Chapter III

METHODOLOGY

Professor Macdonald remarked that in the forming of stone implements, there could be no more ingenuity exercised by one race of men than by another, for they would be made of such form as the rock would permit. The progress of civilization, as indicated by implements, could therefore only be traced in those made of bronze or iron, which admitted of the exercise of greater art.


Even in a paper devoted so extensively to descriptive material and to chronological correlations, there should be some expression of the assumptions and premises — that is, the body of theory, or strategy — which have been used in treating the data and reaching the conclusions. The present chapter is a discussion and explanation of the methodology employed. Much of it is commonplace and implicit in the text, but it may be useful to set it down here rather than to leave it implied and perhaps misunderstood.

(A) "Periods" and "Cultures"

As already stated, the principal aim of this paper is to set out a more precise chronological and spatial framework for the Solutrean in France. At the present time the majority of opinions regards the Solutrean as a rather short, transi-
tory and, in a sense intrusive, period or culture with a restricted geographical range, and somehow different from the preceding and following Upper Palaeolithic assemblages; or at least having very little influence on the general Upper Palaeolithic sequence. All of these views are to some extent justifiable, and the very unorthodoxy of the industry is a cogent reason for closer study than has hitherto been made. The standard subdivisions of the Solutrean set up over a half century ago have stood the tests of time pretty well, with few modifications, and there seems to be a generally accepted belief that they are comparatively firmly fixed (in contrast with, say, the Perigordian or Magdalenian subdivisions) and present a straightforward picture of development. The developmental sequence usually accepted is as follows:

a. Proto-Solutrean. This subdivision was named by D. Peyrony in the 1920's to identify level 8 at Laugerie-Haute: West. More confusion revolves about this term than any of the others. To many writers it is equivalent to any level where pointes à face planes are already well developed, i.e., to what is more usually called Lower Solutrean. Such thinking is an inheritance from the period when the Lower Solutrean was the earliest known manifestation of the Solutrean in France, and Breuil and Parat used the expression Protosolutréen in this sense at Le Trilobite (Yonne) in the early years of this century (Parat 1902a:
sing difficulty in diagnosing it and relating it to equivalent or contemporary industries; generally the problem is taken care of by calling it an "evolved local Solutrean".\footnote{One might mention here a further subdivision, the "Solutréen Final" or Solutréen V, created by Cheynier at Bédeguile. This will be discussed later in its own place, and it is enough to say here that to the present writer this particular creation, based as it is on wholly indefensible criteria, has absolutely no chronological or cultural value and should be dropped. Fortunately it has not caught on to any important extent among prehistorians, but the fact that it was created and has aroused so little protest indicates very well the general absence of critical judgment in carving up the Solutrean.}

Now, as the divisions are set up at present, questions such as, are there any laurel leaves in the Lower Solutrean? can have only negative answers, for the simple reason that when laurel leaves appear the industry is automatically called Middle Solutrean -- just as it is automatically promoted to Upper Solutrean at the first appearance of shouldered points. It is reminiscent of a similar problem involving the possibility of Pliocene Man: as geologists have pointed out, the beginning of the Pleistocene is in part defined by the appearance of man, so the further back man or his works are pushed, the further back goes the Pleistocene boundary. Thus, there can never be a Pliocene man on these terms and the Lower Solutrean can never have laurel leaves.
In a rough sort of way this latter viewpoint can be considered true, but it holds certain dangers. Not so much because it will disguise the first appearance of laurel leaves in the Lower Solutrean -- it is reasonably convenient, after all, to use this as a marker to make the division -- but because it also will disguise any time lag or diffusion lag. Suppose that an isolated Solutrean region did not receive or adopt or invent laurel leaves at all; it will then be classed as "lower" Solutrean even if it persists while the Middle or even the Upper Solutrean develop elsewhere. In a cultural or typological sense it is Lower Solutrean, of course; in a chronological sense it is not. So other criteria or methods must be employed to resolve this problem, so familiar to archaeologists everywhere, of cultural lag and temporal overlap. Unfortunately the relative lack of either statistical or typological changes in the tool-kit, other than for the "typical" or characteristic implements, during the duration of the Solutrean brings us up against an obstacle which can only be negotiated, it would seem, by absolute and relative chronological information (such as provided by radiocarbon dates or stratigraphy) distributed in and linking up the various regions of Solutrean settlement. In the absence of these, an attempt is made here to furnish the same picture by typological comparisons on a more minute level than has hitherto been attempted for the Solutrean.
These comparisons may perhaps come fairly close to the truth, but until they are further substantiated by such absolute and relative methods as mentioned above, their accuracy should be regarded with a certain reserve.

This situation is, of course, essentially the same as the one which the Pecos Classification in the Southwestern United States presented (Kidder 1924). There, too, the inherent problems of overlap and distribution involved in a linear developmental scheme quickly became evident, as Kluckhohn has pointed out:

...the various stages recognized by the Pecos classification do not, necessarily, represent separate and clear-cut time periods even in the same geographical locality... At very least the Pecos classification should take explicit account of the differing periods of development in different areas and of varying genetic sequences in various regions of Anasazi culture (C. Kluckhohn, in Kluckhohn and Siger, 1939:159-60).

Indeed, a little reflection will show that what has happened in the case of the Solutrean (as well as of the original Pecos Classification) has been a confusion between chronological periods and evolutionary stages, on the one hand, and modes, in the sense in which Childe (1944) used the term, on the other.¹ In other words, the question to be

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¹But of course Childe meant each of his four modes (0, 1, 2, 3) to represent a technological stage in human progress, specifically in the use of metals, with Mode 2, for example, meaning the stage when metal was used in handicraft but not in husbandry, and stone tools were still abundant. I doubt that there are any significant technological-economic dif-
asked is, to what extent are the Solutrean "periods" syn-
chronic and to what extent homotaxial? To what extent may a
culture or the subdivisions of a culture transgress time and
other cultures or subcultures? Childe's system of modes
would, of course, free the subdivisions of the Solutrean
from such time-binding expressions as "Lower Solutrean"
where the terms are loaded in favor of relative chronologi-
cal positions. Thus, if the labels Solutrean A, B, C, etc.,
were substituted for expressions like Lower Solutrean, Upper
Solutrean, and so on (very much as Hawkes has recently sug-
gested for the British Iron Age) (Hawkes 1959), it would be
much easier to visualize a situation where a "Solutrean B"
(equivalent to, say, the present Lower Solutrean) might be
contemporary in some areas with a Solutrean C (Middle Solu-
trean), and in other areas might even persist into a Solu-
trean D representing the Solutrean with shouldered points in
the French Southwest or in Cantabria.

If all the assemblages grouped in a single mode (say,
a Mode X comprising all the known Solutrean levels with well
developed pointes à faces plane but without laurel leaves,
shouldered points or willow leaves) are later found to be con-

ferences between any of the Solutrean divisions, so any such
divisions as modes, if used, would have only a stylistic val-
ue. Incidentally, this seems to be the idea applied by Caton-
Thompson to the Aterian when she isolated four typological
styles designated by Greek letters rather than by chronologi-
cal labels (Caton-Thompson, 1946).
temporary and with only regional variations in style, then they can be recognized as such and no harm has been done. On the other hand, if they are found not to be all on the same horizon, then such a distinction between period and culture has justified itself and we are able to see more clearly the gradients of growth and diffusion involved. This is one of the principal objectives of the present study and surely one of the prime aims of all archaeological research. In the final chapter of this paper, which offers an attempt at cultural and temporal alignments of all the French Solutrean, an illustration or application of these ideas of Childe and Hawkes will be given as modified for the present case.

In this paper, when terms like Proto-Solutrean or Middle Solutrean are used, each is intended as an equivalent of what Willey and Phillips have called the phase, which they define as "an archaeological unit possessing traits sufficiently characteristic to distinguish it from all other units similarly conceived, whether of the same or other cultures or civilizations, spatially limited to the order of magnitude of a locality or region and chronologically limited to a relatively brief interval of time... A phase may be anything from a thin level in a site reflecting no more than a brief encampment to a prolonged occupation of a large number of sites distributed over a region of very elastic proportions" (Wil-
ley and Phillips (1958:22)

From a typological point of view the method and purpose of research, in the Upper Palaeolithic at least, involves the tracing out of the combinations and intercombinations of technological habits and techniques (including learned motor habits) with artifact forms and frequencies, both in their representative aspects as indicators of cultural tradition and in their roles as agents of subsistence and other functions. Each "culture" seems to possess a consistent tool-kit which, to some extent, is unique but which can be brought to lend itself to comparison with others. (The basis of scientific investigation, after all, is that attributes of any whole can be abstracted and compared; if the assemblages were not consistent, obviously there would be no points of comparison). For the most part such research involves tracing out patterns of stone working on the assumption that they are consistent reflections or images of more significant patterns. To the extent that tools (whether primary or secondary) reflect economic or subsistence activity, there undoubtedly is a fairly close relationship. The failure to penetrate into the higher orders of interpretation has, of course, already been stressed by Hawkes (1954) when he points out that while in prehistory we know a good deal about techniques, and a fair amount about subsistence economics, our knowledge of socio-political institutions is slight
and of religious institutions and beliefs nearly nil. This is not to say that such is an inevitable situation, of course, and Childe (1944), for one, tried to combine the first two of these orders to give a reconstruction of the third and fourth; but it involves a different conceptual level. Nevertheless, one can create, on the basis of typological studies of worked stone and organic material, patterns of diffusion, migration, invention, etc., which have a certain reality of their own and, it is hoped, have correlates in the "real" culture. As Piggott has recently (1960) discussed, all our attempts at reconstruction of prehistory are based on models, whether they are theological, mythological, economic or technological in nature, in our attempts to discover what he calls the past-in-itself and make it into a past-as-known. And, a most important point, the past-as-known derived from one model is not, by definition, the same as the past-as-known discovered by another; just as, in his metaphor, "what you perceive from Jodrell Bank is not what you receive from Mount Palomar", its relationship or resemblance to the past-in-itself is only an approximation, an attempt to understand by hopefully creating other constructs. It was Sir Edward Tylor, I believe, who once stated that the statistical assemblage of much data can still leave one outside the cultural reality of the life of the peoples one is studying.

The present study, then, is an attempt to create a
technological model, based mainly on the typology of stone and bone artifacts, for an approximation to understanding the "cultural reality" of the users of these artifacts. Obviously, one of the dangers of typological study, whether of stones or of ceramics, is that it creates a curious mystique in which the objects and types seem to interact and breed in a universe of their own with only a perfunctory relationship to the world of men who designed and used them. (The expression "psychoceramics" has been dubbed to one form of this aberration). Seen another way, a typological reconstruction is like the philosopher's shadows flickering from the fire on the cave wall, giving a picture which is often distorted and not wholly indicative of "what happened in history". Whenever Palaeolithic archaeology can be supplemented by other approaches and methods it will gain immensely; but since the typology and technology of stone tools will always remain the backbone of Palaeolithic research, it behooves us to perfect it, to understand it and to know how to interpret the changes which statistical and other methods bring out. For in studying such assemblages we are really studying a rather pure and simplified form of human behavior; always keeping in mind of course the reminder that artifacts are not culture but the means and results of culture. (Neither is all archaeological culture manifested in artifacts, of course; one has only to think of such habits as skull trephination or
dental mutilations which give us cultural information in non-artifactual form.) Artifacts are social tools and this is especially true of implements of day-to-day use. The form or shape of the tool is ordinarily not left to each individual to discover for himself; instead, the proper form, the method of manufacture is passed on as a result of experiments, accumulated experience and improved skills. Therefore, in spite of the frustrations of research in this particularly austere field, where the recoverable evidence has been censored to a minimum by the circumstances of time and attrition, there is an advantage in the opportunity it offers for disciplined investigation in a relatively homogeneous reservoir of cultural behaviour. The conclusions we distill from this pool depend on the fineness of our methodologies and categories. But we should keep in mind that it is particular, continuous patterns of behavior, as reflected in artifacts, that we are studying and not primarily tribal or racial groups. If such ethnic or racial identifications can be pinned on as a result of deductions from skeletal material, art representations, etc., so as to cross-cut the record of the artifacts, well and good; this will be a welcome bonus. But in the final analysis the aims of Palaeolithic typology must be the elucidation, mainly from the artifacts and their contexts, of such human behavioral patterns as the selection of materials, the manufacturing techniques, the morphology,
use and function, change and distribution. In a sense we are calibrating cultural behavior on a rather simple technological level where the economic and subsistence limitations of the societies involved are already known, i.e., we know that the groups were participants in nothing more complex than a gathering-hunting subsistence.

Of course we are involved, at this remove, neither with "societies" nor with "cultures" in the specific sense, but only in the generic sense. In the reference already mentioned here, Hawkes (1954) has pointed out that what we are really dealing with in archaeology are norms rather than cultures, or at least that is what we reduce it to when dealing with the data analytically. Without norms there can be no firm claim of comparability between the phenomena given by the material:

...the human activity which it can apprehend conforms to a series of norms, which can be aggregated under the name of cultures, definable in terms of time and space and recognizable each by its standard range of material products (Hawkes 1954:157).

Thus, it is doubtful that our constructs really apply to societies in the specific sense. For convenience's sake it is often useful to refer to "Solutrean culture" or even to "Solutrean society" of a particular region; but the terms mask a certain vagueness which should not be disregarded. Instead of tribes or societies, a more general term such as aggregate is preferable, in the sense in which Forde has used
it in discussing the historical spread of African popula-
tions:

I do not call them 'societies' because I think they
were population aggregates, continually splitting up
and then re-forming into a succession of societies.
It is not the societies that have the continuity so
much as the succession...of cultural forms and soci-
al structures (Daryll Forde, in Tarx, Hineley, House and
Voegelin, 1953:19-20).

(E) Classification and Typology

As Krieger (1960) has emphasized, these two terms
have different slants: classification refers to any purpose-
ful sorting or designating, while typology infers basic or
inherent divisions, "an orderly system of actions, obeying
certain laws or principles" (ibid., p. 143). A classifica-
tion should throw light on the relation between one set of
facts and another; a typology is a classification that is
"explicitly theoretical in intent as opposed to one intended
purely as a descriptive categorization" (Kluckhohn, 1960:134).

In recent years in American archaeology (but also
to some extent in British archaeology, e.g., in the writings
of Hawkes, Daniel and Childe especially) there have been
rather heated controversies over "types" which in essence
boil down to the question: do types exist as entities in
themselves, as Platonic concepts, or do they exist only in
the mind of the observer? Biologists have, of course, faced
the same dichotomy, as expressed by one of them recently:

For the typologist, the type (eidos) is real and the
variation an illusion, while for the populationist the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different (Mayr, 1959:2).

There can be little doubt that, for the individual or group which fabricated the artifact, the type did exist as a desirable end to achieve. If this were not true the range of inventions in any one culture would, theoretically at least, be infinite. But even a slight acquaintance with Palaeolithic industries (or any other assemblage of archaeological products) shows that in a given industry there is a fairly well defined outer limit to the occurrence of specimens — though this limit is by no means static through time and space — and that within this range a little experience soon brings about an ability to predict absences and presences of forms. In other words, the makers were exercising, consciously or otherwise, a social or cultural choice determined in part by tradition (i.e., persistent attitudes or ways of doing things passed on from one generation to another) and the technical or social needs of the community, in part by the raw materials available, in part by the level of technical skill acquired, in part by the degree of familiarity with the choices of other groups, and finally, partly by the interaction with other types in the same tool kit. But these factors which ultimately determine the "type" (and this applies to the assemblage or "total" type) were not always kept in separate compartments by the makers; they "overflow-
ed" and influenced each other, leading to gradations, variants, combinations and composites where the "type", though existing in principle, is in practice blurred and indistinct, surviving only in a given range where the "pure" type can be pinpointed only for hypothetical convenience.

An illustration of this principle is provided by the Solutrean points à face plane. Here it would seem that the original Solutreans had the idea of, and presumably the need for, a pointed implement made on a flake or a short blade with one face plain, or at least with only the bulb removed, and the other face more or less retouched about its surface. This much, in which the factor of function is usually basic, can be attributed to social tradition and technological need, and might even be considered the reflection of a "value". But the expression of this value, or its attainment, was also influenced by other factors such as (a) the kind of stone available, (b) the kind of technological finish or retouch used, and the degree to which it covered the surface, (c) the interaction with other types or styles in the same kit. All these factors are variable, but the extent or the reasons for this variability are difficult to express, and the resulting wide range in types of points à face plane, both in time and in space, reflects this variability. For example, the relative skill of a group in utilizing flat retouch will create differences or sub-types, as
will the use of certain fine-grained flints or jaspers which invite unusually accomplished specimens. Also, tool types tend to merge into each other, either because of similar re-touch techniques, or because of the temptation to make composite tools, or because of convergence in morphology caused by some pervading style which finally creates in the makers' minds a certain haziness as to the end-product desired (e.g., see the late Solutrean at Le Placard, where, as will be described in Chapter VI, one variety of \textit{pointe à face plane} seems to blend with one kind of willow leaf).\footnote{There is a striking linguistic analogy to this idea in phonology: two sounds very close to each other will, when juxtaposed, introduce an instability into the whole system which will involve readjustments, and any one change in the equilibrium of a total system will involve gradual but inevitable changes in the whole system, and change, like the geneticist's drift, will spread all across the sound system. It is apparently not clear what causes these "mutations" (e.g., why, when Latin was evolving into the Romance dialects, the Latin vowel quantity disappeared and a stress accent was substituted) but it is known that compensations do take place in linguistic changes. I am indebted to Eylet Blockbush for bringing this illustration to my notice.}

It is axiomatic in acculturation studies in social anthropology that the introduction into a society of a new trait or group of traits does not always leave the original structure of the society intact; indeed, usually the introduction will distort or reshape the rest of the culture far outside the immediate area of its use. The introduction of metal tools into stone using societies, with its effects on
such non-technological aspects as social organization and division of labor, is a case in point which has been documented many times.

Similarly, the introduction of a new style or fashion or technique into a Paleolithic tool kit may change much more than the immediate group of artifacts to which it is applied. A kind of shock wave may travel through the rest of the tool kit, and formerly stable "type" of artifacts may be altered into new types. I believe that in the Solutrean we can discern several such waves of innovation, or horizon styles, which have such effects. The fashion of stemmed artifacts is one such; another would be the fad for covering large areas of the surface of tools with flat retouch; a third might be the fashion for miniaturization or slenderizing of certain artifacts in the Upper Solutrean. In all these cases, the new idea has spread out from a single class to other classes of artifacts; it would also be possible that a former type or class might be entirely submerged or displaced by an innovation, such as seems to have happened to several sub-types of points à face plane after the Lower Solutrean.

One of the things which a statistical treatment of industries permits us to do is to see what implements replace others through time, and at what rate; this can often give a broad hint as to the functions of each in the culture
using them. For example, in the Solutrean the decline in
the popularity of pointes à face plane (both in absolute
numbers and in types employed) seems to have been closely
correlated with the growing popularity of various forms of
laurel leaves; and, later, the latter tended gradually to be
displaced in favor of shouldered points. Now, there seems
no doubt that most of the shouldered points were used for
projectiles, and probably this is true for many of the laurel
leaves (though these may not have been used on throwing pro-
jectiles but for thrusting spears); we are thus led to sup-
pose that many pointes à face plane, which were gradually
supplanted by laurel leaves, performed much the same kind of
function, though many might equally well have served as
knives.

Types are consistent choices made within a universe
of potential selections. If the universe is a narrow one --
e.g., in a pebble-tool range -- the individual types within
the range will be indistinct and few, and hard to diagnose.
If the potential range is wide, then the consistent selec-
tions made will be better as types. Now, nearly everything
can be "typed". As Childe (1951:740) said, cuisines can be
typed from their débris if certain animals are consistently
selected out of a potential wide range of fauna available.
But if the fauna in the habitat is limited to a few species,
and all are utilized to the maximum, the possibility of dis-
tistinguishing "types" is poor unless a consistent proportion is found in a number of sites. When these consistent choices become sequentially persistent ones, we are dealing with traditions.

So, to come back to the controversy mentioned earlier: the "type" does exist in a sense -- but only within a flexible framework of shifting and partially independent determinants which make of it a fluctuating ideal pulled about by the changing values of the external forces exerted. The question, then, is not so much whether the type exists, but what determines it and how can the expressed variations be predicted and explained.

Each type (here I am talking only about stone implements, although the same ideas might be projected to embrace other human products) is thus best understood as part of a constellation pulled about by the gravity systems of the variant factors involved. The varying potency of each component or ingredient determines the final product. A factor may at one moment be passive and at another active -- e.g., exactly the same type might be obtained from a fine-grained jasper or from a poor flint if the other factors are made to compensate.

Now, the variant factors listed above need not exhaust the list of potential determinants; but even with the four mentioned, the number of combinations which have to be
evaluated along with the crude morphology (i.e., shape or form) gives a formula much too cumbersome to handle with the elementary typology we are accustomed to use. For example, a single artifact may embrace such elements as: on a long blade, flat retouch on one edge, some inverse retouch, a concave truncation, made of cherty flint, some slight denticulations. In ordinary usage some of these elements may be either ignored, or mentioned briefly, or else given a distorted importance at the expense of others; but, unless there is a very high consistency in their frequency and distribution, the criteria are selected on the basis of what the observer believes is significant. And when the criteria are not selected with consistency, that is, when in one case it is the retouch which is emphasized, in another the shape, in another the technique or manufacture, it is clear that there will be a loss of information. In actual practice this traditional method of seeing distinctions between units works pretty well, as long as it is understood that it is a rule-of-thumb analysis. It is the method which has been adopted, faute de mieux, in the present paper and I have discussed it here only to stress its inherent weakness. Probably the solution will be to devise a classification whereby all the attributes which we can perceive at the present time are subsumed in the description of each object so that possible significant combinations
or correlations which at present are suppressed may be extracted. Our present categories, though serviceable, are much too rigid and impressionistic and too resistant to further analytic breakdown once applied to the materials excavated. In other words, not only do they fail to answer certain questions when applied to the material, but they often give "no information" responses when questions are asked later on. They are intended to ask, and answer, the questions native to their traditional framework, and only fortuitously to answer other future inquiries. This problem is not one restricted to Palaeolithic archaeology, nor to Old World problems, but it is one which will have to be resolved if Palaeolithic studies are going to keep abreast of the increasing amounts of raw data fed into the field and the more demanding and critical inquiries which many prehistorians are provoking. Perhaps the classification system which has been outlined by J.-C Gardin (1958) is the most suitable for resolution of this problem, considering the success it has had in dealing with fairly similar difficulties in such widely divergent fields as iconography, ceramics, metal artifacts and even texts, from a heuristic as well as a purely problem-solving viewpoint. The application of this method to the Palaeolithic industries has already begun, and the results should be most interesting.

As far as this paper is concerned, the definition
of the "type" which has been devised is essentially an empirical one: it is a form occurring in a non-random fashion which has significance in time, or space, or both, in relation to the problem concerned. Thus, it must occur frequently enough (even if only at one site, in one horizon) to remove suspicion of being a fluke or accident or whim. On the other hand, it may be a relatively rare phenomenon whose occurrence, either over a wide geographical area or sequentially in a single zone, takes it out of the random or fluke category or indicates that it was a meaningful form to at least some of the bearers of the culture. To give examples of what I mean: I do not consider that many of Cheynier's laurel-leaf "types" have any reality as types, because most are unique specimens (e.g., the "feuille de gui") with only one or two specimens known and due certainly to accident, whim or experimentation rather than to a fixed category in the makers' minds. On the other hand, I have in this paper isolated a new "type" (or rather, sub-type) of endscraper -- the miniature or "Grimaldian" type -- because I find that it does seem to have a significant spatial distribution in the Solutrean, even if its temporal place is not yet entirely clear.\footnote{However, credit must be given to B. Peyrond (1939-46) for having first recognized the significance of this sub-type.}

I have no wish to become involved in the perennial search for a definition of "culture", and the definition I have adopted is equally pragmatic. It can be most elegantly
presented in a definition of culture which Kluckhohn has recently used, paraphrasing Lévy-Strauss:

A culture is a set of patterns of and for behavior prevalent among a group of humans which, from the point of view of the research at hand and of the scale on which it is being carried out, presents, in relation to other such sets, significant discontinuities. (Kluckhohn, 1960:137).

The viewpoint in this paper is that an archaeological culture is one characterized by assemblages exhibiting a particular pool or elements patterned in a distinctive and coherent combination. (This is not very different from Childe's definition of an archaeological culture as a recurrent assemblage containing the same type fossils [Childe, 1954:74, but it carries it a little further). The regional variants seen in assemblages can be compared to dialects, or, in the zoological realm, to speciation.

It is an accepted notion in anthropological studies today that linguistics offers many analogies with archaeology and can present many insights for investigation. There are two purposes of analysis of linguistic features: (1) to see if the distinctions made are meaningful to the users themselves; this can only be done if the users of the language are available, and (b) to trace historical change, whether or not we know if the users consider the distinctions meaningful. In prehistoric archaeology the former method is the more difficult, but we can often deduce whether the makers or users of an artifact considered a feature (e.g., the
gibbosity on blades or bladelets) meaningful by comparison of the frequency of occurrence, and occasionally by suggestions from studies of modern primitives. But the emphasis in prehistoric archaeology is to trace historical change, and this can be done whether the original users were conscious of the distinctions (i.e., considered them meaningful) or not; in most cases, one supposes, they were aware of distinctions between features, but conceivably there were cases where they did not, e.g., whether a burin was made on a flake or on a blade, or whether parallel or irregular flaking was used on a particular foliate point. They may have observed the difference, of course -- just as the Navaho can observe the phonetic distinction between p and b -- without giving the distinction any significance. To us, on the other hand, the unconscious distinction may be very revealing.

One of the premises or postulates of this paper is that, like linguistic families, Palaeolithic stone industries may be structured along certain lines and that this structuring is consistent and meaningful. As mentioned earlier, this premise of a structure or style has been seized intuitively long ago by archaeologists; indeed, Kroeber is of the opinion that, at the beginning at least, stylistic data can best be handled intuitively.

I continue to believe that statistical correlations can be used in stylistic analysis, but that their main function is to convince those who are, by nature or by inexperience, stylistically insensitive. Sty-
listic qualities and their patterns and interpretations must be taken in through the senses and digested through the subrational process sometimes called intuition, but for which the term 'perception with aesthetic feeling' is adequate. A mathematical approach, being abstract and rational, seems best deferred until the pioneering job of analysis has been pretty thoroughly done by exercise of perception. On the other hand, final proof, in the sense of formal scientific proof, has apparently to be quantitative. I believe that most qualitative data, and perhaps even stylistic ones, can also be interpreted quantitatively in the end; but certainly not so profitably at the beginning (Kroeber, 1956:330-31).

The present paper, though utilizing some very simple quantitative methods, is in large part such an 'intuitive' approach to the problem.

But such "statistical" approaches as these developed by the Bodes have shown, by means of elementary quantitative calculations projected graphically, that each industry has a profile, a structure which is highly consistent and predictable (e.g., "X" industry may have a predominance of burins over scrapers, with a low incidence of perforators and backed blades), and which also reflects temporal and spatial factors; and this to some extent independent of purely qualitative changes. Once the "personality profile" of an industry has been fixed it can be compared with others to give other than impressionistic responses. The structure may change little when foreign elements (equivalent to loan-words) are added, but reveals it immediately in the graph. On the other hand, just as two languages from quite different families may have many loan-words in common through contacts
of various sorts, so two entirely differently structured industries with very different origins may have some common implements or traits.

Unless this is kept in mind there is always a temptation to give blanket names inferring a common genealogical origin to industries which in reality share only a few elements and are basically different in structure and emphasis. The case of the Solutrean and the Szeletian will be discussed later in this light; perhaps another such false family will turn out to be the "Aurignacian" of Central Europe, so-called principally because of the occurrence of cleft-based bone points or steep scrapers like those in the Western Europe Aurignacian. Of course the fact that they do share such elements is interesting and important from the viewpoint of culture history and possible contacts (where it is not merely cases of parallel, independent development of certain simple artifacts to serve similar purposes in more or less equivalent economies), and must be investigated profoundly. But this should not be confused with genealogical evolution or divergence and differentiation of an original "stock", i.e., an assemblage structured along certain stable and efficient lines.

This is not exactly to say that there were originally "pure" assemblages or industries, any more than that there were once pure languages or races. Change, through borrowing,
Invention and loss, has always gone on, but I believe it is as true that there is always some degree of integration on this low level as on the more complex level of civilization. In a most stimulating book devoted to analysis of the style or superstyle called civilization, Kroeber (1957) has shown that neither he, nor Spengler, nor Toynbee, nor even Sorokin have considered civilizations to be haphazard things without consistency, a dump or "thing of shreds and patches"; they only differ in the degree and nature of the integration involved. The same approach to internal consistency or integration of elements in the more humble industries of the Palaeolithic, by indicating the range of selectivity of a group or the degree of toleration to shifts permitted by an industry with a given structure, might be most profitable.

There may be periods of equilibrium when little change can be detected in an industry, of course, and similarly there may be periods of abrupt change, almost of breakdown; perhaps the terminal Magdalenian is an example of this latter. But just how different one industrial structure has to be from another in order to be classed as a different "culture" is a tangled problem which would overburden the present paper. Most likely it would have to be expressed or demonstrated in quantitative terms although, as indicated in an earlier quotation from Kroeber, the difference would al-
ready have been sensed intuitively.

The concept of equilibrium and non-equilibrium has recently been re-introduced into social anthropology with good results by Leach, in a study of changes in ethnic identification and acculturation among hill tribes in Burma. His particular findings need not concern us here, but among his conclusions are some which find possible parallels in prehistoric structures:

My own view is that equilibrium theory in social anthropology was once justified but that it now needs drastic modification. We can no longer be satisfied with attempting to set up a typology of fixed systems. We must recognize that few if any of the societies which a modern field worker can study show any marked tendency towards stability (Leach, 1954:284-85).

Eventually, even, it may be necessary to treat such subdivisions as Perigordian, or Mousterian, as Leach says social systems should be viewed — as scientific fictions, useful but nevertheless fictions.

One of the postulates of this paper is that industries, and to an unknown extent the cultures behind them, often exhibit symptoms of instability or disequilibrium which can be detected by the use of such devices as cumulative graphs and statistical typology. This is not proved, but it is likely. A corresponding postulate is that the degree of instability or change, and its direction, is a patterned effect rather than a random or eccentric one, and that the direction can be plotted; consequently the structure revealed by the
cumulative graph can be used to predict the chronological position of an industry when this is not known by stratigraphy or other means. But -- and this is important -- such prediction is really only valuable within a rather small area containing the local culture zone or culture sub-area. The extent of this varies in each case, and in some cases in the French Solutrean might be less than a 50 kilometer range. I have emphasized this reservation because one criticism of such efforts as Ford's (see Ford, in Phillips, Ford and Griffin 1951), in American archaeology, to use the percentage method of prediction is that what is otherwise a useful tool is strained by trying to extend it too far geographically beyond the range of radiation of the local focus (Willey and Phillips 1958: 28). In other words, for a Solutrean cumulative graph which closely resembles another in, say, the same département, there is a very good chance that closeness of fit means chronological proximity; but no such assumption should be made in our present state of knowledge if, say, a graph from Laugerie-Haute is nearly identical with one from Solutré. These two sites show too many evidences of belonging to variant traditions within the general Solutrean framework, and the distance between them is too great, to permit such conclusions to be adopted unless there are other kinds of evidence to support it. (It goes without saying, of course, that the samples used should have a reasonably high
degree of reliability both as regards the size of the sample and its purity. In the Upper Perigordian, for instance, we have very few reliable collections because of the faulty excavation methods used; for the Font-Robert horizon there is not a single collection which can be relied on.}

Another postulate, closely related, is that by the use of such graphs a number of archaeological levels in a given region which are not all found in stratigraphic positions may be arranged in their correct sequential order. This has been attempted in the present paper, and although no absolute proof is yet available to substantiate the assumption, it has worked out very well in practice; and for the first time it seems possible to align the assemblages from such late Solutrean sites as Pech de la Boissière, Jean-Blanc and Fourneau du Diable (Dordogne) in relation to each other, on other than purely stylistic or impressionistic grounds. The value of this information in suggesting the directions of movement and diffusion of groups and traits does not have to be emphasized.

At the present time we can see, by means of a fairly well defined typology, how the structure of one industry differs from another. For example, it is clear that the Solutrean consistently has a predominance of end-scrapers over burins, practically no backed blades, and tends to have an overwhelming ratio of such items as laurel leaves, pointes
à face plane, etc. What we still do not understand is why the ratios should be so different between the several Upper Palaeolithic "cultures" which all more or less dipped into the same general pool or substratum, yet gave it such individualistic expressions. Their subsistence economy seems to have been so similar that one hesitates to ascribe the differences to this factor; indeed, the disparate responses to basic needs which were in all likelihood very much the same in the Solutrean as, say, in the Perigordian, suggests that there is probably after all no very close correlation between tool type or statistical distribution on the one hand, and economics on the other. Perhaps within certain limits one tool kit answered the purpose as well as another. This question of the factor of tradition will be discussed in more detail later in this paper, especially in Chapter X.

But clearly the distinctive profile of each culture is not due merely to chance; it must be due to social choice or to technological habits conditioned by the society, but it is not easy to see if it was influenced by factors such as the raw material available, the size of the group, the subsistence pattern and the habitat. An hypothesis once advanced by Bordes was that the incidence of Levalloisian technique in Mousterian industries is a reflection of habitat and of movements resulting from climatic changes. Up to the present I can detect nothing of this factor in the varia-
tions in Solutrean technology, but it should not be discounted.

In other words, a pattern approach must be used for archaeology, as it has been in physical anthropology, so that instead of making a comparison of a few items or characteristics in relative isolation, there will be consideration of the whole pattern which they show in combination. As Washburn has put it in discussing the recent revolution in his own field:

The new systematics is concerned primarily with process and with the mechanism of evolutionary change, whereas the older point of view was chiefly concerned with sorting the results of evolution (1951:298).

A total morphological pattern is the ultimate aim in order to achieve a knowledge of the functional relationships between the assemblages and the other facets of the lives of the makers. And, at the present time, the cumulative graphs based on the typologies introduced by Bordes (1950) and by de Sonneville-Bordes and Perrot (1953, 1954, 1955, 1956a,b) are the closest thing we yet have to a presentation of the total profile of a Palaeolithic culture's industrial residue. With all its imperfections, this represents a greater step to the understanding of the dynamics of primitive industries than is generally realized. Like Bennett's area co-tradition concept, it is not merely a descriptive device. Rather, and most important, it is a framework for comparison between areas or traditions in order, first, to isolate the distinc-
tions, and, second, to explain them.

(c) **Culture Areas**

The reference just made to culture areas is not an accidental one. The procedure in the present paper is to discuss the Solutrean sites in France in the order of their geographical distribution according to the drainage systems in which they are located. This arrangement is not only convenient for descriptive purposes. It also holds the implication that, all things being reasonably equal, neighboring sites will resemble each other more closely than will distant ones. And, in spite of the fact that we are dealing with mobile groups of hunters (though the exact degree of mobility is not yet established), and that there are occasional examples of very similar assemblages being found at some distance from each other, nevertheless the assumption works out rather well in practice. There are consistencies between geographical areas and tool kits which seem to indicate that the same aggregates, or very closely related ones, were well enough identified with certain regions so that something like culture sub-areas can be distinguished even in the short-lived and rather concentrated Solutrean culture. If this can be established, its value in assessing the culture history and process of Western Europe during one brief portion of Upper Paleolithic times is obvious.

The culture-area concept is a familiar one to ethno-
ogists, and to archaeologists under this name and in the more elaborate form of the area co-tradition. Of course, as Kroebel once remarked (1947:329), "culture areas" are primarily not areas at all but kinds of culture which are areally limited, and they refer to static formations or moments in a time flow. In all probability, part of Western Europe (more particularly the Cantabrian zone and Southwestern France) could profitably be considered as a co-tradition area in the Upper Palaeolithic by contrast with the surrounding regions; but this is an idea which will be discussed at greater length later. For the purposes of this paper, it is suggested that the greater the contiguity between two sites of the same period, the greater is the probability that the resemblances are due to occupation by the same or a closely related aggregate. That is to say, if two very similar industries are found separated by only a few kilometers, the chances are much greater that they belong to the same "culture" than if an ocean or many hundreds of kilometers separated them. This may sound pretty elementary, but it is the assumption on which culture areas are built. The research for this paper early indicated to the writer that in fact such areas could be distinguished for the Solutrean, and since then it has been gratifying to note that essentially the same idea has been expressed very well by a new World archaeologist in a recent paper:
A higher order of space-form relationship can be derived from the spatial position and formal typology of assemblages. If the loci of the assemblages comprising a culture type form a geographical cluster, then the area occupied by the cluster is a culture type area. Repeated examples of such culture type areas would lead to recognition of a principle of spatial coherence of the component assemblages of a culture type. Finally, the spatial clustering of culture type areas themselves can be investigated to discover whether or not there is repeated association of such areas with one geographical region.... I think it safe to hazard, however, that all archaeologists would agree to the general proposition that artifact form does in fact vary systematically in space. The relationship is a direct one; artifacts or assemblages which are formally close tend strongly towards spatial closeness. The explanation for this phenomenon is obvious, but it is drawn from observations of living cultures, not from the data of archaeology; most formal similarities are the result of person-to-person transmission of ideas and objects, and space is a barrier to this transmission. The converse of the relationship, that artifacts or assemblages which are formally distant tend strongly to be spatially distant, is no better than half true. Space is not the only barrier to transmission; time is equally effective. Hence, we expect two assemblages that are very much alike formally to be close both in space and time. Two assemblages that are very different formally are expected to be distant in space, distant in time, or distant in both space and time (Spaulding, 1960:451-52).

This is not to say that there are always persistent traditions continuing through time in each area inhabited by the Solutreans. The degree of fit changes from one period or horizon to another, since of course probably no one area was wholly sealed off from contacts with other regions. Nevertheless, it seems true at present that through time a given zone, e.g., the Pyrenees or the lower Rhône region, will have a certain internal consistency in relation to other
regions, and that this continuity is probably a reflection of the continuity of aggregates in the region concerned. Similarly, on the same or approximately the same horizon even in such a relatively homogeneous region as the Dordogne-Camprodon-Corrèze area it seems possible to mark off particular industries, sometimes clustered, sometimes scattered, which also seem to represent related aggregates.

What I am trying to express is very much the kind of tradition that Phillips, Ford and Griffin tried to describe in the lower Mississippi valley:

Thus, we have in mind the concept of a continuously evolving regional pottery tradition, showing a more or less parallel development in and around a number of centers, each of which employs a number of distinct but related styles, each style in turn being in process of change both areally and temporally.

The idea of culture areas is by no means a new notion in French prehistory, of course (D. Peyrony, for one, had often suggested such groupings), but I believe the idea has not been as well exploited as it might be. The concept of culture areas is based on the idea of plotting the geographical distribution of elements. The writer is quite aware of the disfavor or negative attitude towards mappings of this kind on the part of some French prehistorians (e.g., see the comments of de Sonneville-Bordes, 1960:24-25), and perhaps the abuse of the method by certain members of the so-called "Toulouse School" justifies this caution. But such plottings can show up significant information even if
the precise time horizon is unknown, and in this paper the significance of the geographical restriction of elements such as eyed needles, certain types of foliates, shouldered points and pointes à face plane, and other implements will be discussed. In other words, geographical plotting is the necessary and normal supplement to vertical plotting and should be equally well defined.

(D) Regional Studies

As Gjessing has recently (1960) summarized very well, in Scandinavia, Great Britain and countries such as Czechoslovakia there has been increasing interest in "socio-archaeology" in late years, i.e., in the intensive study of archaeological "communities", their economy and linkage to their environments and as much as possible of their social system. This contrasts with the taxonomic and distributional studies most favored in Germany and, though he does not say so, in France. Undoubtedly this, especially the ecological approach, is a tremendously valuable tool for research in prehistory. But this approach has not yet been applied to any Solutrean site and, whether we like it or not, we must face the discouraging fact that the one hundred or so Solutrean sites which have been dug in France (and which probably constitute the majority of all the Solutrean localities in that country) have been investigated in a different manner and can now never answer certain of the kinds of
questions we should like to put to them today. So, unless we are to throw away what is known, we have to approach the materials available pretty much on their own terms -- or rather, on those of the excavators -- and try to refine the techniques of study to be used. In other words, we must fall back on the classic methods of taxonomic and distributional studies.

This of course lays the archaeologist open to the charge of stressing "taxonomic rosettes" as Taylor (1948) has expressed it. But it need not lead to what Taylor has considered the great error of such taxonomic studies: of emphasizing comparisons between types of artifacts which are widely separated at the expense of deeper studies of archaeological situations such as communities or camps. The present writer is whole-heartedly convinced that, whether the stress in research is on "socio-archaeology" or on artifacts nearly exclusively (as this paper is), the approach in each case must be based on local or regional studies. Properly conducted, even a taxonomic study can yield a good deal of information about social agglomerations and relationships between sites and regions, in addition to supplying what is absolutely invaluable in any branch of archaeology: a corpus of data whose geographical and chronological ranges are known and whose variations (both temporal and spatial) are understood.
What is required in research in the European Upper Paleolithic at this moment are more really intensive regional studies which will lend themselves to wider correlations. This has been the guide-line in the present work, where the aim has been to link up settlements (where they can be distinguished) in local regions or provinces, rather than make far-flung comparisons between provinces where the unit artifact involved may involve a different context. (To anticipate a little by way of illustration, I do not believe that asymmetrical laurel-leaves "mean" the same thing in the Pyrenees as they do, say, in Corrèze or at Solutré, and hence I believe that correlations or ethnic reconstructions based on such comparisons are fallacious.) For the same reasons, this paper will not discuss at great length the possibility of relationships between the industries with bifacial foliates of Central and Eastern Europe, and the Solutrean of Western Europe. To anticipate again, I should say that I do not think there are any direct relationships between them, and I think that further research will bear out what now seems indicated by a few significant facts to be discussed later. But the point is that Taylor, in this matter, was right in eschewing such long-range correlations except as tentative guesses, hunches or feelers for planned field work in a preliminary stage. Regional studies, spreading out slowly but effectively from sound bases, will in the long run
provide better answers than will the hop-skip-and-jump method; at any rate, it is the method which has been adopted here. That this regional approach is a respectably old principle in anthropological studies is evidenced by the following reference from Graebner, as quoted by Peter Schmidt, and it is not discredited by the fact that it was advanced by the now-moribund kulturkreislehre:

The surest and, in my opinion, the only means to prevent false conclusions in the field of distant interpretation, or at least to reduce them to a minimum, is the greatest possible approach to local interpretations (Schmidt, 1939:133).

I believe that it is through an approach such as this that we can best work towards the ecological approach to the Upper Palaeolithic. If local groups or "aggregates" which represent some approximation to societies, bands or tribes can be isolated, we are obviously well on the way to understanding the processes by which change and invention took place and the nature of the social choices which Graham Clark has aptly described as the decisive factor in the interplay between culture, habitat and biome (Clark, 1953). Indeed, what has been outlined here could in part be described as a demographic study, with emphasis on the size of groups, their geographical distributions and their degree of conservatism of habitat; and, as such, it forms part of the ecological approach in its widest sense which has been described by Movius (1949), an approach which one ex-archaeologist, Daryll
Forde (1943) considers the one which can most successfully unify all branches of anthropology.