Constraints on a Theory of Hominid Tool-Making Behavior

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Introduction

There is a widely held and curious tradition which maintains that the organs of the body are essentially genetically determined while the vastly differing capacities of the human mind are somehow shaped by environmental circumstance. This curious belief in the simplicity of the mind complements an equally puzzling account of the brain's evolution. According to Leakey and Lewin, for instance,

"the evolutionary trend that culminates in the human mind has been a simple, well-ordered and biologically economical one [. . .] The secret of the human mind is that rather than having the ability to learn certain variants of specific tasks or behavior patterns, it simply has the ability to learn" (Leakey & Lewin 1977: 190-192).

An anthropologist from another planet might well consider such views to be unduly anthropocentric. Why, for example, can human babies tested shortly after birth spontaneously make arbitrary categorical discriminations along an acoustic continuum which just so happens to correspond to human phonetic disjunctions (Eimas & Corbit 1973), while Rhesus monkeys (Morse & Snowden 1975) or chinchillas (Kuhl & Miller 1975), whose inner ear structure resembles man's, cannot.1 Perhaps for much the same reason that squirrel monkeys, but

1. Rhesus monkeys can apparently be made to discriminate the voiced-voiceless distinction between /t/ and /d/, while chinchillas seem to be able to discriminate a sustained /a/ from /i/; however, only insofar as some principle is fully and coherently integrated into a well-formed system, the whole of which can be tested with respect to a significant range of phenomena, does it become a principle of any scientific interest. To take any such principle in isolation and "demonstrate" its presence in some other cognitive domain or in some other species, whether in spontaneous behavior or through conditioning, coaxing or evoking, is to demonstrate nothing at all (cf. Atran 1980: 226-227).

probably not humans, recognize squirrel monkey hands from extremely degenerate information, or that humans, but probably not chimpanzees, recognize human faces from very poor exposures to fragmentary data (see comments by Lucas Teuber and Susan Carey, Symposium 1974), human brains are just built differently than the brains of other creatures. Talk of relative simplicity with respect to distinctly specific neurophysiological structures seems hopelessly misleading. Why should there be less innate structure (as the notion of simplicity implies) in the human language faculty or those parts of the brain that are involved with the production of artifacts, than in the retina or the small toe? Why, in other words, should we expect that the few limited years of a lifetime which are scattered through experience be responsible for such rich structures, rather than concede that these structures are largely determined at birth as the complex product of millions of years of evolution? The only conceivable reason for holding a view which has no evident support from neurophysiology is that such a view would provide a phylogenetic excuse for maintaining an equally unsubstantiated belief in some general human “logic”, “classification” or “intelligence”.

Despite the popularity of the idea of “general intelligence”, I doubt whether anything empirically significant is implied by the phylogenetic notion of “adaptation” which supposedly produced it. Nor do I think there is much to the ontogenetic notion of “learning” which is then supposed to account for the mind’s vastly different inferential schema. To say that a learning theory holds across modes of cognition is tantamount to saying that a growth theory holds across bodily organs. True, a reduction is no doubt possible at the level of cellular biology, but this is of no apparent consequence at the level of the structure and functioning of different organs; for example, it is quite implausible that the visual structures of the cortex governing facial recognition belong to the same set of logical models as other neuron systems which determine an artisan’s judgement of shape and perspective, or which determine language, sensory-motorcoordination, mathematics or an appreciation of melody.

Consider the case of stone tools: the species-specific geometric and Gestalt elements which apparently govern lithic production indicate a well-structured figurative space (which represents the form of the object) and topological order (which represents the relative position of geometric entities, independent of their form and size). The capacity to manufacture such tools implies a continuous projection of figures in space and a superposition of transitive operations on these projections which translates the continuity serially, into a temporal space. None of these operations, however, seems to bear the slightest resemblance to the structures of subrecursive hierarchical functions which seem to be responsible for the organization and generative capacity of language, though the contrary is implied in any claim which links linguistic competence to lithic manufacture. Of course, a weaker argument could be made for the connection between language and tool-
making, namely, that the fabrication of tools and the production of language are both "creative". But this reduces to the rather banal point that cognition is far underdetermined by experience; that is to say, cognitive structures of all sorts are able to take an instance of experience and "automatically" predict its extension to an infinite set of complexly related experiences. Such a "creative capacity", which includes schema as diverse as those corresponding to the visual configurations of cats or monkey play, contains too little of substance to warrant interest.

Still, many authors persist in using equally dubious kinds of analogy. Thus, Marshack argues on the basis of "zigzag" lines found engraved in several Mousterian examples of stonework, and on a single Acheulean artifact, that Neanderthal likely had, and Homo erectus may have had, a decidedly human linguistic competence:

"The symbolic artifacts document a cognitive capacity and competence for abstraction, modeling, and manufacture of a different order than that which can be deduced from the [mere] subsistence of tool industries. Although this competence involved a use of hands and tools, it is probably allied to linguistic competence rather than to tool-making, in both an evolutionary sense and in contemporaneous Mousterian usage" (Marshack 1976: 305).

Just how is the competence involved in "a use of hands and tools" allied to linguistic competence?

"A hand can gesture 'snake' and indicate size and direction [. . .] The hand, then, would be involved in what be termed description or modeling with a tendency toward [. . .] 'verbing' or indication of action or process" (ibid.: 309).

It is patently clear that no testable claim of substance is being made here; to confound gestures of action or process with the abstract syntactic category "Verb" would be like saying that because animals, persons, places and things can be the subjects of sentences, it follows that such beings and objects must share significant cognitive features with the grammatical function "Subject of". This is wholly untenable.

For Montagu, tool-making alone suffices to establish linguistic competence. Indeed, human language (of some sort) must have accompanied the earliest tool-making, "well antedating the tool-making of early Oldowans":

"It has been observed by more than one writer on the subject that any description of the processes involved in language or speech could also be employed to describe the process of tool-making, that there exists, in short, a grammar of tool-making in much the same sense that there exists a grammar of language and speech" (Montagu 1976: 269).
This common grammar apparently consists of "certain formal rules relating to the modification, transformation, and positioning of certain stylized chopped-up segments of sound", which "are exactly the kinds of processes that occur in tool-making". And, I might add, in bird songs, frog signaling, etc. But we are informed that at some unspecified, "more detailed level of analysis the parallel between language and tool-making becomes even more striking, but although several authors on the subject have seen this, no one, so far as I know, seems to have made a detailed study of tool-making as a clue to the cognitive processes of early men and the origin and evolution of language and speech. What is needed is a scientific study of tools, a science, as it may be called, of hoplonology (Gr. Οπλον, a tool, implement). The hope is that by giving such a science-to-be a name, it may be conjured into existence" (ibid.: 269-279).

Unfortunately, such a science is virtually guaranteed to remain little more than mere conjuring as long as it is tied to the groundless presupposition that tool-making is, or should be, a guide to the nature of language.

Similarly for Kitahara-Frisch (1980), who works on the assumption that the cognitive system governing cultural forms as a whole does not differ fundamentally from language, early Paleolithic industries may be expected to reflect the earliest manifestations of a capacity for language and symbolic behavior. To cite one example, "both tool-making and language call for a capacity to conceptually relate discrete times and places" (ibid.: 217). But then so does the proven ability of chimpanzees to build picture models of causal paradigms (Premack 1976). Such evidence hardly warrants the claim that "it can reasonably be assumed that to the development of technological skills corresponded a parallel development of linguistic and symbolic competences."

Guilmet even goes so far as to imply that an evolving "technical capacity of mind" ultimately connects tool-use in non-human primates to human language:

"Two other laboratory studies of nonhuman primates demonstrated how the opportunity to play with sticks enabled tool-use for problem-solving [ . . . ]. It has even been suggested that the hominid abilities to manipulate objects in the sphere of action and symbols in language originate from the same neural structures. Holloway has argued that speech and tool-making can both be characterized as the imposition of arbitrary form upon the environment, thus those skills must have evolved concordantly. 'Selection favored the cognitive structures dependent on brain organisation and social structure which resulted in both language and tool-making' (Holloway 1969: 404). Bruner (1972) has stated that the simultaneous appearance of language and tool-using in human children during development suggests that the two may derive from some common programming capacities of the larger hominid nervous system" (Guilmet 1977: 44).
Now consider the chain of this argument one link at a time. First, the notion of “problem-solving” in non-hominid tool-use has little to do with a vast array of different problem-solving capacities which are present in humans. The notion of “problem-solving” itself is as vague in this context as the notion of “intelligence”: for instance, what non-trivial, testable relationship holds between solving a spatial access problem, a semantic puzzle and a problem of facial recognition? Second, even if one could formulate a substantial sense to the notion of “imposition of arbitrary form on the environment” which goes beyond a mere paraphrase of the empty notion of “creative capacity”, there is no reason whatsoever to assume, a priori, that because two cognitive domains possess such a feature they must “originate from the same neural structures”. Thus, “generative capacity”, in the meaningful sense of being able to produce indefinitely many well-structured formulations from extremely limited means, seems to be a property of facial recognition as well as syntactic structure. But what responsible neurophysiologist would just assume they share a common neural program? Third, not only is the selection process entirely speculative, but what is selected, i.e. cognitive structure, is entirely unknown in terms of a theory which deduces empirically testable consequences. Fourth, the claim that because tool-making and language use are ontogenetically contemporaneous they therefore share common “programming capacities” is equally tendentious: teeth also appear at the time language use does but that does not imply that dentition and linguistic competence are linked. Little is yet known about what goes on when billions of neurons are crammed together in an area the size of a basketball, and speculations of this kind are not likely to improve matters. Certainly no respectable biologist would conjecture about the genetic relationships among organs whose structures had yet to be described and about which he therefore knew nothing. It is only after hypotheses for one cognitive domain have been formulated in a reasonable fashion to be

2. For Bruner, it appears that the simultaneous triggering of two seemingly very different cognitive faculties is taken as a priori evidence that the two faculties must therefore be structurally linked together. A similar case of unwarranted leap from simple correlation to cause occurs in Piagetian theory. The mere appearance of one cognitive faculty in the course of individual development is taken as sufficient proof of its being a necessary condition for the appearance of any faculty which follows in time. For example, “Firstly, we need to explain why language appears at one developmental stage and not another; it is no accident that language acquisition in the child occurs after sensori-motor intelligence has reached its final stage” (Piaget & Inhelder 1969: 156). From this simple fact of succession it is concluded that “the progressive and systematic coordinations of sensori-motor schema play an important part in the formation of language”. Consequently, “there is no need to suppose innate linguistic programming, since self-regulations are sufficient to explain the development of intelligence and its influence over language”. Not only is there no apparent structural similarity between linguistic and sensory-motor behavior, however, but the fact that children who suffer from sensory-motor defects (e.g., the deaf, paraplegics, the blind) achieve perfect grammatical knowledge of their language, just as “normal” children do, indicates that there is no testable connection between the two domains of behavioral performance.
empirically tested that they may be compared to the hypotheses proposed for another structured domain. If, subsequently, it should turn out that certain hypotheses are generalizable across a variety of cognitive domains, that would constitute a significant and surprising empirical discovery which is not based upon vacuous analogies.

It appears, then, that untested beliefs in the structural similarity of tool-making and linguistic behaviors, when made explicit, prove to be either demonstrably false or trivially true; how to avoid this, the following pages are intended to show. The constraints proposed here on theory formulation are thus meant to preclude empty theories; but more importantly, they are designed to prevent theories of substance from devolving into false theories or theories which are suspect by virtue of their unwarranted extension to other domains. My aim is not, therefore, to infer a level of cognitive ability from early tool types, to seek out the earliest evidence for specifically human cognition or to provide an analysis of hominid cognitive capacity. There is, however, an attempt to advance understanding of human nature, though not in the positive sense of producing a novel theory or adding fresh evidence to test, and choose among, extant theories; rather, in elucidating what such a theory should not be, I hope to ease the task of understanding current theories and thereby facilitate the search for better ones.

I. — Constraints

If we hope to gain some insight into the nature of the human mind we should turn to areas where the techniques of experimentation are moderately rich and where there already exists some non-trivial results about the cognitive systems involved. Presumably, neurophysiologists could then go on to study the physical foundations of those systems.

N. Chomsky,

The description of mental structures underlying lithic production must meet the requirements of any psychological theory. This entails the simultaneous search for (i) techniques of empirical corroboration, (ii) abstract models of those specific cognitive systems involved, and (iii) theories of underlying biological systems in the cortex.

(i) Analysis of debitage, remontage and experimental knapping, together with a knowledge of hominid anatomy, offer an indirect but rich set of empirical techniques for the reconstruction of the non-arbitrary behavior associated with lithic production. The problem is that most experimental and empirical techniques
have been used in conjunction with attempts to reconstruct type-lists for the purpose of classifying lithic assemblages. Type-lists, however, are inadequate for inquiry into underlying cognitive schema. For example, in Tixier's (1963) classification of an Epi-Paleolithic industry in the Maghreb, five types of burin are listed under the category *burin sur troncation*. The first four listings take notice of the truncated form (concave, convex, rectilinear, oblique). The last type opposes simple (one burin chute) to multiple (more than one burin chute) and is essentially a matter of technique.

Type divisions may thus consist of a mixture of morphological, technological and functional criteria which only confuse attempts to isolate underlying mental structure (as would, for instance, an attempt to abstract a grammar from a list of sentences, some of which are classified by phonology, some by syntax or semantics, and some by what the sentence refers to or what it is used for). Clearly, then, experimental and empirical techniques are only useful in the service of some detailed and specific theory which could isolate the various cognitive factors involved; under responsible idealization of the kind appropriate to any science, this would mean ignoring, say, questions of function when dealing with technique, and concentrating on the purely figurative results.

(ii) In an article which outlines a typically “behavioral” approach to archaeology, Sheets (1975) states: “Archeologists, in their artifact classifications, should be attempting to codify the phenomenal [i.e. behavioral] order rather than the ideational order.” Sheets identifies the “phenomenal” order with the “taxa constructed by the archeologist”, which allegedly deals with the “etic” phenomena of culture “from without”. The “ideational” order is equated with “ancient mental taxa”, which pertain to the “emic” phenomena of culture “from within”. Since there is no way of “testing” (in the sense of positively verifying rather than falsifying) the correspondence between the two orders, anything other than a statistical analysis of the former would be “spurious”. Sheets follows Harris (1975) here in equating emic with what psychologists call “competence”—what people know—and etic with “performance”—what people do. He claims, as does Harris, that an understanding of the latter is both logically and heuristically prior to an understanding of the former. Indeed, a complete understanding of native competence is supposedly impossible because “we cannot assume that all members of a given culture share the same degree of consciousness in the formulation of a given category”.

This view I take to result from a mistaken conception both of the nature of the mind and of science. One cannot study performance (i.e. behavior) directly any more than a physicist can make sense of measurement without a theory of physics. To study performance, that is, the theory of how we put to use what we know in what we do, one must first study competence, that is, what we know.
Such theories are far underdetermined by the behaviors they describe. They have a complex relationship to the physical stimulus which is mediated by abstract schema physically rooted in the brain, and which operate upon abstract representations of behavior and not directly on the stimulus itself. The sensory-motor, geometric, Gestalt and classificatory structures which are involved in the production of stone tools refer, therefore, to abstract theories, unconsciously produced by the cerebral cortex of the lithic artisan, but consciously intuited by the anthropologist.  

Admittedly, people's performances may vary greatly, often seemingly indiscriminately (e.g. predicting the effects of fatigue and motivation on the proper production of a tool or a sentence may be practically impossible). Accordingly, the task of the psychologist and the anthropologist is to first abstract various schema of competence and then go on to study the ways in which these schema couple to form schema of performance. There are undoubtedly severe genetic constraints on such couplings which make it easier to analyze some kinds of performance than would be the case if these couplings were determined exclusively by the environment, as Harris's brand of materialism assumes.

The observational “taxa of the archeologist” represent an epistemological hodge-podge of attributes which may reflect the archaeologist's implicit assumptions about gross morphology, function, technique and so on. This may yield an observationally adequate classification of a lithic assemblage but it cannot provide an explanation, or principled interpretation, of tool manufacture; for, “it is the descriptivist limitation in principle to classification and organisation of data, to ‘extracting patterns’ [ . . ] or ‘habit structures’, insofar as these may exist, etc., that precludes the development of a theory of actual performance” (Chomsky 1965: 15).

Furthermore, there is no reason why conscious appreciation of these schema

3. As an epistemology, rationalism has a significant advantage over empiricism in supposing that mental entities cause behavior rather than the other way around. This could explain how knowledge of other minds existing in radically different environments is possible and would allow the mind to be studied by ordinary scientific methods of theoretical conjecture and experimental refutation. Theoretical arguments from behavioral effects to mental causes would always be non-demonstrative (i.e., scientists, unlike mathematicians, cannot “prove” their arguments), though predictions (or retrodictions) from causes to effects could be tested against experience. It won't do to claim that mental causes are really but probabilistic “dispositions” to behave in a certain way, as logical behavioralists contend; for, if the assumptions we usually make about mind-body interactions are true, they demand that the mental cause of a form of behavior should be an event ontologically distinct from, and independent of, the behavior that it causes. This is a way of saying that when a relation is causal it is also contingent—that any effect might have had a cause other than the cause it did have. But if it is held that the mental is a “logical construct” of the behavioral (as the notion of “disposition” implies), then what is denied is the ontological independence of behavioral and mental events. It is, however, precisely the assumption that mental and behavioral phenomena are distinct states of affairs that is required to give a coherent sense to the notion that one causes the other.
should have any bearing at all on the problem, at least on the native's part. It is the native's unconscious capacity to acquire tool-making or language skills that is most remarkable and worthy of serious attention, since these are precisely the skills which seem most "natural". Of course, the scientist's ability to consciously construct theories about the nature and development of faculties which are so easily and spontaneously acquired by children may be constrained to rather simple accounts by the (largely innately determined) limits of introspection and rational inquiry into such matters—but this we shall just have to live with.

(iii) One problem with building theories of hominid tool-making behavior is that one is still unclear as to what type of hominid behavior belonged to which type of brain. For the earliest Oldowan industries, Mary Leakey (1971: 280) claims that "on the evidence available now" it is likely that *Homo habilis* was the tool-maker while *Australopithecus boisei* was the tool-user. The available evidence, however, is little more than the fact that *Homo habilis* had a larger brain than *Australopithecus robustus*. Such evidence does not, in itself, establish that the smaller-brained creatures were not tool-makers as well. Evidence from Sterkfontein and Swartkrans (SK 847 and *A. capensis*) suggests that *Homo habilis* was responsible for the Developed Oldowan industries, but there are little or no cranial finds of any significance. The absence of direct evidence connecting cranial to lithic finds in the same archaeological layer also hampers attempts to link *Homo erectus* to the Acheulean industries of North Africa.

Moreover, a clear association of one kind of behavior with a specific type of hominid would not get us very far, since the endocranial evidence for neural reorganization is likely to remain fragmentary and poor. According to Holloway (1968, 1975), there is little hard endocranial evidence for extinct hominid species for a variety of reasons: specimens of endocasts are rarely complete, unbroken and undistorted; the internal table of the cranial bone is often exfoliative; large brain specimens provide little sulcal and gyral definition and there is a high variability in the degree to which representations of cortical morphology are impressed with overlying dural membranes into the bones of the skull; and sample sizes of populations are too small to give any indication of variability. In the absence of any direct knowledge which links hominid neural reorganization to specific sorts of behavior, supplementary evidence must be taken from comparative study of human beings and non-hominid primates. Making sense of such evidence, however, is by no means a clear and easy matter (Holloway 1968).

Another disturbing factor is that the manner of combination of various mental properties could preclude absolutely isolating the manifestation of any particular property in actual performance. Thus, an attempt to distinguish figurative operations which are purely geometric from those which involve Gestalt notions may prove to be a difficult task; to interpret a square, for instance, we must take
into account the figure-ground relationships in order to process its geometric interpretation as four lines of equal lengths, connected at right angles to enclose a space. Nevertheless, it may be possible to localize Gestalt and geometric processing in different parts of the brain. According to Bever (1975), the right hemisphere seems more disposed to a global processing of stimuli in terms of “holistic” behavioral templates whereas the left hemisphere appears to be favored for the more difficult task of “analytically” processing stimuli in terms of component parts, which includes recognition of the whole as well; apparently, we can listen to a melody or a syllable, for example, in terms of the individual acoustic intervals or in terms of overall acoustic contour, and we can analyze geometric figures and faces in terms of constituent notions of angle, line and boundary or as “Gestalts”.

This minimal bit of evidence for the neurophysiological foundations of cognitive structures, however, is far from sufficient to constrain theory formation for particular cognitive domains, though it is apparently enough for those who believe in a general intellectual capacity which underlies all cognitive domains. So it is for the psycholinguist Bresson (1976), who extrapolates a general intellectual schema from findings that the left hemisphere operates in a “digital” fashion while the right hemisphere operates “analogically”. The schema of general intelligence simply consists of a holistic-figurative “aspect” located in the right hemisphere and an operational-analytic “aspect” located in the left hemisphere. Cerebral asymmetry would thus “account for” syntactic structure and, by extension, language in general as the product of the part-whole processor. A similar argument would then apply to the fact that a chimpanzee cannot rotate and peel an apple at the same time and, by extension, could not use a tool to make a tool not develop syntactic structures because its brain lacks cerebral asymmetry. Still another argument for a biological foundation to general intelligence takes note of the increase in the posterior parietal and temporal regions of the neocortex which are strongly correlated with object-naming and language in general (Leroi-Gourhan 1964).

Recently, however, LeMay (1976) has questioned the thesis that cerebral asymmetry in primates is to be found exclusively among hominids (see also Groves & Humphrey 1974); while Passingham (1973) contends that there is no clear-cut difference between human and non-human primates with respect to the proportion of association area present in the cortex—the expansion of the temporoparietal association areas at the expense of the visual areas of the occipital lobe may simply owe to the fact that relative sizes of areas within the neocortex change as a function of the total size of the neocortex (this could account for why man could have less striate cortex relative to body weight, more associative cortex relative to total neocortex, and more prestriate cortex relative to occipital cortex). What is more important, even if it could be shown that different cognitive domains
are ultimately rooted in the same cortical structures, these domains may have different functional bases in the cortex.\footnote{4}

Little, indeed, is known about the physiological foundations of cognition. What little we do know of the cognitive functions of the brain comes largely second-hand, from doctors and neurophysiologists who chance upon cerebral aspects of cognition in the course of their medical inquiries into the effects of bullet wounds to the brain, lesions, etc. They fail to test interesting theories of cognition because there are few competent theories around to test. What theories there are usually attempt to link behavioral performance directly to brain structure. Apart from the fact that brain structure is barely known, these accounts ignore the fundamental question of how brains could choose among behavioral options in the first place—and choose they must if tool-making is indeed the non-instinctual, non-arbitrary behavior it is supposed to be (Balout 1954). But in order to choose a behavior hominids would first have to be able to represent that behavior in an abstract way; for, if there is no representation, then there is no way for a hominid to model behavioral options and select his course of action. Thus, what is paramount in any theory of hominid tool-making behavior is not the structure of the behavior itself nor the necessary physiological foundations for that behavior; rather, what is needed is an abstract theory of cognitive representation for specific domains which can eventually be tested against the behavioral evidence at hand and which could ultimately provide logical conditions on the kinds of cerebral systems neurophysiologists are supposed to be looking for. Just what such a theory of cognitive representation entails, is the subject of the next section.

\footnote{4} This view may appear to be dualistic in the sense that it distinguishes the structural operations of the "brain" from the functional operations of the "mind", that different psychologies could have different physiologies or that similar psychologies could have different physiologies; however, an overall physical description of mental properties could still be achieved which could distinguish cognitive structures with essentially similar physiologies. The relative autonomy and localization of these higher order, cognitive functions would result from their segregation in the brain according to similarity of function, and according to the programming of particular brain structures for that kind of function. Mental state types (mental properties) would be thus individuated by reference to the role their tokens (individual processes, events, etc.) play in the stream of activity which mediates the relation between the organism's stimulus and response. In that case, a mental type would be a functional property while a token mental entity would be a particular neurological state. One would go about characterizing a mental property by appealing to any typical cause or effect of its tokens, that is, to their mental causes and effects as well as to their connections with stimuli and responses.
II. — The Object of Study

At least one prominent school of psychology sees the development of human knowledge in an evolutionary perspective:

"The fundamental hypothesis of genetic epistemology is that there is a parallelism between the progress made in the logical and rational organisation of knowledge and the corresponding formative psychological processes. Well, now, if that is our hypothesis, what will be our field of study? Of course, the most fruitful, most obvious field of study would be reconstituting human prehistory—the history of human thinking in prehistoric man. Unfortunately, we are not very well informed about the psychology of *Homo siniensis* of Teilhard de Chardin. Since this field of biogenesis is not available to us, we shall do as biologists do and turn to ontogenesis" (Piaget 1970a: 13).

According to Inhelder and Piaget (1969), two main sets of schema describe the performance of intelligence: the "figurative", which governs conceptions of shape, perspective, angle, line, boundary and so on, and the "operational", which treats problems of seriation and classification. The operational are supposedly dominant, being rooted in the sensory-motor mechanisms which create "a multitude of meanings, space-time patterns, permanent objects, causality, etc. which provide the sub-structure of verbal semantics". The differences between figurative and operational judgements are, in the last analysis, idiosyncratic; in other words, the various neural structures generating schema which may be responsible for all kinds of inference are merely variations on an essential "logic" of intellectual structure taken as a whole. Thus, the only apparent difference between, say, the ability to draw pictures and the ability to utter a sentence owes to the fact that they involve different modes of organizing perception; as far as intelligence is concerned, they are both variations of schema governing sensory-motor coordination, which is the only hereditary structure.

Leroi-Gourhan (1964) reasons that the manufacture of stone tools appears to provide an unmistakable and continuous repository of evidence for the formative psychological processes in hominid development. In an analysis in many ways similar to Piaget's, Leroi-Gourhan claims that the "operational syntax" of tool-making reflects the same general schematic properties of intelligence (chaînes opératoires) as the syntax of language. On this account, a study of the evolution of tool-making should yield directly relevant information about the corresponding evolution of language. To support this claim, Leroi-Gourhan notes that the development of the speech centers of the brain proceeds in proportion to the development of that part of the cortex governing motor movements. The fact that the neurophysiological mechanisms governing the phonological patterns of speech may have little to do with the syntactic, semantic or referring functions
of language is apparently ignored. Such a position precludes an understanding of the various component structures of a cognitive domain because it assumes, a priori, that they must be logically equivalent.5

Until recently, speculation into the theory of transformational grammar employed several classical algebraic models. These models made use of the notion of a “kernel”, which is a standard structure common to a class of homomorphic structures and is used in building up traditional algebraic structures (e.g. in Ring Theory) as well as structures of a more fundamental nature (e.g. Category Theory). Psychologists and anthropologists were quick to seize on the apparent success of these algebraic structures in syntactic theory and tried to extend them by wholly unwarranted analogy to such diverse domains as kinship, cooking, narrative structure, folk taxonomy, etc. Piaget and Inhelder correctly noted the relationship between syntactic “deep structure” and the kernel corresponding to the algebraic model, but they went on to equate this with the underlying “kernel” of intelligence, that is, the schema of sensory-motor intelligence; for, if there is only one kind of intelligence, then there can be only one kernel:

“Chomsky [. . .] assumes the existence of an innate kernel within his transformational grammar. But it is not necessary to take this extreme approach; what Chomsky supposes innate in the capacity for learning language can no doubt be explained by the earlier ‘structuralization’ due to the development of sensori-motor intelligence (or intelligence preceding language)” (Piaget & Inhelder 1969: 121).

More recently, however, speculation by grammarians into the empirical nature of the kernel has largely been abandoned; it seems to have been more an interesting part of the particular model used than of the phenomena which the model attempted to describe (cf. Atran 1980: 158). But neither the acceptance nor rejection of the notion of kernel in transformational grammar was in any way related to a “kernel” of sensory-motor intelligence (whatever that is).6

5. Piaget & Inhelder (1969: 156; Inhelder & Piaget 1969) emphasize their claim that language has no primary role in thought because the deaf can think. But recent work by Bellugi and associates (Bellugi & Siple 1974; Newport & Bellugi 1978) indicates that the deaf do, in fact, have language with a syntactic and semantic structure that closely corresponds to the syntax and semantics of normal children. The complex phonological systems described by Chomsky & Halle (1968) provide further evidence that a phonological component of grammar operates somewhat independently of syntax and semantics; the operation of certain phonological rules requires some input from syntax, but syntax alone cannot determine the phonological make-up of language. For much the same reason, Piaget and Inhelder also hold that there is no epistemological distinction between syntax and semantics or between semantics and reference, though they acknowledge that such distinctions may be “necessary in linguistic theory”. But in order to dispense with what is necessary in linguistic theory, one would have to reduce the concepts of linguistic theory to some other theory without loss of generality and predictive ability. No such reduction exists.

6. Piaget and Inhelder offer a number of equally unwarranted analogies between the sensory-motor intelligence and language which supposedly demonstrate that the structure of sensory-motor intelligence suffices to account for the interesting features of language:
In another case of unwarranted analogy, Holloway argues that an understanding of the kernel and deep structure of transformational grammar might facilitate an understanding of the underlying (thus “deep”) structures and a basic (thus “kernel”) set of techniques in tool manufacture. “Kernel” productions would be represented by bifaces and other “basic” tools found, and probably produced, in abundant quantities because they seem to possess a kind of “completeness”:

“Stone tools found from the Oldowan through the Chellean-Acheulean to the Levalloisian, show a basic pattern (flake deflection) overlain by a set of ‘rules’ about how many flakes (approximately) shall be deflected and where” (Holloway 1969).

Yet the logico-mathematical structures which model syntactic competence can no more “approximately” model flake detachment than they can model kinship or the structure of bacteria.

The final criticism I wish to make about the presumed relationship between language and tool-making concerns the “functional” equivalency argument. On this account, early species of hominid must have possessed a language equal in complexity to the level of communication required to maintain lithic industries from generation to generation and to effect the apparent division of labor (Leroi-Gourhan 1964; Harris 1975). Now it is a logical error to confuse the structure of language with one of its functions. Other systems which operate upon principles quite different from those of human language could conceivably serve the purpose of communication; machines using “structure-dependent” rules, for instance, are adequate for communicating many kinds of messages that are also transmittable by the “structure-independent” rules of natural language. In short, language

“at the age of 18 months, before the infant can talk, he can order spatially and temporally; he can classify in action, or apply different action patterns to one object; and he can relate objects to objects and actions to actions. The linguistic equivalents of these capacities are concatenation, categorization and function, where categorization means the major categories (noun phrase, verb phrase, etc.) and function in grammatical relations (subject-of, predicate-of, etc.).” (Piaget & Inhelder 1969: 148).

Such analogies are hardly compelling and indefinitely, many others would be possible, as there are no empirical tests which could conceivably falsify the claim. They are, therefore, scientifically uninteresting.

In Chomsky’s “standard theory” deep structures were the only syntactic structures relevant to semantic interpretation (Chomsky 1965). Semantic interpretation was thus independently considered from the transformations which would convert deep structures to surface syntactical forms. Piaget and Inhelder interpret this as follows:

“This principle adds an important link to psychology, since it supposes an invariant for the system of transformational rules. One of the most important acquisitions of the sensorimotor period is precisely a first invariant, namely the permanency of the object” (Piaget & Inhelder 1969: 148).

In the more recent “extended standard theory”, Chomsky was motivated by evidence and critical argument to abandon the idea that deep structure alone is relevant to semantic interpretation (Chomsky 1973, 1980). So what becomes of the analogy now?
is not communication and may, in fact, be used to a wide variety of ends such as
the refinement of one's own thoughts, to confuse or deceive, to carry out inference,
to play language games and so on.\textsuperscript{7}

\textit{Wynn's Analysis}

With these considerations in mind I want to explore in some detail one of the
most recent and serious attempts to characterize hominid representational capac-
ity. Using behavioral evidence from stone artifacts found at the Isimila Prehis-
toric Site in Tanzania, Wynn (1979) seeks to describe the intelligence of later
Acheulean hominids in Piagetian terms. In support of the decision to use Piaget-
ian theory, he argues, correctly, that "genetic epistemology is a developmental
theory of intelligence that encompasses all intelligent behavior and is consistent
with, indeed requires, the evolutionary perspective". But there is a more com-
pelling reason for considering genetic epistemology in an evolutionary perspective
than Wynn has in mind; for, insofar as I am able to tell (and exegesis in the case
of Piaget is notoriously a muddle), an evolutionary perspective is the only possible
perspective. It is not simply that an ontogenetic theory of this kind is im-
plausible, but as a theory of ontogenetic development genetic epistemology is, in
a significant sense, logically impossible.

For Piaget, conceptual development progresses through a series of "stages"
beginning with the infant's "sensori-motor coordinations" and culminating in
the adult's "formal operations". Each stage is characterized by a "logic" which is
"weaker", that is, which has less representational power than its successor. The
way a child overcomes his conceptual inferiority to adults is to "learn" progres-
sively "stronger" logics which contain their predecessors as proper parts. Consider
the case of the child's passage from "pre-operational" thought, whose advent is
marked by the child's acquisition of language and a general "semiotic" function,
to the stage of "operational" thought:

"In observing this kind of behavior we undeniably meet with the advent of
logic, but we should note that this logic is limited in two essential respects:

\textsuperscript{7} Now it is possible that we could discover in the evolution of tool-making a development
of more general behavioral attributes such as agent (the ability to willfully construct a tool),
instrument (using a tool to make a tool), patient (knowledge of the object of action), locative
 spatia1ly posing) and so forth. We might also discover that all languages possess gram-
matical "cases" for transmitting such behavioral notions. It is even conceivable that basic
aspects of culture are somehow related to these notions; thus, morality and ethics may be
impossible without a recognition of agency, the idea of place and territory may require a
conception of location, exchange might require a knowledge of instrument and patient, etc.
If so, then an interesting relationship of \textit{compatibility} would seem to hold between tool-
making, language and culture, but this is far from sufficient indication of common \textit{cause}.
And it is precisely the distance from correlation to cause that science must cover. Conse-
quently, until an empirically testable deduction can be framed for such a relationship it
should remain outside the scope of any serious inquiry.
such ordering or classifying or setting up of correspondences does not involve reversibility, so that we cannot as yet speak of 'operations' (since we have an inverse), and because of this, there are as yet no principles of quantitative conservation [. . .] So we should view this stage of intellectual development as a 'semi-logical' stage, in the quite literal sense of lacking one-half, namely, the inverse operations” (Piaget 1970b: 64-65).

The acquisition of a logic capable of expressing the inverse operation characterizes the advanced capabilities of the succeeding stage:

“Between the ages of roughly seven and ten the child enters upon a third stage of intellectual development which involves the use of operations [. . .] He now arranges things in a series and understands that in lining them up, say, in order of increasing size he is at the same time arranging them in order of decreasing size; the transitivity of relations like ‘bigger than’, and so on, which previously went unrecognized or was noted as a mere matter of fact, is now something of which he is explicitly aware [. . .] the conservation principles which earlier were lacking are now established” (ibid.: 65-66).

Piaget sums up the progression of stages as follows: “a given stage S is not the most likely at the beginning of development but it becomes the most likely once equilibrium is attained at stage S−1, first of all because the acquisitions in S−1 are necessary to the constructions in S and, secondly, equilibrium that has been attained affects only a limited area, is therefore incomplete and gives occasions for new forms of desequilibrium which will account for the transition from S−1 to S.” Thus, while “equilibration” sets the stage for the wider purview of experience incorporated in the succeeding stage, it takes an act of “reflective abstraction” to actually increase the expressive power of the concepts (logical predicates) in the next stage. What is equilibration and how does reflective abstraction actually work? Piaget can offer nothing more in the way of explanation than vague metaphors about the equilibrium of gases and the “leaps” of evolution. Until these notions are made clear and testable Piaget is caught in a dilemma. As Fodor relates it:

“Suppose, e.g., that you are a stage one child trying to learn the concept C. Well, the least you have to do is learn the conditions under which something is an instance of (falls under) C. So, presumably, you have to learn something of the form \((x) \ (x \ is \ C \ if \ x \ is \ F)\) where F is some concept that applies whenever C does. Clearly, however, a necessary condition on being able to learn that is that one’s conceptual system should contain F. So now consider the case where C is, as it were, a stage two concept. If something is a stage-two concept, then it must follow that it is not coextensive with any stage one concept; otherwise, the difference between stages wouldn’t be a difference in the expressive power of the conceptual systems that characterize stages. But if the stage one child can’t represent the
extension of $C$ in terms of some concept in the system available to him, he can't represent it at all since, by definition, his conceptual system just is the totality of representational devices that he can use for cognitive processing. And if he can't \textit{represent} the extension of $C$, then he can't \textit{learn} $C$ since, by hypothesis, concept learning involves projecting and confirming biconditionals which determine the extension of the concept being learned. So, either, the conditions on applying a stage two concept \textit{can} be represented in terms of some stage one concept, in which case there is no obvious sense in which the stage two conceptual system is more powerful than the stage one representational system, or there are stage two concepts whose extension \textit{cannot} be represented in the stage one vocabulary, in which case there is no way for the stage one child to learn them” (Fodor 1975: 90).

Piaget is obviously stuck on the second horn of the dilemma. His account has it that “preoperational” children cannot fully acquire the concept of conservation because they simply lack the algebraic operations to do so. But if the child’s preoperational logic cannot even afford him the means to \textit{represent} the conditions under which quantities are conserved, he could not possibly learn that those \textit{are} the conditions. And, in fact, Piaget is unable to offer a \textit{theory} of the processes whereby equilibria are achieved: “equilibration” just paraphrases the unexplained process of stage-to-stage transition.

Considered as a \textit{theory} of concept \textit{learning}, then, genetic epistemology is either logically impossible or so hopelessly vague as to constitute no theory at all. Nor could it function as part of a theory of concept development in evolution, simply because no one has the slightest idea of what a conceptual \textit{theory of evolution} would look like. At present, all we can say about the appearance of some trait in the course of evolution is that it occurred in some species as a mutation—which is arbitrary for all we know—and contributed to the differential reproduction of that species in such a manner that the trait survived. \textit{If} one did succeed in demonstrating the presence of a trait such as “operational intelligence” in some hominid group, \textit{and if} one could describe its usefulness for a wide range of selectively advantageous cognitive tasks, \textit{then} one could argue that it is plausible that the trait had adaptive value for the group.

This is not, however, what Wynn has in mind. As he puts it: “It is only Piaget’s characterization of the final ontogenetic stage, that of adult thinking, that is of particular interest here.” This is the stage of “operational thought”. Operational thought presumably organizes phenomena by means of regulators and of operations: “the two fundamental regulators of operational thinking are reversibility and conservation. They can be recognized in the organisation of the behavior of modern adults and, as I will show, in the behavior of later Acheulean hominids” (Wynn 1979: 373).

Now if operational thought appears as a universal \textit{stage} of ontogenesis, then
Wynn is justified in looking to evolution for its origins. But if operational thought is a purely descriptive account of an ontogenetic stage—and here I leave aside the serious doubts about its universality—, then any attempt to locate the same descriptive range of phenomena in phylogeny would be unwarranted; for, ontogeny does not recapitulate phylogeny and is not isomorphic with it. Suppose, for example, that psychologists were able to isolate the function of one type of reasoning, say, transitivity, in some descriptive domain, such as block-building play. Suppose further that the same operations were found, at the same period of the child's mental development, in another descriptive domain, such as kinship. Suppose also that transitivity could be inferred at some stage in hominid phylogeny in a descriptive domain analogous to block-building play, such as tool-making. Given these suppositions, one would in no way be justified in then assuming that transitivity was employed in hominid kinship, even if the presence of kinship at that stage in phylogeny could be demonstrated on independent grounds; correlation, in itself, establishes nothing at all. Still less could one justify an inference based on these suppositions to the argument that transitivity in tool-making implies the existence of kinship. It won't do to argue as Wynn does and claim that only "possible" existence is implied because the modal argument is just as disastrous as the non-modal one; there just isn't any possible a priori justification for including that some property x is (possibly) correlated with property y at some time T−1 simply because x and y are correlated at T.

Both kinds of unwarranted inference are to be found in Wynn's analysis. The first kind derives from the mistaken belief that Piaget's genetic epistemology constitutes an absolutely general theory of development, wherein if some property of intelligence is present anywhere, then it is present everywhere. The second mistake owes to an uncritical acceptance of Piaget's doctrine that a restricted set of intellectual operations exhaust the significant forms of cognition, whereby if one has the "kernel", then one can generate the rest as by-product or "husk".

Nevertheless, I intend to show that Wynn does use certain structural techniques of genetic epistemology to produce some valuable insights into tool-making behavior. But one must be careful to separate the specific structural methods of genetic epistemology, which can lead to interesting analyses in particular cases, from the general theory, which more often than not draws unwarranted conclusions from these analyses. A more detailed appraisal of how it is possible for particular methods to succeed in Piagetian analysis where the general theory fails can be found elsewhere (Atran 1980: 103-105). For now, I simply wish to separate the particular from the general in Wynn's characterization of hominid intelligence. By so doing, I hope to highlight the kinds of abstract structures which a cognitive theory of tool-making might profitably deal with.
Provided the recent re-dating of the Koobi Fora formation at Lake Turkana, Kenya, is correct, it now appears that the oldest known lithic industries come from the Hadar region of Ethiopia (2.4-2.6 million B.P.). Among the artifacts discovered in these early industries were choppers, scrapers, polyhedrons, discoids and spheroids (Corvinus 1975; Roche & Tiercelin 1977). Many tools are characterized by a “functional” symmetry produced by discriminate flaking (see fig. 1). The evidence collected so far seems to confirm findings from similar industries in Kenya at Olduvai and Lake Turkana (East Lake Rudolph) and supports the claim that the earliest knappers showed skill in the working of multiple edges, bifacial flaking of discoids and in the patterning of spheroids and sub-spheroids (Leakey 1971; Isaac 1976).

Taken as a whole, the evidence from the earliest East African Oldowan industries offers a number of clues regarding hominid cognitive capacity, and also undermines a number of previous speculations on the subject. First, the persistent use of the principle of conchoidal fracture can hardly be deemed an accident. Thus, chimpanzees can be trained to strike a flake from a core (Tixier, personal communication) but apparently are unable to recognize the striking of a flake in a sequence of strikings that produce a tool “Gestalt”. Second, in discovering the properties of persistent conchoidal percussion, Oldowan knappers were required to know the characteristics of both flake and core. Consequently, it would be a mistake to try and separate “core” from “flake” industries on grounds of conceptual competence. The relative frequency of use of core and flake would be merely a matter of contingent performance. Third, evidence of multiple working edges and bifacial flaking would seem to refute any notion that the evolution of human tool-making capacities proceeded in a gradual, monotone progression from ad hoc tool-use, to a single strike, to multiple random strikings, to bifaciality. Once the principles of conchoidal fracture were discovered, all of these factors would become expressions on a single conceptual theme. There is thus no indication, at least for the moment, of a gradual evolution from proto-hominid to hominid tool-making. With the Oldowan industries, it appears that a conceptual “rubicon” of sorts had been crossed. But if there is no reason to suppose that tool-making was the gradual progression it is often made out to be, why, then, should tool-making be a progressive guide to the evolution of language (which is even more likely marked by drastic evolutionary mutation, and at a much more recent date)?

The Oldowan knapper was apparently not only capable of displacing techniques in space and time (chimps can do this) but to develop schema of production which were grounded in cognitive structures of quite specific natures. There are, for instance, the sensory-motor coordinations including asymmetric serial operations
of left-hand rotation and coincidental right-hand detachment. There are Gestalt configurations which allowed the knapper to differentiate core from flake, to distinguish one flake from another in a series producing overall symmetry, and to see in each block, a potential flake, and in each flake, a potential core. There are also “proto” geometric structures conforming to “incipient” dihedrals, trihedrals and spheroids. These latter structures, however, are barely elaborated: “The artifacts hitherto recovered show competence in stoneworking but a certain simplicity in their lack of design” (Isaac 1976: 563).

Most analyses of the development of tool-making capacities simply go on from here to note that sometime in the course of habilines-erectus evolution techniques were developed in the later Oldowan and early Acheulean industries which included a working of the striking platform, more anticipation of future strikes, a greater regard for symmetry, some preparation of the anvil, and retouch on flakes. Wynn, however, chooses to interpret this behavior in terms of a theory of hominid organization of geometric space. So far as I am aware, he proposes the first hint of an explanation of the organization of a cognitive domain involved in tool-making which goes beyond mere description of performance. At the same time, he offers the possibility of a principled account of the transition from the proto-geometric “pre-operational” structures of the earlier industries to the fully geometric operations of the later industries. Unfortunately, these achievements are difficult to notice, embedded as they are in a theoretical motley.

In an argument designed to convey an implicit distinction between the functional symmetry of Oldowan tools and the “true” bilateral symmetry of Acheulean artifacts, Wynn (1979: 383) states: “Vaguely symmetrical artifacts may represent not a concept of symmetry on the part of the hominid knapper, but a concept of symmetry on the part of the archaeologist.” For Wynn true symmetry occurs

“when a form is ‘mirrored’ across a reference line. The mirroring is accomplished by inverting the form from one side of the reference line onto the other side while maintaining congruency. All the distances perpendicular to the reference line must be reversed, e.g. A \rightarrow B + B \rightarrow A \ldots 

But bilateral symmetry is even more complex than this because it requires both the symmetry operation and the operation of spatio-temporal substitution. For two forms to be congruent in Euclidean geometry all the analogous internal dimensions of the forms must be identical” (ibid.: 382).

What is intriguing about this presentation is the reference to Euclidean geometry for, what is implicit in Wynn’s entire analysis is the idea that later Acheulean hominids were producing tools in conformity to rules governing topological relations and figurative projections in Euclidean space.
Hominid Spatial Structuration

According to Wynn, three other kinds of operational spatial organization, complementary to symmetry, were employed in the production of Isimila tools: (i) whole-part relations, (ii) qualitative displacement, and (iii) spatio-temporal substitutability. Each kind is allegedly representative of some “infra-logical” universal of “logico-mathematical classification”. But while I accept Wynn’s distinction of kinds, I do not accept their respective “infra-logical equivalents”, as my remarks will indicate.
(i) In considering the case of the late Acheulean biface, Wynn argues that bilateral symmetry was achieved by minimal discriminate retouch because the maker had competence in the relation of whole to parts. The fact that four short sections of discontinuous retouch were placed relative to one another to produce overall shape “indicate that the knapper anticipated the final shape and knew precisely what had to be done to achieve it. Such analysis of whole into parts is one of the most sophisticated of topological relations, and [...] requires reversibility in thought” (Wynn 1979: 377; my emphasis).

Instead of exploring these significant and complex topological relations, however, Wynn simply concludes: “The maker must have been able to conceive the desired shape in terms of potential constituent elements, in this case flake removals, and then combine these elements in additive fashion into a finished whole” (ibid., loc. cit.; my emphasis). Now why should such topological relations be combined “in an additive fashion” and why must this require “reversibility”? Apparently, for no other reason than by so combining they would exemplify the “infra-logical equivalent of logico-mathematical classification (A + A’ = B, B + B’ = C, etc.) because separate parts are combined in an additive fashion to create a whole”.

Yet, there is no “additive reversibility” shown here. Indeed, it is precisely the fact that the whole cannot be processed simply in terms of its separable parts that distinguishes configural part-whole relations from additive classes of feature lists. Nevertheless, Inhelder and Piaget try to equate the two kinds of properties:

“There is no reason to suppose that the various sets to which they [part-whole relations] give rise will be limited to content which is discontinuous. Such sets as the addition or multiplication of classes can be paralleled by sets involving a continuous content. The operations which will effect the various transformations of such sets will be identical except that they bear on spatial or spatio-temporal parts” (Inhelder & Piaget 1969: 38; my emphasis).

Configural part-whole relations, though, are not “identical” to property features, nor does any “parallel” logic seem to go beyond unwarranted analogy. According to Garner, for instance, configural relations, such as “symmetry” and “repetition”, differ from features, such as “square” and “red”, or feature dimensions, such as “form” and “color”, in two important respects:

“First, both of them [symmetry and repetition] refer to relations between other parts of components of the stimulus; and, second, neither symmetry nor repetition can be changed without changing some of the stimulus components. This last point is [...] especially compelling for considering that configurational properties are of a different kind than component properties. With either a feature or a dimension, the level on the attribute can be changed without changing other component properties of the
stimulus, but if symmetry is changed to asymmetry, then other (component) properties are also changed” (Garner 1978: 124).

Configural relationships are better expressed in terms of such operations as “translation” (over an axis), “reflection” (through an axis) and “rotation” (about an axis) than in terms of “addition” and “seriation” (cf. Shepard 1975).

Let us grant, however, that Wynn has succeeded in demonstrating that the logico-mathematical equivalent of whole-part relations is a formal classification of the type \( A + A' = B \). Given this definition of classification, Wynn goes on to argue as follows:

“This powerful organisational tool is crucial to much of human behavior. Kinship systems, for example, are based on classification. Distinguishing cross-cousins from all cousins and affines from consanguines would be impossible without the ability to form classes and classes of classes. True, classification is unnecessary for recognition of simple relations, e.g., mother-offspring; but kinship systems are never so simple and anything more complex requires combining individuals by means of arbitrary and abstract features, i.e., classification. The infra-logical equivalent of classification has been shown to have been employed by the Isimila hominids. It can therefore be concluded that these hominids were at least capable of creating kinship systems as complex as any existing today” (Wynn 1979: 382).

Such reasoning seems to me entirely too speculative. Few, if any, kinship systems are organized strictly in terms of the hierarchical feature classifications which Piaget describes. What Wynn apparently has in mind are terminologies for unilineal descent groups (ibid.: 374), but whether the formal aspects of hierarchical classification, or any other “formal” system, define the underlying kin relations is seriously open to question (cf. Needham 1971). The classificatory aspects of many other kinds of “kinship system” (assuming there is such a thing) are likely to be better described in terms of non hierarchical “permutation groups” (Sperber 1968) or even as attempts to build formal analogies to natural kind classifications (Lévi-Strauss 1962).

Even if every kinship system resembled a unilineal descent group, however, and even if all such systems exhibited the same “formal” structure as models of tool-making do, we still could not conclude that evidence of tool-making is evidence for the possibility of kinship. To say that two systems are “formally” equivalent is to say nothing at all; for one theory to have some non-trivial relation to another theory, the predicates (concepts) which fall under the rules or laws of the first theory must be mapped in some consistent fashion onto the predicates of the other theory. But for this to happen, the formal systems of both theories must be completely interpreted, that is, the predicates must be given a meaning and reference. Yet, there is no fully interpreted model-theoretic account for “kinship” as a whole nor is there one available for tool-making behavior. No one is even close to
understanding what the predicates common to such theories are (or even what they would look like). And even if one had such fully interpreted theories, the tricky problem of reducing one theory to another would remain.8

(ii) The knapper’s apparent intention to occasionally produce tools with straight working edges, as evidenced by careful retouch, provides Wynn with his example of “operational qualitative displacement”. This operation differs from the part-whole relations in that “it regulates elements not as they relate to a conceived whole but as they relate to each other” (Wynn 1979: 378). His analysis turns on the notion of the straight line as a projective figure:

“Projective geometry is a geometry of perspective in which a shape varies according to the source of a projection or the viewpoint of an observer. Certain aspects of the projective figures (most notably straight lines) must remain constant for the figures to be equivalent. A straight line is a projective figure because all the points on the line must be related to a constant viewpoint. When a straight line is viewed from the appropriate position the terminal point of the line masks all the subsequent points and the entire line appears as a point. Actual sighting of course, is not necessary, even if imaginary. Each point on a straight line is related by this projection to all other points on the line in reversible fashion. If point D is ‘in line’ with points C and B, and C and B are ‘in line’ with A, then D is also ‘in line’ with A because of the reciprocal reversibility of the relation ‘in line’ (ibid., loc. cit.; my emphasis).

I agree that the relation “in line” is a primitive and important notion in our analysis of space and, if Wynn is right, in the spatial analysis of later Acheulean hominids as well. My objection, again, is that I find little justification for conceiving of this relation as “the infra-logical equivalent of logico-mathematical sertation, e.g., \( A < B < C \)” (ibid.). Consider the following:

8. Actually, Piaget’s argument for the connection between classificatory logic and kinship is even more tenuous than Wynn’s. For Piaget, “general mental structures and general social structures are identical in form and therefore [...] there is a natural affinity between them, the origins of which are no doubt partly biological [...] When Lévi-Strauss wishes to characterize the structures of affinity, etc., and to give an adequate expression of this anthropological structuralism, he resorts to the larger structures of general algebra (groups, lattices, etc.) so that the sociological explanation thus coincided with a qualitative matematization similar to that which occurs in the setting up of logical structures, the progress of which can be followed in the spontaneous thinking of children and adolescents” (Piaget 1973: 32).

But just because algebraic models of similar or different sorts may be able to capture certain regularities in structures as diverse as kinship, transformational grammar or the behavior of bacteria this does not in any way indicate that the empirical domains themselves share causally related properties (cf. Atran 1980: 103-105).
"The relation <, or the converse >, is a transitive asymmetrical relation. It finds its exemplification in some discovered qualitative domain which is sufficiently homogeneous to allow identification as a well defined range of a single quality. Within this domain the character studied must be capable of such a serial gradation that a transitive asymmetrical relation can be discovered to hold between the discriminated elements" (Nagel 1930: 20; my emphasis).

The point is that asymmetric transitivity can be interpreted for any number of domains, including, for example, density of liquids, relative spatial position and color saturation. Now, it is true that in discovering the relation of density of liquids, scientists consciously applied an asymmetric transitive relation (e.g., where > is the relation "more dense than", then a is more dense than b if b floats on a, and if b is more dense than c, then a is more dense than c if c floats on a). For relative spatial position and color saturation, however, it is obvious that asymmetric transitivity holds, but it is not necessary that anyone should, or that presapient hominids ever did, discover this fact. The spatial judgement "to the right of (or to the left of) on a line", governing the fact that A, B, C and D form a linear series of points on a straight edge, is a case of a priori knowledge, as is the case that "olive green is darker than canary yellow but lighter than navy blue". These are intriguing examples of innate human knowledge acquired in the course of evolution. Such examples could thus turn out to represent significant objects of study, such as human structuration of space and color, which could be explored with ordinary scientific methods of theoretical conjecture and empirical refutation by linguists, psychologists and anthropologists alike. But I do not believe that there is a priori significance to the fact that both the relation "is to the right of on a line" and that of "darker in color than" can be represented in terms of asymmetric transitivity. In other words, I doubt that the phenomenal study of space will lead to an interesting account of human color categorization or vice versa. This view may well be mistaken, but if it is, then someone will have to show how.

Again, let us grant that Wynn has demonstrated "formal" classification, this time in terms of transitivity. Following Piaget, Wynn argues that this suffices to establish (the possibility) that later Acheulean hominids had the ability to number. But it does nothing of the sort; transitivity and classification are necessary axiomatic preconditions for the introduction of number in a logical schema, but they are by no means sufficient.9 Nothing in tool-making behavior indicates the presence of these sufficient conditions to construct even a finite numbering sequence.

9. What is still lacking is evidence for a concept of number itself, such as the Frege-
Perhaps the most significant knapping operation described by Wynn concerns what he calls “spatio-temporal substitution”. This involves the ability to recognize the form as a whole from different partial points of view and thus to converge myriad strikings to one final end. As Wynn puts it:

“This infra-logical operation of spatio-temporal substitution is attributable to Isimila hominids on the basis of regular cross-sections of bifaces. Most of the cross-section of a biface could not be directly perceived by the knapper because he could not ‘see through’ the external surface to the configuration of a planar intersection, e.g., one of the oblique intersections. How, then, can such cross-sections be made to conform to a desired shape? The relations between the elements, in this case flake removals, constitute the shape of the cross-section. During flaking the modification of the surface to regularise the cross-section from one point of view must not be allowed to ruin other cross-sections, most of which are directly observable [. . .] The observable effect of flaking must be translated into effects on viewpoints that are unavailable. This is again a matter of projective geometry. The knapper must construct unavailable viewpoint from available viewpoints by constructing mentally the rearrangement of elements and relations which would constitute the cross-section, if it could be observed, much as the traveller reconstructs landmarks approached from a new direction by rearranging familiar elements and substituting new ones” (Wynn 1979: 380).

Here, as before, the elaboration of the conceptual constraints on knapping technique in terms of projections in Euclidean space would be most welcome. Instead, another “logico-mathematical equivalent” is cited (i.e., $A_1 + A'_1 = A_2 + A'_2 = B$) which runs up against the same kind of difficulties as those discussed in connection with whole-part relations.

In conclusion, Wynn claims that the operational concepts governing human spatial organization were fully developed by the late Acheulean. In fact, there is evidence, both practical (cf. Montagu 1976) and conceptual, that the operations described by Wynn were already present in the early Acheulean; the principle of spatio-temporal substitution as described above, for example, also appears to have operated in the formation of early Acheulean trihedrals (see fig. 2). Wynn then dismisses the claim that later lithic productions represented a conceptual advance over the Acheulean. His argument, if restricted to such spatial organization as evidenced in stoneworking, seems valid as far as it goes; thus, the Levallois technique is just as refined in the Acheulean as in the Mousterian, and the principles of the soft hammerstone, the preparation of the striking platform and the anticipation of the final flaking series from the first strike are all present in the Levallois technique.

Russell notion of a class of similar classes, as well as evidence that the purely arithmetic axioms, such as Peano’s, are satisfied.
Figure 2

Specimen courtesy of Michel Dauvois from the STIC excavations, Casablanca (Morocco). Paris, Muséum national d'Histoire naturelle, Laboratoire de Préhistoire.
From this speculative but plausible position, however, Wynn jumps to the conclusion that *Homo erectus* (or whoever made Acheulean artifacts) possessed the full range of human cognitive abilities, in a sense meant to include linguistic competence and the capacity for cultural performance in general. Take the case of language: at one point, Wynn reasons that because the sensory-motor techniques employed by Acheulean hominids in lithic manufacture were essentially those employed by Upper Paleolithic *Homo sapiens*, and because there is no clear-cut separation between the morphology (including brain shape and size) of *Homo erectus* and *Homo sapiens*, there is likely no clear-cut difference of "intelligence". Yet, not only is there evidence that brain size and shape are significantly different for *Homo erectus* and *Homo sapiens* (Ariens-Kappers & Bouman 1939; Holloway 1975), but there is also some doubt that the specific morphological requirements of human phonology (and, hence, syntactic contour) are present in any species save *Homo sapiens* (Lieberman 1975). Moreover, it is quite possible that the production/perception apparatus for human language represents a mutation which was not attended by any gross morphological change. Such an apparatus might, rather, mediate the relation between two physical systems which antedate it: the ear-mouth mechanism which, it so happens, can transduce verbal signals, and the central nervous system which, it so happens, can process whatever information verbalizations might communicate.

Wynn's analogies notwithstanding there is no hard evidence whatsoever to indicate that *Homo erectus* possessed human language or "culture". Possibly, the delicate attention to Solutrean "leaves", together with the "mass production" of blades in the industries of *Homo sapiens* does indicate the attainment of some sort of cultural watershed, as does the proliferation of technical "styles", specialization of tasks and techniques, trading of artifacts and so on. Perhaps language, with its inferential capacity to compute dimply perceived consequences from original formulations, together with its capacity to combine concepts and so give memory greater play, made the differentiation of cultures possible. What seems implausible is that *Homo erectus* should have possessed such an incredibly powerful representational faculty as human language without ever having employed it in any clearly recognizable sense. Wouldn't it be odd if, by analogy, some species of bird possessed functional wings but never flew?

Thus, Wynn's speculations on language are wholly unsubstantiated (and, given his data and theory, essentially insubstantial), as are his conjectures on cultural symbolism. They rely on the argument than an understanding of causality is dependent on concepts of conservation. By "conservation", Wynn means to attribute the operation to later Acheulean hominids; however, there is clear evidence to indicate that chimpanzees can master simplified, though abstract, schema of causality (Premack 1976). The further claim is made that if hominids did understand causality, then they would probably have recognized (putative) causes
for most phenomena. So, if no “real”, verifiable cause could be found they would have invented one by way of magic or myth because “if no cause were discernible it is reasonable that one would have been created” (Wynn 1979: 387). This step of the argument requires a truly daring leap of faith—why hominids should have been compelled to fill such causal gaps in their knowledge, or even to recognize them as gaps, God only knows. Admittedly, concedes Wynn, there is no direct evidence to point us one way or the other, the only indirect evidence coming from Piagetian analogy. Such unfounded analogy, as I have tried to show, is best left outside the domain of any serious inquiry into the cognitive capacity of pre-sapient hominids.

Conclusion

The significance of Wynn’s analysis comes to light only if we lay aside the Piagetian prejudice that any spatial structuration is merely another example of some vaguely general operational intelligence, and consider a modified version of the Kantian view instead. On this account, Euclidean geometry constitutes one of a number of a priori (innate) mental structures acquired by human beings in the course of evolution, and which provides them with their (species-specific) means for interpreting the phenomenal world in which they exist. Similar arguments could be made for the a priori validity of such phenomenal structures of common sense thought as the bodily object, color categories and the taxa of natural kinds (Atran 1981). Thus, along with such innate categories as “substance”, “color” and “living kind”, Euclidean geometry would organize the normal, everyday world of human perception; however, there is no reason to suppose, a priori, that a theory of one such domain should be applicable, in whole or in part, to any other domain.

To sum up, then, it has been my purpose to argue that a cognitive theory of hominid tool-making behavior should set itself the relatively modest goal of trying to elaborate the significant point that Wynn makes about the way hominids specifically organized their working space, viz., in terms of topological configurations and projections in Euclidean space. At least this approach appears to offer the promise of a tangible return for the effort invested. The case appears to be otherwise for any direct, wholesale account of such a complex and confused notion as “human intelligence”. This is because a study involving a Euclidean analysis of human spatial structuration could plausibly meet the three initial constraints on a theory of tool-making behavior outlined in the first section, whereas a study of general intelligence could not. First, the theory could be constrained by an empirical criterion of observational adequacy. This would confine the theory to a particular phenomenal domain, namely, that for which
archaeologists and paleoanthropologists have developed a rich enough set of empirical techniques to recover data that directly bear on the theory. Second, this theory could then go some way towards fulfilling the psychological requirement of principled adequacy by restricting itself to a specific sort of abstract structure which has already undergone significant theoretical elaboration, that is, of the kind responsible for Euclidean configurations. Third, such a theory might thus eventually meet the philosophical requisite of ultimate conformity to a materially causal explanation insofar as it would be limited to abstract structures whose underlying physiological foundation neurophysiologists already seem to have some idea of.

Perhaps all we can ever reasonably hope to learn about the evolution of human cognitive capacity concerns the rather limited domain of spatial structuration which is implicated in hominid tool-making; speculations on the evolution of language and culture in general are likely to remain just speculation. But a truly profound, principled explanation of even this restricted aspect of human cognition may prove of greater significance for understanding human nature than any all-purpose account of general intelligence.10

10. Following submission of this article, Y. Coppens indicated that the Oldowan tools found at level B2 of the Usno-Shungura formation in the Omo valley have been dated at approximately 3 million years B.P. Only one apparent type of hominid, *Australopithecus africanus*, has thus far been found at this level (Coppens 1975).

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Abstract

Scott Atran, Constraints on a Theory of Hominid Tool-Making Behavior.—This article aims to show that any serious theory of tool-making behavior must meet the requirements imposed by a rationalist psychology. These involve techniques of empirical corroboration, abstract models of the specific cognitive systems implicated in lithic production and, ultimately, theories of underlying biological systems in the cortex. An account of the Euclidean operations apparently applied in hominid stoneworking would plausibly seem to meet such requirements; however, considerations of other aspects of human culture, including language, which are derived from conjectures about hominid tool-making behavior would likely remain unconstrained speculation. In line with Kant, it is argued that these operations refer to one of a number of innate mental structures acquired in the course of evolution which provide human beings with their species-specific means for organizing the phenomenal world of normal, everyday human perception.

Résumé

Scott Atran, Préliminaires à une théorie du comportement technologique des hominiens. — Cet article a pour but de montrer qu’une théorie rigoureuse du comportement relative à la fabrication d’outils préhistoriques doit tenir compte des exigences d’une psychologie rationaliste : techniques de corroboration empirique, modèles abstraits des systèmes cognitifs particuliers impliqués dans la production lithique et, en dernière analyse, théories des systèmes biologiques sous-jacents dans le cortex. L’exposé des opérations euclidiennes apparemment utilisées par les hominiens semblerait devoir répondre à ces exigences. Toutes théorisations sur d’autres aspects de la culture humaine, dont le langage, dérivées de conjectures sur le comportement technologique des hominiens, ne seraient que pures spéculations. La thèse soutenue ici, d’inspiration kantienne, est que ces opérations renvoient à l’une des nombreuses structures mentales innées, apparues au cours de l’évolution, et qui fournissent aux êtres humains les moyens d’organiser le monde phénoménal de la perception humaine normale et quotidienne.