached from the viewpoint of social psychology to be holistically understood. The issue is, how were the perceptual sets, the cognitive orientations toward particular clusters of environmental stimuli, formed and organized in the first place? Obviously, there are questions here of socially imbedded rules, regardless of whether the motor actions and perceptual sets are acquired through observation and imitation or through symbol-mediated instruction.

A stone tool is, for the purpose of this essay, more than a simple object which had some X use some time Y ago. The stone tool manifests another design feature not shared by communication systems using the vocal-auditory channel. It has "*no fading*," or rather, it has "*rapid-fading*" (Hockett 1960), only to the extent that we refuse to read from the tool the processes that must logically have gone into its formation. Obviously, it is impossible to specify precisely the details of the process of making, e.g., a Chellean handaxe. One could estimate approximately the number of blows a cobble received by counting flake scars, and one could assume that the tool was rotated at least once in the hand before another blow was directed. I am not concerned here with these details, as they are unnecessary to the theme of my discussion and a more detailed manuscript is in preparation. I am concerned with the more general aspects of socially mediated rules involving a set of operations that produce an arbitrary form. Stone tools, even of the Oldowan type, give evidence of a very simple but significant fact: *conformity of behavior*. They also demonstrate the simple (or complex) fact that the perceptual processes of their makers were selective. It is the matrix of selected orientations and how it is generated that is of interest here. I have found the writings of Mead (1934), Sherif (1966), and Sherif and Cantril (1947) useful for this purpose, because they are concerned with the genesis of conformities in behavior, and have elegantly summarized the relevant psychological literature regarding the formation of perceptual sets, the integration of frames of reference in the shaping of such sets, and eventually, attitudes. While these social psychologists have hardly been concerned with stone tools, they have formulated the problem of shared perceptual sets which underlies the basis for the conformities that the archaeologist unearths. For example, Sherif (1966: xi) states:

> . . . the problem formulated was *under what conditions are people guided by objective factors of the world around them* and *under what conditions do they become reciprocally susceptible to each other's influence to establish a stability in their perception relative to their surroundings?*

Also (p. 106):

> The psychological basis of the established social norms, such as stereotypes, fashions, conventions, customs and values, is the formation of common frames of reference as a product of the contact of individuals. Once such frames of reference are established and incorporated in the individual, they enter as important factors to determine or modify his reactions to the situations that he will face later—social, and even non-social, at times, especially if the stimulus field is not well structured.

Sherif and Cantril (1947: 29) note:

> When an individual reacts repeatedly in a characteristic way (positive or negative) in relation to a certain stimulus object, we infer that the members of the group have an established social attitude in relation to it.

Most stone tools (but certainly not all of them) are objects with *standardized* forms restricted to but a few definite shapes, which, as argued above, are largely arbitrary or non-iconic. These objects, the tools, have passed from an *unstructured* condition to one with an *imposed structure* by way of plural sets of activities, each involving selective perception. Tools have symmetry beyond chance or the physical laws of impact of one hard substance against another. For example, a well-made Acheulean handaxe has symmetry around three axes or planes. Flake scars are not produced randomly; the tool has been rotated to effect similar operations on the obverse face; there is an edge, and it has a variable extent along one plane of the tool; there are terminations to certain sequences of motor and perceptual activity once some set of attributes is judged as complete, adequate, or matching some internalized conception. There are the antecedent stages to the actual processing of the stone blank or cobble (or large flake), involving selection of suitable materials, where figure-ground relationships are variable, yet standardized in the sense that certain objects will be selected (become figural) and others rejected (remain ground). That is, out of the welter of variations in the environment, only certain objects will be discriminated and selected from the rest. The same applies to the use of hammerstones and very likely to subsequent stages of the use of the manufactured items. How many frames of reference there were is a problem for future research, and specifying them will be a dangerous procedure because the experience is not available to us. My point here is less a matter of worrying about these specificities than of showing that many

[18] This does not mean that there are no innate predispositions for humans to perceive certain things in certain ways. The human infant's reactions to the adult face is a possible case in point, and there are probably other examples. Nor do I mean that there cannot be sets which are essentially automatic, without immediate cognition. (See also Sherif and Cantril 1947: 43–50.)

frames of reference must have been operating.[19] This passage from an unstructured condition to one of structure has involved the maker in a constant interaction with conformities both at the motor and perceptual level. *The genesis of these conformities must have been imbedded in social relations.* Again, it is worth noting that all motor and perceptual activity is concatenated, hierarchically organized, and with cut-off points once some criterion has been met (see Miller, Pribram, and Gallanter 1960 for a general framework describing these operations; also Harris 1964). Fish, ants, vultures, monkeys, apes, and men do it. The point is, that when the motor actions and perceptual sets are *established* for arbitrary or non-iconic configurations which cannot feasibly be explained on the basis of innate mechanisms, we are dealing with socially determined rules. We are dealing with human behavior. We are dealing with an important attribute of "culture" which other animals do not have. We are dealing with "significant symbols" (Mead 1934). These are not operations of unwrapping some flint wrapper from tool-candy, or washing the potato off in the ocean. These are not simple motor responses learned from the alpha male or precocious deviant child. They are acts producing structure where there was none before, where the final product has no necessary relationship to the initial object. Through social rules, shared frames of reference, socially transmitted strategies for producing structure, arbitrary form was imposed on the environment, *and rules imposed upon the society's members.*

Certainly language is adaptive, particularly in making possible communication as to the nature of the environment, planning, learning, and transmission of knowledge over generations, and it hardly takes any imagination to see why it was selected for in hominid evolution. Because it is based on arbitrary symbols and grammar, it has productivity that far exceeds that of systems based on instincts or innate releasing mechanisms. How could any system encode the ever differentiating environment, whose manipulation is a key to adaptation, in a wholly iconic way except by visual pictographs? Arbitrary symbols are, after all, *impositions* which *structure* perceptions and interactions between the organism and its environment, particularly other organisms of the same species. Symbol systems *enforce* figure-ground distinctions, and enforce interrelations between organism and environment (whether it be a stone tool, an animal's scent or footprint, or another member of the group) and standardize them by the simple fact of representation that must ignore a certain amount of idiosyncratic variation or lack of non-essential uniformity. Symbol systems *are social and material control*, and surely social control was a key element in hominid evolution, given that co-operation and elemental social groupings meant selection for different dimensions of affect-impulse control, cognition, perceptual sensitivity, play, hostility, and communication. Arbitrary symbols enforce consensus of perceptions, which not only allows members to communicate about the same objects in terms of space and time (as in hunting) but also makes it possible for social relationships to be standardized and manipulated through symbols. It means that idiosyncracies are smoothed out and perceived within classes of behavior. *By enforcing perceptual invariance, symbols also enforce social behavioral constancy, and enforcing social behavioral constancy is a prerequisite to different task-role sectors in a differentiated social group adapting not only to the outside environment but to its own membership.*

Symbol systems are rules about the world; they standardize perceptual selection by enforcing actions to objects and relationships perceived and symbolized. The transmission of these rules requires stable and predictable relationships of interpersonal perception and, ultimately, rules of conduct. This can only be half of the story, however, because any such formulation must also provide an explanation of the generation of conflict. The dialectical argument is appropriate here, because symbol systems are enforced on animal natures resplendent in sheer egoism, and surely much of social structure represents behavioral-organic responses to the invention and social processes of symbolization. Imposing form generates also its opposite, variability and resistance.

Putting the argument another way, arbitrary symbols standardize the *decoding* (interpreting the environment; Osgood and Sebeok 1965) operations of members of a social group; they also standardize the *encoding* processes, that is, how the intentions of the members are expressed. These are expressions or instances of social control, and these may be carried out without reference to strictly biological variables. Standardizing either input or output processes, however, does not guarantee standardization of *processing*, that business that goes on inside the "black box." That is one reason why the behavioristic accounts, such as those of Skinner, are not appropriate to human processes, except for a very limited range of descriptions.

How were these frames of reference, rules, perceptions, generated and transmitted? Were symbols the vehicles? Was language involved? I have already indicated my *bias* to answer this affirmatively, as did Sapir and others. I can see no way of proving it, but I believe the framework offered herein makes as convincing a case as any. I am not concerned here with questions regarding the sophistication or primitiveness of the possible symbol system. I am concerned with making the point that there is no a priori basis for denying the possibility of a primitive symbol-system or language when one has stone tools made to a clear-cut standardized pattern. To raise objections on the basis of brain size, gradations in biological and social evolution (the necessity for a protocultural stage), or the presence of learning processes in apes and monkeys, finches, ants, and sea otters is an exercise in futility. Objections based on brain size must invoke assumptions regarding the structure of early hominid brains. Objections based the logical constraints of a

[19] Likewise, I am purposely omitting any discussion of the problems facing the archaeologist who discovers such materials and who must decide upon frames of reference to recognize patterns of standardization. I am not concerned with the archaeologist's perception, but with that of the hominids. There is, I hope, some concordance between what the archaeologist perceives and uses in his analysis and what the makers of the tools used as frames of reference. Thus I am purposely omitting a large literature from archaeology concerned with such problems. Rouse (1960) has already given himself to such problems in his development of "modes" (any standard governing behavior of artisans) and his discussion of "Analytic Classification" (pp. 313–15). He is not concerned, however, with *how* the perceptual sets or frames of reference become established or why. While I have not provided any quantitative data regarding the number of sets or frames of reference, I submit that they are too numerous to be explained in terms of innate releasing mechanisms and that their organization presupposes social mediation of a symbolic nature.

gradation framework do not tell us where in the sequence the behavior emerges. Objections based on learning models alone are Aristotelian errors which reify the learning achieved under widely different interrelations of organic structure and environmental stimuli as identical phenomena: learning for an ant is surely different than learning for a man. These objections, I repeat, do not vitiate the fact that both symbolic language and stone tool-making are processes imbedded in social situations where arbitrary form is imposed upon the environment.

Similarly, I am not interested (at least in this paper) in deciding whether this or that hominid had "culture" in the sense I have discussed it. Hallowell (1965) has decided that the australopithecines were without it. This decision is not based on their brains, their teeth, their bipedalism, or the stone tools found associated with some of the finds, but on the basis of a logical necessity determined by his definition of a protocultural stage. I prefer to say that if there is evidence of stone tools made to a standardized pattern which is non-iconic, one has evidence of behavioral specificity of the human (*Homo*) sort. *Australopithecus* passes or fails the "culture test" on the basis of his products, not his morphology.[20] For my part, I believe that *Australopithecus*, or whoever made the Oldowan choppers, was human and possessed culture. The stone tools will not "tell" us precisely how the hominid organized his experience; they "tell" us that he possessed a cognitive structure necessary for language, a structure harmonious with language. This does not mean that the genesis of the system was not gradual, or that components of the system were not rooted in many sectors of behavioral continuity with other primates, indeed, other mammals. Nor does this framework deny that once such systems emerged they produced veritable revolutions. It means that the basis for decision about the presence of an emergent is to be based on analysis of the products of cognitive structures alone, and not brain sizes, abstractions from the behavior of baboons, or philosophical systems.

Culture is ours alone, by the facts of arbitrariness and imposition. Logical frameworks which necessitate a priori decisions regarding the placement of an event in the past in a psychological framework will not determine the presence of "culture:" an analysis of stone tools will. A return to the essential problem, the "imposition of arbitrary form upon the environment," might serve as a stimulus for discussion that will eventually return culture once again to our own domain.

[20] The elements of fossil morphology are, of course, important clues to the adaptations that the animal made to its environment; but such features as bipedalism, teeth, or brain size, either separately or together, cannot tell us whether the animal possessed "culture."

Abstract

It is argued that a number of recent writings based on primate studies and on analysis of early hominid evolution have blurred certain central issues regarding human and non-human primate behavior. The central problem of how man organizes his experience and how he interacts with his environment is seldom squarely faced. A framework is provided here which examines tool-making in terms of psychological processes. It is argued that both tool-making and language come out of the same cognitive structure. The framework attempts to provide a means by which the appearance of emergent human behavior may be gauged from the fossil record. Two attributes, *arbitrary form* and *imposition*, are defined. It is argued that these two dimensions are specific to the human psychological structure, and that stone tools made to any standardized form satisfy the requirements of emergence in cognitive structure. Tool-making is analyzed using models for language behavior, and strong parallels are shown with certain design features that are specific to human communication. Tools are then viewed from the perspective of social psychological frameworks relating to the acquisition of norms of reference, perception, and the passage of objects from an unstructured to structured condition. This analysis suggests that arbitrary symbols played a major part in the development of social controls adaptive for early hominids utilizing strategies of division of labor, since symbols produce invariant relationships that can be defined outside of strictly biological relationships.

Comments

by JEAN BENOIST☆

Montreal, Canada. 10 II 69

Holloway's work leads to interesting results in two directions. First, social anthropologists are not usually concerned with the biological sciences, and they have come to ignore the fact that some of the phenomena they study are deeply rooted in zoology. In the past few years, however, there has been an increasing tendency for those who undertake the study of man to reconsider the separation between what is cultural and what is biological. Holloway has shed new light on the intermingling of the two levels and on what must be allotted to each. Second, one is grateful to him for his effort to free himself from the theories of "consensus anthropology" and to open up new vistas.

Some questions do arise, however, on each of these points. While he refuses to accept the—often artificial—criteria that separate the partisans of a critical-point theory and those of a gradation framework, is he not, in fact, trying to set up other criteria of discontinuity? Above all, I would appreciate more precision regarding his main argument, "imposition of arbitrary form" upon the environment. Is the difference really convincing between a stick prepared for termite-eating and the pebbles made into the first human tools? Isn't the relationship between "invariant product" and "shape of original object" in the human tool and, for example, the bird's nest identical (independent, of course, of their functions)? And is, then, the transition from an "unstructured condition" to one with "imposed structure" really the essential dividing point?

by ROBERT N. BOWEN☆

Honolulu, Hawaii, U.S.A. 11 II 69

Holloway is concerned that recent primate studies have blurred the behavioral distinctions formerly made between hominids and non-hominids, and he attempts to re-establish them by focusing on cognitive patterns he feels are uniquely hominid. He cites "imposition of arbitrary form upon the

environment" as unique and equates this with *culture*, thus reinstating the latter as a specifically human phenomenon. His study centers on language and stone tools, focusing on the latter as being the "singular repository" of clues to arbitrary imposition in early man. He considers Oldowan tools as being *non-iconic* (that is, not suggested by the stimulus itself), while the tools of Goodall's (1963) Gombe Stream chimpanzees are *iconic*. I agree that there are basic differences in the cognitive processes out of which these two tool-making patterns arise, but I do not think that "imposition of arbitrary form" is the critical factor. Both types of tool-making involve altering natural materials toward more effective environmental exploitation; that is, arbitrary form is imposed in both cases, granted that more alteration may be associated with Oldowan tools. Unaltered sticks seem to me no more iconic than unaltered pebbles.

The hominid pattern differs from that of the chimpanzees in that it involves: (1) stone, which is much more difficult to work than wood, (2) the use of a tool to make a tool, (3) greater initial selectivity, and (4) possible carrying and storing. These features reflect more complex cognitive patterns and are related to Hockett's (1960) unique design features in hominid vocal communication.

The author compares human language structure, as described by Hockett (1960), Greenberg (1967), and Morris (1946), with the structure of early standardized stone tools. I find his analogies clear and useful. He errs, however, when he cites *traditional transmission* as a design feature unique to human communication; rather, Hockett considers this feature to be found in both hominids and pongids. In addition, Hockett refers to the extremely important *displacement* feature, accompanied by *productivity* and *duality of patterning*. *Displacement* and *productivity* seem to be reflected in Oldowan tools and *duality of patterning* in later industries. The author also discusses *arbitrariness*, which Hockett attributes to non-hominoid primates as well as man and the apes. Holloway dismisses this feature in gibbon vocal communication, saying "it is not the kind(?) of arbitrariness which is *imposed* on the environment by man." This evaluation seems subjective, and yet the point is critical to his thesis.

I agree that Oldowan tool-making and human language come out of the "same cognitive structure," but do not agree that the appearance of stone artifacts reflects the *emergence* of human cognitive behavior (although it may represent the first relatively non-perishable evidence for it). Rather, I see the emergence occurring long before the time of the australopithecines, specifically near the time when full bipedalization emerges with all its associated behavioral patterns. According to present evidence in the form of *Ramapithecus* (Pilbeam 1966), bipedalization may have developed as early as 14,000,000 years ago, and perhaps earlier. This is 12,000,000 years before the earliest evidence of stone tool-making. It seems logical to envision millions of years of altering more easily worked perishable materials before stone alteration began, perhaps while stone was being used but not altered. The point is that in order to discover *emerging* uniqueness, we cannot rely on stone tools, but must look to paleontological evidence to furnish clues. Holloway says, "changes in the brain cannot be used reliably to pinpoint the appearance of the human revolution," and refers to cranial capacity as "almost useless." Cranial capacity cannot reveal cognitive processes; nevertheless, increasing cranial capacity and full bipedalization are extremely important changes in the Primate order and furnish *clues* to unique behavioral patterns. The alteration of stone comes much too late in the record to tell us anything about the *emergence* of hominid behavior.

Holloway feels it is important to have a clearer understanding of areas of continuity and discontinuity. I agree, since the application of the common terms *man* and *human* depends upon where physical and behavioral continuities and discontinuities are seen. If one is citing relatively discontinuous points in hominid evolution, the first would be full bipedalization, facilitating increased fine manipulation and alteration of natural materials, perhaps to a considerably greater degree than the chimpanzee tool-making observed by Goodall. From this period, through the appearance and differentiation of stone artifacts, I see no behavioral discontinuity until the domestication of plants and animals in the Neolithic, but rather a relatively slow and continuous increase in elaboration and effectiveness (although some students might recognize a stone utilization discontinuity). The next discontinuity occurs with the exploitation of carbon fuels, and others may occur in the future with nuclear and solar energy exploitation. The terms *man* and *human* have been applied to segments of hominid evolution in an amazing number of ways, with little consensus. The view I favor, as reflected in my comments, is to apply them to the entire family Hominidae, including *Ramapithecus*, placing primary emphasis on the first discontinuity, full bipedalization, and all that it implies in terms of structure and behavior. If it is difficult for the minds of "gradation" students to accept this relatively sharp "critical point," three or more stages can be arbitrarily superimposed, such as *formative*, *developmental*, and *full*. Similar stages can be applied to Hockett's design features. Trying to establish a human and non-human division at other discontinuities mentioned leaves us in the embarassing position of labeling some *Homo sapiens* populations as being human and other non-human. Drawing the line at other points because of subjective impressions that "human nature" is or is not present is unscientific.

My last comment concerns the alternate title, "The Myth of Animal Culture." In keeping with the author's thesis, it should read, "The Myth of Non-Human Animal Culture."

by ALEXANDER DAWIDOWICZ☆

Warsaw, Poland. 3 II 69

The author has made an important contribution in his very persuasive review. Particularly convincing is his argument that evolution favored the cognitive structures dependent on brain and social organization, which resulted in specific and unique human culture and human behavior.

There can be no doubt that some elements of the hominization system are "rooted in many sectors of behavioral continuity" with other species, both infrahuman primates and other mammals. All life forms came from the same stock. Therefore, some elements of behavior and, above all, elements of metabolism, genetics, and immunology are similar or even identical in various species. Nature is not very inventive; the number of chemical compounds invented by man in the past few decades is larger than the number of compounds created by Nature over millions of years.

Evolution is slow, and the same pattern repeats itself over and over. Man, with his cognitive structure and, indeed, with imposition of arbitrary forms upon the environment, plays a special role in this process. Man is also, however, a product of evolution. Therefore it is not surprising that some traces of this evolutionary process appear in his physical and psychological makeup.

by THEODOSIUS DOBZHANSKY☆

New York, N.Y., U.S.A. 14 II 69

The problem so pointedly and penetratingly discussed by Holloway is by no means new. Darwin showed almost a century ago that remote ancestors of mankind were not men. But how far has mankind diverged from its animal ancestors? The spectrum of proposed answers ranges all the way from that man is nothing but an animal to that man differs not in degree but in kind

from any animal. In a sense, the extreme views are both equally true and equally trivial. Man is nothing but an animal, because he has no immaterial soul. On the other hand, only man can learn to read CURRENT ANTHROPOLOGY or to pilot jet aircraft. Yet to define just what complex of traits and abilities really distinguishes the human from other species of animal is by no means a trivial problem.

Biological classification was almost exclusively, and still is very largely, based on bodily morphology. On this basis, mankind belongs to the family Hominidae, of which it is the sole living representative. A familial rank is warranted, because the nearest related family, Pongidae, differs from Hominidae in the structure of many body parts. However, there are no sufficient morphological distinctions to exclude man from the order of primates, from the class of mammals, from the phylum of vertebrates, or from the kingdom of animals. On the other hand, man's psychological attributes separate the "human domain" from the animal domain far more radically. Whether or not Holloway has succeeded in defining precisely the nature of the discontinuity may be debatable; I believe that he has at least put the problem in sharper focus.

The human species is a prime example of quantum evolution, to use G. G. Simpson's term. In a short (on geological scale) interval of time, this species evolved an altogether novel mode of adaptation to the environment—through culture, which involves what Geertz calls "imposition of an arbitrary framework of symbolic meaning," or in Holloway's version "imposition of arbitrary form." This framework or form constitutes the adaptive zone, in this case the uniquely human adaptive zone of our species. If biological classification were built according to psychological instead of morphological attributes, one would have to divide the animal kingdom into humans and non-humans, instead of conventional phyla.

What is involved here is, however, more than a matter of classification. In producing man, biological evolution has transcended itself, in the same sense in which the evolution of inorganic nature transcended itself in producing life. Of course, I am aware that some authors would like to restrict the term "evolution" to biological evolution only. Such a restriction is neither necessary nor desirable. On the contrary, it should be stressed that the inorganic (or cosmic), organic, and human evolutions are parts of a single inclusive evolutionary development. This development does not flow at a uniform rate; from time to time it involves apparent breaks of the continuity, giving rise to something radically new. The two transcendences mentioned above are, at least on earth, the major ones in the over-all evolutionary development. In stressing the breaks of the evolutionary continuity, it should also be made clear that, in Holloway's words, "a literal overnight or single-generation propulsion of apehood to manhood" is not involved. Quantum evolution is rapid on geological time scale but certainly not instantaneous. Furthermore, like any other major evolutionary change, it can only occur by compounding a novel system from genetic elements which existed, or were gradually formed, before the quantum change took place. I must therefore disagree with Holloway's statement that "We are not learning about human behavior or human evolution from primate studies." These studies may help us to locate the building blocks from which the uniquely human adaptive system has been compounded.

by WALTER GRAF☆

Vienna, Austria. 11 II 69

Holloway has made an interesting interpretation of stone tools as "the singular repository of any clues to behavior of a discontinuous sort," arguing that whether or not a phenomenon is " 'protocultural' or 'cultural' will depend, not upon . . . brain sizes, reconstructions of ecology, or logical imperatives from a constraining framework of stages (although these can be supporting arguments), but on the analysis of artifacts." But his argument "that both tool-making and language come out of the same cognitive structure" and his discussion of "two attributes of human existence . . . arbitrary form and imposition" perhaps need some supplementation.

Both tool-making and language are phenomena that developed gradually over a very long period of time. Concerning language and its sound-symbols, many theories have been advanced by phoneticians, linguists, psychologists, biologists, and so on. For example, Werner (1959) concludes that thinking and experience for the beginning of onto- and phylogenetic development was complex and not specific. Stopa (1966) holds that speech in the beginning was very complex, both in its sound-symbols, especially their phonetics, and in its structure (e.g., one-word sentences). On the other hand, it has been argued (Roger 1943) that *Pithecanthropus* did not have a convolution of Broca and that Neandertal man had only a rudimentary one.

However this may be, it seems to me that in comparing the structure of language with the structure of tool-making their development must be taken into account. Is it not remarkable that tool-making, practiced for some hundred thousand years, produced only a small number of really different specific forms? Certainly, we are dependent on the remains, and, on the other hand, man produced especially what he wanted, the way he wanted it. Nevertheless, it took a very long time to get from eoliths to handaxes to flakes, and so on, especially in regard to the basically different forms. Here Holloway's approach would suggest a rough dating of the psychological process underlying the tool-making as well. Both these approaches depend upon inferences from known (recent) psychological or manufacturing processes.

by MARY W. HELMS☆

Chicago, Ill., U.S.A. 7 II 69

Holloway's thoughtful essay redirects our attention most persuasively to a major issue in human evolution and our understanding thereof. Although I find the major arguments compelling, I cannot help but wonder to what extent Holloway's analysis tells us more about the manner in which an erudite member of modern *Homo sapiens* views the perceptive abilities of early man than about the actual nature of early man's awareness. In other words, is all that appears to Holloway as non-iconic behavior in early man actually non-iconic, or could some of the material which he offers as evidence for rule-standardization represent lack of rule-standardization as well? When *Australopithecus* or *Pithecanthropus* addressed himself to a rough chunk of stone in order to fashion a tool, might his perception and thus his techniques not have been more impeded by the properties of stone than would be those of modern man, whose increased non-iconic perception sees fewer hindrances and more alternatives for shaping it? To the extent that Holloway's approach reflects more than methodology and becomes an actual description of early man's cognitive processes, due weight should be given to our unavoidably retrospective viewpoint.

by A. W. R. MCCRAE☆

Entebbe, Uganda. 21 I 69

Anthropology is a pseudoscience with a pseudolanguage, and is bristling with pseudoproblems. Holloway appears to be wrestling with one such pseudoproblem, the demarcation of intellectual territory.

If one is discussing human culture, most arguments obviously fall well within the scope of straightforward anthropology. But if one is discussing the *origins* of

human culture, as this article seems to be, one enters a field where speculation and controversy are rife. Much of this controversy may be pseudo-, that is, due primarily to differences in technical language. When jargon has been reduced to a minimum we may realize that such attempts at demarcation are futile, because what we are really considering is a continuum. I find it hard to believe that a worker possessing the amount of erudition so clearly displayed by Holloway can have missed this point, yet the kind of thinking he presents went out with the search for the Missing Link. The kindest interpretation which I may therefore put to this article is that the writer's common sense has been overcome by pressures to appear impressive through sheer verbiage.

To raise two specific points: I cannot agree that tools are to be solely "viewed from the perspective of social psychological frameworks." Tools are functional objects. They may be viewed as physical extensions of the user (or group of users), and will in this sense be adaptive organs, falling within the scope of interpretation by evolutionary theory. Tools cannot be regarded *only* as psychological symbols; if this is only partly so, then Holloway's thesis is pointless.

Secondly, CURRENT ANTHROPOLOGY appears to be a journal which permits the word "analysis" to mean "discussion." This appears to be indicative of a tendency to upgrade the significance of words in the sociological sphere so that jargon comes to mean "science."

The basic fallacy in Holloway's thinking seems to be that he considers different conclusions reached from the same data to be mutually exclusive. This, in the soft sciences, is far from being always so. It is up to intelligent workers to resolve these apparent contradictions, not to evade them by erecting barriers between disciplines.

by JOSEF WOLF☆

Prague, Czechoslovakia. 12 II 69

Holloway's study is remarkable; yet I cannot help feeling sceptical about some of his conclusions. As an anthropologist, I can hardly agree that "culture . . . is also the imposition of arbitrary form upon the environment." Unlike Holloway I see culture not as the single defining characteristic of man, but as a complex of characteristics, including not only man's use and making of tools and his speaking and thinking, but also the structure and function of family, clan, ethnic group, and society, their behavior and all the relations between their individual members, etc. From this point of view, it is important to have not only a concept of man, but also a concept of culture and of the relations between the two. That is the reason why man is no longer studied only as a biological individual but as a social being in a certain cultural environment, and why he is considered not only as an individual or a type but also as part of a certain human race, nation, and social formation.

Thus contemporary anthropology must deal not only with the problems of man, but also with the problem of the concept of anthropology itself in its delimination as a science of man.

In the attempt to solve the problems of man, complicated and unique as they are, there is the danger of adopting an extreme point of view—either a purely zoological one, or a purely philosophical one, or one which sees man as isolated from the rest of the nature and from his social and cultural environment (which involves the common errors of anthropocentrism and anthropomorphism).

From the anthropological point of view, man is indeed exceptional, for he can walk, crawl, swim, run, and live on a wide variety of foods. From the morphological point of view, he stands in contrast to all other mammals, and from the point of view of his cerebral activities, to the whole of living nature.

In contemporary anthropology, *Homo sapiens* is therefore characterized not only as an animal producing and using tools, but also and primarily as a cultured and speaking animal and a social and moral being. Of course, as to the precise phylogenetic and taxonomic classification of man in nature, he is only one of the most highly developed and most highly perfected animal species.

Most of the skeletal discoveries of man's ancestors are found with remains of culture, the results of human work. Culture forms an indivisible part of human evolution, and this is even more so in contemporary ethnic groups and societies. There has never been man without culture, and culture came into being together with man as a biological phenomenon. These are convincing reasons for the anthropologist to turn his special attention to the study of man and culture, their mutual relations and proportions.

The fundamental tasks in contemporary cultural and social anthropology are: (*a*) to explain the character of man and human societies; (*b*) to ascertain whether there exists a parallel development of culture and of man as a living being, or whether the cultural evolution of man depends on the biological evolution of man as the species *H. sapiens*; and (*c*) to contribute effectively to the development of man, as an individual and in society. Anthropology does not yet have the solutions to these problems. Instead, we find different concepts of culture and man and various ideas as to the ways these problems might be solved.

Holloway's experiment, in my opinion, is only one of many contributions to the solution of the whole complex of basic problems in anthropology. Man is neither a perfect machine nor simply a biological being. Each individual has his own behavior, character and experience—each is unique and never to be repeated; but only relations between people, cultural and social life beyond the bounds of individuality, enable man to express himself as a cultural and social being. In the near future, the study of man will be influenced by the broad distribution of information from all over the world that technical development will soon permit. This will allow man to acquire a new skill—to become an expert in human relations in the middle of cultural and social changes in his society.

Changes in human relations and human behavior are going to be the centre of interest in contemporary anthropology—the science of man and his activities in nature and society, someday to be a science preserving and perfecting mankind.

Reply

by R. L. HOLLOWAY

I have found the comments disappointing in that only those of Benoist, Helms, and McCrae raise truly pertinent points for argument. Dobzhansky's comments are all well appreciated, but I do not see that they shed any light on the problem I have raised. My statement regarding learning about human behavior and evolution from primate studies seems his major point of disagreement. I believe that primate studies undertaken in the context of mammalian behavior in general have been extremely valuable contributions to our knowledge; I said so in my article. But I also submit that much of the writing on primate behavior, and particularly the most popular of such works, has been directed by fairly long involvement with the problems of *human* evolution. That is, the problems of primates are often seen through the colored lenses of human behavior and evolution. It remains to be seen whether we can validly assume that present-day primate behavioral patterns were in fact present 10–30,000,000 years ago. These latter studies may indeed help us—it would be insane to argue otherwise—

but it will be by providing ideas that can be tested against the fossil hominid and archaeological evidence. The "building blocks" to which Dobzhansky refers (but which he does not describe) probably antedate Darwin, and as far as I can tell many of the terms (e.g., dominance, binding, bonding, sociality, etc.) were understood in the context of adaptation prior to primate studies. Many studies of mammalian behavior in general have offered as much food for thought as primate studies, and have provided frameworks for the adaptive nature of social behavioral processes upon which primate studies have offered few if any genuinely useful new specifics. It seems to me that there are two types of primate studies, or two sets of writing about primate behavior: (1) those of field-workers, in which the behavior is coldly described and measured, and (2) those of people trying to prove how much we have learned about human behavior and evolution from the study of primates. The first kind is usually of excellent quality and cannot be seen as anything but a tremendous addition to our knowledge. The second type is often nonsense and simple bandwagoning.

Wolf has apparently been misled by what I thought would be an obvious reference to Tylor's classic definition of culture. I do not see culture as *only* "the imposition of arbitrary form upon the environment." We do not disagree about culture and its complexities. Certainly, we do disagree very significantly as to the basis of man's exceptional status, which Wolf sees in his morphological attributes. Man differs from other animals, of course, but he is hardly in "contrast to all other mammals" with regard to eating, locomotion, or cerebral activities.

Graf's comments lead me to try once again to clarify one of my points: I did not mean that stone tools were the *only* clues to behavior, continuous or discontinuous (whatever these rubrics mean). Every piece of bone, artifact, or context is a clue to behavior. What I meant was that if we are to wrestle with the problem of whether this or that hominid had culture, we must consider stone tools as the singular repository of clues as to whether or not behavior was standardized through some agency other than genetically programmed behavior or mimic learning.

Bowen's comments are more to the point. He sees no difference, from the point of view of the imposition of arbitrary form, between stone tools and the chimpanzee's sticks. Perhaps the problem is that he is trying to compare the unaltered sticks with the unaltered pebbles. I wouldn't know how to compare these, and that is not my concern. Nor am I concerned with the ways in which a stick tool differs from a pebble tool. My concern is to compare the *process* whereby the stick is made into a tool with the process whereby the pebble is made into a tool. Bowen's items (1), (2), and (4) do not strike me as important in dealing with the *process* of standarization of form, but (3), selectivity, is, and in the latter sections of the article I tried to show why it was important in the evolution of adaptive social behavior. In short, Bowen is concentrating on the materials, and I am concentrating on the processes. Obviously, stone tool-making reflects "more complex cognitive patterns," but the question remains, "Is there a difference beyond degree of complexity (should we be able to measure the latter)?" Bowen sees the emergence of human cognitive behavior "long before the time of the australopithecines." In what does he "see" this cognitive behavior? Full bipedalism. I would hardly argue that bipedalism and increasing cranial capacity are not clues to behavior, but I find it impossible to use these bits of evidence to argue about cognitive capacities. I fail to see how full bipedalism, increased fine manipulation, and alteration of natural materials are "relatively discontinuous."

Mary Helms's concern as to the extent to which our modern perspective may bias our view of early man is an important one. I wish I knew the answer. Perhaps it is to be found partly in her second point —the extent to which the perceptions and techniques of early man may have been impeded by the properties of stone. As I indicated in my article, there must be an element of determination in the physical properties of stone. A wide variety of different stone types was used for the manufacture of tools, not all of them with exactly the same physical properties relating to fracturing. These differences do not alter the fact that there is considerable standardization of form in the cultural remains of *H. erectus* if not *Australopithecus*. Certainly, hominid skills did increase with time; but already by the time of *erectus* and perhaps earlier, standardization of form had been achieved in different tools made from stone with slightly different properties.

I would answer Benoist's interesting questions as follows: yes, the products are invariant, but not the beginning unaltered structures. We cannot simply think in terms of reduction or accretion of pieces, but rather in terms of change in the structure of the objects. I still argue that iconicity is higher in the bird's nest (in addition, its form is more determined by genetics than are the forms of stone tools) than in, for example, a Levallois flake. As to his final question, I must answer "no": the essential difference is arbitrariness, that is, absence of any necessary relationship between the original and the final forms.

Perhaps McCrae is right about anthropology. But until he shows exactly how anthropology is a pseudoscience . . . bristling with pseudo-this-and-that, he has simply raised his own pseudoproblem. For his information, the demarcation of intellectual territory was not my problem —rather, it is his. Actually, I was trying a bit of integration, and the problem I was concerned with was whether stone tools, and the processes behind them, raised anything worthy of discussion relative to the nature of man and his adaptation. The kindest interpretation I can make of McCrae's response is that he becomes emotionally disturbed when he encounters "intellectual territory" he hasn't traversed before. To overcome this pressure (to borrow a turn of phrase), he turns to insult and blatant distortion to clarify his own pseudoproblems.

I ask him to show me where I said that "tools are to be solely viewed from the perspective of social psychological frameworks" *in the sense that that was their only importance*. We all know tools were important in human adaptation. There is no need to state it again and again, or refer to them as "adaptive organs," which is simple nonsense. What else do they mean? That is the question I tried to answer—correctly or not. Also, where did I say that tools can be regarded *only* as psychological symbols? I don't understand this distortion. Whether McCrae likes it or not, tools were "adaptive organs" only if used in certain ways and the processes of manufacture learned and passed on through the generations. All of that involved, sorry to say, psychological processes, and that was the problem I was concerned with—not the rest of McCrae's hyperbole.

My paper offers an "analysis," whether correct or invalid. McCrae's response is a discussion based on emotion, filled with distortion, and therefore fallacious. Any further discussion of it would be a waste of time.

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