CHAPTER-II

ARCHAEOLOGISTS AND THE CONCEPT OF CULTURE

PART- A : V. GORDON CHILDE AND DAVID L. CLARKE

Much of the research effort in prehistoric archaeology in the late nineteenth and the early twentieth century was expended on cataloguing an accumulated mass of excavated and explored archaeological material, and organizing it into rudimentary regional sequences. These concerns have continued to-date to dominate some schools of prehistoric research, for example, in West Germany, post-war German archaeologists, cautious as a result of the memories of racial and nationalist use of theory by the National Socialists, have tended to concentrate on the description and typological classification of materials and avoided theoretical discussions (Arnold and Hassman 1995: 70-81; Härke 1991: 197).

By the early twentieth century, with the recovery of vast amounts of archaeological material in Europe and the near East, it was increasingly being realized that the broad terms like 'epochs' or 'ages' were not entirely suited to the nature of the archaeological evidence because of the variations witnessed in the plethora of archaeological assemblages. It was at this stage that the concept of culture was introduced into archaeology. The introduction of the concept is generally considered to be a major turning point in the history of the discipline. And the credit for this, to a large extent, goes to V. Gordon Childe.

V. Gordon Childe (1892-1957)

In an autobiographical article, 'Retrospect', written a few weeks before his sudden death in 1957, in which he looks back to his career as a prehistoric archaeologist, Childe writes that his most important and useful contributions to

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1 During the second half of the nineteenth century, with the impact of Darwinism, began the development of evolutionary archaeology. Archaeologists tended to view the record of man's development in terms of a series of stages. Social evolution was viewed largely as a continuation of biological evolution and, like the latter, was assumed to be unilinear.
prehistory were "... certainly not novel data rescued by brilliant excavation from the soil or by patient research from dusty museum cases, not yet well founded chronological schemes nor freshly defined cultures, but rather interpretative concepts and methods of explanation" (1958 : 69). And rightly so. Childe's originality lay in the way he used material remains to understand prehistoric communities. He realized that an abstract sequence of epochs was totally inadequate for dealing with traces of human societies.

*Introduction of the Culture Concept in Archaeology*

Childe pioneered a new approach in archaeology in his classic, *The Dawn of European Civilization*, originally published in 1925.¹ It was as Glyn Daniel put it "a new starting point for prehistoric archaeology" (1950 : 247). In this work Childe introduced the concept of culture to archaeological material. He "supplemented an exclusively chronological by a cultural approach" (Clarke 1976 : 5). This does not mean that he rejected the need for periodization, but he saw the temporal sequence as only the beginning and not the end of archaeology. "The Dawn", writes Childe in "Retrospect" (1958 : 7), "aimed at distilling from archaeological remains a preliterate substitute for the conventional politico-military history with cultures instead of statesmen as actors and migration in place of battles."

Childe stressed that each culture should be delineated individually in terms of constituent artefacts and that one should not be content with building up bare space-time subdivisions. Childe interpreted the prehistory of Europe in terms of a complex mosaic of cultures, represented by maps and tables, in *The Dawn*. The maps show the spread of different cultures at four periods of time spanning the Neolithic and the earlier Bronze age and the chronological tables relate to a number of defined geographical zones and their cultures. A detailed tabulation of the chronological and geographical distribution of all cultures in

¹The book subsequently underwent five revisions, the last in 1957, the year of Childe's death. Childe was in the habit of revising his books regularly in order to bring them up-to-date.
the Danube valley was published in *The Danube in Prehistory* (1929). These charts became the prototypes for ones that other archaeologists would use to represent regional cultural chronologies around the world (Trigger 1989: 170).1

Archaeology, therefore, for Childe was a study of cultures and not culture. Each culture had a specific distribution in space and its own time, and was defined in terms of specific material artefacts. In this way local cultural sequences were constructed and the historical relationships between these cultures analysed in terms of the established concepts of migration, diffusion, and internal development (we will analyse the importance of these concepts shortly).

**Definition of Archaeological Cultures**

As has often been remarked, even as Childe employed the term culture in the archaeological context in three of his major texts of the twenties, *The Dawn* (1925), *The Aryans* (1926) and *the Most Ancient East* (1928), he attempted no definition of the concept. In fact it was not until 1929, in a preface to *The Danube in Prehistory*, that Childe in a discussion of archaeological procedure, first defined the term:

We find certain types of remains - pots, implements, ornaments, burial rites, house forms - constantly recurring together. Such a complex of regularly associated traits we shall term a cultural group or just a 'culture'. We assume that such a complex is the material expression of what today be called a 'people' ... The same complex may be found with relatively negligible diminutions over a wide area. In such cases of the total and bodily transference of a complete culture from one place to

1 A good example is R.E.M. Wheeler's *Early India and Pakistan* (1959) in which he gives seven distribution maps ranging from those of PGW and NBP ware, to those of microlithic industries and of Gangetic copper hoards and megalithic cists.
another we think ourselves justified in assuming a 'movement of people'.

(1929: i - ii)

It needs to be noted that here Childe limited the definition of culture in the archaeological context, to the material level -- in fact as a unit of classification for archaeological remains. Two important points emerge: first, the archaeological phenomenon represents "a people". Second, pottery alone does not constitute an archaeological culture. This is important because the one mistake that had often been made in the past (and in fact continues to be made even today) is to regard pottery (probably because it is the most visible, quantifiable and plentiful class of remains) as the mark of the archaeological entity, more basic than others and to consider everything else in relation to it. And moreover assume that it directly reflects a culture territory and its boundaries. So Childe's understanding that ceramics alone do not constitute a culture was important. Pottery "defined" but did not "constitute" culture (1958b : 70) and it was peoples/s who produced the archaeological mass. Thus the new concept provided access to the people behind the data (Mc Nairn 1980: 48).

Childe maintained the above definition of archaeological cultures until the end. In Piecing Together the Past (1956: 111) he wrote:

Similar assemblages of archaeological types are repeatedly associated together because they were made, used or performed by the same people at the same time. Different assemblages of associated types occur at the same time because they were made by different peoples.

[The same definition is repeated in a letter to Soviet archaeologists written in 1956 (Harris : 99)] Childe was thus distinguishing the types that comprise an assemblage or culture and those that distinguish the entity from others of similar rank. The latter types were referred to as 'type-fossils' and are presumably to be recognized after a full analysis. Clarke too stressed on this in Analytical Archaeology in 1968.
In an *Introduction to Archaeology*, also published in 1956, he repeats the definition of archaeological cultures (p.19):

A culture, it will be recalled is just an assemblage of types repeatedly found in association at a number of sites. Now a type is a type because it is the result of distinct actions all inspired by one and the same tradition. Types are associated because the several traditions expressed in them are maintained and approved by a single society. The same assemblage of associated types recur on a number of sites because all the sites were occupied by members of the same society. What sort of unit that society was -- a tribe, a nation, a caste, a profession -- can hardly be decided from archaeological data. But these societies, however they are to be designated, do provide archaeologists with actors in an historical drama.

In "Retrospect" (1958b: 70) Childe briefly states that he was influenced by the German concept of culture. In *Piecing Together the Past* (1956a: 28) Childe quotes Kossinna's classic definition "Sharply defined archaeological culture areas correspond unquestionably with the areas of particular peoples or tribes".

**Gustav Kossinna's Contribution to Archaeological Theory**

Linguist - turned - prehistorian Gustav Kossinna made very important contributions to archaeological theory in the pre-war years and laid the groundwork for a nationalist German prehistory (although he died a year before Hitler's rise to power). According to Arnold and Hassmann (1995: 72) there has been a tendency to set up Kossinna as a sort of "straw man" whose scholarly work is presented as the primary basis of later archaeological research under the Nazis. According to the above authors this approach is flawed because
"...it lumps all of Kossinna's work into an indistinguishably negative and ideologically tainted mass while ignoring his potentially valuable theoretical contributions" (ibid.). They echo Becker (1985) who remarked that it is now too easy to overlook that Kossinna's Siedlungs-archäologische methods were epoch-making for the whole profession.

Kossinna developed his approach to prehistory with the aim of documenting the antiquity of the Germans in the new nation state of Germany. In the course of this he created a set of methods and interpretative principles for the discipline of prehistoric archaeology. This method involved the definition of archaeological 'culture provinces' and the interpretative principles postulated a link between such culture provinces and the territories of prehistoric peoples (Shennan 1989: 8). The distributions of distinctive artefact types used to identify cultures reflected 'cultural provinces', according to Kossinna. And clearly defined 'cultural provinces' coincided with settlement areas of tribal or ethnic groups. This approach Kossinna referred to as settlement archaeology (siedlungs-archäologische).

Kossinna proposed that from upper Paleolithic times onward the archaeological assemblages of Central Europe could be organized as a mosaic of cultures (Kulturen or Kultur-Gruppe). And he further argued that it was possible to determine where a particular tribal group had lived at different periods of time by mapping the distribution of types of artefacts characteristic of that group. He believed that by identifying historically known tribal groups of the late Roman period with particular archaeological cultures it would be possible to trace them backward in time archaeologically! Kossinna used his settlement - archaeology method to show the descent of the Nordic Aryan, German race (who were the most superior) from Indo-Germans and to demonstrate the outward movement of influences from this superior core area¹. He used archaeological

¹He did not consider that migration is only one possible explanation among many for the geographic spread of an artefact-type.
finds to argue that parts of Poland had in fact been Germanic since the Iron age. Therefore Kossinna was very specifically relating race with culture.

Childe, no doubt, developed his concept of culture under influence from Kossinna's work. Like Kossinna he tended to view archaeological cultures as material expressions of particular peoples who were united by a common social tradition. And Childe, developed and elaborated upon Kossinna's concept of "cultural provinces" as defined by artefact distributions. However Childe's independent contribution was the emphasis on material assemblages rather than individual artefact types, and a focus on the social rather than ethnic or racial interpretations of archaeological cultures (Veit 1984). Childe rejected Kossinna's equation of cultures with specific races except in one of his earliest writings, The Aryans. This book published in 1926 went through only one printing in his life time; and was later ignored by the author altogether. In fact in the 1930's, concerned about the alarming growth of racism in Nazi Germany, Childe rejected the entire Aryan thesis of progress. This book does not even find a mention in "Retrospect" (1958).

However The Aryans did raise some important issues. For one, it gives central importance to language (this probably had a lot to do with Childe's early interest in philology). Childe argued that a common language implied a common mental outlook. And that "language ... is a more subtle and pervasive criterion of individuality than material items like flints and sherds or race"(p. 4). Those who shared similar languages therefore had similar cultures. The reconstruction of primitive Aryan culture by him in chapter IV of his book was based not so much on archaeology as on the properties of language. Infact Childe's treatment of the archaeological evidence is very slight in The Aryans.

But it was not only Germany which witnessed politically motivated uses of archaeology, but many other European countries too, in the pre and post war years (Hodder 1991; Kohl and Fawcett 1995).

Yet when Childe wrote The Dawn in 1925 and The Aryans in 1926, he meant the two to complement one another. While the former attempted to trace the spread of material culture from Near East to Europe, the latter sought to demonstrate how the benefits of this process had been reaped by Indo-European speaking peoples.
Race, People and Culture

From the 1930s, Childe began to take a closer look at terms like 'race', 'people' and 'culture' in archaeology and related disciplines to clarify their meaning. This might have had to do with his increasing awareness of the works of American anthropologists like Boas and Kroeber (signalled by his three visits to the United States in the 1930s) who stressed that language, race and culture are separate entities. In the articles entitled "Races, Peoples and Cultures in Prehistoric Europe" and "Is Prehistory Practical?", both published in 1933, Childe brings up the question of race and culture in detail. The issue was of increasing concern to Childe, as it was to Boas, because of the claim of the racial superiority of the Aryans under Nazi rule in Germany. Childe now voiced his concern against such theories. He argued that 'culture', race and 'language' comprised distinct entities, not necessarily coterminous. In "Is Prehistory Practical?" (1933 : 417) Childe states that "In the prehistoric past, as obviously today, culture was independent of physical race, was not a matter of biological heredity but of social tradition". And in Social Evolution (1951 : 40) specifying this point, he wrote:

So for the archaeologist the unit or society must remain the group enjoying the same culture-- i.e., giving concrete expression to common traditions. ...we might call its (group) members a people, but we should have no right to assume that this people as a whole spoke a single language or acted as a political unit, still less that all its members were related physiologically or belonged to one zoological race.

Cultural Evolution

In the mid 1930s there is a slight shift in Childe's theoretical orientation as exemplified by his presidential address to the Prehistoric Society, "Changing Methods and Aims in Prehistory" (1935). The shift was to a large extent an
outcome of a brief trip to the Soviet Union in 1934. "In that year", writes Childe. "I visited for the first time the U.S.S.R. and secured some typical Russian works on prehistory. From Kruglov and Podgayetsky, Krichevskii and Tretyakov I learned how neatly even the Maarist,\(^1\) perversion of Marxism explained without appeal to undocumented external factors the development of certain prehistoric cultures in the Union" (1958b: 71)\(^2\). Childe gradually introduced Marxist terms and theories into his interpretation of archaeological data, for example, 'savagery', 'barbarism' and 'civilization' (terms originally formulated by the nineteenth century anthropologist L.H. Morgan). The Neolithic and Urban revolutions (which Childe had mentioned in New Light on the Most Ancient East (1934), and then in his Prehistoric Society address), separated these stages. In 1935 he clarified his conceptions of the revolutions -- as major cultural transformers which changed whole social structures -- and published the results in Man Makes Himself (1936)\(^3\). In this work he interpreted the archaeological record as evidence of a directional process whereby the increasing scientific knowledge accumulated by human beings gave them ever greater control over nature and led to the formation of new and more complex socio-political systems. Thus within the materialist framework Childe approached cultural development at the level where society is materially in interaction with the environment.

\(^1\) Maarism was the name given to the Japhetic theory of N.Y. Maar that languages necessarily develop by an autochthonous process.

\(^2\) The work of American cultural evolutionists like White and Steward cannot be compared to that of Childe (Flannery 1994: 112-113). Their source is more Darwin than Marx. "In other words, we have one set of people who are following Marx and Engels and another group who are saying, 'Must there not be a cultural evolution that parallels what Darwin said about biological evolution?' "(ibid.).

\(^3\) As far as the Neolithic revolution was concerned food production was taken as the feature distinguishing the Neolithic from the earlier Palaeolithic and Mesolithic. And food production was seen as resulting from dessication which compelled large number of people to subsist off a small area of land (Childe was influenced by Pumpelly's 'oasis' hypothesis). According to Childe Neolithic people learnt to co-operate with nature to increase their food supply. And with the increase in food supply, "man ceased to be purely parasitic ... and became a creator emancipated from the whims of his environment" (1928: 2).
His paradigm of the Neolithic revolution is essentially ecological in content. Childe was the first archaeologist to think of Neolithic origins in terms of ecology. So too with the Bronze age. The adoption of Bronze tools and weapons doubtless gave their users "enhanced control over their environment" (1935 : 7). Childe's 'second revolution' implied a more complex economic and social structure and was characterized by an increase in population, size of settlements, large scale movements, birth of literacy, beginning of exact and predictive sciences etc.-- all of which was due at least partly to the "emergence in addition to the farmers of a new order of professionals who did not grow or catch their own food" (1958 b: 71) but were supported by the surplus above domestic needs produced by farmers, fishers and huntsmen. The new order of professionals "comprised not only artisans and craftsmen but also rulers, officials, priests and clerks" (1951 : 24). The surplus wealth was accumulated by kings and temples most of which was used to provide for themselves and sustain the other non-agricultural classes. One of the strongest legacies of Childe is the holistic concept he employed for the study of the revolutions.

In What Happened in History (1942), Childe combined his system of revolutions with Morgan's stages of savagery, barbarism and civilization with food production forming a "servicable and scientific differentia" (1951 : 24) between savagery and barbarism, and the urban revolution between barbarism and civilization. Though more or less a revised version of Man Makes Himself, Childe now attempted to focus not only on technical knowledge as a prime mover but social, political and economic institutions as well to explain cultural changes. In accordance with the principles of dialectical materialism, he viewed every society as containing within itself both progressive and conservative tendencies that are linked by a dynamic unity as well as persistent tension. The latter provides the impetus that in the long run brings about social change. Hence every society contains within itself the seeds for the disintegration of its existing structure and the creation of a new form. But in 1957, Childe himself criticized What Happened in History for the lack of advance in its conceptual framework since Man Makes Himself (1958 b: 73).
Scotland Before the Scots (1945) was another attempt by Childe to explain the development of prehistoric cultures in terms of internal social changes. But however much he played with Marxist theories, he honestly admitted in "Retrospect" that he "...just had to admit migrations¹ and the impact of foreign cultures: the internal development of Scottish society in accordance with 'universal laws' simply could not explain the archaeological data from Scotland; reference to Continental data actually documented the solvent effects of external factors" (1958 b: 73).

In Social Evolution (1951), Childe came to the conclusion that the development of societies varied from area to area (though all had developed from the same Neolithic base), depending on local environmental and economic conditions and upon the process of diffusion. Contact between different societies, according to him played an important part in cultural development (1944 : 76-77). In 1958 (c) Childe states that "it is arguable that all progress, indeed all change, is due to the stimulus of contact with other societies" (p. 6). And as societies grew more complex inter-societal contact became a more important source of innovation. Thus for him no society existed in a vacuum. His materialist model denied the existence of closed systems and the completely internal evolution of society. Hence every prehistoric culture was influenced by the vagaries of diffusion and by its historical antecedents, no less than it was shaped by its relations of production.

What is significant about Childe's books like Man Makes Himself (1936) or Social Evolution (1951) is the emphasis on cultures as products of human action -- on human cultures as social constructs rather than as products only of their environmental and technological contexts. He was not an ecological or a technological determinist (Trigger 1994; Renfrew 1994; Rowlands 1994). He attributed technological change to human beings applying their powers of

¹ Thus while heartily approving of his Soviet colleagues' efforts to explain changes internally, Childe rejected Nicholas Maars' repudiation of external causes like migration and diffusion as valid historical processes. It was according to him only "a pseudo-Marxist materialism" (1935 : 12) which ignored diffusion and migration as promoting cultural change.
inventiveness to control nature more effectively. For example, in his model of the Neolithic Revolution (in which increasing desiccation is the prime mover) it is by deliberate human intervention that plants and animals are domesticated. Though he never precisely discussed how this might have occurred.

According to Daniel (1958: 66) the great puzzle of Childe at all times was to what extent he was a Marxist.¹ What needs to be remarked upon is that Childe's attitude to Soviet archaeological scholarship changed during his lifetime. Klejn makes up a parable of a knight's (Childe) unlucky romance with a lady (Soviet archaeology): the courtship, love, disappointment and final frustration (Klejn 1944: 75-90). In one of his last essays, "Valediction" (1958), Childe highlights some of his reservations about Marxism (Maarism to be more specific). He writes:

Maarist's appeal to "uniformities of social evolution" while it seemed to make intelligible the development of each individual culture to which they applied it, completely failed to explain the differences between one culture and another and indeed obliterated or dismissed as irrelevant the differences observed. So it made prehistory unhistorical. To restore historicity it is needless and illegitimate to appeal to undemonstrable genetic factors or to a mechanical determination by the non-human environment on the one hand, or to the unpredictable genius of great men or interpositions by an inscrutable providence on the other. The individual and the unique can be made

¹ This issue dominates, if only in the background three recent monographs about Childe -- McNairn 1980; Trigger 1980; Green 1981.
intelligible by presentation as an historical conjuncture of general and familiar processes and patterns.

(1958 c: 7)

And a few weeks before his death Childe wrote, "Now at last I rid my mind of transcendental laws determining history and mechanical causes, whether economic or environmental shaping its course" (1958 b: 73).

**Functionalist Approaches**

Alongside Marxism, the other concepts which exerted a sharp influence on Childe from the 1930s onwards were those of contemporary social anthropologists like Raddcliffe-Brown and Malinowski. In "Retrospect" Childe admits that:

"I took from Marxism the idea of the economy as the integrating force in society, but I was just as much influenced by Malinowski's functionalism and tried to stick the archaeological bits together by reference to their possible role in a working organism."

(1958 b: 72).

It was in his 1935 "Changing Methods and Aims of Archaeology" that Childe for the first time referred to a functional view of the study of archaeological cultures.

The study of living human societies as functioning organisms has revealed to archaeologists this approach to their material. It has led to the correct definition and interpretation of culture.

(1935: 3)

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1 Both were in turn greatly indebted to Durkheim. Clyde Kluckhohn in fact always maintained that Radcliffe-Brown's major contribution to anthropology was his ability to read French, meaning he had stolen it all from Durkheim (Geertz in an interview with R. Handler 1991: 607).
According to Childe an archaeological culture should be viewed "not as a dead group of fossils but as a living, functioning organism" (ibid., : 10). Till the last of his writings, for example, Piecing Together the Past (1956 : 34), Childe held that culture should be viewed as an organic whole and not as a mechanical aggregate of traits.

Another prominent British archaeologist who by the 1930s had realized the importance of viewing archaeological evidence not merely as isolated fossils from the past but as parts of functioning social and cultural systems was Graham Clarke. Clarke however finds Childe "not in the slightest degree being influenced by the functionalism expounded by Raddiffe-Brown or Malinowski". (1976 : 6). And that "when years later he showed signs of being aware of what they were getting at, it was rather as a bystander" (ibid.). However Childe's writings are a testimony to the influence of the functionalist approach because in them he emphasized the need to look at archaeological cultures in systemic term. In this way, Childe pre-empted the New Archaeology of the 1960s which had as its underlying notion cultures as totalities or systems consisting of not merely component elements but their inter-relationships as well.

Childe in his 1935 address also referred to culture as an adaptation to an environment. However unlike either Radcliffe Brown or Malinowski, it was "material culture" which he saw to have the main adaptive potential (1935 : 9). This was a distinctly "Childean" approach. According to Childe "a culture is the durable material expression of an adaptation to an environment ... that enabled a society to survive and develop" (1951 : 15). But Childe did not consider the concept of "adaptation to the environment" useful unless the "environment" included among its significant elements the "social environment" (Trigger 1980 : 172; Mc Nairn 1980 : 73). Quoting Childe:

Thus prehistory can recognize peoples and marshal them on the stage to take the place of personal actors who form the historian's troupe. Their interactions are no less a part of the drama of
prehistory than are their reactions to the external environment.

(1933: 417)

Childe's sentiment is aptly summed up in his reaction to Prehistoric Europe - The Economic Basis by Graham Clarke, a leading advocate of the ecological approach: "Yes, Graham, but what have you done about society?" (Clarke 1974: 55). I think this question is immensely important. It is a nuance to which the New Archaeology does not appear to have been sensitive.

Childe understood the "economic basis" of prehistory in a wider sense than did Clarke and others, to include not only the strategies of getting food and producing surpluses (by adapting in particular ways to the environment) but more importantly, also the social relations within and between societies by which resources were procured, produced and distributed.

New perspectives in the culture concept

By the late 1940s and early 1950s, Childe turned his attention to the more theoretical concepts in archaeology, as is evidenced in History (1947), Social Evolution (1951), Piecing Together the Past (1956), Society and Knowledge (1956), and finally the Prehistory of European Society (1958). By this time he had been working with the idea of culture for over twenty years. It was thus expected that his depth of understanding of the concept would have increased.

By 1951, Childe also felt it necessary to examine the relationship between the archaeological and anthropological view of culture in some detail and in Social Evolution (1951) he devoted a chapter to the meaning of culture in both these sciences. This was the first time that he had explicitly differentiated between the two usages (McNairn 1980: 59). Basically he argued that the archaeologist's

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1According to Trigger (1980: 110) by the 1950s Childe was becoming increasingly depressed and pessimistic about the validity of his interpretation models of culture change. And according to Tringham (1983: 97) one of the weakest points in Childe's research (which was a combination of induction and deduction) was the deductive process of validating his models with appropriate archaeological data and this thus must have been a source of much of his frustration with his data.
culture differed in degree rather than in kind from the anthropologist's. As before he defined an archaeological culture as "an assemblage of associated traits that recur repeatedly" (p. 30) with the qualification that "these traits are mostly material objects" (ibid.). Culture in the anthropological sense he saw as basically a holistic concept comprising all aspects of human action that are not physical reflexes or instincts. In addition to this holistic level, Childe also saw culture as the acquired behaviour and patterns of particular groups. It was with this level that he equated the archaeologist's culture. In *Social Evolution* Childe also stressed, as mentioned above that culture did not necessarily correspond to either a linguistic grouping or a single society. Childe was thus very cautious as to what type of anthropological/sociological grouping the archaeologist's culture corresponded. It was the same caution David Clarke (a later day British archaeologist) applied in his understanding of archaeological cultures, in his seminal work *Analytical Archaeology* (1968).

*Social Evolution* (1951) was an important work in the sense that some new perspectives were added to the culture concept. Whereas previously Childe had considered the archaeologist's culture to be an empirical entity immediately apparent on inspection of the archaeological record (1935: 2), he was now aware of the subjective element in the classification of data (1951: 40):

Culture and society are abstractions ... The subjective element comes in when deciding which idiosyncrasies should be ignored when defining a culture. Frankly, it is hard to say which should be disregarded as purely individual and which should be taken as social traits, the differentiae of new cultures.

Childe's next major theoretical work, *Piecing Together The Past* (1956 a) was based on his lectures dealing with archaeological classification and interpretative concepts relating to the archaeological record. *Piecing* indicated the complex problems involved in the interpretation of data; Childe sought in this work to present systematically the assumptions with which archaeologists work.
To extract information from archaeological data, an expression of the behaviour of men past, the archaeologist must understand the context of his data and classify it accordingly. The classification according to him must be on three distinct basis: functional, chronological and chorological. Quoting Childe (1956a: 14):

The first basis of classification is functional: what was the purpose of the act that produced the datum or the use of the latter. In practice this should generally mean 'what was the object for? ... then in each functional group we still recognize a vast number of different types. One reason for the observed differences may be age ... so we rearrange all our collections and inventory cards on a chronological basis... But within each chronological group thus formed we may still be faced with different types of adze, dagger, razor, dwelling, grave... We shall then observe on consulting the excavation reports that a given type of adze is often found with a given type of razor or dagger in a given type of tomb or dwelling... In the light of these associations we reclassify the contents of our chronological groups on a chorological basis.

Artefact-Types

In *Piecing*, Childe proposed an archaeological hierarchy of type, industry, culture and culture-cycle (a hierarchy which was to be rationalised and interpreted in terms of systems theory by David Clarke in the late 1960s). For Childe, the type was the smallest unit of archaeological classification. And when types were significantly associated i.e., in a context which indicated contemporary

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1 Childe admits to have "taken this rather ugly word from the Austrian, O. Menghin ... I have not seen it previously used in English, but know no better equivalent" (1956: 14 (f.n))
use then the notion of culture arose. Childe made it clear that when defining a culture some important points had to be kept in mind (1956 a: 33-34). One, that not all types to be assigned to a culture need recur in every assemblage indicative of that culture (but to qualify for inclusion, a type should be present in at least two representative sites and by more than one example). Two, some types, indicative of significant cultural behaviour, will be seen to occur in other areas and other times and hence these would be of little value for diagnostic purposes. Three, a type distinctive of one culture might turn up sporadically associated with another culture, but this does not imply that the distinction between the two is invalid -- it could be indicative of some sort of interaction, for example trade, between the cultures.

Childe was therefore hinting at the use of a polythetic model for archaeological entities such as types and cultures -- a model elaborated upon by David Clarke in a detailed and sophisticated manner in Analytical Archaeology (1968). There Clarke specifically mentions that before the publication of Childe's Piecing there was no book in Britain which discussed methodology in such an explicit fashion (1968: 40. f. no. 5).

Also published in 1956 for the general public was a volume entitled A Short Introduction to Archaeology. It was a kind of summary of his theoretical arguments in Piecing. Childe was at pains to point out that as an important source of the past, archaeology was not a mere collection of objects: "All archaeological data are expressions of human thoughts and purposes and are valued only as revelations thereof" (p.11). Childe's discussions on culture in the last few of his writings, like the above and the following clearly pre-empted the cognitive and symbolic concerns of the post-processual archaeology in the 1980s.

In Society and Knowledge (1956 b) Childe argued that a prehistorian must treat artefacts and monuments "always and exclusively as concrete expressions and embodiments of human thoughts -- in a word of knowledge" (p.1). Knowledge was mostly a study of practical or technical knowledge. This work was a culmination of Childe's long interest in technology -- the implications of which are both social and economic. Childe admitted that ideological beliefs were
important for the prehistorian but, they left little material expression in the 
archaeological record.

Childe's last, posthumously published, book, The Prehistory of European 
Society (1958a) "...exemplifies better than any other work how what everyone 
will accept as history could be extracted from archaeological finds" (Childe 1958 
b: 94). In this work Childe tried to illustrate how Europe eventually achieved 
pre-eminence in science and technology having outstripped the Orient and "achieved a 
culture distinctively its own" (ibid.)¹. To understand how and why this was so, he 

wrote:

I invoke no agencies external to the observed data, 
no eternal laws transcending the process as 
empirically given, but historical conjectures of 
well-established environmental circumstances and 
equally well-known patterns of human behaviour 
legitimately inferred from their archaeological 
results. The archaeological data are interpreted as 
the fossilized remnants of behaviour patterns 
repeatedly illustrated in ethnography and written 
records. Together with the relevant features of the 
non-human environment they are presented as 
instances of more general known processes. So the 
specific events are explained as individual and 
perhaps unique conjunctures of known universal 
factors. Such explanation is scientific as well as 
historical.

(ibid.)

¹The same theme is reflected in his rewriting of the New Light on the Most Ancident East in 
1954 and in the last revision of the Dawn in 1956.
Stressing the importance of the above work Trigger (1980) points out that it was more than just a synopsis of Dawn because in it Childe, differentiated with clarity, more than ever before, between the concept of society and that of culture.

Quoting Trigger (1980: 160):

He developed the idea of society as the structural matrix in terms of which individual items acquired their functional significance... Thus Childe, apparently quite independently, arrived at a view of how archaeological data might be studied in terms of their relationship to social systems that in many ways resembled what had been achieved a few years earlier by Gordon Willey in his Prehistoric Settlement Patterns in the Viru Valley, Peru.

In 'Valediction' (1958c), in which Childe sums up the conclusions of almost forty years of work, major aspects of his use of the cultural concepts in archaeology are highlighted, the most important being that culture should not be viewed as a finished and static organism. Instead he stressed on its dynamic aspects—"It must not only function, it must change, and the observed changes must be described and explained" (1958 c: 5). This idea ran counter to the practice of social anthropology in Britain at that time. And in explaining change, Childe thought that external factors should be invoked only when "compelled by cogent concrete evidence" (ibid.)²; and "as far as possible, changes should be explained by internal development including thereunder adjustments to documented changes in the non-human environment" (ibid.).

¹This study of Willey’s (1953) marked the beginning of settlement pattern analysis and the societal interpretation of archaeological data in America.

²Here Childe specifies that before talking about diffusion to explain an innovation, we must in the first place have found in the archaeological record one society where the device was current in earlier times. For this precise relative chronologies are required. If invasion is to be admitted as the mechanism of change then for Childe the invading culture must be well defined in its homeland, and bring with it a substantial number of distinctive archaeological traits (1958 c: 6).
Another conclusion Childe reached at the end of his career was that archaeologists must resign themselves to adopting only a behaviourist position. "I believe foredoomed to failure any attempt to recapture the subjective motives or emotions that inspired the overt acts, the results of which alone survive in the archaeological record" (ibid.: 3).

Childe concluded with the following note on the future of archaeology:

... the future of archaeology lies, I believe, with the historical rather than the naturalistic disciplines. It is a source of history rather than of generalizations claiming the dignity of natural laws.

(ibid.: 7)

(Later Clarke was to take issue with the idea of archaeology being a source of history; he was to insist on the separate lines of enquiry of the two disciplines).

Childe's above position was in sharp contrast to that adopted by contemporary American archaeologists like W.W. Taylor and Gordon Willey; and later day American 'New Archaeologists' like Lewis Binford. Childe, infact, was ignored by the American New Archaeologists because of his adherence to a historical framework: he was declared 'particularistic' and 'irrelevant' to the search for general laws of human behaviour (Tringham 1983: 93).

It is a testimonial to the dynamism and wide range of Childe's scholarship that he continues to be read so widely till today1; And that his writings still invite debate and controversy. In Sherratt's words: "Prehistory is still in dialogue with the ghost of Childe" (1989: 185). The Childe Centennial Conference organized at the London Institute of Archaeology in 1992 aimed at assessing Childe's legacy to archaeology; and the relevance of his work in the 1990s. The general consensus was that even if his interpretational models are considered outdated, his

1According to Trigger "Vere Gordon Childe, although dead since 1957, remains the most renowned and widely read archaeologist of the twentieth century" (1994: 9). And a reading of Ucko. (ed.)- Theory in Archaeology (1995) reveals that on the international archaeological scene his is the most profound individual influence, from Ireland (Cooney) to Japan (Tsude).
theoretical work continues to inspire and to resonate with the perspectives of successive generations of archaeologists and with new fashions of archaeological interpretation" (Trigger 1994: 10).

It must be emphasized that Childe, first and foremost, will be remembered as a "man who made order out of archaeological chaos" (Flannery 1994: 110). From a vast body of disconnected and fragmented archaeological data on prehistoric Europe (and some parts of the Near East), he produced a framework of successive developmental and technological stages that have provided us "food for discussion for more than half a century" (Flannery ibid.).

Trigger goes so far as to say that Childe contributed to the three main archaeological movements of the twentieth century: culture-historical, processual and post-processual (Trigger 1994: 24). He was no doubt one of the bounders of the culture-historical archaeology. He also helped pioneer the ecological and economic approaches that in the 1960s gained much importance within the processual 'school' (or New Archaeology) framework. And finally in the last years of his life, he developed an interest in cultural behaviour that anticipated many of the concerns of post-processual archaeology.

However, throughout Childe's forty-year career with its inevitable changes of view (under the influence of new data and varied theoretical impulses) the one concept that remained central to his work was 'culture'. He distinguished it from its anthropological usage by defining it specifically in terms of material remains. And he was cautious in his application of anthropological terms like 'race', 'people', 'language' and 'tribe' to the archaeological data.

Archaeological cultures, as Childe understood them, were dynamic systems, defined in terms of their constituent artefacts, but each having a specific distribution in terms of space and time. Childe sought to explain culture change by analyzing internal as well as external factors. Archaeological cultures were not "lifeless assemblages of accidentally connected types" (1951: 14) but the residue of peoples and societies. Thus for Childe archaeology was a humanistic rather than a natural science.
Childe did not introduce the idea of culture into archaeology. Neither did he tussle with the problem of cultural boundaries, as Clarke would some years later. Yet what Childe did was to give this rather vague concept a clear definition in specifically archaeological terms; and to demonstrate how it could be used to interpret the accumulated archaeological data of Europe in a systematic fashion.

**David L. Clarke (1940-1976)**

I will now move on to another British archaeologist, David Clarke, who contributed immensely to the systematic building of a conceptual and terminological structure in order to establish archaeology as a mature discipline. Clarke proposed a purely archaeological theory as compared to his American contemporary, Lewis Binford's anthropological orientation. In this (and in many other ways) Clarke was following Childe. A detailed analysis\(^1\) of his monumental work *Analytical Archaeology* (1968) will show how. It is surprising though that when discussing the history of archaeology in his introduction, Clarke did not even mention Childe,\(^2\) even if he did begin his chapter on 'Culture and Culture Group' with the following words of Childe: "In the classification of relics perhaps the first step should now be to assign them to their proper cultural group" (1978: 299). In fact, in their respective approach to archaeological cultures there was a convergence of views. Although Clarke differed from Childe in the way culture should be defined, both Childe and Clarke conceived of cultures as entities representing the cultural traditions of human groups. "Both adopted classificatory expedients to remove the untidiness in the cross-cutting distributions..." (Shennan 1978: 13).

Clarke, like other British archaeologists at this time, was also beginning to be influenced by the locational analysis and general systems approach of the NewGeography that had developed at Cambridge University. Amongst the major

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2. This notable omission was pointed out by Bob Chapman in his 'notes' to Chapter I in the second edition of *Analytical Archaeology* (1968).
texts of the New Geography were Hagett’s *Locational Analysis in Geography* (1967), Chorley’s *Models in Archaeology* (1967) and Harvey’s *Explanation in Geography* (1969). There are many references to Hagett and Chorley in Clarke’s *Analytical Archaeology*. Clarke also tapped the then developing discipline of cybernetics in his attempt to construct models for archaeological processes. He refers extensively to an elementary textbook by Ashby. According to Doran (1970: 295) cybernetics, the objectives of which are clearly related to systems theory, was not a well-defined or coherent subject with practical and theoretical results to its credit. Yet some of its concepts, homeostasis, positive and negative feedback, oscillation, dynamic equilibrium and information theory, were borrowed by Clarke. Clarke specifically applied information theory in his model of culture as a system, as I shall shortly illustrate.

Archaeologists of this time also had the advantage of the advances made in radiocarbon and other applied archaeological technologies, which were independent sources of relative chronology. This enabled archaeologists to focus on the content of the archaeological record itself. And it was precisely on the "archaeological record" that Clarke focused in *Analytical Archaeology*.

The volume was a "bombshell" (Champion 1991: 130; Malina and Vašíček 1990: 117). It was simply not the kind of text British archaeologists were used to. In Malina and Vašíček’s words:

Clarke’s book had an explosive effect. After such a massive attack, archaeology woke up from her virginal slumbers and all the previous tentative honeymoons were forgotten. Instead of a few recipes, she was given a whole cookbook which has been, and still is, extensively used for meal preparation, even though what is cooked very rarely corresponds to Clarke’s original version.

(1990: 119)
Not all the impressions of the book were quite so positive. When it first appeared, according to Chapman (1979), there were extreme reaction—some ignored the book, others ridiculed it criticizing its jargon. Some scholars said it contributed nothing. Some even described it as un-British! And after the initial outbursts, the book remained little more than a curiosity.¹ Champion (1991 : 131) doubts if many now share such extreme reactions or if many now even read this book.

In an interview with Richard Bradley (1993 : 75) Colin Renfrew tries to analyze the reasons for this non-acceptance. He points out that the theoretical aspects of archaeology were not particularly interesting for British archaeologists at that time. Second, criticism of certain British archaeologists in his introduction to Analytical Archaeology antagonized many. And three, Clarke's language was not easily comprehensible— it was "heavy jargon" (ibid.)².

These objections aside, Clarke outlined with greater clarity than any other scholar of his time the full depth and potential of archaeology. He has left as his legacy an emphasis on archaeological methodology³, awareness of its assumptions and conceptual weaknesses and refinement of procedures. Prime amongst his contributions is the notion of culture. Clarke showed a concern for the analysis of culture for its own sake (something which was largely missing from much of the processual (New) archaeology of the 1960s and 1970s) as we shall shortly discuss).

Also unlike the New Archaeologists and Binford who viewed "archaeology as anthropology", Clarke proclaimed "archaeology is archaeology..." (1978 : 11)—an independent discipline in its own right,

¹According to Shennan (1989 : 831) Clarke's Analytical Archaeology has had virtually no influence whatsoever.

²Jacquetta Hawkes (1968) remarked that Analytical Archaeology did not suit her perception of the way the English language ought to be used.

³It was his earlier, extensive work on Beaker cultures which actually led Clarke to look for new means of resolving classificatory problems involved in processing complex archaeological material.
concerned with "archaeological data which clusters in archaeological entities displaying certain archaeological processes and studied in terms of archaeological aims, concepts and procedures" (ibid.).

The central concern of Clarke, like Childe, was material culture—artefactual entities, their scale, and the processes by which they come into existence and disappear. "The archaeologist's facts", wrote Clarke, are "artefacts and the information observed about their contextual and specific attributes" (1978 : 13). For Clarke material culture was subject to its own rules (Shennan 1989 : 833). Clarke held that material culture residues should be constructed as a system; and only when we can understand it in systemic terms should we attempt to explore the relationship between this system and other behavioural systems. And Clarke pushes ahead with the task of construction of material culture as a system through an understanding of archaeological procedures, entities, concepts, and processes.

Clarke lists three aims or objectives of archaeology. The first of these was, very importantly, the definition of its fundamental analytical entities: attribute, artefact and artefact-type, assemblage, culture and culture-group. Clarke built a schematic hierarchical model of archaeological entities. Attribute and artefact were the lowest level entities and culture the peak. He insisted on a systematic treatment of the four levels before moving on to the last, culture. Clarke devotes separate chapters to the specific analytical entities. Each of these "is composed of aggregates of lower level components and as such they can be treated as micro-systems" (ibid. 150).

As stated above, the lowest level entity recognized by Clarke were the attribute and artefact-type. The essence of higher level entities rests on the definition of attribute, which is defined as "a logically irreducible character of two or more states, acting as an independent variable within a specific frame of reference" (ibid. : 156). Steiger (1971) criticises this definition. He disputes the term "logically irreducible" claiming that if taken literally this would restrict the attribute to the world's four major dimensions: length, time, mass and electric
charge (p. 67). He also contests the idea that an attribute is an independent variable within a specific artefact system. He claims that this is a relativistic concept.

Nevertheless, Clarke's is a good working definition considering that none had hitherto been attempted in such detail. Moreover Clarke clarifies that attributes are of three kinds: 'inessential', 'essential' and 'key' attributes and a detailed analysis of the data will determine which kind of attributes there are. He cautions against,

the intuitive and arbitrary spotting of key attributes in a system, before proper analysis, leads to the arbitrary definition of group boundaries based on 'type fossils'. Such set markers may be ultimately established but the 'typical' or the key attribute must be the terminal result of the analysis -- not the premise of the opening gambit.

(1978 : 155)

Clarke defines the artefact-type as "an homogenous population of artefacts which share a consistently recurrent range of attribute states within a given polythetic set" (ibid. 209). He clarifies that:

We are now concerned not with the static phase pattern of a single attribute within a population but with the cluster of tens or hundreds of different attributes that integrate to define artefacts as complete entities.

(ibid. 208).

Clarke provides insightful examples of truly multidimensional models of attributes within artefact-types, in his analysis of British Beaker Pottery (1979 : 211. fig 51), Hallstatt Swords (ibid. 213. fig 53), and Iron age Fibulae (ibid. 214. fig 54).
The next level of analysis is the assemblage. This entity according to Clarke has a reasonably established definition as "an associated set of contemporary artefact types" (p. 245). But he stresses than an assemblage must be distinguished rigorously from the loose physical or geographical 'aggregate'. Clarke points out that illusory cultures have been created from time to time by "palimpsest distribution mapping" (ibid.) of assemblages of artefact types occurring roughly in one area though not found in contemporary associations. The first question to be asked of several different artefact-types of any distribution map," writes Clarke, "is how many of the types occur together in contemporary association, in what combinations, and on how many occasions" (p. 246). Therefore both the spatial, temporal and contextual aspect of particular assemblages have to be taken into consideration.

Clarke defines culture, which he admits was probably the most important single concept in prehistoric studies (ibid.), as "a polythetic set of specific and comprehensive artefact-types which consistently recur together in assemblages within a limited geographical areas" (p. 247). Clarke's definition was a contrast to the old ('culture brick') models (which assumed that each assemblage was a homogeneous and self-evident group of artefact-types (defined on the basis of the presence or absence of certain types, which were often derived from the assemblages of a type site or intuitively considered to the most important attributes in the definition of a particular culture) with identical distribution boundaries. Where transitional forms occurred at geographic boundaries, the tendency was to equate them with different cultures. The polythetic analysis of culture, on the other hand, allowed for variation of artefact types within a particular area. Clarke admitted that the boundaries of a culture area only occasionally coincide with the boundaries of any specific artefact-type or group of artefact-types, though he specifies that the "distribution pattern is not without regularity, since the density of type distributions do fall away rapidly in frequency of occurrence towards the boundaries of the dispersion area. However, because this gradation is more an exponential than a steady gradient, even the boundary
assemblages of very intermixed neighbouring cultures will necessarily contain a greater number of types from one cultural set and lesser numbers from others" (p. 265-66).

Clarke's was probably the most comprehensive, though complex, purely archaeological statement on the entity archaeologist's call culture. Clarke insists, as did Childe before him, that it was not right to construct cultures from one aspect of material culture alone. Attempts to define cultures on the basis of single pottery types in such a manner that the domestic heavy-duty pottery and the finer wares are put in different cultures, or attempts to define a culture based upon hoards of widely traded and mass produced bronze artefacts, not entirely coinciding in space and time, are examples of the abusive use of the culture concept (p. 248).

Clarke also brings to the fore the notion of the subculture defined as "an infra-structural segment or activity alignment characterized by a specific type complex" (p. 250). He isolated five types of subcultures which according to him register in the archaeological record: ethnic, regional, occupational, social and gender. Each may have a type complex unique to it, albeit only under "certain conditions" (p. 251). He goes on to say that the "artefact-type complex of such subcultures may also include a distinctive linguistic vocabulary, habits, customs, clothing, art styles as well as a specific set of material equipment" (ibid.). He illustrates each one of these examples. For instance, Beaker culture enclaves of late Neolithic Britain have been interpreted as remains of intrusive immigrant minorities maintaining their cultural identity from c. 2000-1500. Clarke recommends that we keep our minds open on subcultures and we need not elevate them to the status of full cultures.

However I think it would not be so easy to delineate the various subcultures in the archaeological record since material equipment is not so easy to slot into such subgroups. For instance, migrants adapt to the society which they enter, and adopt its customs. And occupational, ethnic and gender distinctions may not have any spatial dimensions. A subculture does not necessarily have
every material culture item unique to it—for example, house-form, stone tool types etc., as Clarke said, are uniform across techno-complexes (i.e., a range of cultures). We would expect pastoral enclaves in an agricultural zone to be distinguishable. But only one or two items may be specific to say the Buddhist monks in the iron age Gangetic plains. Gender also creates problems: present day practices do not necessarily indicate that femininity in prehistoric times was displayed in particular modes of dress or ornament. Finally, it is also theoretically possible that a person living in the Gangetic valley in the iron age was a woman, born into a potter's group, and also a Buddhist nun!

Clarke emphasized that the perception of archaeological culture as a complex material phenomenon should not allow one to forget that "the phenomenon was generated by groups of people enjoying territorial areas through time and is the product of social behaviour" (p. 269). This immediately brings to mind Kossinna's classic definition that archaeological culture areas correspond with areas of particular peoples or tribes. The link in this idea from Kossinna to Clarke was obviously Childe. Clarke clarified that it was only through the correct definition of archaeological entities, such as culture was it possible to reconstruct the behaviour of past people.

Also like Childe, Clarke clarified that an archaeological culture cannot be equated with a historical tribe or a racial group or a linguistic unit. He perceived it simply as an archaeological unit. Though he admits that with precise, well defined definitions of archaeological entities and a good archaeological model "we may with a margin of error be able to identify an archaeological entity in approximate social and historical terms" (p. 12). I will shortly take up just how Clarke attempts to do so.

Following his discussion on culture and culture group, Clarke goes on to discuss the technocomplex, "a group of cultures characterized by assemblages sharing a polythetic range but differing specific types of the same general families of artefact types, shared as a widely diffused and interlinked response to common factors in environment, economy and technology" (p. 230).
Clarke discusses three types of techno-complex categories: hunter-fisher-gatherer, pastoral-nomad and agrarian techno-complexes. He attempts to describe these categories in a coherent manner; as also compare and contrast the variety within the techno-complexes on the one hand and between each other on the other. He also studies the relationships between pairs of techno-complex systems (preferring to use the term 'coaction'). Therefore he refers to the hunter-fisher/pastoral nomad system coaction, the hunter-fisher-gatherer/agrarian system coaction and the agrarian/pastoral nomad coaction. For example, in the case of the third study he refers to the symbiosis of various pastoral groups with agricultural groups.

Thus Clarke's archaeological entities are polythetic.

A group of entities such that each entity possesses a large number of the attributes of the group, each attribute is shared by a large number of entities and no single attribute is both sufficient and necessary for group membership.

(Clarke 1978: 36).

Clarke agreed wholeheartedly with Childe in that no group of cultural assemblages from a single culture ever contains, not did ever contain, all of the cultural artefacts (Childe 1956: 33; 1963: 41). And thus it would be foolhardy to take up a single attribute for group membership. However Clarke points out that "although the polythetic interpretation is immensely more realistic and powerful than old trait monothetic models, the fit is still only approximate" (1978: 413). First, because archaeological material is never "fully polythetic"; and secondly, polythetic relationships can be of varying types.

The major argument that Clarke presents is the need for greater sensitivity to variation in hand-made artefacts which could make classification difficult, as boundaries would never be clear. According to Clarke:

For the first time it is possible to see that this awkward variety and range of variation of our entities and their attributes underlies their original
potential to change by mosaic and multilinear
development and not by an agreed mythology of
simple unilinear typologies.

(p. 37)

Clarke correctly observes that such a polythetic model "...not only precludes the
definition of fossiles directeurs but also the construction of maps showing the
precise location of prehistoric cultures and their boundaries, and of pristine and
clear temporal boundaries demarcating different phases (Plog 1975 : 211).

I think Clarke's acknowledgement of the variation in the archaeological
record¹ and his efforts to explain such by formulation of a polythetic model
of artefacts and archaeological cultures is his major contribution to theoretical
archaeology. Klejn however thinks of Clarke's model as too "mechanistic" and
"oversimplified". In an interview with Timothy Taylor (1993 : 278) he says : "It
(Clarke's scheme) viewed the entire material as some mixture, a porridge, a mess
of different elementary cells which have completely equal weight; distinguishing
among them you can then combine elements in order to obtain the units of the
next level, and so on, from each level to the next. This would allow everything to
be done automatically". It could be argued however, that this porridge is the very
reality of the archaeological record.

Clarke also discussed a general systemic model for change in artefact
types and cultures over time (p. 416):

The general dynamic system² model provides a
framework against which the particular changes of
particular processes upon specific archaeological
entities can be studied by postulating and separating
the inherent general changes, providing in addition
a set of appropriate analytical procedures.

¹ Binford, too needs to be given credit for this.
² All archaeological entities, according to Clarke are kinds of systems and hanging entities are
"dynamic systems". A general dynamic systems model serves as a model for entities and their
systems and processes at all levels.
Doran (1970) discusses the main features of the complicated model (complex in keeping with the complexity of topics Clarke was dealing with): One, there is a system which follows a trajectory, depending on the history of the system and on the influence of the environment. Second, regulatory mechanisms keep the variables of the system within limits inspite of major environmental fluctuations. Third, the system is complex and hence not observable in its entirety. To go around this problem Clarke makes use of the notion of the 'Black Box' (1978 : 59-66). In his view, with the application of this notion it was theoretically possible to produce an accurate model of obscure systems "inspite of their shroud". According to Doran in using the concepts and analytical entities of information theory Clarke uses several technical terms "well outside their normal sphere of reference" and this has inherent dangers (p. 292). Doran, a computer scientist, claims that since information theory is highly mathematical, a non-mathematical application like Clarke's may be misleading (ibid.).

In Salmon's (1978) opinion little can be gained by applying systems theory to archaeology, primarily because it is too general a theory and does not specifically address itself to the specific interests of the archaeologists. But despite the apprehensions about the use of systems theory language to archaeology, it did provide new and different ways of looking at the archaeological record.

1 In using this notion one ignores any known structure of the system. Instead, one regards the system as represented by a black box (opaque) within which is hidden the unknown system. The analyst interrogates the black box with an input and observes the response of the box, or the system it represents. In other words, when the analyst has posed sufficient number of questions and answers, he can have inductively discovered major patterns of systems behaviour, assuming it indeed behaves regularly.

2 In the opinion of both Doran (1970 : 292) and Steiger (1971 : 67) it is difficult to submit the kind of archaeological systems considered by Clarke to treatment as a Black Box.

3 Lewis Binford, too was making use of the systems prospective in archaeology though in a different way than Clarke.
In *Analytical Archaeology* after a detailed analysis of archaeological entities at all levels, followed by the formulation of systemic models for archaeological procedures, entities and processes, Clarke finally tried to establish an equation between archaeological entities on the one hand and the people who produced them on the other. As Clarke put it:

> At some stage a statement or hypothesis must be formulated to relate the ranks of these artefact entities with the rank of social, linguistic and racial entities. The range of connection of the archaeological entities must be specified in human terms if their information value is to be maximized.

(1978 : 363)

Clarke was well aware that a simple equation between differently based entities like archaeological 'culture', 'people', 'tribe', 'language' and 'race' is not possible. This is the lesson that had come down from anthropologists like Boas and archaeologists like Childe (though Clarke does not mention either of them, in this context). Clarke begins his chapter on 'Group Ethnology' with a quotation from the American archaeologists Willey and Phillips, "the unit 'Iroquois culture' seems to have been shared by tribes that were not only outside the famous confederacy but in some cases not even affiliated linguistically" (p. 363). Yet this lack of exact correlation between the entities did not mean "there is no correlation whatsoever -- it simply emphasizes the complexity of the relationship" (p. 365). An archaeological culture must have been a product of a group of people, belonging to a certain tribe and speaking a certain language, even though 'culture', 'people', 'tribe' and 'language group' could not have shared exactly coinciding boundaries. Clarke specifies (p. 369) that,

> The archaeological culture maps a real entity that really existed, marking real interconnection -- that this entity is not identical to historical, political, linguistic and racial entities does not make it the less real or important.
In order to assess the implications of his archaeological taxa in human terms, Clarke suggests three lines of investigation. First, the internal approach, i.e., "in having defined our hierarchy of entities with great care we may examine them to see whether their conditions naturally imply certain kinds of correlation. Second, an ethnographic approach; And third, the historical approach, which tried to work out the correlation, or lack of it between archaeological entities and historically documented peoples.

On the basis of the above three approaches, Clarke took up a survey of the culture artefacts of sixteen hunting-fishing-gathering tribes of the Californian coast of America. The survey was based on Drivers comprehensive ethnographic study of these tribes. The sixteen tribes (including four confederate tribes) spoke between them four language groups, twelve languages and sixteen dialects. To analyze the sharing and distribution of artefact-types amongst neighbouring tribes with diverse languages and cultures, Clarke took up a central group, the Yurok (speaking an Algonkian language) and made a contoured map linking Yurok elements with those of every tribe of that area. Clarke surprisingly found that there was a strong link between geographically distinct but linguistically akin tribes. In the second analysis, each tribe was connected by an arrow to that tribe with which it shared maximum artefact-types. Once again tribes speaking one language though separated by many miles and non-linguistically related groups, seemed to be linked. Clarke then took several tribes of the particular area and plotted exponential fall-off of their artefact-types beyond their own boundaries. The model once again predicted "unusual local, linguistic or topographical ties or barriers" (p. 389).

Another study was undertaken to contrast the Pueblo farming tribes and the nomadic Apache/Navaho tribes of North America. Two experiments in this case illustrated the severe division in language and culture of the two tribes. It was a case which illustrated that complementary techno-complexs do not necessarily accelerate converged developments.
Clarke concludes his evidence of the study of the ethnographic tribes as such:

In the tiny sample of tribes studied, the evidence supported the view that individual site assemblages within a tribal area express polythetic variations on the total tribal assemblage, with a customary level of 65-95 percent of shared elements or types linking pairs of assemblages from a single tribe. There does not appear to be any confirmation of the concept of a focal 'tribal hearth' within which all the tribal elements appear and thence decline rapidly in concentric zones. Beyond the tribal boundaries the tribal artefacts and elements still occur amongst other tribes but these fall-away in an approximately exponential decline in all directions from the tribal margins. Exceptions to this exponential decline do occur, with shared elements even reaching 87 percent between tribally and linguistically unrelated assemblages in one exceptional case ... Such exceptions are clearly related to unusually good or bad lines of intercommunication and interconnection introduced by unusual topographic, linguistic, historic or socio-cultural ties and barriers.

(p. 394)

Clarke's Analytical Archaeology was, thus a very important work of its time; And, in fact, till today remains unparalleled, along with Childe's Piecing Together the Past (1956), in respect of its contribution to the field of theoretical archaeology. It made important contributions in terms of refinement of archaeological methodology and explanation of various archaeological
classificatory terms with 'culture' being the prime amongst them. *Analytical Archaeology* represented something quite different from Binford's *New Perspectives in Archaeology*, both of which were published in the same year, and hailed as ushering in a "New Archaeology". This is primarily because, as Stephen Shennan (a former student of Clarke) put it, *"Clarke took culture seriously"* (1989 : 833) [Emphasis mine]; and Binford most certainly did not. The culture concept was central to the entire body of *Analytical Archaeology*. Clarke, like Childe, took up the culture concept as the organizing unit of analysis in archaeology, refining it further. Shennan in a review of Clarke and Binford's 1968 books, "21 years on", points out that "*Analytical Archaeology* represents... on the one hand the last gasp of Childean cultural tradition, on the other a groping towards the future of a new cultural archaeology ..." (ibid.).

It is indeed ironic that the two scholars, Childe and Clarke, who provided such valuable insights into the entities of archaeological analysis and are amongst the very few archaeologists who talk about culture in specifically archaeological terms, did not have many followers to carry on their work. Tringham notes about Childe that no research student at the University of Edinburg or the Institute of Archaeology "took on the burden of Childe's theoretical models nor his stature of synthesis; no one followed up or tested his interpretation" (1983 : 99). The same holds true for Clarke also (though it must be remembered that he died young). Petrequin's (1988) is one of the very few studies which actually operationalizes the ideas of *Analytical Archaeology*. His work on the cultural trends in the late Neolithic of Eastern France, documents the changing frequencies of a whole series of different artefactual attributes and relates them both to one another and to the social, economic and ecological information derived from other sources and approaches.

Binford's *New Perspectives in Archaeology*, unlike *Analytical Archaeology* was a success, and over the years, in Shennan's words : "taken over the subject, first in the United States, then in Britain and other English speaking
areas and increasingly in the rest of the world" (1989 : 831). Clarke was initially totally ignored by Binford and his followers in America and later referred to only as a disciple of New Archaeology — a false claim that ignored the distinctiveness of Clarke's work.

It is precisely because of the distinctiveness of Clarke's work and his approach to the culture concept in archaeology that I have considered him with Childe in Part (A) of this chapter, rather than with the "New Archaeologists" like Binford in the following Part (B).
Lewis R. Binford, is credited with having begun the campaign for "New Archaeology" in America in the 1960s. The term "New Archaeology", though, was not coined by Binford but by his critics and students who came to be known as the "Young Turks". The latter included Stuart Struver, James Hill, Leslie Freeman, William A. Longacre, Robert Whallon, Kent Flannery, James Brown and Sally Binford.

Binford and his students presented unconventional papers at meetings of the society of American Archaeology and American Anthropological Association in the early 1960s -- papers which reflected their growing discomfort with traditional archaeology. It was at this time that a number of Binford's articles began appearing in print, each one an attack on traditional archaeology (1962,63,64,65,67). And in 1968, with the publication of the Binford edited New Perspectives in Archaeology, the "New Archaeology" had finally arrived. In a retrospective, (1989 : 831-35) twenty years after its publication, Shennan writes that: "... the kind of archaeology represented by New Perspectives has taken over the subject, first in the United States, then in Britain and other English-speaking areas and increasingly in the rest of the world as well". New Archaeology in many respects did set the agenda for a whole generation of research1. As Alison Wylie said (1989), the New Archaeology of the 1960s became everybody's archaeology in the 1970s and few wanted to be left out of the new theoretical movement of those years.

The question that needs to be raised is: Was "New Archaeology" really new? Did its advocates really contribute "revolutionary" ideas, or did they simply reinterpret old ideas? Some old timers like Waltar Taylor saw nothing new in the New Archaeology and dubbed it as "old wine in new skins" (1972)

1 According to Redman (1991: 295), "although most archaeologists did not claim to agree with all aspects of New Archaeology nor could more than two or three people agree on what it was, virtually no one rejected it outright" (emphasis mine).
Graham Clarke too stated that many of the ideas expressed by the New Archaeologists were merely rediscovered by them. But Binford argued that New Archaeology was indeed revolutionary in the sense that the ideas and theories of science though old were being applied to the field for the first time (1968 : 120).

To my mind, most of the ideas of Binford and his students that were clubbed as "new" were really not so new. A brief research on the people and ideas that influenced Binford during his student years (University of North Carolina, 1955-57 and University of Michigan, 1957-61), and later as an assistant professor at the University of Chicago (1961-65) will argue for this.

Binford came into archaeology from two previous academic disciplines, forestry and wildlife, and anthropology. While studying anthropology at North Carolina, he began to realise that "...if anthropology was going to cope scientifically with the problem of why cultures changed, the anthropologist had to have data with time depth. I began to go for regular weekend field trips and archaeology became my commitment" (Binford 1968 : 1). In 1957 Binford transferred to the University of Michigan for further graduate work in anthropology and archaeology under James Griffin, Alfred Spaulding and Leslie White. As mentioned earlier, in the United States, prehistoric archaeology (as distinct from classical archaeology) has been taught primarily in the departments of anthropology. Binford's anthropological background, thus, led him to give archaeology a strong anthropological orientation. And his first paper was aptly titled 'archaeology as anthropology' (1962). It discussed archaeology's potential contribution to the field of anthropology.

Walter Taylor's *A Study of Archaeology* (1948), undoubtedly had a strong influence on Binford during his student years; and in his words: "It still sits on my bookshelf - full of stratified marginal notes, reflecting something of the changes in my thinking since 1955" (1968 :1). Taylor's was a polemical work that assessed on the one hand the aims and accomplishments of archaeology upto

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1 It was only in the 1980s that attempts were made to separate archaeology from the departments of anthropology (Watson 1995 : 684).
Taylor examined the difference between the old and the new interests and as a remedy to the discrepancy suggested an alternative, the conjunctive approach. This emphasized that the working of culture was the main subject of archaeological inquiry; and culture was conceived of as systemic or integrated. Taylor sought to view archaeological cultures not as a collection of traits, but as systems.

Taylor further stressed the study of functions and functional connections in a context. According to Willey, he focused on the actual finds in an archaeological context, in and on the ground (1994:64). This was carrying things beyond what had been customary -- beyond the traditional typological and classificatory arrangements of the data. In Taylor's words: "Culture is integrated to such an extent ... that cultural manifestations cannot be truly depicted or understood apart from their contexts -- with the corollary that construction of cultural context is an absolute requisite for anthropological archaeology (or archaeological anthropology)" (1972:29). He further emphasized that "...context is needed because cultural isolates can neither be understood nor properly used without it" (ibid. 31). This was the old Boasian doctrine which Taylor stressed repeatedly, but which unfortunately, Binford and the other New Archaeologists skipped over.

Taylor's point that had immense appeal for the New Archaeologists was that archaeological information can be used to generalize about the nature and

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1 British functionalist views were being popularized in American anthropology departments during the 1930s and 40s, particularly at the University of Chicago and Yale (Malinowski had in fact taught at the latter University from 1938-42). As Leach pointed out, Binford's remark that "human behaviour is the by-product of the interaction of a cultural repertoire with the environment" may be proto-typical of the "new" archaeology, but to a social anthropologist, it read like a quotation from Malinowski when functionalism was at its peak (1973 : 761-62). This view may be extreme, but Renfrew also states that to examine connections between sub-systems as in systems theory "is, of course, simply a statement of anthropological functionalism that differing aspects of culture are all interrelated"(1972 :24).

2 American archaeologist Willey too, by turning towards settlement patterns tried to broaden the context of archaeological information.
working of culture. Taylor (1948: 156-67) embraced the establishment of general laws as being the supreme goal of archaeology (as well as ethnology). He saw his own conjunctive approach as being the intermediate step towards this goal. Thus the adoption of explicitly nomothetic goals by New Archaeologists did not represent a major break with the past. In fact the level of critical awareness Taylor created has been the backdrop in which New Archaeology grew (Leone 1972: i).

And in Taylor's own words:

Much of the "new archaeology" is operating with a conceptual scheme which is virtually identical, in its basic ideas, with that anticipated in A Study of Archaeology. What does surprise me, however, is that it has taken the many years since 1948 for the conceptual scheme to take hold!

(1972: 29).

Binford, even as he accepted many of Taylor's ideas, did not accept his reduction of culture to ideas or mental constructs. According to Taylor, a group of people were said to be members of the same culture because they shared the same mental template. However, he concluded that while culture was ideational, many aspects of culture were reflected archaeologically. And, therefore, culture was something which could be inferred from material remains. Taylor's 'normative' views on culture were shared by archaeologists like Willey and Phillips and Irving Rouse. Binford rejected this 'acquatic view' of culture in which:

Culture is viewed as a fast flowing stream with minor variations in ideational norms concerning appropriate ways of making pots, getting married,

1 Willey and Phillips (1958: 5-6) likewise viewed explanation as the highest goal of archaeology although they believed that, as of 1958, nothing of importance had been achieved at that level. In the writings of anthropologists Leslie White (1945), historical activities tended to be viewed as essentially descriptive, while the ultimate aims of archaeology are characterized as being processual, that is to say, concerned with the formulation of general laws of cultural behaviour.

2 Rouse in an interview with Peter Siegal narrates how he as a normative archaeologist had become one of the "whipping boys" of the New Archaeologists in the 1960s (1996: 676).
Changes observed in the archaeological record, according to Binford, were the product of a variety of different reasons, and not merely of the fact that different people in different places have different ideas about the proper ways of doing things (Shennan 1978 : 12). In opposition to the 'acquatic view' of culture, Binford offered an approach in which "culture is not reduced to normative ideas about the proper ways of doing things but is viewed as the system of the total extrasomatic means of adaptation" (1972 : 205). We will revert to this point shortly.

According to Binford (1989 : 686) the real excitement at Michigan during his student years in the late 1950s and 1960s was in the department of anthropology where there were White, Service and Beardsley; and Steward, though retired from the faculty by the time Binford joined continued to maintain a hold, well into the 1960s, via his ideas, particularly on 'cultural ecology'. And Binford was no doubt influenced by this idea. In his words:

The comparative study of cultural systems with variable technologies in a similar environmental range or similar technologies in differing environments is a major methodology of what Steward has called "cultural ecology", and certainly is a valuable means of increasing our understanding of cultural processes.

(1962 : 12)

At Michigan, Binford was particularly influenced by White. When he was his student, "theory became a meaningful word" (Binford 1972 : 8). It appears to me that White influenced Binford's theoretical position in two major ways:
(1) Binford's perception of culture as systemic had a lot to do with White's influence (besides of course Taylor's). For White, as discussed earlier, the culture of mankind as a whole, or of any people or group of people or area was a system with interrelated, interdependent subsystems such as technology, social organization or philosophy; with technology playing the most important role in culture change. From White Binford learnt that:

Culture was not some ethereal force, it was a material system of interrelated parts understandable as an organization that could be recovered from the past, given the language of Spaulding.¹

(Binford 1972: 8)

Binford also borrowed from White his definition of culture, i.e., culture as man's extrasomatic means of adaptation to his environment. It was implied by this definition that it was not man who moved and did things, but culture(s). Therefore culture had agency.

(2) The philosophy of science, as stressed by White, is echoed in Binford's work. He too, like White, has been against collection of facts for themselves and has insisted on generalizations and the development of a scientific study of cultures. Referring to White's classes at Michigan, Binford writes:

We were searching for laws. Laws are timeless and spaceless; they must be equally valid for the ethnographic data as well as the archaeological data. Ethnology and archaeology are not separated by a wide, unbridgeable gap.

(ibid.)

Thus, Binford tried to realize a synthesis of the methods of White and Spaulding to provide some meaning to the "endless taxonomic schemes of the archaeologists" (ibid. 9).

¹ Spaulding taught how with the aid of statistical techniques the organization inherent in an archaeological record could be exposed and explained.
Binford was also greatly influenced by the philosopher of science, Carl Hempel (to the works of whom he was introduced by his teacher Spaulding). In 1942, Hempel wrote an article, 'The Functions of the General Laws in History', in which he claimed that historians explain the events of human history in the same way in which natural scientists explain physical events. For logical positivists like Hempel a causal explanation of an event involves deducing a descriptive statement of the event from one or more general laws in connection with certain unique statements about initial conditions. The procedure was formalized by Hempel as the deductive-nomological (D—N) mode of explanation.

Binford accepted the D—N model of scientific explanation for the testing of conclusions derived from archaeological data. This was unfortunate because the D—N model requires laws, and the archaeologist, unlike the physical scientist, does not have a ready stock of laws with which to construct explanation (Salmon and Salmon 1979 : 64). Moreover the D—N model based on general laws is too simplistic to deal with the complex nature of the archaeological record. Hempel, himself, in his classic 1948 paper, explicitly acknowledged that not every scientific explanation could conform to a deductive model (reprint in Hempel 1965 : 250-51). It is ironic that Binford's scientific procedure which depended "exclusively" upon the Hempel model was, already by the 1950s, a philosophical approach under attack! Binford seemed to be running a little behind time.

1 Binford's students too accepted this model. According to Lambery-Karlovsky, "The D—N model became, in fact, a commodity reduced to a fetish; it become something quite fashionable which without presented alternatives was responded to by 'I'll buy that!'" (1989 : 6).


3 Not all archaeologists were impressed by the D—N model. Many, early on, shared the sentiment "Unfortunately, his [Binford's] students and followers have been over impressed with the great simplicity and apparent logical power of the deductive—nomological paradigm and scientific explanation and have pushed the discipline in a questionable manner (Sabloff, Beale and Kurland 1973 : 105).
Finally, Binford's work was aided, in a major way by the expansion of computer methods and analysis and the coming of radiocarbon dating.

Looking back on the various influences on Binford, it seems that his New Archaeology was neither radical or revolutionary. Binford simply put together Taylor, Spaulding, White and Hempel to build his archaeological methodology.

What Binford was doing in America in the 1960s was parallel to some extent with what was happening in Britain as exemplified by David Clarke's *Analytical Archaeology* (1968). As Paddayya (1990: 5) pointed out, both Binford and Clarke were principally concerned with the idea of culture process. And that both were taking recourse to the systems theory to see "multiple causal links between the structural components of cultural systems themselves and these of the ecosystems in which they were placed" (ibid.). However Binford laments in his 1987 interview with Renfrew that Clarke's book though focusing on pattern-recognition work never questioned the basic premises of traditional archaeology (p. 690), i.e., had not departed from the normative-additive concept of culture. Binford could not have been more wrong.

*Binford on Culture*

While at the University of Chicago, which he joined as an assistant professor, Binford published his first major article "Archaeology as Anthropology" (1962). The basic ideas encapsulated in the article were:

1. The archaeological record represents the structure of the total cultural system
2. It is possible for the archaeologist to reconstruct total extinct cultural systems, i.e. it is possible to witness the dynamics of the living system as it existed in the past

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1 In an interview with Renfrew (1987: 686) Binford talks about how in the early 1960s he had been working analytically on many "cultures" in Midwest America that were defined almost exclusively on mortuary data but with the introduction of C14 technology he realized that what he was looking at was probably not independent "cultures" but components of the same system. And, this is when, according to him, he began to worry about his methodology and began to look for new ways of obtaining information about the past and about matters other than chronology.
Change in the total cultural system must be viewed not as a result of diffusion of ideas, migration of people etc. but in the context of an adaptive environment as also society.

In this article three major functional subclasses of material culture were discussed: technomic, sociotechnic and ideotechnic (a'la White).

In 1965, appeared "Archaeological Systematics and the Study of Culture Process", in which Binford for the first time offers an alternative approach to the normative view of culture, which he considers inadequate for the generation of fruitful explanatory hypotheses of cultural processes. Instead,

An approach is offered in which culture is not reduced to normative ideas about the proper ways of doing things but is viewed as the system of the total extrasomatic means of adaptation. Such a system involves a complex set of relationships among people, places and things whose matrix may be understood in multivariate terms. (p. 210)

Binford thus adopted an ecosystemic conception of culture. Culture was now referred to in terms of "extrasomatic" and "adaptive" (under the influence of White as discussed). Culture was wrongly conceived of as an "abstract mechanical relation" (Hodder 1991: 32) not situated either in place or time. Culture was too far generalized and removed from any historical or material specificity. Binford's approach was thus different from Clarke's in this respect. The central focus of Clarke's Analytical Archaeology, as pointed out earlier was material culture.

Within the New Archaeology's framework of the culture concept, the individual had no independent role. The ecosystemic view ruled out human inventiveness and innovations as forces capable of bringing about change. Obviously individuals did make decisions but since they could not be recovered by archaeologists, they found it more useful to study and understand the system "whose behaviour is detectable over and over again" (Flannery 1967: 119).
The New Archaeologists stressed on the systemic integration of cultures. In lieu of the previous "shreds and patches" and trait-list conceptions, cultures were now seen as consisting of not merely component elements but their interrelationship as well. In Struver's words:

For the systemic theorist, culture is made up of parts, structurally different from each other, but articulated within the total system. More broadly, culture and its environments represent a number of articulated systems in which changes occur through a series of minor, linked variations in one or more of these systems. A major objective of archaeology is to understand the linkages between parts in both cultural and environmental systems as reflected in the archaeological record.

(1971:10)

As adaptive systems, cultures were seen to change in the direction of equilibrium. For example, in Flannery's systems model, self-regulation within the sociocultural system functions to keep the variables in the subsystem in balance so that they do not threaten the existence of the system (1972:409). Similarly Binford states: "We can ... expect variability in and among components of a system to result from the action of homeostatic regulators within the cultural system serving to maintain equilibrium relationships between the system and the environment" (1972:107). But when the equilibrium was upset by external factors like environmental changes, further adjustive changes rampified through the cultural system. Feedback mechanisms in cultural systems thus operated both negatively -- toward self-correction and equilibrium and positively -- toward equilibrium and directional change.

Binford and his followers also criticized the anti-scientific, anti-theoretical and particularistic approach of traditional archaeologists. Their aim was to make archaeology scientific. The scientific method included observation (on the archaeological record), hypothesis formulation (hypothesis linking
archaeological remains to events or conditions in the past that produced them) and the testing of hypothesis. And if validated, such hypothesis would be raised to the status of laws regarding the role of archaeological remains in the functioning of extinct cultural systems. According to Binford:

Most of my own efforts and those of my colleague in the "new archaeology" have been directed towards the disproof of the old principles of interpretation which gave the ring of plausibility to traditional reconstruction's and interpretations. We seek to replace these inadequate propositions by laws that are validated in the context of epistemology of science, so that we may gain an accurate knowledge of the past.

(Binford and Binford 1968 : 120-21)

And at another place Binford writes:

... Further if we hope to achieve the aim of reconstructing culture history, we must develop means for using archaeological remains as a record of the past, as a source of data for testing propositions which we set forth regarding past events, rather than as a record we can read accordingly as a set of a priori rules or interpretative principles whose application allow the skilled interpreter to "reconstruct" the past. We know much too little about both archaeological data and process of cultural development to make "reading the archaeological record" anything but a shallow and suspicious pastime.

(ibid. : 12)
Some New Archaeologists of the later period (we will call them the late-phase New Archaeologists) even declared themselves to be part of a Kuhnian revolution\(^1\), insisting that their programme represented a sharp discontinuity, a paradigm shift from traditional archaeology (Gibbon 1989 : 83\(^5\)).

New Perspectives in Archaeology was published in 1968\(^3\). Most of its 16 essays (almost all of which are individual case studies with a social, economic and ecological orientation) dealt primarily with the question of variability and change in the archaeological record\(^4\). All try to "... explain variability scientifically, rather than by conjecture or by "hunch" with the ultimate goal being "the formulation of laws of cultural dynamics" (Binford 1968 : 27). The underlying assumption was that all archaeological site remains are patterned in keeping with the patterned behaviour of the members of the extinct society that utilized the site. Thus the structure and mutual co-variation of classes of archaeological data at a site could be revealed; which would further allow them to offer testable hypotheses concerning the social organization of the extinct society.

Sally Binford's article in New Perspectives (1968 : 49-69) tried to offer explanations other than the arrival of a new set of people or diffusion to explain change in the Mousterian artifacts from sites in Palestine. Variability, according to her, must be examined within a functional frame of reference and that

\(^1\) Kuhn (1962) rejects the view that science progresses through accretion; instead he emphasizes the revolutionary process by which an older theory is rejected and replaced by an incompatible new one. Kuhn uses the term "paradigm" rather than theory to denote what is rejected and replaced during revolutions.

\(^2\) However Binford himself criticized the extreme scienticity of these later-day New Archaeologists (1983 a : 14-17).

\(^3\) The result of a one-day symposium organized by Binford and his "mafia" (as Binford and his students came to be called) in 1966 to present their "new" ideas.

\(^4\) Binford since the early sixties had become concerned with the meaning to be given to variability in the archaeological record, specially the variability associated in European Mousterian industries by Francois Bordes.
variability between assemblages at the same time may represent "activity variants" rather than "culture change" or occupation by bearers of different traditions.

Deetz (1968: 41-48) discusses how the archaeological record can inform us about residence and descent rules in a family but he is not very successful in this effort and admits that "inference of descent from archaeological data, at least in terms of patterning, is a much more difficult and complex problem" (p. 47).

Longacre (1968: 89-102) put forth his studies of Carter Ranch site in East Central Arizona. The site was excavated by him over a period of two field seasons, 1961 and 1962. Particularly significant are Longacre's study of pottery designs and the inferences to be based on them regarding social groups and inheritance:

(a) The pottery designs (175 analyzed) were clustered in terms of two major architectural units.

(b) The clustering of designs in rooms, trash areas, and burials argues for the localization of female potters in architectural units at the site over a period of several generations.

(c) This suggests that inheritance was probably in the female line.

These inferences were based on one assumption, that potters were women, even though this assumption did not follow from the archaeological remains. Stanislawski (1973: 117-22) points out the following defects:

(1) Conclusions were drawn on the basis of the untested model of matrilocal and/or matrilineal group/s.

(2) Though explanations were based on analogies from traditional ethnographic works, Longacre claimed that such concepts could not have been reached without his use of systems analysis (1972: 52).

(3) Many of Longacre's inferences were incorrect. For example, he assumed that the findspots of the potsherds are the same as the loci of production—but discard locus is not necessarily the same as production locus.
Thus Longacre's argument was not deductively valid. It was an attempt "...to support a hypothesis by making plausible (but not deductively valid) chain of reasoning from the hypothesis to observationally available data" (Morgan 1973: 272).

The Late-phase of New Archaeology

Guy Gibbon (1989: 62) makes a distinction between the early (1962-70) and the late phase (1970-78) of New Archaeology. By the late phase, archaeologists had come to regard their programme as "explicitly scientific", "positivist" and "anti-empiricist" (ibid., 83). Binford however became critical of some of the later development as we shall presently discuss.

In the early 1970s some important works were published which were widely cited by the New Archaeologists. Amongst these were the writings of Fred Plog and the publication of Explanation in Archaeology (1971) by Patty J. Watson, Steven A. Le Blanc and Charles L. Redman. Plog's M.A. thesis Archaeological Survey: A New Perspective (1968), though never published was greatly praised by the Binfordians. Plog opposed the strict inductivist research design and presented a new form, one he believed would place archaeological research in its proper "new posture". Plog argued for the use of survey data in problem-oriented research and clarified the writing of an archaeological research design using hypothesis formulation.

Binford, however viewed Plog's works particularly his article "The Explanatory Research Design" (1970), as also Watson, Blanc and Redman's Explanation in Archaeology (1971) as setbacks in his attempt to develop a sound archaeological methodology (1983 a: 14). Criticizing Plog, he writes:

... all the test implications are inductively reasoned stipulations of meaning. There is no argued

1 In this phase New Archaeology was under severe attack. It does not mean it was dying out.

2 It was based on a work done by William Longacre and Mark Leone in the upper little Colarado area of Arizona.
theoretical position, nor are there any hypotheses deduced for testing. Test implications become expanded operational definitions. No consideration is given to the problem of using the archaeological record as a meaningful fact, and there is no recognition of the differences between researching the character of the archaeological record and the historical processes of the past. Nevertheless Plog's work has served as a model for many of us as to how "new archaeology" should be done.

(ibid. : 15)

This criticism of Plog's work could well hold true for Binford's own work.

Explanations in Archaeology (1971) was criticized both by Clarke and Binford for trying to arrive at a scientifically based framework for archaeology (Watson, Le Blanc, Redman (1971 : xii). Clarke (1972 : 237-39) states that it is a mistake to impose the philosophy of any one discipline on that of another. For example, principles of reasoning employed in mathematics do not hold good for chemistry. What, according to Clarke, is required is internal analysis (with external aid) and explicit development of valid principles of archaeological reasoning.

Binford claims to have found explanations in archaeology as "very frustrating" (1983 a : 15). And adds that there is no way to achieve the programme laid out by Plog or Watson, Le Blanc and Redman as long as the Archaeological record is seen as the source of information for use in testing propositions about dynamics. When any proposition links the dynamics of the past to the static derivates remaining in the present, there is no way of testing such a linkage with only static facts.

(ibid.)
Binford, therefore, by the early 1970s was becoming increasingly critical of some of the arguments about deduction and hypothesis testing. Assessing the work of the 1960s, Binford in an interview says that New Archaeology quickly dismantled the methodology of traditional archaeology but could not offer a strong alternative to what had been dismantled (Renfrew 1987:697).

You had a programmatic posturing coming out of ecology, geography, sociobiology, or some place else, but there was not the fundamental inductive research needed to put in place a strong alternative to what had been knocked down. There was clear inertia here; people didn't know what to do.

(ibid.)

( Years later Binford took upon himself some of the blame for what was happening [1983b:107] but added, "I should like to think I am not entirely responsible").

Binford had by the late 1970s and early 1980s come to the realization that he had really nothing very "new" to offer by way of methodology. He also came to realize that the archaeological record is static though the archaeologist is interested in the dynamic nature of the past, i.e., how people lived, what they ate etc. This then was the fundamental problem: how to make inferences from statics about dynamics. Binford became convinced that one needed to study processes actually occurring in the present. Ethnoarchaeology seemed to him "... to be the only chance we have for the development and perfection of methods of inference dealing with humanly generated artifactual material" (ibid.:104). It was only by making observations in the present that archaeologists would be able to construct a "Rosetta stone" with which past material culture could be translated.¹

¹ However Wylie (1989:107) in criticism of Binford's approach has correctly pointed out that: "it is simply indefensible to assume, a priori: that all aspects of the past are accessible if only the appropriate actualistic research is undertaken; the available evidence makes it clear that there is no determinate Rosetta Stone code to be cracked that will provide comprehensive access to the past".
Binford's Nunamiut Eskimo ethnoarchaeological data revealed that a single group of people do not generate sites which are internally homogenous. The study of site structure i.e., the spatial distribution of artifacts, features and fauna, revealed different patterns of assemblage variability which could be understood in terms of different functions performed at different locations. For example, at the Anavik springs site complex of the Nunamiut Eskimos in Alaska, three different types of locations (separated by only a km.) were identified where different kinds of functions were being performed- (1), hunting camp (including a specialized 'lovers' camp) (2) A 'kill' site with specialized areas for butchering (3) A series of stone caches where meat was stored. All the locations were components of the same major task (i.e., the exploitation of caribou) and were used by the same group of people. According to Binford:

There was no question that all the sites I observed and all the events I described pertained to the same ethnic group and were performed by a limited number of known men who did not change their culture from one episode to the next. My Nunamiut study ... was a demonstration that there was a substantial variability in the behaviour of the same individuals, resulting in very different archaeological remains from site to site.

(1984: 178)

Another good example of "situational variability" is found in Binford's, "An Alyawara Day : flour, spinifex gum, and shifting perspectives" (1984: 157-82). Here, he discusses differences in the several descriptions of resin processing by the Alyawara Australian aborigines, as another example of situationally variable behaviour.
In the 1970s, in the course of his ethnoarchaeological work Binford adopted a new phrase 'middle range theory'. It involved discovering in the living world general correlation's between static entities, of a sort that might survive in the archaeological record and systemic processes that do not. The correlation's thus permitting archaeologists to infer unobservable processes that went on in past times on the basis of material remains.

Binford in his ethnoarchaeological studies at this stage, has not, however, gone beyond the understanding of culture as man's extrasomatic means of adaptation (see 1984 : 177) or of culture as systemic. So for him:

One of the most important lessons to be learned from my enthoarchaeological research among the Nunamiut Eskimo is that one has to conceive of all their sites as part of a larger system ... archaeologists must identify each type of behaviour that took place at each site they find and then begin to fit the pieces into place to make up a prehistoric system ... "


Even his commitment to the scientific method was as strong as before. In Debating Archaeology (1989) Binford writes: "The New Archaeology was dedicated to the scientific growth of knowledge; this goes on today in spite of much opposition" (p. 24). And further defending the use of the deductive approach in archaeology he writes:

There we never a claim for the absolute priority of deduction over induction, as many critics have

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1 According to Binford as it became clear that the strategies to both the development and testing of archaeological theory could not be applied to the development and testing of general theory, the use of the term 'archaeological theory' began to seem increasingly ambiguous and thus the adoption of 'middle-rang theory' (1983 a : F.no. 5 : 18). The underlying assumption, of course being that objective yardsticks or instruments of measurement could be obtained for past systems and their archaeological residues (Binford 1978 : 45).
asserted. We fully acknowledge the crucial role of induction, while at the same time we realized that inductive arguments were not verifiable by simple reference back to the manner in which they might have been constructed in the first place.

(ibid.)

**New Archaeology in India**

In India, by and large (till about the 1960s and in fact, even till date) all discussions and interpretations of archaeological data have been mainly on the descriptive level. A few scholars like Malik tried to create awareness regarding strengthening the theoretical base of Indian archaeology (1968). According to Malik, "the time is now ripe for the sophistication of our discipline since ... (it is) at a 'naive' theoretical stage" (p. 14) and no attempt has been made to break away from the old mould of the 19th century concepts: "Is Soan a separate culture?" (p. 33). Malik's *Indian Civilization: The Formative Period* (1968) aims "... to stimulate interest in the development of different approaches to the study of archaeology rather than to present any definitive results" (p. 14).

In India, New Archaeology made its impact in about the mid-1970s. H.D.S. Sankalia chose the New Archaeology and its relevance to India as the subject of the D.N. Majumdar lectures (1974). Dr. Sankalia examined in detail the writings of Binford and Clarke and their relevance in the Indian context. These lectures were subsequently published as a book (Sankalia 1977). Dr. Sankalia also introduced recent advances in theoretical archaeology to his students at the Deccan College, Pune (Paddayya 1990: 43). Large scale excavations were also undertaken by Sankalia, Dhavalikar and Ansari at the site of Inamgaon to understand the chalcolithic phase of Western India from a "processual point of view" (ibid. 47). "In our own way", says Dhavalikar the New Archaeology was applied to excavations at Inamgaon and it led to "interesting results" (1979: 36). Dhavalikar also applied the settlement pattern studies to the entire chalcolithic period in Maharashtra. Through its application he tried to work out why the
concentration of Chalcolithic sites in the Tapi valley is not on the banks of the Tapi itself, but on the tributaries, as is the case today. The results were indeed illuminating.

Dhavalikar also tried to infer some of the social organizational aspects of Chalcolithic Inamgaon through the application of the conjunctive approach. On the basis of structured and probable sex differentiated activity areas he thinks that "Late Jorwe was a polygamous society" (1988 : 24). Although it was no doubt, a bold attempt at reconstructing social archaeology but there is not sufficient archaeological evidence to reach such a conclusion.

In 1986, Deccan College, Pune conducted a series of lectures on New Archaeology. Binford was invited for the programme. In 1988, The Indian Council of Historical Research, New Delhi also organized a seminar on 'New Archaeology and India'. Unfortunately it turned out to be on everything but New Archaeology!

Moorti (1990 ; 1994) tries to apply the methodology of processual archaeology (the systemic approach) to the megalithic problem in South India. He attempts to study the megalithic in the context of their environmental setting. Moorti (1990 : 1-64) also attempts to decode the organizational aspect of the megalithic period through a study of the available material evidence which, following Binford (1962 : 219-20) he divided into three major functional sub-classes, viz. technomic, socio-technic, and ideo-technic.

K. Paddayya has written an insightful book on New Archaeology from outside the Anglo-American world (1990). And Archaeologists like Paddayya (1982), Murty (1985), Raju (1988) have attempted to adopt, in their studies on stone age culture, some insights of New Archaeology; prime amongst them being of viewing prehistoric cultures as adaptive systems as also the adoption of the settlement-system approach to the archaeological record (on the basis of ethnoarchaeological models etc.).
Problems with New Archaeology:

The stir created by the New Archaeologists in the 1960s and early 1970s did not result in any substantial gains. "In my opinion", writes Binford, "the New Archaeology was something of a rebellion against what was considered sterile and non-productive endeavors by archaeologists. Rebellion cannot continue simply for rebellion's sake" (1977: 9).

Walter Taylor in "old wine and new skins" (1972: 28:33) worried about the lack of cultural contexts in the work of New Archaeologists. Impatient to get on with their hypothesis-testing, they did not provide either themselves or their readers "the contexts which alone can set their tests and results in an appropriate and necessary relevance" (ibid.: 31).

Taylor also questions the definition of culture as man's extrasomatic means of adaption (ibid.: 32). This definition is fundamentally flawed and we have to be extremely wary about using adaption to the environment as the basis for explaining society and culture. As Hinde points out, a "fit" between societal practices and the environment does not necessarily imply selection for these particular practices. "At most it implies that man's propensities are such that, within limits, sociocultural structures can accommodate to circumstances. But, within limits, that is little more than a truism" (1987: 162). According to Bargatzy (1984: 399-415), "adaptation", one of the key concepts in current anthropology has outlived its usefulness and should be abandoned.

The adaptive view of culture adopted by New Archaeologists gave little or no emphasis to individual creativity and individuality. Ignoring the role of human agency in culture change is one of the major limitations of

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1 In this particular article Taylor examines his own study of 1948 and the theoretical innovations of the New Archaeology in the 1960s.

2 There were of course exceptions like Plog. According to him a concern with the individual represents "a necessary realization that the items of material culture with which we work were made and used by individuals. Consequently, the pattern in the distributions of artifacts we study are there because individuals in particular but variable circumstances made and used artifacts in particular but variable ways" (1977: 14). Martin too, though being a forerunner of the processual view was willing to incorporate "humanistic ideas", believing that there may be "many roads to the truth" (1974).
New Archaeology. This results in only a "partial analysis" of cultures. As Gibbon (1989:109) put it:

Viewing people as malleable plastic figures entirely shaped by external changes in the environment is not only partial analysis but one that distorts social life in profound ways.

After all, is it not people who produce social actions? And therefore, is it not them who have the ability to exert purposive control over it? A few adherents of New Archaeology admitted their failure to consider the individual as important within the culture concept. For example, Renfrew (1972:496) admitted that:

While the behaviour of the group of many individual units may often effectively be described in statistical terms without reference to the single unit, it cannot so easily be explained in this way.

While discussing the place of people's in the understanding of culture, I'd like to quote here a brilliant passage by Stuart Piggott¹:

There is surely something seriously wrong when archaeological and historical reality have been sacrificed in theory... The prehistoric Europe of this book's title is inhabited not by human beings--stinking, likable, witless, intelligent, incalculable real awful people--but by the pale phantoms of modern theory, who do not live but just cower in ecological niches, get caught in catchment areas, and are entangled in redistributive systems. I started reading this book with high hopes, but as, snowbound, I finished... I almost felt I could crazily rush to opposite extremes as an escape from deterministic systems of regular human behaviour...

(Piggott:146)

¹ A review article by him of Prehistoric Europe (1984).
New Archaeology also suffered from ahistoricism or refusal to consider past development trajectories of the societies studied. Ecological functionalism pushed into the background historical reconstruction's of past peoples. The theories and methods of New Archaeology also ignored formative phases and ideological influences and symbolic systems of the past. The view of culture as extrasomatic thus entailed a reduction of internal, historical, ideological and individual dynamics.

The New Archaeologists were also, unfortunately, unable to produce any laws of culture change as they had claimed. Instead explanations and even tests of hypotheses were nearly always presented as hypothetical sketches or incomplete constructions which required systematic restatement and testing. As a result, issues of confirmation were never grappled with, and the task of the New Archaeologists was never clearly laid out (Renfrew 1987: 690).

To a large extent much of the theorizing in New Archaeology resulted from a defensive attempt to establish the discipline as a "hard" science (Bayard 1969: 382). However archaeology like anthropology is not a "hard" science but a discipline dealing with incomplete and imperfect data. And it is to this data that all archaeological constructs must connect. And it is precisely at this level that New Archaeologists failed, in my view. For example, Binford's deductive testing programmes remained largely rhetorical and polemic, and were not incorporated to any significant extent in actual practice. Problems and objectives were often formulated by New Archaeologists like Binford only as armchair generalizations. The fit between their theoretical framework and data was not a smooth one. It is a source of constant surprise to me as to how the kind of archaeology represented by Binford and his "mafia", in the late 1960s and 70s has "taken over the subject" as Shennan put it (1989: 831); whereas David Clarke whose ideas were truly revolutionary, has had not much influence.

Binford's later-years ethnoarchaeological research has also come under the critical eye of Wylie (1985) and Watson (1986: 1991). Wylie (1985) points out that observations made in living (dynamic) societies are just as theory-dependent as is inference from the archaeological record with the result that the problem of
interpretation is simply transferred from one discipline to another. According to Watson (1991), Binford began work in a living society to enable direct observation of the variables that concern him (refuse disposal patterns, hunting strategy, butchering etc.) but he does not specifically address the inferential problems in recording and generalizing that information. And secondly, once the inferential problems of obtaining accurate and adequate ethnographic information are solved, Binford still faces the same issues confronting any archaeologist who wants to use contemporary data to help understand the past. She further adds that there needs to be a bridge to relate the ethnography plausibly to the archaeology.

In my opinion, Binford's major contribution to archaeological research has been his continual stress to study variability and diversity in the archaeological record (1968, 1978, 1980, 1983, 1984). Archaeological methods of inference have been generally inadequate when dealing with variable patterning in the archaeological record. Binford has been focusing on this problem since the 1960s and has emphasized that variability between assemblages at the same site may well represent "activity variants", not necessarily culture change or occupations by different people¹. The differences noticeable could thus be "... functional or referable to differing situations that are not necessarily ethnically significant" (1984 : 178)². Thus Binford arguments on functional variability have added a new perspective to the study of archaeological patterns.

¹ I don't know if Binford was aware of the work of anthropologist D. F. Thompson, who as early as 1939, in an article entitled "The seasonal factor in human culture", pointed out that the same group of Australian hunter-gatherers would use totally different material culture assemblages in different seasons of the year.

² In all his arguments regarding functional variability, Binford has denied neither the existence of ethnicity nor its potential importance as a conditioner of variability in the archaeological record. My only concern, Binford says "is for the unambiguous identification of archaeological properties with a clear ethnic, symbolic, or ideological referent." (1984 : f.no. 12 : 181).