

**SOCIETY FOR
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NUMBER 1

DECEMBER 1977



THE POPULATING OF WESTERN NORTH AMERICA

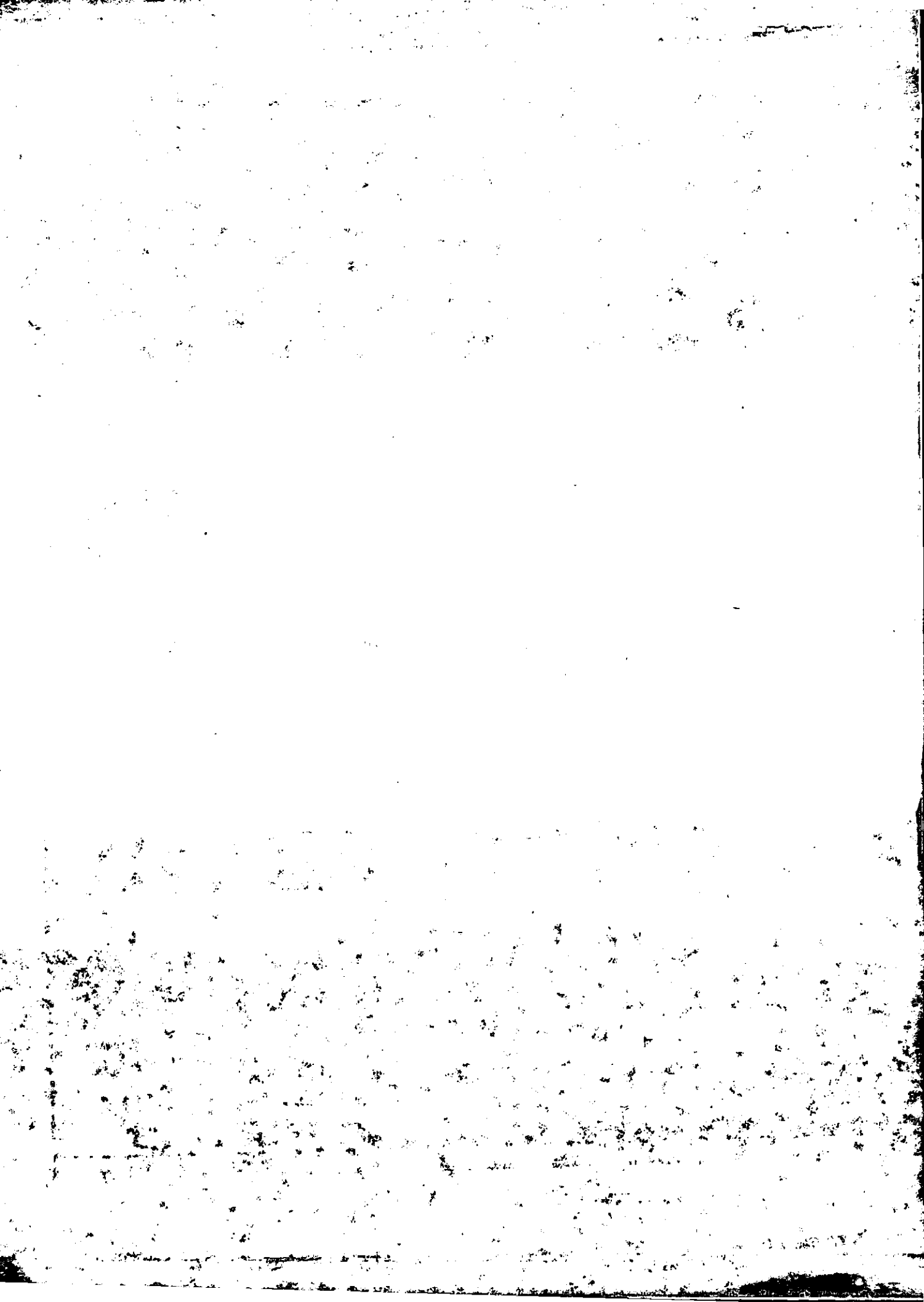
by Grover S. Krantz

and

INVESTIGATIONS INTO COMPUTER GRAPHICS:
ARCHAEOLOGICAL APPLICATIONS

by Stephan R. Samuels

**METHOD AND THEORY
IN
CALIFORNIA ARCHAEOLOGY**



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GARY S. BRESCHINI, SERIES EDITOR

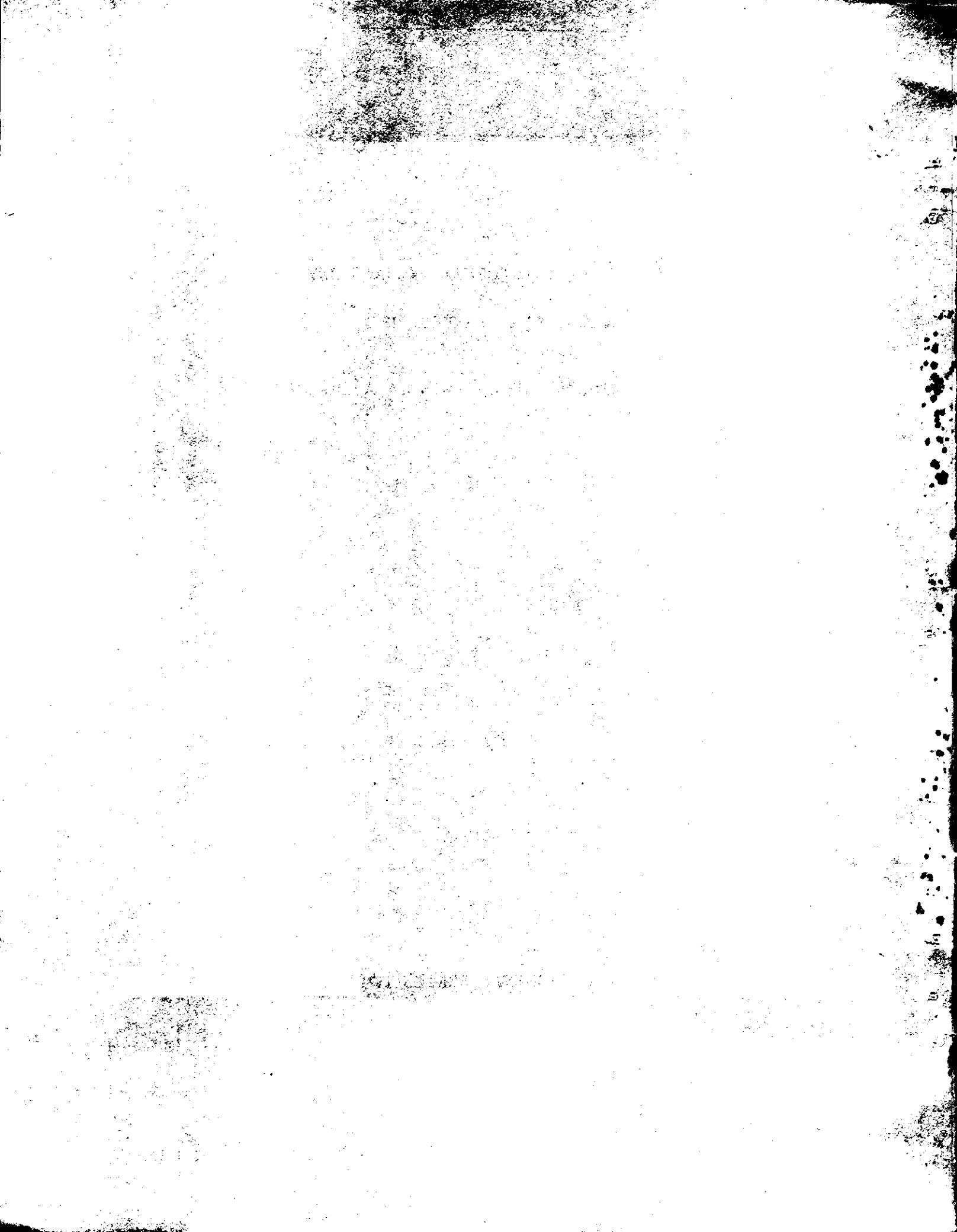


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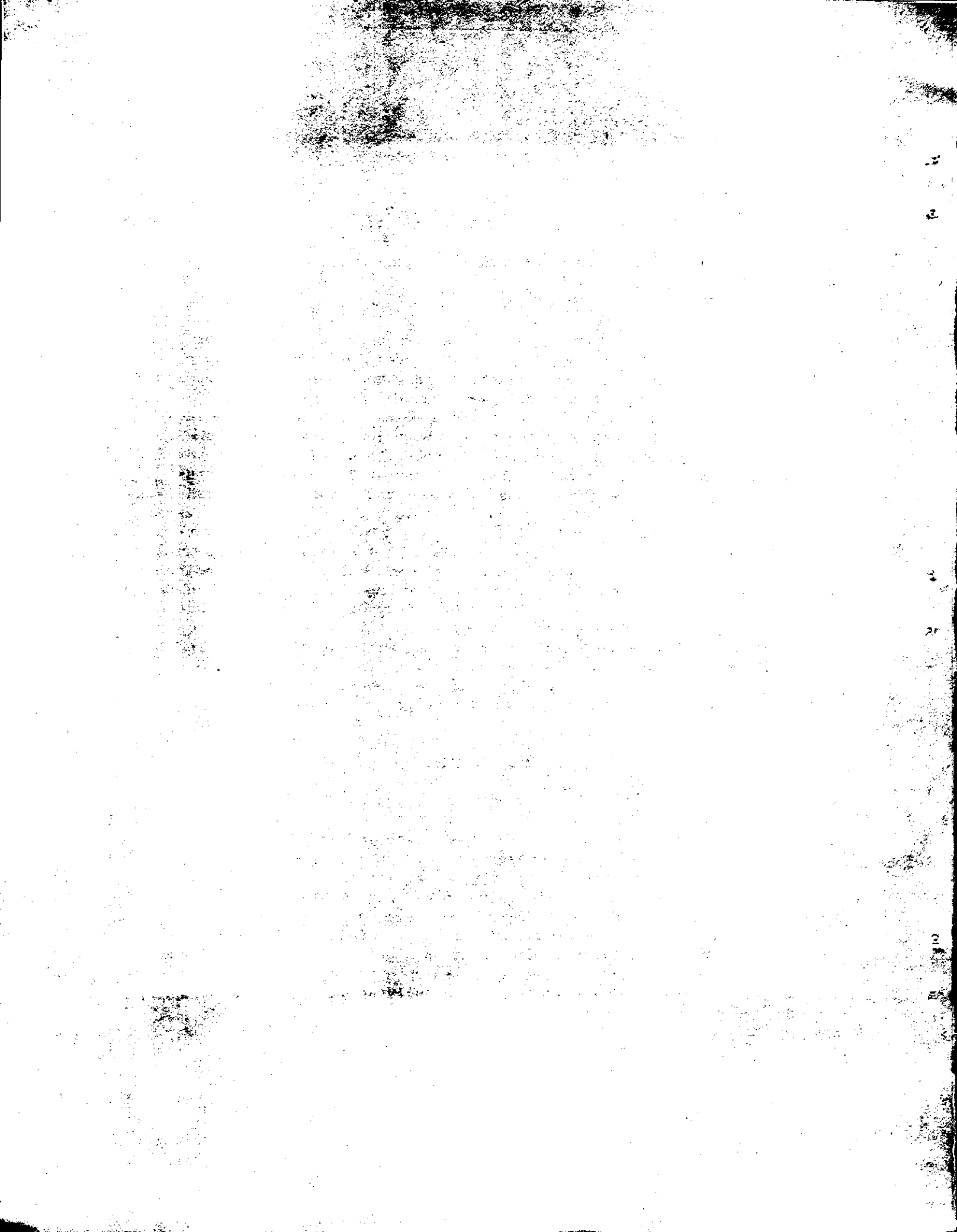
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THE POPULATING OF WESTERN NORTH AMERICA.....1
By Grover S. Krantz
Department of Anthropology, Washington State University

Abstract--The recent distributions of Indian languages in western North America can be accounted for with a single wave of immigration and with no great movements of people into or through any territory already occupied by another people. The initial immigration can be set at 12,000 years ago through the ice-free corridor in Alberta and a second opening through the Alaskan panhandle to the coast about 800 years later. The frontier of occupation would have been channeled between and around various barriers in the western U.S., thus breaking up the expanding population into groups that would eventually become the major American language phyla. The rate of frontier advance would vary according to several geographic factors so that initial areas of occupation by the various groups can be mapped. A major depopulation of arid lands is postulated with subsequent repopulation which put almost all groups in their modern locations. Some relatively minor readjustments of boundaries show regularities and can be accounted for by temporary discrepancies in cultural advantages. Language group affinities and time depths of separation reconstructed here are mostly in agreement with the general consensus of linguists. Some discrepancies on the west coast are accounted for by proposing that rates of linguistic drift are greatly affected by population size, technological changes, and foreign contacts.

INVESTIGATIONS INTO COMPUTER GRAPHICS: ARCHAEOLOGICAL APPLICATIONS.....65
by Stephan R. Samuels
Department of Anthropology, Washington State University

Abstract--This paper describes a new computerized method of data input and display. Contour maps, representing topographic or stratigraphic archaeological information, are accurately and efficiently converted into a form suitable for computer use with a sonic digitizing slate. This information is then drawn as perspective surfaces by a computer controlled pen system. Computer plots have been found to be an effective means of conceptualizing complex surface element relationships. Symbols representing object locations are able to be superimposed upon the surface representation. This method has good potential as an analytic and reportage tool for the archaeologist.



PREFACE

The following two articles make up the first issue of the new series titled *Method and Theory in California Archaeology*, published by the Society for California Archaeology as a part of their *Occasional Papers*. The other series under the heading of *Occasional Papers* deals with Cultural Resource Management. It is hoped that these two titles will compliment each other, and prove useful to the archaeological community.

The *Occasional Papers in Cultural Resource Management* will seek to publish examples of CRM reports currently being produced, as well as papers dealing with data--site and survey reports, preliminary reports dealing with new discoveries or findings, syntheses of laws and regulations, methods of site preservation, survey techniques, EIR guidelines, etc. Brief comments, book reviews, guest editorials, short articles and full length reports will all be included within this format.

The *Occasional Papers in Method and Theory in California Archaeology* series will concentrate specifically on new and different interpretations or explanations of existing data, and new methods of gathering, analyzing and interpreting data--and *well reasoned* speculation is especially encouraged. Replies to previous articles, comments, book reviews and editorials will also be included within this series. Purely descriptive reports are discouraged unless they contain highly significant data which will generate new and different interpretations.

This series, then, centers around new ideas--new ideas for the treatment, gathering, and analyzing of data, and new ideas of what the data means. As a result, there will be considerably more speculation in this series than in most. This follows Kroeber's belief that:

Now and then it seems permissible for the student to leave off his daily association with specific facts and rise above them on the gyroscope of his imagination to discover if a broader view may not give him new insights into their relations, or alter his conception of their setting in the larger landscape of nature as a whole. Such flights indeed appear almost incumbent on him at intervals if his occupation with his materials is close and unremitting (1923: 125).

Many new ideas, because they fail to conform to established theory or because they cannot be completely documented, are rejected by journals as too controversial, as unsupportable, or as speculation. This series will seek to publish these new ideas, and in subsequent issues will also publish replies and comments generated by them. It is hoped that in this manner, many papers will be published that otherwise would have been rejected, and that the readers of this series will be exposed to a large number of new and different ideas. Of course, not all of these new ideas will be incorporated into existing archaeological theory--some will probably be shown to be hopelessly wrong. Others may prove to be the foundations for a new generation of archaeological theory. Any papers which stir thought will be considered successful, since, while they may be totally wrong, they may lead someone else to a discovery, a new concept, or to a new approach to existing data.

Because many of the papers to be included within this series will be

speculative in nature, ample opportunity will be available for reviews, comments and replies, and even guest editorials. In this way, speculative articles will be reviewed by the members of the profession, and their values and shortcomings pointed out, their relative merits discussed. Through this process, new ideas can be aired and discussed, and then discarded, modified or accepted as the case may be, at the judgement of the profession.

It is within this framework that the two articles in this issue are presented. One is highly speculative, dealing with the movements of populations into and through western North America, while the other presents a computerized method of data input and display. In subsequent issues of this series, reviews and comments upon these two articles, and other articles to follow, are welcome--indeed are solicited. Also solicited for forthcoming issues are additional articles, book reviews, comments, rejoinders, replies and editorials. These may be submitted to the address listed below for consideration.

Gary S. Breschini, editor

379 Corral de Tierra
Salinas, CA 93901

Reference Cited

- Kroeber, A. L.
1923 The History of Native Culture in California. *University of California Publications in American Archaeology and Ethnology*, Vol. 20: 125-142.

THE POPULATING OF WESTERN NORTH AMERICA

by Grover S. Krantz



INTRODUCTION

The classification of Indian languages into families and phyla is largely complete for western North America. The distribution pattern presented by these language groups is very complicated and until now has defied all attempts at explanation. It will be shown that this complex distribution of peoples is not only explainable but is quite as it should be, considering the various geographical and cultural factors which have been in operation. The approach used here is essentially that of animal geography where the animal is man. The human subgroups are identified or tagged by their linguistic affiliations rather than by such things as coat color or tail length as in other animals. These subgroupings can be followed from their origins, through their dispersions, and to their ultimate distributions according to reasonable rules of behavior.

It is assumed here that ancestral derivations of people are best reflected in linguistic distributions rather than in other cultural items or even in anatomical traits. Material culture and anatomy respond to environmental pressures, but "genetic" relationships among languages are not so affected. While civilized people do learn new languages or force others to learn theirs, this generally does not happen on a large or regular scale among hunters. Probably Alfred Kroeber best expressed the idea that language, rather than any other measure, most closely represents actual biological relationships:

It is sometimes thought that because a new speech is readily learned, especially in youth, language is a relatively unstable factor in human history, less permanent than race. It is necessary to guard against two fallacies in this connection. The first is to argue from individuals to societies; the second, to believe that because change is possible, it normally takes place. As a matter of fact, languages often preserve their existence, and even their territory, with surprising tenacity in the face of conquest, new religion and culture, and the economic disadvantages of unintelligibility (1948: 220).

This is nevertheless not a linguistic study in any usual sense of the word. It is an attempt to account for the distributions of the generally accepted language phyla, but not by any means of analysis of the languages themselves. Linguistic affiliation is used here merely as an identifying tag for each population on the assumption that this best indicates historical relationships.

What results from this study is a pattern of human divisions with rather exactly defined distributions which follow automatically from geographical and biological considerations. It happens that this pattern of natural divisions coincides almost exactly with the distributions of the postulated language phyla. It is true that I had the language distributions in mind when constructing this model, but it stands on its own merits quite independently of all linguistics. Because the natural human groups and the

language phyla correspond so closely I have made the assumption that we are thus dealing with one and the same thing. A few instances will be noted where my geographical groupings of people will be at variance with commonly accepted language affiliations. In these cases I suggest that it is the proposed linguistic relationship which somehow does not correspond to actual genealogy.

The attitude has been adopted here that the distributions of all human groups follow entirely from describable reasons. The location, extent, and relationships of every American Indian language group can be described in terms of laws of human behavior which admit no exception. This is perhaps not completely achieved here, but the procedures followed here at least come remarkably close to this goal.

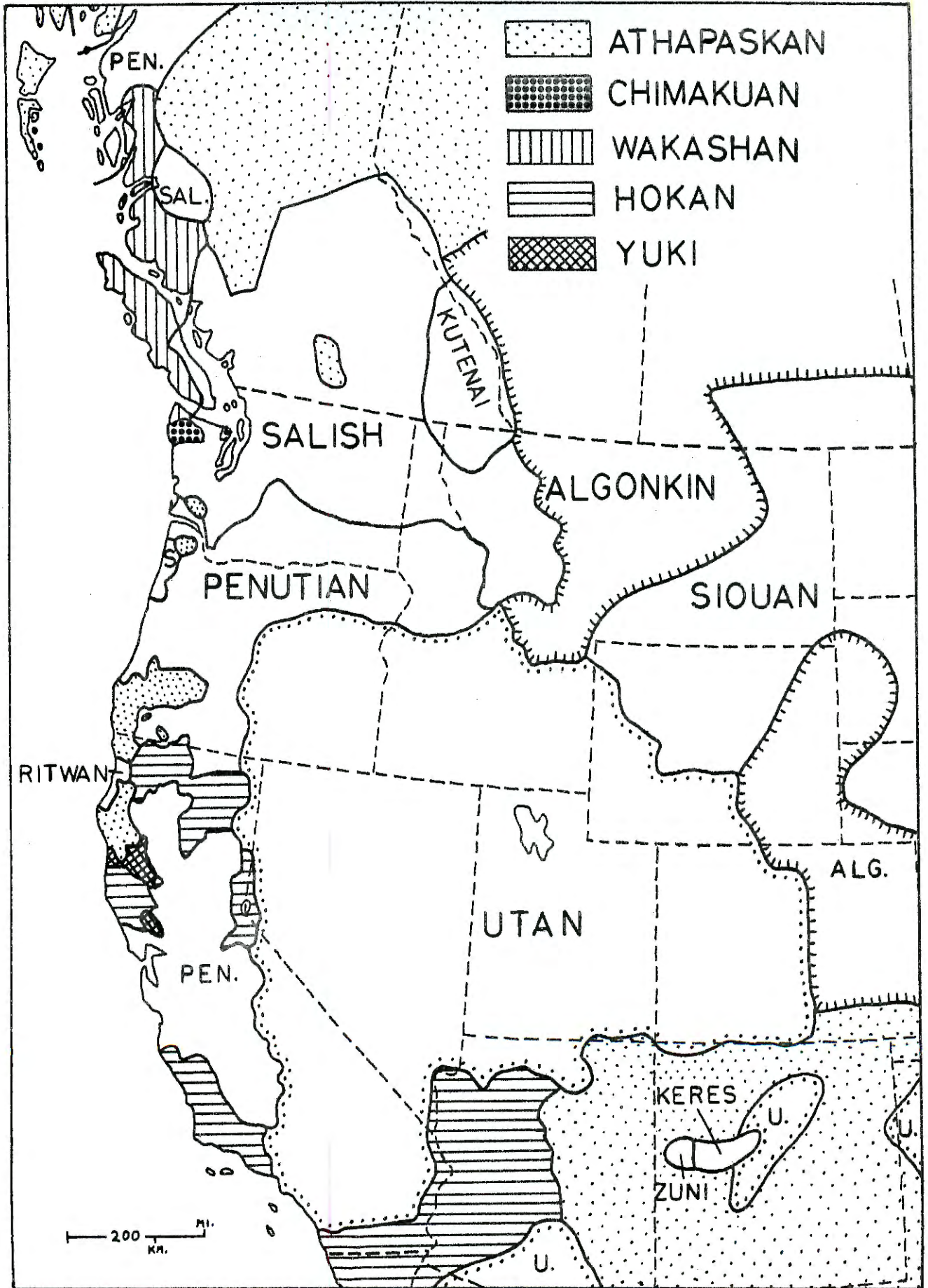
Western North America is the subject of this study because it was here that I first saw how the various laws of movement could be applied. It was noted that western North America is one of the linguistically most complicated areas of the world that is at the same time well studied and mapped (See Fig. 1). Postulated human distributions can thus be tested against language distributions. It was also realized that the rules being used here are applicable only to hunting and gathering peoples. The rules for agricultural populations are somewhat different and have not been fully worked out.

Previous attempts to account for these language distributions have involved postulated migrations of various kinds and are too numerous and too futile to be worth reviewing here. Whenever a human migration is postulated, in order to be considered seriously, it should include at least some kind of an answer to each of the following five questions:

1. Why did the people move at all rather than simply remaining where they were?
2. Why did they move where they did rather than to some other location?
3. Why did they move when they did rather than at some other time?
4. Why was it they who moved there and then rather than some other people?
5. How was the move accomplished, especially if it was into or through an area already occupied by other people who would oppose such a mass action?

If any of these questions cannot be answered the proposal is weak. If none of them are answered it is hardly a proposal at all. To postulate a migration without answering these questions is only to restate the fact of lin-

Figure 1. Generally accepted language groupings in western North America at the "Ethnographic Present." Tlingit and Haida are included with the Athapascans, as this will be supported here. Tsimshian and Chinook are shown as Penutian, but this will be challenged. Salish, Wakashan, Chimakuan, and Kutenai are mapped separately, because it will be shown that combining them into a "Mosan" phylum is not realistic. Ritwan, Yuki, Zuni, and Keres are shown separately, as their assignments here will differ from those of many authorities. (Major sources for these distributions are Kroeber 1925, Berreman 1937, Stewart 1966, Voegelin and Voegelin 1966, and Jorgensen 1969).



guistic distribution as though such a restatement explained something.

In addition to answering the above questions it is to be desired that each answer be in the form of a law of human behavior. By a law I mean a statement that any human group under the same circumstances would have done the same thing. This is specifically to avoid historical explanations where behavior is "explained" because people were led, pushed through, chose, persevered, or otherwise did something unique. Such historical explanations are of no value, because like an act of God, they are not predictable and are not susceptible to disproof by evidence. This is to say that an historical explanation, once postulated, makes further investigation or elucidation pointless. It is the regularities, not the irregularities, of human behavior that must first be discovered.

It should be explained here that among hunting peoples my use of the term migration does not refer to geographical displacement or relocation of a group, but rather to the extension of a frontier. Thus a migration is not one that leaves an empty space behind--large groups of hunters will not voluntarily abandon a perfectly habitable area. The analogy is not to a rolling ball but rather more like an unrolling carpet. The rolling ball analogy may apply to individuals and small groups like families or bands, but larger groups would require some special reason to abandon their homelands.

Of all anthropologists who have attempted to explain some of the linguistic distributions dealt with here, only Melville Jacobs (1937) made a real effort to account for the movements with a set of universally applicable rules. His effort largely failed because it was too limited in scope and his rules of movement were not all correct. Still, Jacobs' effort stands in sharp contrast with others, like Sapir (1929), who postulate movements of people for no reason at all.

RULES OF MOVEMENT

The actual rules of movement for hunting bands are rather simple in principle but tedious to explain. (1) The first group into an area tends to occupy that area indefinitely; (2) technological advantages may cause regular shifts of language boundaries. Each of these requires some elaboration before it can be applied to the actual situations.

First Group In

Stated simply, this rule is that no hunting people can move into or through an area which is already occupied by another similar group. At first glance this would appear to raise an impossible barrier to any theory involving migrations of any kind, but it will be shown that in the long run it actually makes things much simpler. This proposition is elaborated in detail elsewhere (Krantz 1976) and will be only summarized here.

History is full of migrations where people have moved through inhabited regions and displaced others in settling new lands. What is generally not appreciated is that all such movements were made by people who had the power to do so, and that this generally included a food producing economy--agricultural and/or pastoral. Without thinking about it, most anthropologists have assumed that hunting people were equally capable of overrunning their neighbors. Those cases that have been observed may be results of contact situations where one group has imported weapons or the other has been dec-

imated by new diseases.

Successful migrations are made possible only when the movers have the military capacity or its equivalent to overcome any real or passive resistance. When two agriculturally based peoples engage in a dispute over territory, the one with the greater resources will prevail. When an agriculturally based people disputes with a hunting and gathering group the agriculturists will ordinarily prevail for obvious reasons. Farmers have more people per square mile, free seasons in which all military effectives are available, and transportable food supplies. Often they also have control over larger areas to draw upon for resources, better political organization, greater social prestige, transport animals and vehicles, and superior weapons. The hunter may be a better fighting man but his limited numbers and his need for almost daily foraging for food will always lead to his defeat in the long run. The hunter simply has none of the military or other advantages mentioned above.

When two hunting groups contest a piece of territory, neither side has any of these advantages. If they do fight, both sides have the same minimal capacities, with one major exception. The group defending its own land will know the territory and its resources, thus giving them a significant edge. When all else is equal, the defender should always prevail. Verne Ray saw this clearly for eastern Oregon Indians, but failed to generalize on the principle:

In these contests the Shoshoneans often pushed as far north as the Columbia River...but the invaders never remained long and in no case established permanent camps. Any attempt would doubtless have resulted in failure, for the balance of power was at all times very even and the Sahaptins were on home ground (1938: 391).

If a permanent movement of hunters is proposed to have happened, some indication must be offered of how this could have been accomplished in the face of resistance. While such movements can and did occur, they were rare, and followed only from exceptional circumstances that can be discovered.

In general, the rule is that if hunting peoples expand their area, it is only into essentially empty territories, and never at the expense of previously settled inhabitants. The obvious application of this rule is to the initial occupation of a continent like North America, which must have been a single event and not a series of waves of immigration. An equally obvious application is in the reoccupation of regions that became temporarily depopulated for some reason. (Depopulated is defined as the removal or drastic reduction in the number of people.) Since one or more major desiccations apparently emptied large parts of western North America, their reoccupation would follow the same rule. Populations surrounding the largely empty spaces would, with the return of favorable conditions, expand their numbers and territory to move inward as long as the original inhabitants were too few to recover significant population numbers in the same time. It will be shown that this process can lead to significant changes in linguistic boundaries without involving any people greatly interfering with the territory of any other.

A relatively minor variation on the rule of first occupancy is that large groups of people will tend to linguistically absorb smaller groups when in close contact. A rough rule, to judge from ethnographic data, is that any

group numbering fewer than 500 people will be linguistically overwhelmed by their neighbors if these include more than twice as many people. This tends to eliminate many small groups.

Mountain ranges often double as linguistic divides. Sometimes this results from the simple fact that they are physical barriers when the area is being occupied. People who reach one side of a mountain barrier first may be stopped long enough for the other side to become occupied by another people. When the barrier is less than absolute, people may pass over it in small numbers only to be swallowed up by greater numbers arriving later on that side. This would leave the mountain forming a linguistic dividing line as if the people had arrived on each side of it simultaneously.

Movements of individuals or family groups across language boundaries will ordinarily have no lasting effects. Such small groups moving on whim or for personal reasons tend to go in both directions over any boundary and are soon linguistically absorbed by the larger community.

Technological Advantages

The second major rule of movement is that linguistic boundaries may be shifted when those on one side of the line acquire some advantage which enables their people to take over new territory regardless of resistance. Such an advantage would most obviously be some technological innovation which increases the possessors' prestige, wealth, numbers, military capacity, or any combination of these in comparison with their neighbors. Any such innovation might enable one people to acquire some territory from a neighboring group before the innovation is diffused and becomes fully exploited by the receivers.

These innovations may often be discovered indirectly by their effects on linguistic boundaries which will tend to be uniformly shifted in a particular direction. When distributional discrepancies are discovered which are not accounted for by application of the rule of first group in, and if these occur in a regular pattern, then a technological innovation may safely be postulated. Five examples of this kind were found outside of the agricultural southwest and the innovations responsible were easily identified.

The mechanism of boundary shifting was probably often peaceful and was by means of infiltration and sheer weight of numbers from the expanding population. While these movements may have involved hostilities at times, this was probably not the primary method. Once peaceful exchanges of ideas and personnel have diffused the innovation, and its results are fully developed, then equilibrium will return and no further boundary shift can occur. The recipient population then puts pressure on its other neighbors and the process is repeated. This domino effect continues until an essential resource is no longer available, or the process is interrupted by outside intervention.

These are the basic rules that determine the human movements which are recorded in linguistic distributions. While the same rules apply in general to agricultural populations, such peoples have further capabilities which follow from their greater numbers, free seasons, and transportable food. Commercial and "cultural" activities of agriculturalists are still other factors which complicate the potential movements of post-Neolithic peoples. Because the effects of these additional forces have not been fully understood and quantified, the agricultural southwest will here be dealt with in only a

rather superficial manner. Elsewhere in western North America one of these forces, that of commercial activities, proved to be a significant factor in explaining the peculiar situation of the Chinook Indians.

DATE OF ENTRY

The first human occupation of America beyond the unglaciated parts of Alaska is set here at 12,000 years ago on the carbon 14 time scale following Haynes (1964). There is much difference of opinion on this timing, but I believe it can be settled with relatively little difficulty. In general, the reconstruction developed here is a sequence of events that is not dependent on any particular absolute time scale. Fixing the date of initial immigration at 12,000 years ago is not especially critical in itself--8,000 or 18,000 would do just about as well--but the event is closely tied to the paleogeographical situation. Glacial distributions locate the two very specific points of entry, so if these initial openings are dated, then so is the appearance of man in this region. Also, I am using the standard carbon 14 dates as commonly given without the newly proposed correction factor (Renfrew 1973). This correction would set back all the dates in a regular manner and would not affect the sequence of events in any way.

One argument for the 12,000 year date is simply that if the immigration occurred at any other time in the glacial sequence the resulting distribution of peoples would not be that which in fact exists. An earlier or later flow of immigration would have followed a different pattern coming from different entry areas and these cannot be reconciled with known linguistic distributions.

The actual archaeological sites which are dated in excess of 12,000 years ago can be used to argue against their own validity from at least three points of view. (1) If one simply observes the plus-minus figure given after each date it will be found that many of these dates may actually fall within two standard deviations of the time scale accepted here.

(2) The pattern of early dates, plotted by century, presents an interesting picture. Archaeological sites understandably decrease in frequency as one goes back farther in time and this ought to be more or less a gradual change. In America, however, there is an abrupt decrease in sites beyond 12,000 years ago. If the area were in fact inhabited at some earlier time, we are hard-pressed to explain the great difference in the number of sites dated in the millenium preceding 12,000 B.P. as compared with the millenium following that date. This discrepancy was acceptable when only a handful of sites was known of such antiquity, but now the contrast is too great to be ignored. It is easier to accept the idea that some error has crept into the dating of a few supposedly early sites than it is to explain why their time range has not included many more discoveries by now.

(3) While all American sites with dates older than 12,000 years have been questioned on some technical grounds it remains to be demonstrated just what the sources of error may be. This is still not a satisfactory reason to accept any of them, even for the time being. There are also many sites which have yielded carbon 14 dates that are clearly too recent to be correct. Often these spuriously young dates are not published, though every archaeologist is aware of some examples. Those that are published rarely receive the special attention they deserve. The significance of these inexplicably recent dates

is that they also are often quite secure and no flaw can be found in their determination. Prehistorians who can reject spuriously young dates without proof of dating error should be capable of rejecting a comparable number of dates that may be spuriously old. In fact, logic requires that rejection of a set of too young dates be matched by a rejection of some too old dates. You cannot have it both ways--if the very old dates are tentatively accepted, then all well controlled carbon dates must be equally accepted, however recent. I find no difficulty in dismissing as spurious-for-unknown-reasons a few archaeological dates, some too old and others not old enough.

RECONSTRUCTION BY BACKTRACKING

With all of the above principles in mind, one can roughly reconstruct the major events in North American prehistory by backtracking from the present situation, peeling off one layer at a time. Starting with the distribution of language groupings in the ethnographic present (Figure 1), the first step is to remove the horse-based Indians from the High Plains. This leaves a largely empty strip running from southern Alberta almost to the Gulf of Mexico, regularly inhabited in only a few places and sporadically elsewhere. The next step is to remove the Apaches from the southwest and to replace them with larger areas for the various indigenous agriculturalists. Still earlier, these agriculturalists must be backed off to some degree from territory they usurped from hunting peoples, probably mostly of Utan affiliation. (It should be noted here that I am using the name Utan for the phylum to include the Uto-Aztecan and the Kiowa-Tanoan families. This is partly for the sake of simplicity, and partly in recognition that Utans have continuously occupied the group's homeland since its origin. This also avoids the necessity of changing the phylum name every time a family is added or removed.)

The great geographical extent of Utan speakers can largely be explained as a consequence of refilling vast areas which had been temporarily depopulated (or greatly reduced in population) during the Altithermal. A core area for the Utans may be postulated in the Colorado Plateau (adjacent parts of Utah, Colorado, and Arizona) which they occupied from the beginning, and from which they expanded in all directions at the end of the Altithermal as shown in Figure 14. While the depopulated areas may have been previously occupied mostly by non-Utans, the reoccupation would tend to divide them about equally between Utans and non-Utans alike (see Figure 2).

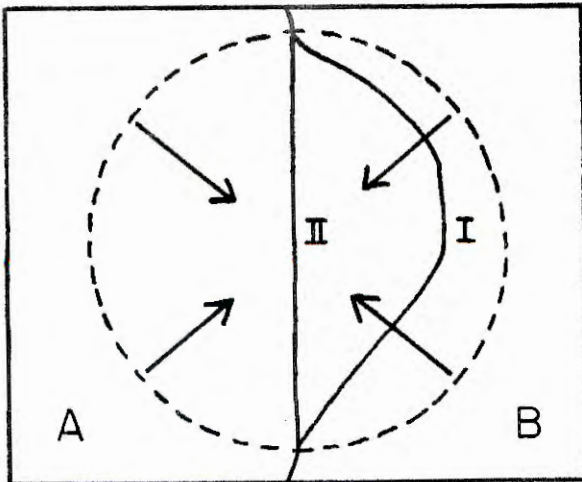


Figure 2. Idealized picture of how a boundary shifts as a result of depopulation and reoccupation of an area. Languages A and B meet initially along line I. This is followed by desiccation seriously reducing the population within the dashed central area. Most of the depopulation affected language A. Upon return of better climate, people expand into the area from all directions resulting in boundary II in the center. Language B thus gains considerable territory without actually displacing an entrenched population.

Backtracking to the pre-Altithermal, we need some mechanism to distribute the original inhabitants in their core areas--Utans on the Colorado Plateau, Hokans in most of California, Penutians in western Oregon, and Salish in western Washington and British Columbia. East of the plains would be a rather homogeneous Algonkin territory, interrupted only later by agricultural people. Some other items which remain to be explained are the diverse language families of the Northwest Coast, the Athapaskans on the U.S. Pacific coast, and the Penutians in Central California. If the above described distributions can be accounted for at an early stage, then all subsequent relocations are potentially explainable.

GEOGRAPHICAL SETTING

The initial occupation of America below the Canadian ice field stemmed from two sources--down the ice-free corridor in Canada, and through south-east Alaska to the coast. The opening of the corridor at 12,000 years ago would place the first inflowing Indian population at Edmonton, Alberta, as a convenient starting point for present calculations. The other opening was from northwest Canada along the Stikine River to the coast about 800 years later. The major flow from the Edmonton area will be dealt with first.

The physical geography of western North America would automatically cause a division of the first immigrants into several separate groups which ultimately should become marked as language phyla. The earliest people expanding through the ice-free corridor would spread out rather freely to the south and east, but in the west there were only four major routes around and through the mountains with their glaciers and Pleistocene lakes. As people expanded from the plains area along these four routes they would have been separated from each other for several centuries--enough to form separate dialects, if not languages.

In order to predict the rate of movement of the expanding human frontier from the glacial corridor, a number of estimates must be made regarding their maximum expectable reproductive potential and of the various delaying factors. As an initial working hypothesis (later modified) it was assumed that the original hunting bands would have to be treated as though organized into macrobands or dialect tribes of some 500 people each and covering areas with a typical diameter of 100 miles (160 kilometers). Such a tribal unit on the frontier would increase in population at its maximum potential rate and expand its territory into previously unoccupied lands. After the tribe has doubled its numbers and area it would then subdivide into two such units, one retaining the original area and the other becoming a new frontier tribe which would then repeat the process. This would go on repeatedly with more and more tribes being left behind with each bifurcation on the moving frontier. A zone ranging from 100 to 200 miles deep (160 to 320 km) would thus constantly be feeding new individuals, families, and/or bands into the uninhabited areas. It is assumed that the human reluctance to enter new areas and preference for living near kinsmen would mean all areas would be filled to near carrying capacity immediately behind the frontier.

The time required to double the population of such tribal units under various circumstances then determines the speed with which the frontier advances, 100 miles per doubling time. Actually, while tribal organization might have existed we may never be able to determine this, nor does it make any difference. A frontier-feeding zone 100 miles deep with lesser contri-

butions for another 100 miles would satisfy this picture regardless of the higher social organization. Thus the doubling time refers to the feeding zone, whether it is tribally organized or not, but it can be treated as though it were made up of such tribes.

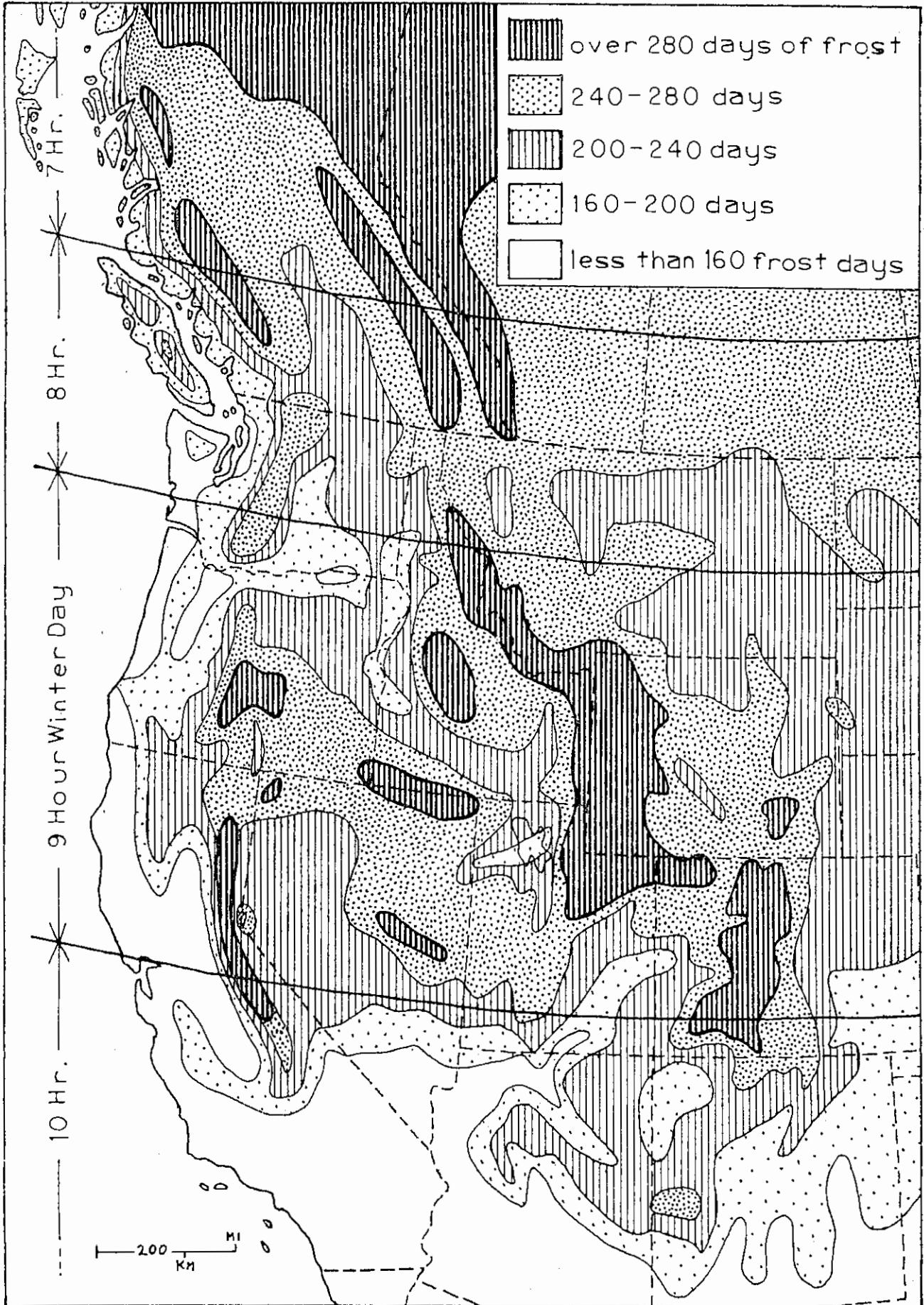
The actual rate of advance of this expanding frontier is very significant and will vary with circumstances from place to place. There will be a basic reproductive rate which can be variously reduced by such variables as length of winter season, number of daylight hours in midwinter, geographic fanning effect, and delays in changing major ecological zones.

A common estimate of maximum human reproductive capacity over a sustained period is to double the population in each generation of 25 years. An advance of the frontier by 100 miles every 25 years (over 6 km per year) should therefore occur under ideal conditions.

Winter is a season of increased casualties. Its various hazards on occasion lead to deaths from starvation, exposure, and carnivore predation. These losses of personnel can be expressed in terms of an increased length of time for a given population to double its numbers. Longer and more severe winters would lengthen this doubling time more than in the less intense winters. *Goode's World Atlas* (1960, or any recent edition) provides a convenient map of zones of winter duration in the United States, from average first to last frosts of the season. Intensity will generally parallel duration and thus it provides a suitable measure of this debilitating phenomenon. *Goode's Atlas* divides the U.S. into six zones which are copied, slightly simplified, in Figure 3, with the two mildest zones being combined into one which was considered to have had no measurable impact on human mortality. The next, more severe, winter zone was arbitrarily assumed to have added five years to the normal doubling time of any population living within it. For the next zone 10 years were added, then 15, and finally 20 years were added for the most severe winter zone. Accordingly, people living in the areas of the worst winters are assumed to have required 45 years to double their numbers and territory, all else being equal. Admittedly these values are not based on any particular study of human mortality rates, but they are merely rough estimates of what might reasonably have been the case. The extension of these same winter zones into Canada was based on a study of maps of comparable vegetation, altitudes, latitudes, and continentality.

The factor of high latitude by itself, in addition to contributing to longer winters, also results in fewer hours of midwinter daylight. This puts limits on the amount of time during which people can move about hunting, foraging, and collecting such items as firewood and water, and thus tends to increase winter survival problems. Accordingly, I added daylight zones to Figure 3 in terms of the number of hours in each midwinter day. A ten hour day was assumed to have had no significant deleterious effect; for a nine hour day an arbitrary five years were added to the frontier-population doubling time, for an eight hour day 10 years were added, and for a seven hour day 15 years

Figure 3. Climatic measures of winter severity. Five zones of winter length are shown in terms of average number of days from first to last frosts. This modern situation would hold approximately for terminal Pleistocene times though the number of days would have been different. The number of hours of midwinter daylight are given in four zones. (Winter lengths from *Goode's World Atlas*, 1960.)



were added. Using both winter length and daylight hours, the extreme condition could be as much as a 60 year doubling time for the most adverse conditions in contrast to only 25 years for the most favorable.

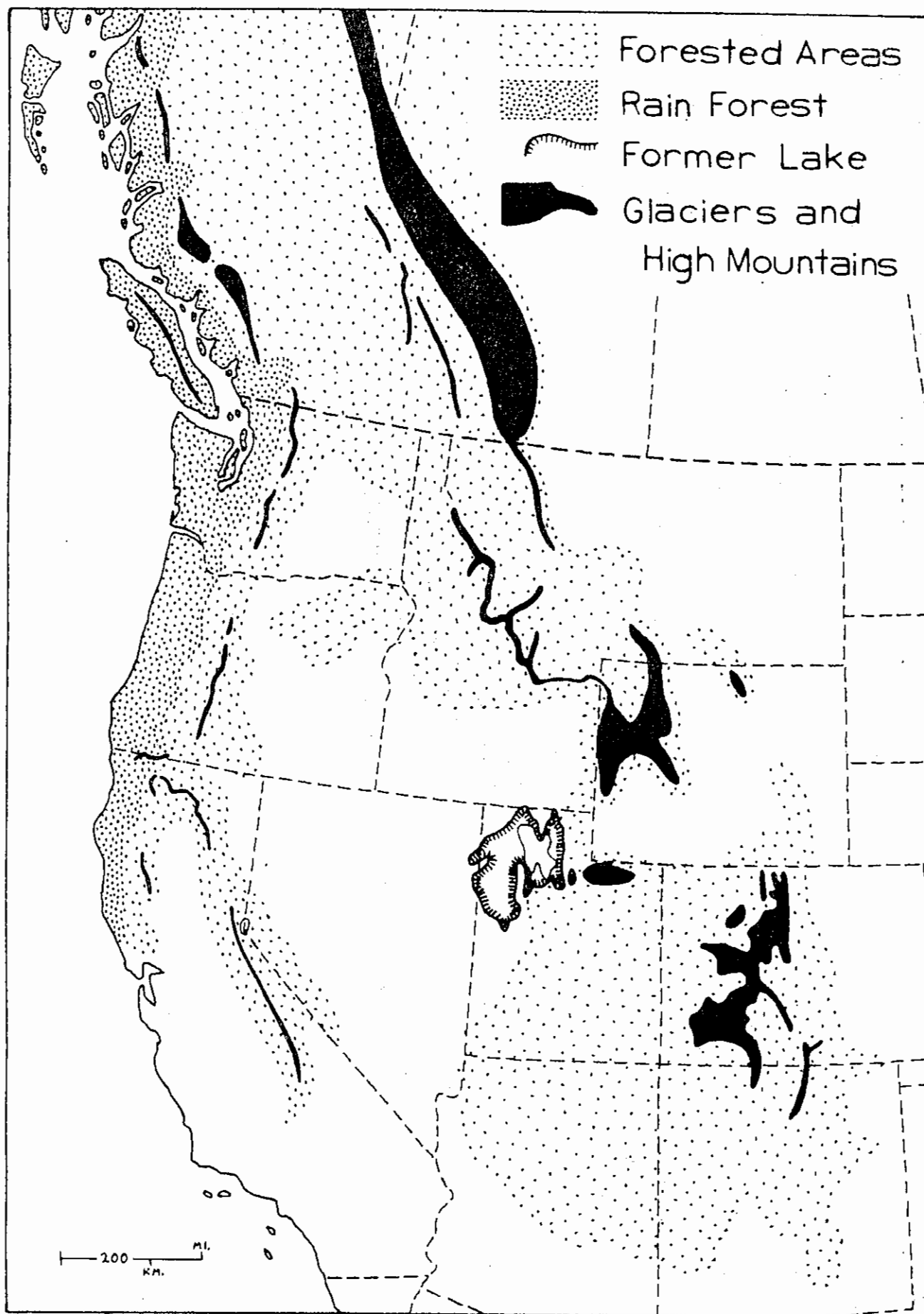
When an advancing frontier moves beyond a restriction and is able to fan out into a wider area its reproductive potential is diverted into more than one direction instead of just straight ahead. This was quantified simply by noting that an opening of one "flank" would divert the population in two directions, thus doubling the time required to double the population, and reducing forward movement by a factor of two. For an opening of both "flanks," as in moving from a mountain pass area into a broad plain, the reproductive doubling time becomes three times greater, thereby expressing the three directions in which the frontier temporarily moves. These fans are marked in Figure 11 with the letter "F" at each appropriate location. It should also be noted that these fans are variously used along the entire front or just on the diverging flank depending on the relative sizes of each. A wide front was considered unaffected by a small diverging part, but narrower ones will be delayed in their entirety as some of the reproduction is diverted into the branch.

A complementary funneling effect was also considered, but there appeared to be almost no locations where it would have been especially pertinent. One possible case of funneling in northern California will be mentioned in the text below, though it is better described by other means.

Major changes in ecological relationships could also cause significant delays in the frontier expansion. People would learn to hunt new animals and to hunt familiar animals which have new habits, they would discover and learn to exploit new vegetable food resources, their clothing and shelters would be changed according to the new needs and resources, and finally the inevitable casualties would result during this reorientation. Many of these environmental adaptations would be so gradual as to cause no measurable delay, as for instance in the southward movement from Canada to the gulf coast of Texas. In other places the changes would be much more abrupt--entering or leaving a forested area or in passing from a simple woodland into a temperate rain forest. Full desert conditions probably did not exist 12,000 years ago.

All ecological changes are gradual to some degree, but the various forest zones generally show their major effect in less than 100 miles. A three generation (75 year) delay factor was arbitrarily assessed for the crossing of each of these lines of ecological change. The areas of open country, forest, and rain forests are shown in Figure 4. These ecological zones represent a considerable simplification from the latest available maps of potent-

Figure 4. Physical barriers to human occupancy at the end of the Pleistocene. High mountains, with or without glaciers, and the Stansbury level of Lake Bonneville are treated here as absolute barriers. These are shown according to their probable extent at the time each was first encountered by people following the scheme presented here. Thus the east side of the Canadian Rockies is at 12,000 years ago, and for other areas the time gradually moves up to about 11,000 B.P. (Data drawn from many sources and personal examination of most areas.) Plains, forests, and rain forests present ecological boundaries which should have significantly delayed human movements. These zones would have been different in the past, but in lieu of complete mappings, the modern zones must be taken as reasonable approximations (simplified from Küchler 1966).



ial natural vegetation issued by the United States Geological Survey and contained in the National Atlas of Canada. It may be rightly argued that these maps do not represent the conditions of 11,000 to 12,000 years ago when the climate was different. Since no appropriate maps are available of the entire area for the terminal Pleistocene conditions, there was no obvious alternative. While the amount of precipitation and tree growth was for the most part greater at that time, the general picture of comparatively open plains versus forests would have been at least similar to the recent condition. Because of this problem a certain amount of error will occur as to where and when the frontier crossed each of these boundaries, but the fact of a certain number of major ecological readjustment delays would probably remain essentially as given.

Figure 4 also shows the total barriers of glaciers, high mountains, and bodies of water which people could not have crossed at that time and which therefore channeled the advancing frontier.

The glaciers do not represent any single time, but rather a "creeping effect" of their condition when people first encountered them. The eastern edge of the Cordilleran ice sheet is shown in Alberta at about 12,000 years ago as a black area. Its other side in British Columbia is of a later date after the immigrants had advanced some distance into the U.S., moved around its southern edge, and started back north again. The mountain glaciers are shown with the same idea in mind so that the great glaciers of the Sierra Nevadas, for example, are reduced to a thin line by the time of human arrival.

Mountain barriers are shown in Figure 4 as thick lines which are not obviously distinguishable from the glaciers. In many cases it is not clear whether the high mountain ranges, glaciated at one time, were ice covered or not when people first reached them. In any case, these taller mountain ranges would present some ecological as well as physical obstructions, and would certainly have delayed human expansion if it did not actually stop it altogether.

Of all the Pleistocene lakes only Bonneville constituted a substantial barrier at that time. This lake, previously much larger, was at that time no higher than the 4,500 foot Stansbury level (about 1,370 m), and functioned as a westward extension of the Uinta mountain barrier that stretched across much of northern Utah. Its sister lake, Lahontan in Nevada, was apparently dry at this time because its presence could have blocked human movements which appear to have been unaffected. No other glacial lake was large enough, if extant, to have had a noticeable effect.

Enough information has now been presented to reconstruct the actual expansion routes and rates of advance of the first Americans. Figure 11 shows the major possible lines of movement through western North America along with some of the more important branches. The starting point is at the lower, open end of the ice-free corridor, arbitrarily placed at Edmonton, Alberta. All of the lines are drawn to pass comfortably around each of the various absolute barriers and continue until they meet one another or reach the ocean.

THE CENTRAL IMMIGRATION

The first aboriginal Americans entered a continent with no other human occupants who could contest the territory. A maximum rate of reproduction would be expected along their advancing frontier, subject only to the geographical and climatological restrictions described above. This immigration

would thus not be uniformly rapid in all directions nor would it present an even front. The pattern of initial occupancy would instead follow particular routes at various rates of advance. These can be predicted with some accuracy.

The Eastern United States

Incoming Indian populations in what is now southern Canada would have advanced to the south and southeast at a relatively slow rate at first and their frontier would expand in concentric arcs. Throughout the central and eastern U.S. this expansion would continue relatively unimpeded. The rate of advance would increase rapidly to the south, and somewhat to the east, as milder winters and lower latitudes were reached. Only one significant ecological transition, that of entering the eastern forests, would briefly slow down this advance. The Great Lakes at that time were barriers comparable to their present sizes and people would have had to move around them. Early Lake Superior would have divided the oncoming people for about 250 years before they met again at its eastern end. With continuous occupation around the lake ever since that time, the presumed dialect distinction at that meeting would probably not have developed into anything more significant.

Most of the eastern U.S. would have been fully occupied in about 1,000 years, or by the year 11,000 B.P. Eastern Canada and all areas to the north of the ice margin could have been occupied only as the ice melted. This northward movement therefore may not have been nearly as rapid as the people's reproductive potential would permit.

The only major separation of eastern populations would have been the division between Newfoundland and the rest of eastern North America. This geographic phenomenon is nicely paralleled by the linguistic distinction of Beothuk from the otherwise rather uniform Algonkin language family. The linguistic complexity of the southeastern U.S., as well as some northward interjections, are the result of agriculturally based peoples and will not be further dealt with here.

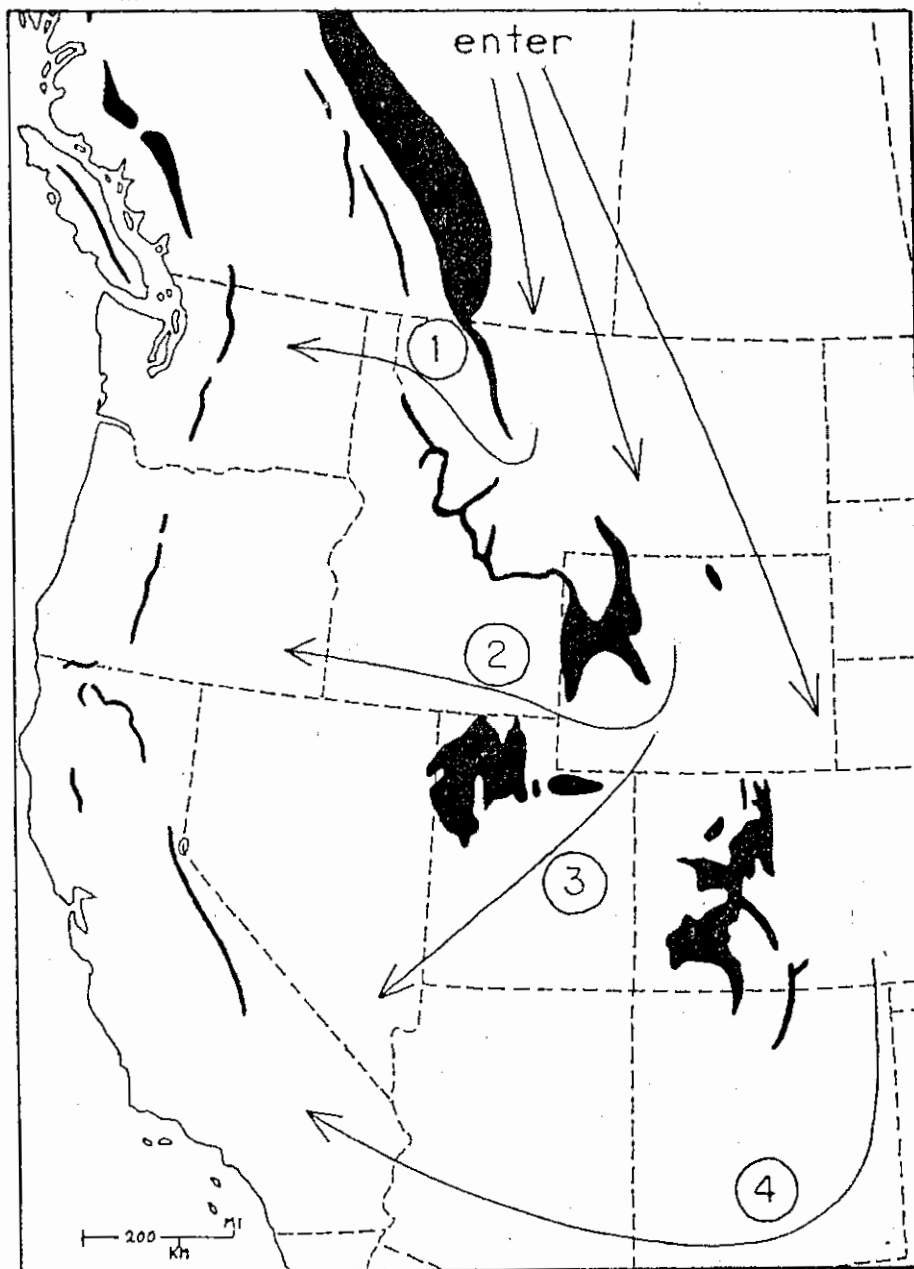
Northward moving Algonkins in Canada followed the retreating ice margin. At the same time early Athapaskans would have moved toward them from the far northwestern part of Canada, likewise occupying territory being vacated by the waning Laurentide ice sheet. The dividing line between these Algonkins and Athapaskans is about where they should have met when the ice finally disappeared. There is no need to postulate a more recent Athapaskan intrusion, overriding all peoples in their way, to account for the present location of these arctic hunters. To explain the Athapaskan distribution by such means would raise the questions of why they moved, why there, why then, why not someone else, and how it was accomplished. To explain the Canadian Athapaskan distribution as I have just done answers all of these questions.

One might wonder if the Algonkin expansion into eastern North America could have occurred at some other time. At no later date would their boundary with the Athapaskans have been located where it is (if it would occur anywhere at all). At a much earlier date the linguistic unity of the Algonkins would be impossible to explain; linguistic drift should have broken it up much more than is actually the case. The Algonkin near-unity is difficult enough to account for as it is, and will be dealt with in the linguistics section of this paper.

The Western United States

There are only four major routes of entry from the High Plains into

Figure 5. The four entry routes into western North America from the plains area. Barriers of glaciers, high mountains, and water are shown in solid black. Human immigration would be channeled between and around these obstacles. (1) the Salish route passes through western Montana to Washington; (2) the Penutian route is from southwestern Wyoming and into Oregon; (3) the Utan route separates from the above and runs across Utah; and (4) the Hokan route passes entirely around the Rocky mountains to the south, then continues west to California. The arrows marking these routes are stopped in this figure at the points reached by the advancing frontier at about 11,100 years ago, as calculated here.



western North America, the area of major concern here (see Figure 5 above). From north to south, these paths were followed by peoples who are obvious candidates to have eventually become Salish, Penutian, Utan, and Hokan speakers. The major lines of movement are also shown in Figure 11, each line being marked at 100 mile (160 km) intervals, and at each of these marks is shown the calculated date at which the expanding frontier passed that point. Tables 1 to 4 give a breakdown of the calculations used to arrive at each of these figures.

Anyone interested in seeing the correlation of all the factors described here could have copies made of Figures 3 and 4, then superimpose these on a light table. For more detailed study I recommend obtaining the U.S. Geo-

logical Survey maps of each western state at the 1:500,000 scale and with 500 foot contours. Similar maps are also available of Alaska and Canada. For still greater detail the reader might also examine all the pertinent areas and mountain passes in person, as I did, but this could take several years.

The Salish Route

The presumed Salish route is the northernmost of the four, and its terminus, which centers on western Washington, is the basis of the linguistic affiliation postulated here. Since it is not likely that anyone could subsequently displace a population from the well-watered Washington coast, the first occupants must be the ancestors of the present Salish speakers. The first immigrants following this route (see Figure 6) would pass around the glaciers in west-central Montana and then find open access to the west over modest passes not exceeding 6,000 feet (about 1,800 meters). This section of the frontier would fan out to the south as well as to the west, but the southern movement would be blocked at the Bitterroot Mountains in eastern Idaho. The western movement passes easily into the drainage of the Clark Fork River, a major tributary of the Columbia. Further expansion can only occur to the northwest along several parallel valleys, with some major fanning into interior British Columbia. The expansion continues then straight west across northern Idaho and into Washington's Columbia Plateau.

The northern Salish frontier could advance only with the retreat of the ice cover. The dates of this progress are given according to the calculated rate of movement into unoccupied territory. These dates could be significantly too early because the glacial retreat may not have been as rapid as populations were able to expand. The limit of northward expansion would be where these people encountered Athapaskan speakers moving southward on the

other side of the same ice field. The meeting line cannot presently be delineated with any precision on geological grounds, so it was placed in a "reasonable" area consistent with later linguistic phenomena. This meeting line should be at least far enough north to include the area of the Bella Coola, a Salish group,

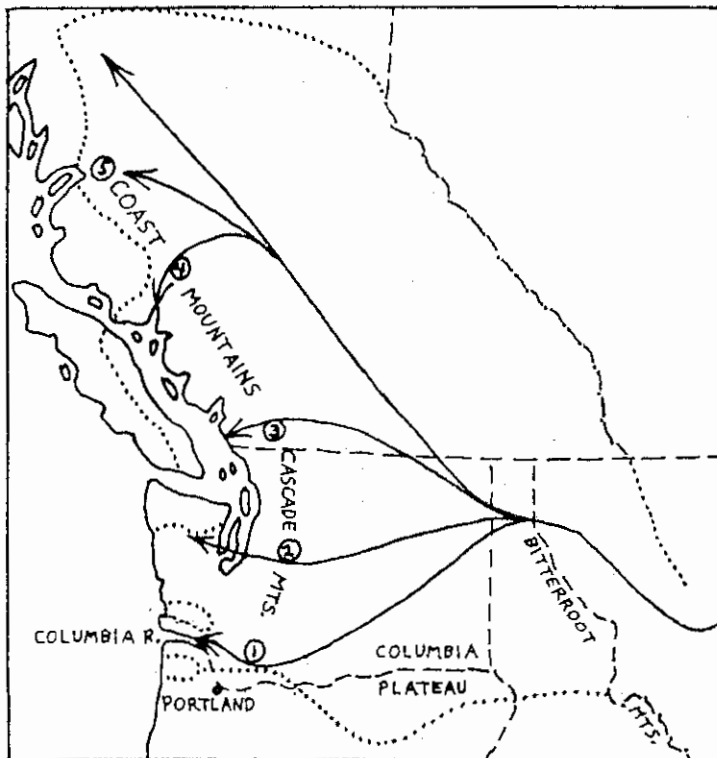


Figure 6. The Salish immigration route. The ultimate Salish boundary at the first stabilization is shown by the dotted line. Access to the coast was possible at (1) Columbia River, (2) Snoqualmie Pass, (3) Fraser River, (4) Homathko River, and (5) Dean River.

and tentatively still farther north so as to bring Salish speakers into contact with the Athapaskans who should then have occupied the Skeena River area.

To the south, the Salish frontier would eventually meet that of the next group still farther south, here presumed to be ancestral Penutians. This meeting of frontiers would have first occurred just to the west of the major Idaho mountains after a separation of at least 600 years. As the mountain glaciers in Idaho melted, these Salish and Penutian peoples would later have come into limited contact with each other all along this divide.

Farther west, the Salish-Penutian contact would continue to extend according to the relative speeds of advance of the two groups, by that time linguistically quite distinct from each other. These movements appear to have been such that the Penutians advanced fast enough so that they eventually would have occupied both banks of the Columbia River between the present locations of Portland and The Dalles. After passing Portland, the river turns abruptly to the north and should then have been occupied on both sides by Salish speakers from there to the river's mouth. These last 150 miles of the Columbia are critical for later movements, as will be shown, but the meeting line farther to the east is of little consequence ultimately. Most of these interior people were later replaced.

In their general westward spread, these early Salish would have encountered the Cascade Mountains in western Washington and the Coast Mountains of British Columbia. For the most part these mountain barriers, whether glaciated or not, would have stopped all human progress. There appear to have been only five openings through which these people could readily have passed in order to reach the coast. These openings correspond well with the five major divisions of Salish on the coast today. South to north, these correspondences are: Columbia River area--Chehalis group; Snoqualmie Pass (adjacent to Seattle)--Puget group; Fraser River--Lower Georgia group; Homathko River--Upper Georgia group; and Dean River--the Bella Coola. The first four of these language groups are in contact with one another while the northernmost is, and probably always has been, separated from the others along the coast by alien people.

If one disallows movements of one people over the territory first occupied by another, then the Kwakiutl, who intervene between the Bella Coola and Coast Salish proper, must have reached their territory at about the same time as did the Salish groups. Since the Salish were the only possible occupants of the interior lands from the Columbia River up to the retreating ice sheet, the Kwakiutl ancestors must have arrived there from the seaward side of the Coast Mountains. There is no other direction from which they could have come. It will be seen that the interior groups of people who reached the coast to the south of the Salish all arrived there at about the same time, and thus could not have figured in the origin of the Kwakiutl. To the north, however, the possibility is open for a sea-oriented people to have been expanding southward and occupying all suitable coastal sites just when the Salish were approaching the coast from the interior. This would automatically result in the Salish being the first to reach only those parts of the coast that are deeply indented, such as Puget Sound, Straits of Georgia, and the inlets where the Bella Coola were located, while the sea-oriented people would occupy the less indented parts of the coast. (See Figure 7.)

The inferred coastal people who gave rise to the Kwakiutl could not have been an isolated phenomenon, but must be traceable to their place of origin. Similarly, they must be expected to have continued their expansion

to the south along the coast as long as unoccupied territory was available to be settled. North of the Bella Coola are more Kwakiutl-related people, and to the north of them are the Tsimshian, Haida, and Tlingit which might at first glance be assumed to be part of this original coastal population. To the south of the Kwakiutl are the related Nootka, the uncertain Chimakuan, and finally a series of identifiably Athapaskan enclaves stretching down to northern California. The obvious temptation is to relate them all as remnants of this original coastal population.

The possibility of a Salish relationship for Chimakuan can be eliminated. In order to provide for the full original connection of the coastal peoples, a member of their group had to occupy at least part of the Olympic Peninsula at the time they first spread through that region. There is no apparent reason why an early branch of peninsular Salish should have become so linguistically separated from all their relatives. Finally, the proposed original Chimakuan area is the last place the Salish would reach, and is also the first place in Washington where the sea-oriented people should arrive. Thus the Chimakuan represent part of the coastal immigration rather than the inland people.

A final observation about the coast Salish concerns their evident propensity to occupy both sides of significant bodies of water. Linguistic similarities are generally just as strong across water gaps as they are for similar distances along each coast. This condition must basically stem from the original occupation, as there would be no easy way to establish such cross-water connections at a later date. Some such crossings could be made under special circumstances, and others could be intensified in later times. This pattern of relationships clearly indicates that social intercourse at significant distances was always easier by water travel than overland. Anyone familiar with the dense, dark, directionless forests of the Pacific Northwest will appreciate how difficult



Figure 7. The coastal Athapaskans' immigration route. Ultimate Athapaskan boundary at the first stabilization is shown by the dotted line. These people passed from northwest Canada through the earliest break in the Coast Mountain glacial barrier at the Stikine and Iskut Rivers. They reached the coast about 11,200 years ago and rapidly spread north and south with a sea and riverine orientation. Their expansion stopped when they encountered territory occupied by other people spreading west from the interior. Lightly held parts of the coastal strip were lost to the more numerous inlanders.

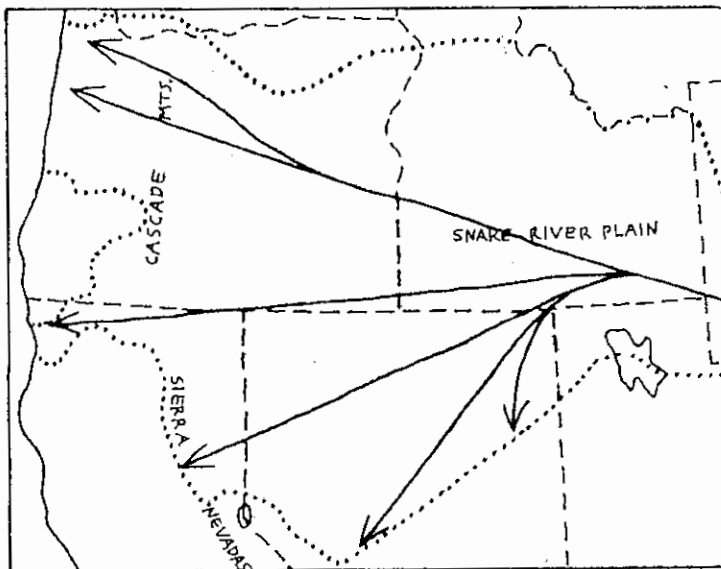
travel can be without roads. Aboriginally, the best roads were the waterways. Prior to the introduction of the dugout canoe such water travel was accomplished with smaller frame craft. Some major waterways, however, such as that separating Vancouver Island from the Olympic Peninsula, were probably rarely crossed and would have been linguistic barriers for some time.

The Penutian Route

The presumed Penutians will now be traced from their place of differentiation after leaving the common homeland of the High Plains. The available entry area to the west begins with the extension of the plains environment into southwestern Wyoming. From there two passages were open, westward across forested mountains into Idaho for the Penutians, and southward onto the Colorado Plateau for the next group. Those expanding to the west would reach the Snake River plain of southern Idaho and encounter no barriers against continuing into Oregon and eventually to the Pacific coast (see Figure 8).

Their meeting boundary with the Salish to the north has already been described. To the south they would first meet the expanding Utan ancestors west of Lake Bonneville, after a separation from them of just over 300 years. The developing Penutian-Utan boundary would extend across Nevada where it would stop at lands occupied by Hokan speakers.

Again it is the western frontier of the Penutians, just as with the Salish, that is the most complicated and has the most crucial results. The Oregon Cascades presented a central mountain barrier which could be bypassed only from both ends. The northern passage was along the Columbia River and over the lower mountains just to the south of the river. The southern passage was from the headwaters of the Klamath and downriver to the coast just inside of northern California. Entry into the western third of Oregon was evidently possible from only these two directions. Penutian occupation of western Oregon was thus substantially delayed, and much of this area was first inhabited by coastal Athapaskans who arrived there according to definite rules.



From southwestern Washington to northwestern California, these Athapaskan enclaves follow a clearly describable pattern. By the rules proposed here, these Athapaskans were necessarily the first to arrive in their areas. Despite

Figure 8. The Penutian immigration route. Ultimate Penutian boundary at first stabilization is shown by the dotted line. The central line indicates where Penutians reached the California coast at the mouth of the Klamath River.

their linguistic similarities to the Canadian Athapaskans, these coastal peoples must have been separated over 11,000 years ago in order to have been there first. The basic pattern of movement is that these sea-oriented people were expanding down the Pacific coast, occupying the coast itself at all usable locations and moving inland along all the various rivers. Given an aquatic orientation, their inland limit would have been, at least for some time, the drainage basins of these waterways. I would suggest that initially these Athapaskans moved into all such rivers, skipping none, until they reached an area in California where the rivers and coast were already occupied by other people. Had they been able to continue unopposed, they would have gone on indefinitely to the south and inland as well, eventually changing their ecological adaptations where necessary.

There are a number of exceptions to the above rule for the coastal and river occupation by the Athapaskans, but the exceptions all follow distinct and reasonable rules of their own. Most of the smaller river valleys are not in Athapaskan hands today, except where access to the small river is blocked from inland by other larger rivers occupied by Athapaskans. The rule is simply that when the inland people arrived at the Athapaskan's inland frontier their weight of numbers and population expansion pressure would have overwhelmed all valleys below a certain size. This need not have involved conflict (although it may have), but given a fair amount of intermixture along the frontier, any small valley with fewer than 500 inhabitants would be absorbed by the larger population.

At the other extreme, the two largest rivers, the Columbia and the Klamath, are also without Athapaskan inhabitants. Here again the outcome depends on a meeting of numbers, this time a matching of forces within the river valley itself. The number of inland people upriver would have hopelessly outweighed the coastal immigrants, even if the latter may have numbered over 500 people. Just how long this settling out would have taken is uncertain, but somewhere between two generations and two centuries should be a good guess.

Only the middle sized rivers and "protected" streams remain. These were small enough to be fully occupied by the coastal Athapaskans in the first round, yet were also large enough to resist the population pressure of the inland peoples. These river valleys are shown in some detail in Figure 12. The Willapa River area is an irregularity, as shown, because its eastern half is now drained by the south fork of the Chehalis River. This is a recent drainage capture as can be seen on direct examination of the streams, the eastern part showing youthful, active downcutting. At 11,000 years ago it almost certainly all drained to the west. Another apparent exception is the rest of the middle sized Chehalis River just north of the Willapa, but in this case the divides are so low as to constitute no barrier to the westward moving Salish. The entire area would have functioned essentially as a large river valley wherein Salish numbers would prevail.

For the Rogue River in southwest Oregon the timing was such that coastal Athapaskans initially occupied the lower two-thirds of its drainage and inland Penutians the upper one-third. The coastal people would have had sufficient area and numbers to hold their ground, while the inlanders had easy contact with relatives over a low divide behind them so they too would be stable. In California, the main fork of the Eel River was similarly divided between coastal people and the inland Hokans who held the smaller upriver portion but with an accessible backup population.

The only place that does not fit any of these rules is the Trinity River, a tributary of the Klamath near the coast. This shows by its Athapaskan inhabitants that these people first held the lower Klamath, then lost it to the inlanders, but continued to hold this tributary. Perhaps there is a rule here, but with only one application, no regularity can be demonstrated.

The reader may have noted by this time that the Ritwan language family at the mouth of the Klamath, by the logic used here, is necessarily of Penutian origin, and its closest affiliation should be with the Takelma on the Rogue River to the northeast. Arguments for a connection between Ritwan and Algonkin (Haas 1958) are not compatible with any geographic interpretation. The distinctive character of the Ritwan languages would follow from considerable Athapaskan influence as the two original populations merged. Not only would there have been considerable borrowing by the Penutians who eventually predominated, but they also would have undergone considerable internal changes.

The rules of movement would have the last part of the Penutian frontier reach well into northeastern California before closing in with the presumed Hokan speakers moving up that state. Here the Penutian section of inland people would have been the first to reach and occupy the northern section of the Sierra Nevada Mountains. This was not arbitrarily decided, but followed automatically from the same rules of movement that applied to all other population movements. This was later to become critical as it is the second best watered region of California and would have remained well populated when most of the rest of the state became depopulated.

Immediately north of the Sierra Nevadas and south of the Klamath River is a high plateau on which I have marked the end of the westward Penutian advance with a mountain barrier in Figure 12. This is a rather inhospitable region, but there is actually no such clear-cut mountain barrier. For subsequent linguistic distributions to make sense it was necessary to stop the Penutians along this line somewhat before the Hokans reached it. This halt can be explained rather as a temporary diversion of much of the population increase into the Klamath River drainage and accelerating that movement in a far more desirable environment. This could be called a "funneling effect" and is the only clear case that had to be dealt with.

The Utan Route

The Utan opening would have been from southwestern Wyoming to the south. Any special relationship that may exist between the Penutian and Utan language phyla may in part result from their short common history in Wyoming. Further similarities between these two may follow from the relatively short time they were separated by the geographic barriers in northern Utah. This separation of some 300 years was shorter than that between any other pair of basic groups and should have permitted a more than usual transfer of linguistic innovations across that developing language barrier for some time.

Utan frontier expansion, following the formula laid down here, was much too slow for them to have reached the Pacific coast as did the other groups. They would end up bordering with Penutians to their northwest, and with Hokans for the remainder of their periphery (see Figure 9). In general, people expanding through the Utan gap would have occupied the Colorado Plateau and south to the Mogollon highlands. They would be the first to reach

most of Utah and Arizona, half of Colorado, and parts of New Mexico and Nevada.

Most of the Utan frontier advance is simple and straightforward, but it involves some very close timing at its southwest limit in Arizona. If the movement rates were altered slightly, the Utans might have spilled out of the Mogollón highland forests and into the open country of southern Arizona. Had this occurred, they would have blocked the passage of the presumed Hokan ancestors moving west over this area. This would have caused Utans to occupy most of California. Hokan could not be a subdivision of Utan and had to enter the area from a separate route into the west. The relationship noted between languages from California Hokan to the the Comecrudan group on the Rio Grande River also argues for an earlier continuous barrier of occupation against further southern movement of the Utans at that time. All of this adds up to a rather limited and landlocked initial allocation of land to the Utan group.

The Hokan Route

The Hokans are the only candidates for the last entry route to the west--around the southern end of the Rocky Mountains (see Figure 10). Their borders with their northern neighbors have already been described. During the initial occupation of North America there would have been a wide continuity between the Algonkians of the east and the Hokan ancestors in the southwest. While all four western divisions would have

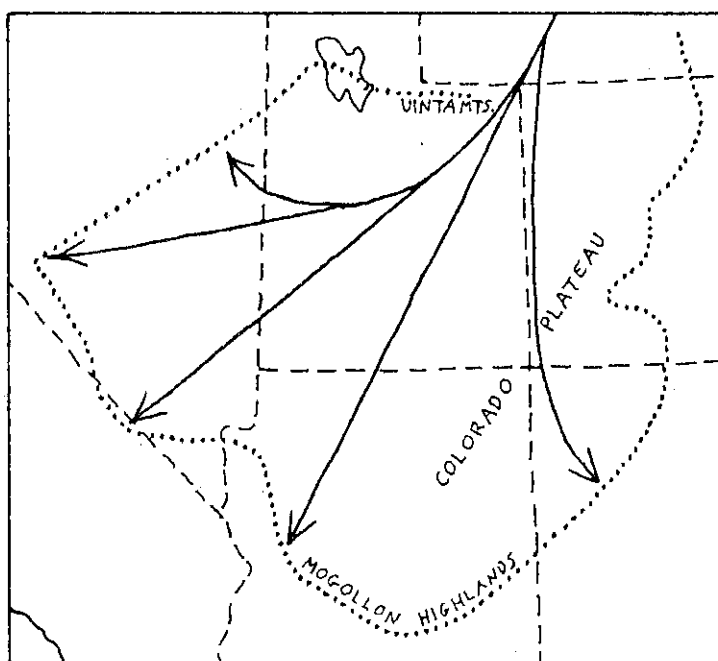
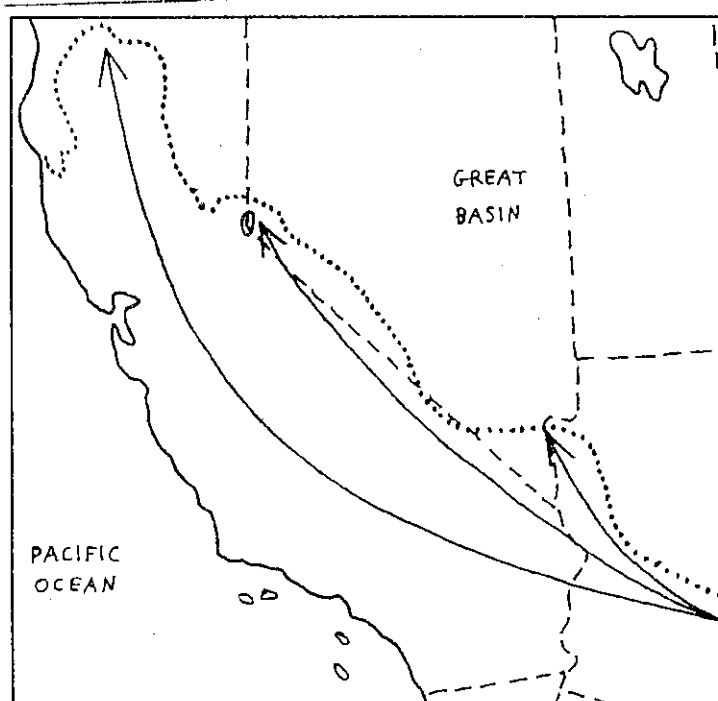


Figure 9. The Utan immigration route. Ultimate Utan boundary at first stabilization is shown by the dotted line. At this time the Utans are stopped far short of the Pacific coast which shows in the lower left corner.

Figure 10. The Hokan immigration route. Ultimate Hokan boundary at first stabilization is shown by the dotted line. This is only the western extremity of Hokan territory which includes the lower Rio Grande and everything to the south.



maintained some continuity with each other and with the Algonkins, this particular pair had the broadest connection. Even the ultimate breaking of these ties with the desiccation of the High Plains was apparently never a complete break across southern Texas.

Given a somewhat arbitrary dividing line from Central Texas to the Gulf Coast, the Hokan side of the line would be the ultimate source of all other American Indian languages from California and the southwest through to South America. No such linguistic grouping has been proposed, but barring transoceanic contacts, and following the logic used here, there is no other available source. Small enclaves of Hokan languages have been identified in much of Mesoamerica. These would not be people who were "dispersed" through the area as suggested by Kroeber (1955), but rather they are little-modified linguistic remnants of the original wave of immigration. The agricultural peoples (except for Aztecs and other Utans) with large populations would be greatly modified derivatives of the same original Hokans. The linguistic implications involved will be touched on at the end of this paper. The large agricultural populations of the eastern U.S. may also be in large part derived from this Mesoamerican source. Sapir's (1929) suggested Hokan-Siouan grouping may reflect this ultimate relationship.

The original Hokan spread across the southwest was by far the fastest of the four lines of westward expansion. All the proposed factors determining rates of movement favored this particular route such that the Hokans would not only reach the Pacific coast by the longest way around, but would also arrive there first, and move north through most of California before encountering the other groups.

Most of the intermountain forested regions would become Utan territory, but much of New Mexico would have been occupied first by Hokans. The dividing line would have been more stable except for the later use of agriculture which permitted some additional movements. Thus the Zuni and Keresans cannot be linguistically identified on these geographical grounds alone.

The Hokan's route proved to be the first into the southern Sierra Nevada and they would have continued up both sides of this mountain range. They would necessarily be the first into most of California with the exception of the far north. There, our timing would have them encounter a long front of Penutians and a group of the coastal immigrants in and around the Eel River.

The Yuki Indians can be reconstructed only as a branch of the Hokans who first reached the headwaters of the Eel River. They would have been under heavy linguistic pressure from the more numerous Athapaskans downriver to the north. Their greatest distinction from other Hokan languages would follow from these circumstances. Later linguistic influences from the Penutians would add to their un-Hokan appearance. Their offshoot of Coast Yuki will be accounted for later as an acorn-based population overflow into relatively under-occupied territory.

Figure 11. Major immigration routes available to the first American Indians. These lines divide to pass between and around the various barriers, and meet on the other sides, just as the earliest human populations would have done. The routes are marked at 100 mile (160 km) intervals with the calculated time of arrival shown at each of these points. Segments that would include the fanning effect are marked with the letter "F." Only the major routes are shown here, but all lesser splits were allowed for in the maps developed later.

The separated southern Yuki, or Wappo, can be explained only by assuming the Yuki originally occupied all the intervening territory. This means the Athapaskan linguistic influence in the Eel drainage must have affected a considerable area to the south. Beyond the Wappo, there is no need to delimit this influence because the subsequent depopulation cut them off here. Reoccupation of former Yuki territory, by Pomo from the coast and Penutians from the interior, isolated the Wappo enclaves which were too small to re-expand significantly. They were large enough, however, to avoid assimilation.

This completes the reconstruction of frontier advances of the various branches of the inland immigration from the ice-free corridor (see Figure 13). Each group has been traced, rigorously following the rules of movement previously laid down, which are based on climatic variations, fanning, ecological changes, and absolute barriers.

As a note of explanation I should add that in developing this picture I saw that people passing through the four western access routes would reach their appropriate destinations only if they advanced at quite different rates. All the factors affecting these rates of movement were then postulated on general information and impressions with this goal in mind. When these factors were first applied to the actual map, the result was a satisfactory fit and no significant adjustments had to be made. The largest alterations were the addition of two fans along the Salish route and the closing of a possible pass in the central Oregon Cascades against Penutian passage. These all appeared reasonable on later examination of maps and visiting the actual terrain.

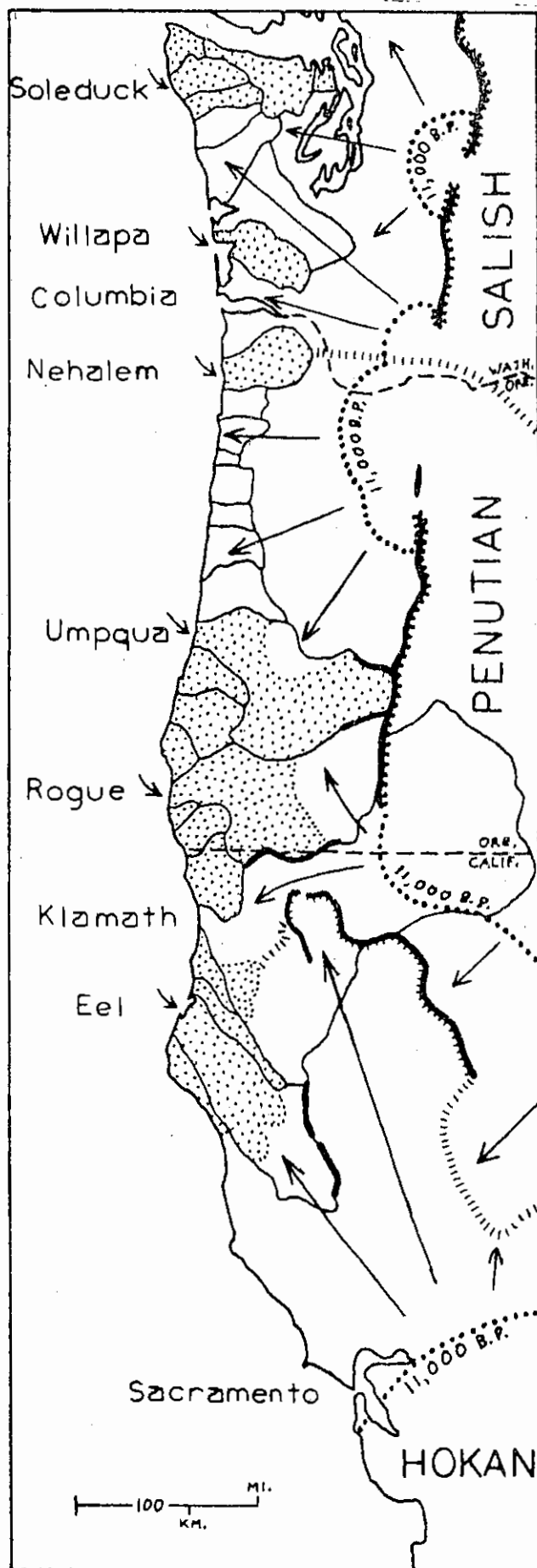
Another decision related to the primary access route of the Penutians. On first examination this looked equally likely to have been a split from the Salish route in western Montana then south across the Bitterroot Mountains into the Snake River plain. The choice between this and the Wyoming route could not be made even by personal examination of the passes in question. The decision made here is based largely on the timing of subsequent events--had the Montana access been used, the Penutians would have arrived in their western area slightly too soon. This also fits better with the suspected relationship between the Penutian and Utan language phyla.

This is not to say that the rate of movement factors used here are necessarily exactly correct. Somewhat different assumptions could have been made which would have led to the same results. In particular, a delaying factor based on difficult terrain could have been introduced, and any or all other factors toned down a bit in order to achieve this same picture. Beyond this or similar minor variations, any other significant changes from the factors given here would lead to distributions that would offer no foundation from which the modern linguistic distributions could be derived.

THE COASTAL IMMIGRATION

The coastal Athapaskans appeared unexpectedly in this scheme. At first it was assumed that some other mechanism would have to be found to account for their curious distribution, though none seemed obvious. Upon implementing the rules of movement for the inland people it became evident that if these movements were stopped at about 10,800 years ago, their collective frontier was remarkable close to that of the Pacific coast Athapaskans. Further investigation showed that if these Athapaskans were introduced along the

Figure 12. Detail of the initial occupation of the central coast region. This map shows the regularities of territorial distribution following the meeting of coastal and interior groups at about 10,800 years ago. The heavy dotted line represents the advancing frontier of the flow of population from the interior toward the Pacific coast at 11,000 years ago. At this time the coastal peoples would have been spreading down the coast at a rapid rate. The heavy black lines represent mountain barriers, glaciated or not, that effectively slowed or stopped the inland people's movements. The "fence lines" of parallel dashes are the boundaries between the three major inland divisions, Salish, Penutian, and Hokan, both before and after the 11,000 year ago time line. Small river valley drainages along the coast are outlined and names are given for some of the more significant ones. At the time of meeting, these small valleys would have been mostly occupied by the coastal Athapaskans who would also have penetrated some distance up the Columbia and Klamath Rivers. The long arrows indicate major continuing expansions of the inland peoples after 11,000 B.P., overrunning certain of the coastal peoples after their meeting around 10,800 B.P. The smallest valleys were linguistically absorbed because their inhabitants would have been too few to hold out; the largest valleys were dominated by the greater upriver populations in the mixing. Some drainages in the north are so poorly divided they provide no barrier to the inland peoples. Only the well-bounded, medium-sized river valleys continued in coastal Athapaskan hands. Some of these were divided between the opposing sides. The stippled areas are those which remained to represent the original coastal strip. Short arrows off the coast mark the main entry points of lasting occupation.



coast, expanding rapidly from north to south at about 11,000 years ago, most of their distributions would coincide exactly with the modern ones (see Figures 7 and 12). This was too much to dismiss as chance, regardless of what may have been said about time depths of linguistic separations. There are a few points of exception to this fit that will later be accounted for in a regular manner by cultural factors.

Human occupation of new territory normally progresses continuously with no areas being bypassed, particularly none that are inhabited by other people. The coastal Athapaskans must then be the southern end of an initially continuous strip of human occupation reaching all the way back north to its source. There are people occupying this entire strip who have not been clearly linked to any other interior groups. The obvious conclusion is that these people are in fact the modern descendants of this ancient Athapaskan strip. Two northern members of this group, Tlingit and Haida, are commonly accepted as distant relatives of the Canadian and Pacific coast Athapaskans. The other language families, Tsimshian, Wakashan, and Chimakuan, show no relationship to each other or to the Athapaskans, but after 11,000 years this is hardly surprising. A "Mosan" phylum has been proposed by some to link Wakashan and Chimakuan to Salish and Kutenai, and in turn to relate the whole assemblage to Algonkin. There is little agreement that the actual relationships are any more than areal--similarities accumulated through long term contact and diffusion. Since the linguistic affiliations might be interpreted in several possible ways, the safest initial approach should be to classify the various peoples according to the most obvious geographical considerations. The resultant grouping will be referred to here and on the maps by the name Athapaskan rather than the broader Na Dene. The reasoning was that if the California representatives of the group are included in Athapaskan, then they all should, no matter how linguistically deviant they have since become.

At 12,000 years ago the coastal mountains of western Canada and southern Alaska formed a continuously glaciated barrier preventing any movement of people from the interior to the emerging coastal region. If this barrier had remained until much later times, all of western North America would have been populated by the interior flow of immigration from the ice-free corridor. The source of a separate stream of immigrants down the coast would have to be an early break in that barrier. Judging from the timing of the coastal people's arrival in California, and the evidently small time differential between there and its appearance in Washington, the first opening should have been just a few centuries before 11,000 years ago.

The location of that opening was suggested indirectly by Krauss (1973: 925) when he noted that Pacific coast Athapaskan perhaps most closely resembles the Tahltan of all the variations of Canadian Athapaskan. Examination of modern topography and glacial maps shows that the most obvious site for the first passage through the barrier was the Iskut River, a tributary of the Stikine, opening into what is now Tlingit territory. Not surprisingly, the Stikine and Iskut Rivers drain the present Tahltan territory. In simple terms, about 11,200 years ago the first opening in this glacial barrier would admit Tahltan Athapaskans onto an uninhabited coast, along which they were able to spread from Alaska to the south until blocked by other people. Glacial geologists T.D. Hamilton and William Mathews (personal communication) would allow that these geographical conclusions of time and place may quite possibly be correct, but that they are not demonstrable at our present state of knowledge.

A bit of support for this coastal strip's linguistic continuity comes

from the demonstration of phonetic correspondences between Tlingit and Oregon Athapaskan by Melville Jacobs and his wife (see Hymes 1956). I have not examined the material in question, and would not be competent to evaluate it anyway, but this is just the kind of evidence that might be expected to appear.

Most authorities have tried to derive the Pacific coast Athapaskans overland from the nearest Canadian source, the Chilcotin in southern British Columbia, some 2,000 years ago. The impossibilities inherent in this concept should be obvious. There is no way such a migration could have occurred over well inhabited territory. Even if such a migration did occur, the entire route should now be occupied by Athapaskans. It might be supposed that these Chilcotins had some unusual advantage like the bow and arrow which probably was a major factor in moving the Apaches south on a different route at a similar time. If this were the case the location of the surviving Athapaskans is still quite unaccountable. Not only do they occupy some of the areas they would have been least likely to be able to penetrate, but also their final distribution corresponds exactly to that of a sea coast introduction.

It might be argued that the coastal regions were so thinly populated that a penetration should have been easy. However it must be remembered that any invading group, dependent on the same resources, would be equally thinly spread. This is like thinking it should be easy to push my hand through a brick wall, since the spacing between atoms means the wall is almost entirely empty space.

The only other route for a 2,000 year old introduction of the Athapaskans would be from the coast. This would involve a long journey bypassing the Northwest Coast peoples from Tsimshian to Chimakuan without leaving any record of their passage. Without some kind of major military or commercial influence this would not be possible. Also, their locations are not those of powerful intruders, but rather are placed just where first habitation would occur. Not only would it be virtually impossible to move into those areas if they were already inhabited, but the exact pattern of occupancy would make no sense.

As a final attempt to make things fit a maritime incursion, it could be supposed that the entire coastal strip remained uninhabited until 2,000 years ago. This might permit the Athapaskans to enter at this date and still be the earliest inhabitants. In support of this, there is little archaeological evidence of earlier occupation in northwestern California and western Oregon. The likelihood of this lack of earlier occupation would appear extremely remote given the expected population pressure and the human tendency to occupy every habitable place. Furthermore, many places on the coast to the north have archaeological sites older than 8,000 years, thus effectively blocking any supposed Athapaskan coastal spread later than that date.

In discussing this area, Elsasser and Heizer (1971: 228-229) concluded that the Athapaskans probably moved in through western Oregon before it became Penutian territory, and that the Yurok must have been first to arrive and "...staked out the best living areas." Their implication is clearly that the first inhabitants would be permanent. One of two conclusions must follow from this. Either the coast was not inhabited until 2,000 years ago, or else this settlement pattern goes back some 11,000 years. Perhaps because both of these conclusions seemed impossible, they dropped the subject without pursuing it further.

It would appear that every other possibility has been fully tried and found wanting. The only explanation that fits the geographical realities is an 11,000 year old expansion of Tahltan Athapaskans down the Pacific coast, and the linguistic affiliations, or lack of them, will have to be explained on other grounds. That this is quite possible will be discussed below where it will be shown that rates of linguistic change, over time, may have actually progressed at extremely uneven rates.

Early linguistic subdivisions of this coastal strip follow in large part from fairly obvious causes. The Haida, before the advent of sea-going dugout canoes, were rather geographically isolated from their neighbors. Tsimshians were concentrated on two river mouths away from most of the others, and likely were subjected to strong linguistic influence from the inland Salish for some time. Kwakiutl were separated from Nootka by the central mountains of Vancouver Island and (originally) the wide water gap at the north end of that island. The Nootka were long separated from the Chimakuan of the Olympic Peninsula by the Straits of Juan de Fuca. Chimakuans were separated by mainland Salish from the Athapaskans to the south. The various Athapaskan groups were also separated into four groups by Salish and Penutians, but this did not result in major linguistic differentiation because of their small numbers.

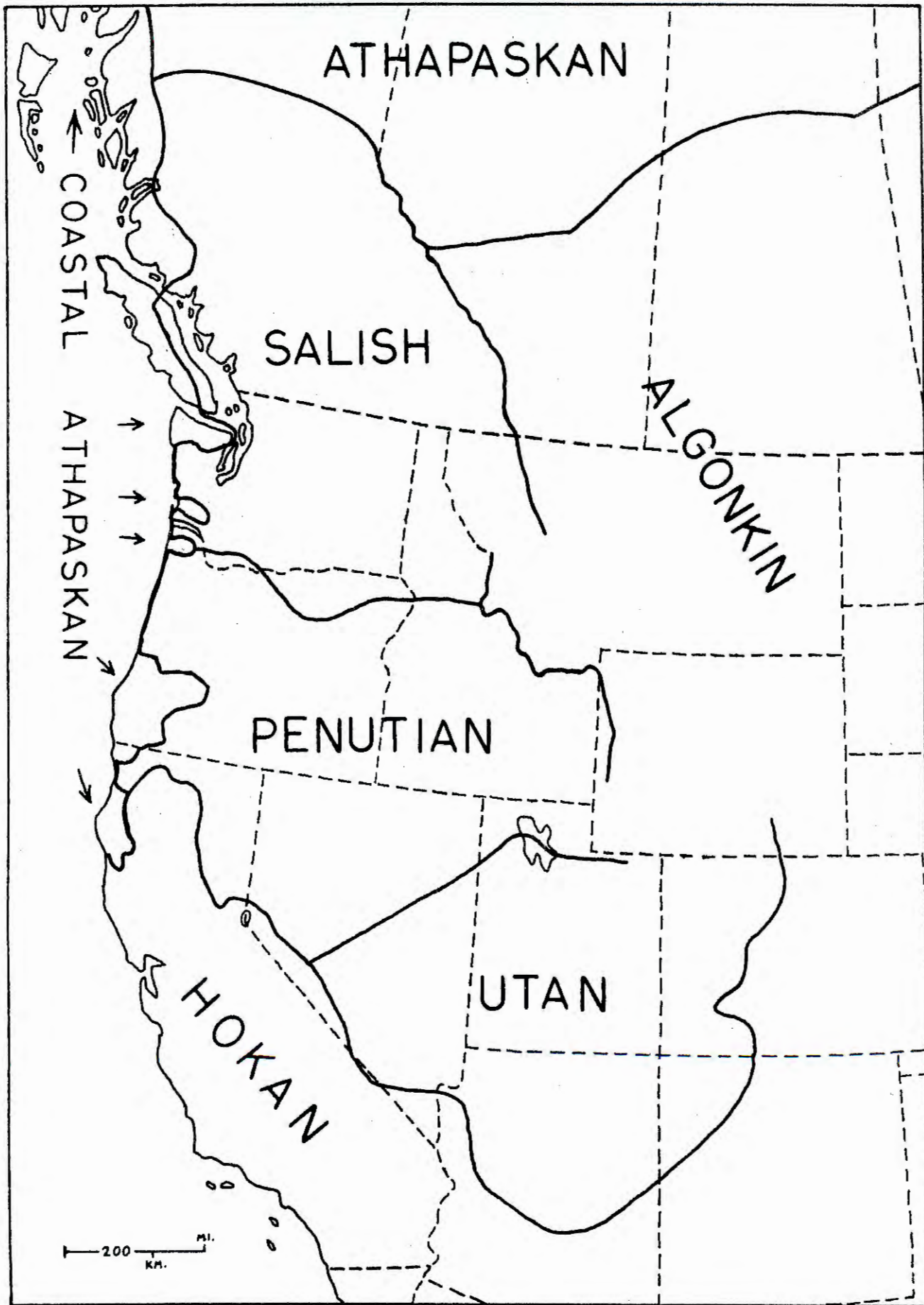
With this the initial occupation is complete and all areas have been occupied, basically following the rule of first occupancy. This may be called the first stabilization period which persisted for several thousand years (see Figure 13). The initial immigration would evidently have been by what are now referred to as Paleo-Indians--most noted as the big-game hunters who made and used the Clovis type of projectile points. The dates assigned to archaeological traces of these people are fully consistent with the time schedule presented here.

THE GREAT DESICCATION

The reconstructed human distributions during the first stabilization conform with present linguistic distributions to a large degree. In the well-watered areas of the northwest the fit is remarkably good, requiring only some minor adjustments in response to cultural events. In the more arid interior, however, the fit is not nearly so perfect. Most especially, the Utans in fact hold a far greater area than the first occupancy would allow them. There is no way the rules of immigration rates can be altered to give the Utans more territory without at the same time totally destroying some or all of the other distributions.

Most of the discrepancies in this interior region can easily be resolved by postulating a temporary depopulation of vast areas. After this the surviving peoples would re-enter the empty areas from all directions. On linguistic grounds the extent of this proposed depopulation can be drawn with

Figure 13. Linguistic divisions as of 10,800 B.P. Each of these population blocks would have become automatically separated from one another by temporary obstacles long enough for them to begin linguistic differentiation. Other factors of climate and topography determine their extent as given here. The actual language types given here are merely presumptions, but they are based on very suggestive locations which will be shown to develop subsequently into exactly the modern distributions of the named groups.



some precision. This amounts to arbitrarily postulating the disappearance of the people who do not fit the modern distribution, and moving back other borders of occupancy to where they may then be "released" to expand at more or less equal rates and reoccupy these lands according to their modern distributions. This hypothetical map of depopulation just happens to correspond rather well with present areas of low rainfall and sparse vegetation.

It should be stressed here that the people did not move out of the depopulated areas, but mostly just dwindled in numbers where they were. There was no way they could move out; all other places were fully occupied and could admit no more than a few stragglers. If anybody moved out of the worst areas they would next enter adjacent areas of declining productivity which would hardly be able to handle new mouths to feed. Those people in the declining areas, unable to feed migrants, would still be able to resist intrusions by still weaker peoples. Retreats, even more so than advances, are movements of frontiers and not movements of people.

Evidence of paleoclimatology includes a period of higher temperature and lower rainfall which matches the proposed depopulation quite well. This period, known as the Altithermal, could be described as a gradually intensifying dry period beginning around 9,000 years ago, peaking at about 6,000 B.P., and with a return to present conditions a thousand years later. Archaeologists disagree about the effect of the Altithermal on human occupancy of the affected areas; Jennings (1968: 136) thinks the effect was minimal, while Baumhoff and Heizer (1965) see a drastic decline in numbers.

Of course the linguistic redistributions proposed here do not depend on the Altithermal per se, but simply on a depopulation of certain specifications. It is just a fortunate coincidence that the Altithermal evidence happens to be remarkably similar to what is required. Had there been no indication of an Altithermal period it would have been postulated here anyway.

The loss of population from the affected areas did not have to be total, but certainly was severe. The measure of depopulation would not be simply the number surviving, or even their percentage, but rather how well they were able to respond to the return of more favorable conditions at the end of the Altithermal. If an area was repopulated by the surviving local people, then the area is not classed as depopulated, no matter how much their numbers may have been reduced. If the area was reoccupied by other people, expanding their numbers and moving their frontier into the area in question, then it is classed as having been depopulated. Thus one region might have been reduced to 10% of its original inhabitants, yet be so far removed from other surviving groups that it had time to recover. Another region might have been reduced to 25%, yet be near to a large population that later expands into the depopulated area so rapidly that the local inhabitants are overrun and linguistically incorporated.

This depopulation should be archaeologically almost invisible. Excavation sites are selected for a number of reasons including evidence of recent habitation and availability of food and water. One thus looks for what would have been a comfortable living area for the Indians at that time. Such ideal habitation sites are just those most likely to have supported some people while the general area was effectively emptied. After a reoccupation from outside sources these ideal sites would continue to support their original populations which might persist for centuries before being absorbed into the general populace. Thus an abrupt cultural replacement will be least likely to

be seen in just those places where it is usually looked for! In addition, occupational hiatuses may actually occur in certain well stratified sites without being recognized. The presence of a hiatus can often be determined, but the absence of such evidence does not necessarily mean a hiatus was not there. Thus not only is archaeology of little use in recognizing past linguistic affiliations, it often cannot even determine whether an area was significantly inhabited at all during a specified time.

The total area which had to be involved in this great desiccation is rather impressive--over three-quarters of the western U.S., the northern half of Mexico, and a large part of western Canada. Figure 14 illustrates the presumed extent of this desiccation. The areas of effective depopulation are delimited, as far as possible, on the basis of geographical regularity. In general, all areas which are not forested today are classed as having been depopulated. This is similar to the low rainfall regions but differs in a few local particulars. The southern Sierra Nevadas and the southeastern fringe of the Rockies are shown as being effectively emptied of human population even though they are forested today. There may be some geographical reason for this which is not obvious, perhaps relating to the topography or to particular species of flora and fauna. Nonetheless they are shown here as depopulated simply because a continuing occupation by Hokan speakers in these two areas would have been evident in later language distributions.

In the north, the limits of depopulation are not so clear. They have been drawn so that the surrounding populations, upon their re-entry, would automatically occupy areas corresponding to recent linguistic boundaries. The area has been outlined to be as consistent as possible with climatic expectations. All interior areas of low rainfall are shown as depopulated. This also includes some forested mountain areas where the previous language assignments do not fit the recent distributions. The northernmost limit is especially arbitrary and further investigation might suggest changes.

The returning populations after the desiccation would spread out from their areas of continuous occupation and into the depopulated region. Their rate of expansion would be about the same as in the original immigration of 7,000 years earlier, but only if more favorable conditions returned abruptly. Actually, the recovery was probably much slower than the human reproductive capacity, so this recovery rate would determine their speed of re-entry. Another factor at this time would be a differential adaptation of various populations to the various environments. Some would move more easily into dry climate areas, others might move along riverine environments, and still others might favor mountains or foothills. Considering all these possible variables the pattern of re-entry is subject to various interpretations. Those given here can simply be described as reasonable, but not proven.

The pattern of reoccupation is relatively simple for the most part, as seen in Figure 15. Algonkians will enter the Canadian Rockies as the Kutenai, who otherwise could not have penetrated this far west. The possibility was also considered that the Kutenai are instead a remnant of the original inland Salish, but their core area is not the one most likely to have survived a general desiccation. The Algonkin affiliation is chosen here for its geographical fit as well as for the reported linguistic similarity.

The southward expanding Athapaskans would be expected to occupy central British Columbia and to isolate the Bella Coola before these Salish speakers could move far back into the interior. A further extension of the Atha-

paskans just east of the Coast Mountains, and the isolated Nicola still farther south, are not explained here. This may seem a minor matter, but it is the largest inconsistency I can see in the whole scheme. Another explanation for them at a later date will be suggested below.

It was the exploitation of three great river courses that dominated the return of populations from the west coast area into the interior. The southern river was the Columbia-Snake which was followed by the Penutians who held both sides of the lower Columbia on the edge of the empty area. Their expansion was basically to the east as far as central Idaho, stopping only at the watershed and at solid Utan occupation. They would also be expected to dominate the eastern side of Washington's southern Cascades, thus pushing back the Salish line slightly from its original position. This move would also close the Snoqualmie Pass area from Salish use.

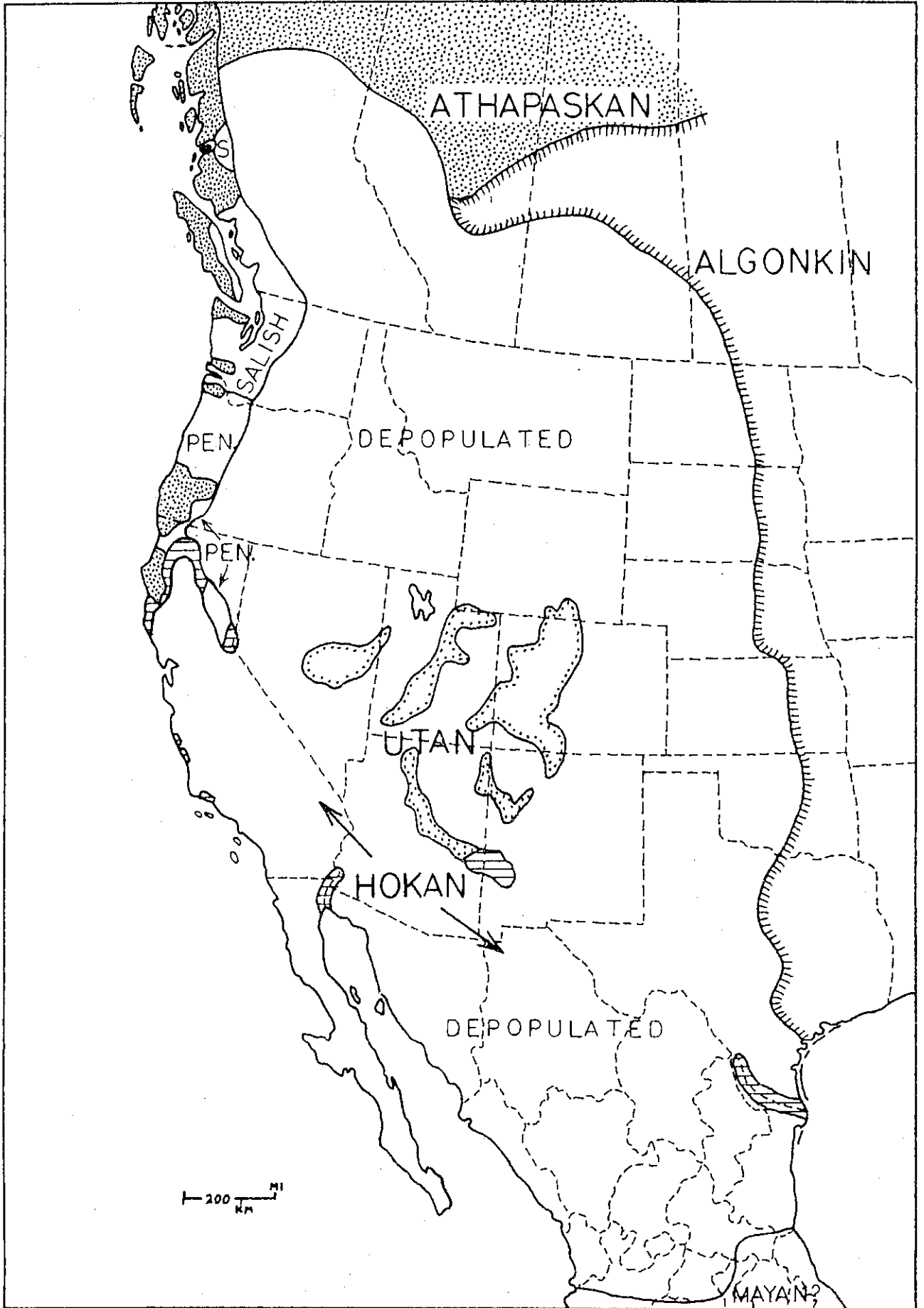
While the Salish would have been outweighed on the Lower Columbia, they would have had sufficient time and numbers to occupy the middle reaches of that river at about the level of Steven's Pass just to the north. From this point, the Columbia and all its tributaries were open for the Salish to follow to the east and to reoccupy their previous territories all the way back to the divides in western Montana. Some of the original inland Salish may have survived and been incorporated into this returning wave.

The northern route was the Fraser River which afforded easy access for the returning Salish to occupy the area up to where the Kutenai had taken over. The present division of interior Salish into two major linguistic sections follows the divide between these two river drainages. It is not clear whether the returning Salish occupied the northernmost bend of the Fraser River or if this was first taken by Athapaskans as shown in Figure 15. If not, then the Salish must have crossed some highlands and picked up the headwaters without ever occupying this bend of the Fraser. On the other hand, if the Athapaskans overran this part of the Fraser at some later date, this might well have been part of the same movement that caused them to extend down as far as the Nicola penetration.

I sense a pattern here. It is possible this represents part of a general southern push by the Athapaskans some 2,000 years ago with the bow and arrow--a move that extended only this far in British Columbia, but which on the empty plains took the Athapaskans as far as Mexico. The biggest difficulty with this idea is the absence of other language boundary shifts that can be described as results of this same process. Perhaps bow-equipped hunters can make inroads only against people who are at the same time both sparsely distributed and relatively sedentary.

The Utans would spread out from their area on the forested Colorado Plateau and would meet the Penutians spreading out from their forested area in western Oregon. The meeting line is now closer to the recent one, but it is still too far to the southeast.

Figure 14. The great depopulation, presumably from desiccation during the Altithermal around 6,000 years ago. Areas of continuing population are labeled according to the presumed affiliation of the group that occupied each place earlier. Those areas with low rainfall, latitude, and altitude were so reduced in population that they were later reoccupied from elsewhere as though they were empty lands.



In the west, Utans spread over most of Nevada and southern California for lack of any significant back flow of people from that direction. The Hokan enclave around Lake Tahoe (Washo) held out well, but was too small to re-expand very far. The most significant event in the west was the Penutian spread from the northern Sierra Nevadas into all of central California. These Penutians would have been the only large population occupying a well-watered area immediately adjacent to the Central Valley with no intervening hills or exceptionally dry country to cross first. The California coast from the San Francisco area almost to Los Angeles was probably at least thinly occupied throughout the Altithermal. The northern section of this coast would have been reached first by incoming Penutians in sufficient numbers to overwhelm the area. The southern section of the coast would have had more time for surviving populations to recover their numbers.

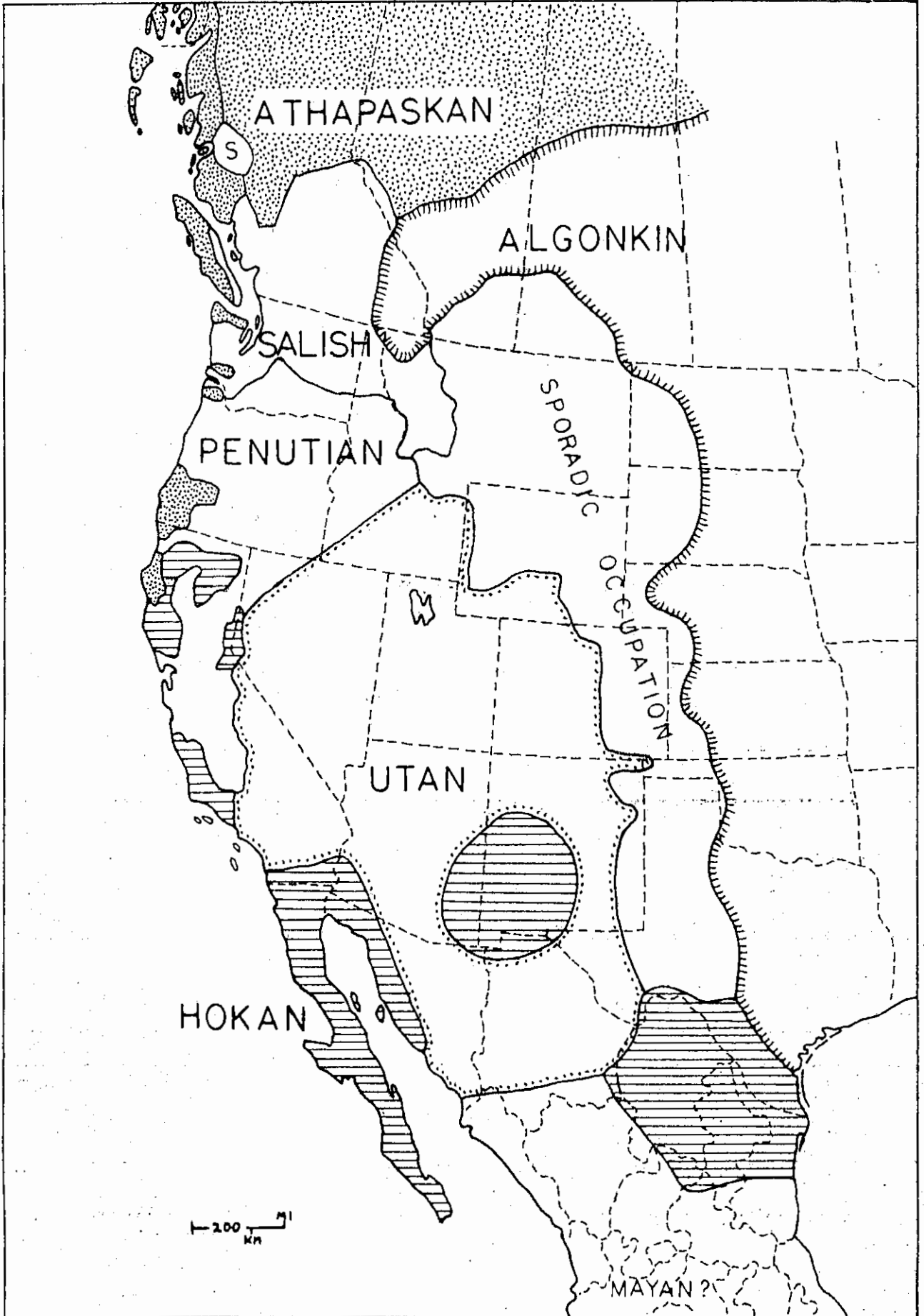
A 300 mile stretch of the lower Colorado River apparently continued as Hokan territory throughout the Altithermal because of its fresh water supply which supported a strip of river plain life. The upper Colorado would have been quickly overrun by the expanding Utans, but the lower part would have had more time for the population to recover and spread to both sides. Their spread down the west coast of Mexico is only a presumption, but it more easily accounts for the Seri who still live at the southern end of this proposed extension. Their spread down Baja California was automatic once they closed off the upper end of this peninsula and blocked entry by any other peoples.

The New Mexico Hokans, who's existence is postulated here, should have spread out in various directions, but mainly away from the greater weight of expanding Utans. It is also a presumption that these Hokans were surrounded and by-passed, but it is interesting that this leaves them in approximately the same area of the ancient Mogollon culture. Further Utan spread into Mexico would eventually have met returning Mesoamericans, presumably Tarascans or Mayans or their common ancestor, along a line somewhere near where I have drawn it.

A considerable Hokan survival along the Rio Grande would have been absorbed by Utans in its upper reaches, but would have had time to recover as the Comecrudan group and spread out from the lower reaches. Finally, along the plains border, Utan expansion may have brought them into some contact with the Algonkins returning from the east. There is archaeological indication that the High Plains were generally not well occupied, so I have left a separating zone on the map between them.

With the end of this great desiccation, the reoccupation of the depopulated areas leaves a picture of ethnic boundaries which is now remarkably close to that of the ethnographic present. This may be termed the second stabilization period. All that remains now is to consider some boundary adjustments caused by cultural factors which gave temporary and local advantages

Figure 15. Reoccupation of the depopulated areas automatically results in this distribution. The Penutian movement into formerly Hokan central California follows from their favorable location in the northern Sierra Nevadas which gave them the most direct access with the largest population. The Utan expansion is the other very significant result of this recovery. The reoccupation pattern of the New Mexico and Rio Grande Hokans is largely guesswork, as is the meeting line of Utans with the Mexican language group.



to certain peoples. After these are applied, according to regularities which will become evident, the ethnic distribution will be exactly as recorded in recent times.

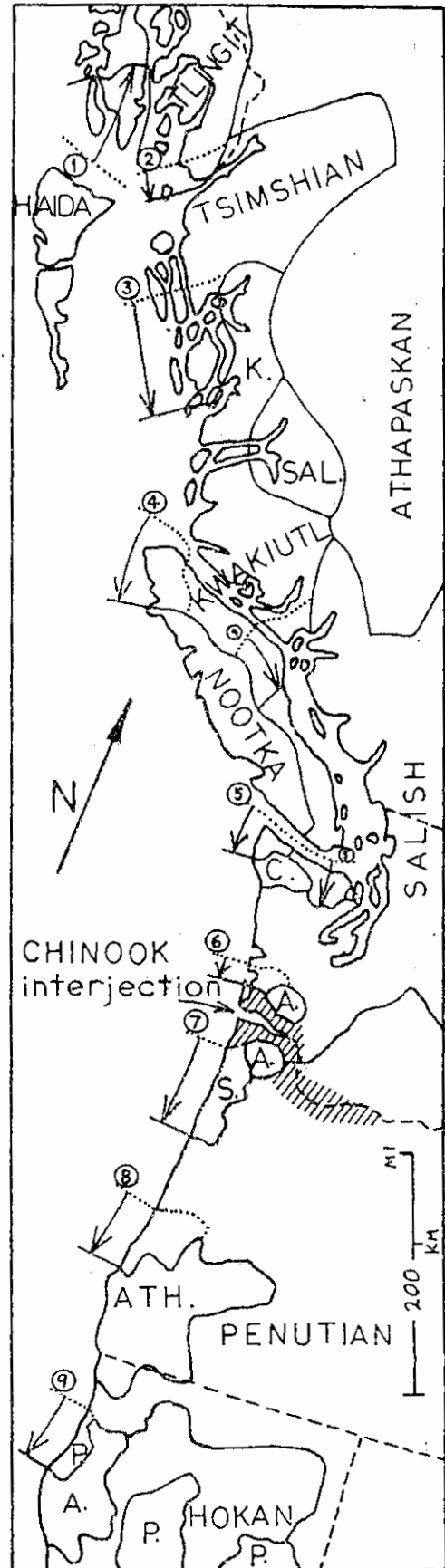
NORTHWEST COASTAL SHIFTS

Throughout the entire length of the Northwest Coastal strip are found unusual southward dislocations of language boundaries, as shown in Figure 16. The basic pattern is that each group holding a substantial piece of the coastline has this territory extended at the expense of its neighbors to the south. These extensions range up to 80 miles in length and include inland portions only about as far as a maritime oriented people could easily dominate.

Some of those who should have had only short coastal footings like the Willapa and Klatskanie lost these footings and could not extend themselves to the south. This shows that the cause of these shifts was a push or intrusion from the north rather than some kind of attraction from the south. This also points to the source of this domino effect as being at the northern end of this chain.

These dislocations were discovered simply because actual distributions could not be made to agree with the processes determining the initial occupation of the area. In some places, as with the northern

Figure 16. The coastal shifts, from Alaska to California. All the normal, or expected, boundaries to the sea coast are drawn with dotted lines, while the actual boundaries are in solid lines. The discrepancies and their directions are shown with arrows along the coast, all but the Haida being to the south, and ending where the coastal forests end. These are numbered from 1 to 9 with 4 and 5 being on both sides of Vancouver Island. The causative factor assumed here is the development of the seagoing dugout canoe. The area occupied by Chinookan speakers is also indicated. It is assumed here that this was an interjection from Tsimshian land over the resident Salish and Penutians. This could have been only after the dugout innovation when seaborne commerce was well developed.



Kwakwiltl, it was evident because the headwaters of an inlet were occupied by people with affiliations to the south, while the mouth of that inlet belonged to their northern neighbors. Many river drainages on the coasts of Oregon and California are divided in the same manner--the population at the river mouth always having connections to the north, presumed here to be intrusive.

Still other shifts were recognized where rather wide stretches of water, such as the Straits of Juan de Fuca, were occupied on both sides by closely related people who ought to be located only on the north side. While Northwest Coast people in general tend to occupy both sides of every water body, some of these seem too wide and should rather have served to cause linguistic divisions in early times, especially before water craft were very well developed. Given a major advance in boat building, however, these wide straits should be expected to change from barriers to highways. If both sides have the same people today, then whichever side first acquired the major maritime innovation would have extended itself to dominate the other side for a short distance.

The dugout canoe is the most obvious explanation for these shifts. As each group acquired the techniques of its manufacture and use, they were able to dominate their south-coast neighbors in economy, prestige, military potential (a big canoe could carry up to 60 armed men), and possibly also in total population. As such domination progressed, the neighbors under pressure would be adopting the same device and developing it until the opposing forces were equalized. Then these neighbors, in turn, would apply the same pressure to the next group to the south. This would be expected to continue until the limit of the dugout canoe building environment was reached, a short distance into northern California.

It has been stated that the Northwest Coast culture ends in northern California for no physical reason since the environmental base continues as far south as San Francisco (Drucker 1965: 7). This is not the case, as may be determined from some maps and by driving the back roads along the coast at Cape Mendocino. From this cape just south of the Eel River mouth on down to the Russian River there are almost 200 miles of coastline with no rivers big enough for salmon, few beaches, and a difficult ocean to navigate. For some distance the forests do not reach the coast, sea cliffs are precipitous, and the streams are too small for boating. The physical base for the Northwest Coast culture simply ends.

The only major language group that did not make the expected southern shift was that of the Oregon Athapaskans. Most of their territory, especially of the Tututni, was relatively unproductive. The limitations encountered below Cape Mendocino also apply to the southern Oregon coast as well, though to a much lesser degree. Even with good canoes, these people would not have had the population or economic power to overwhelm their Yurok neighbors to the south.

Backtracking to the north we find only one exception to the direction of these shifts. This is the Haida extension to the north into what should be Tlingit territory on Prince of Wales Island. While this is commonly stated to have occurred in the 18th century, I would rather think it was for the most part included in the general pattern of shifts perhaps 3,000 years ago.

The Haida are considered to have been the best boat builders of the Northwest Coast in recent times. The regular pattern of coastal shifts leads

back to them and there ends. The Tlingit shift at Tsimshian expense was minimal, which suggests the two groups were under dugout canoe pressure at nearly the same time from a source that was almost equidistant from both of them. Thus all fingers point to the Haida as the source of this innovation.

The most striking aspect of these coastal shifts is their regularity. Two boundaries did not move because the population was too small (Chimakuan) or the area was too unproductive (Oregon Athapaskans). Two more did not move because they were totally overrun by shifts from farther north (the lower borders of Willapa and Klatskanie). The remaining nine language lines are all displaced downcoast in a very regular manner. Even the boundaries on the east side of Vancouver Island are shifted in the same direction. Historical explanations might account for one or two such cases, but the complete set can only result from a single, uniform cause.

THE CHINOOK INTERJECTION

There remains to be explained only one problem in Northwest Coast language distributions. This is the Penutian affiliation of the Tsimshian and Chinook languages that was postulated by Sapir (1929) but never demonstrated. This is most easily solved by first denying any such connection as Kroeber was inclined to do:

As for the Chinook and Tsimshian, the structure of both is obviously different from what is common in the pattern of California languages, that I have been inclined to attach little importance to occasional seeming similarities in vocabulary (1955: 96).

This is not to say that Kroeber was necessarily the better linguist, but rather that this is one of the few "hunches" of Sapir's that turned out to be wrong. It is wrong because it cannot be reconciled with the facts of geography and reasonable expectations of human behavior. The immediate proximity of the Chinook on the Columbia River adjacent to accepted Penutians may appear to be a strong arguing point (see Figure 16). But then the clearly related Tsimshian could not have reached their present location far to the north except by sea transport. Given the dugout canoe and the known development of great commercial activity based on its use, the possibility of such a movement by sea becomes worthy of investigation.

The Tsimshian are situated around the mouths of the Skeena and Nass Rivers which provide the major access to the interior of British Columbia. Whoever occupied this area was automatically in control of a vast network of potential trading relationships between the interior and the various coastal groups when suitable water craft were developed. The Chinook occupy a corresponding position around the mouth of the Columbia River and are automatically in a position to dominate trade between the coast and this even more important interior drainage basin. It might be natural to expect the same people to dominate both of these lucrative trading locations if they ever had the opportunity to do so. If a trading system arose at one focus and extended its connections as far as the other focus, while still dominated by people with one language, then the process should have been automatic. As traders from the original source develop contacts at the other end, bilingual residents would then become significant people, then actual colonization would be added. Together with the prestige and wealth of commerce, these would ultimately interject a large body of people speaking the same language as was spoken where the trading system began.

The question then is which river mouth was the primary focus, and which was the interjection? The northern source of dugout canoes would argue that the Tsimshian are the primary focus, but Sapir's language classification argues that the Chinook are at the source of it all. Fortunately, the answer is easy because the commercial system and its sea-borne population movement had to follow the development of the dugout canoe. The above described pattern of coastal shifts would have included a shift by the people at the primary focus, but the interjection at the secondary focus would occur after this shifting and be unaffected by it.

The mouth of the Columbia should logically have been occupied by the Salish who had earlier overwhelmed the coastal Athapaskans. These Salish then extended themselves to the south as the Tillamook following the pattern of coastal shifts when they acquired the dugout canoe. If the Chinook were the original inhabitants, the area to the south should have been occupied by Chinook relatives instead of the Salish Tillamook. This makes the Chinook necessarily an interjection of some kind.

The mouth of the Skeena River should first have been occupied as part of the initial spread of the coastal Athapaskans some 11,000 years ago. The population here would have been linguistically influenced by the earlier inland Salish, and for some time isolated enough from the other coastal populations to develop a distinct language. If these were the ancestors of the Tsimshian, then they should have participated in the dugout-caused coastal shifts. The Tsimshian show just such a loss of territory to the Tlingit on the north, and a considerable gain of territory at the expense of the Kwakiutl on the south. If the Tsimshian were a later interjection from a Chinookan source these coastal shifts should not have occurred--but there they are.

It clearly follows that the Tsimshian are the in-place natives who capitalized on their location to develop the great trading network; it is hard to see how they could have done otherwise. This commercial activity then spread over the entire Northwest Coast using Tsimshian and the lingua franca of trade. The Columbia eventually became a second major focus of this trade and was linguistically dominated by the trade language which still survives today under the name of Chinook Jargon.

This interjection of Tsimshian speech would be superimposed on a Salish speaking population which had long occupied the mouth of the Columbia. Farther upriver this continuing linguistic intrusion would be over a base of Penutian speakers. Both of these sets of original inhabitants would be expected to contribute something to the eventual development of the Chinook language. A certain number of words ultimately of Salish and Penutian origin would doubtless be incorporated into the general trade language and eventually reach Tsimshian lands as well. It is apparently the Penutian share of this mixed vocabulary that has been seized upon to demonstrate a supposed "genetic" connection with the Penutians. Not surprisingly, Sapir's work on the Chinook was concentrated on an upriver speech, Wishram, which by this reconstruction has a Penutian substratum. Had he worked more on Coastal Chinook with a Salish substratum, the Penutian connection might never have been postulated.

ACORN LEACHING

A number of language distributions in northern California and southern Oregon are reconstructed at this time as being rather close to their present boundaries, but there are still some discrepancies. The lowland Takelma along

the middle reaches of the Rogue River seem inappropriate there, especially as they surround two enclaves of Athapaskans on Galice and Applegate Creeks. The Karok and northern Shasta block a section of the Klamath River which should have had a continuous occupation from the upland Takelma to the Yurok on the coast. The Wintun of the Central Valley extend farther north than can be explained with their immigration after the Altithermal; they now occupy some of what should have been Hokan (Shasta) territory. These examples show a three step northward displacement (see Figure 17).

Wintuns also occupy western territories which cannot be accounted for at this point; they hold most of what should have been Chimariko area, and interject far enough into Pomo lands to cause an enclave of these to become isolated. Lake Miwok appear to have lost the eastern half of their area to the Wintun. The Yuki have a westward extension to the coast which is out of place. All these are westward displacements from the direction of the Wintun area.

To the south, Wintuns extend farther than should be expected and divide the Miwok. Southern Yuki (Wappo), Miwok, and Costanoans all appear to have lost territory to the Wintuns. There are no comparable boundary shifts evident to the east.

These discrepancies take the form of a pattern of displacements radiating outward from Wintun territory to the north, west, and south. The implication is clear that some cultural innovation entered here and spread out, leading

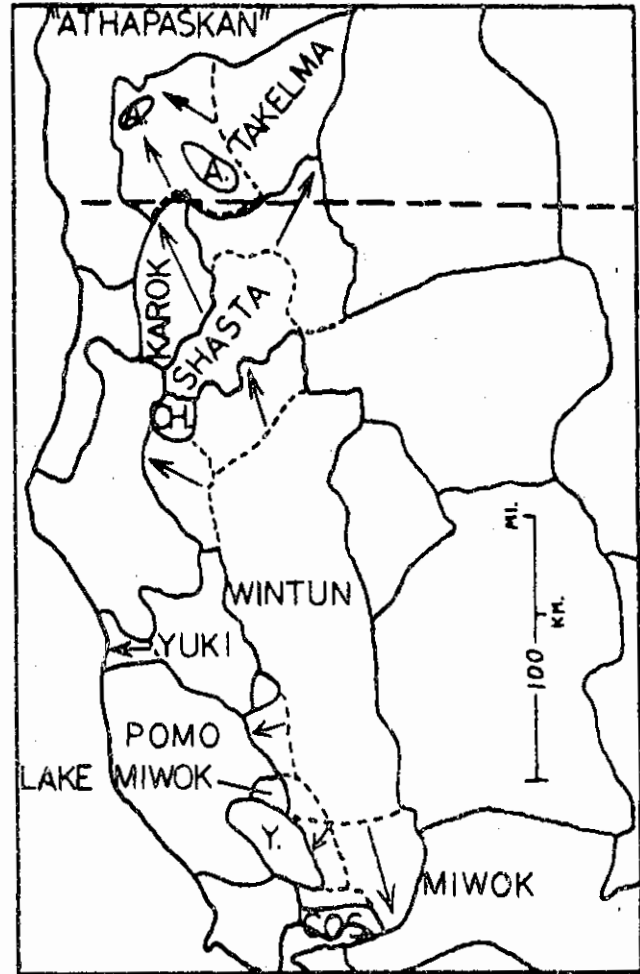


Figure 17. Border adjustments in California and Oregon. The expected boundaries are in dashed lines and the actual ones solid. Arrows indicate the directions of the shifts. Ch. is for Chimariko, Cos. for Costanoan, Y. for Yuki, and A. for Athapaskans. Acorn leaching and its resultant population explosion is the obvious explanation for this radial pattern of shifts. The source of this innovation is in east-central Wintun territory--either by invention there, or by introduction there from the Sierra Foothills. The southern border of the Miwok (off the map) in the foothills also appears to be located too far south and may reflect this same phenomenon.

to this outward shift of peoples. An obvious explanation for this pattern would be that it results from leaching acorn meal.

Acorns are the largest part of the diet of most California Indians. The introduction of this staple food permitted a population level several times larger than would have been possible without it. In order to make acorns palatable, however, most species have to be leached in water. Apparently this was long done by soaking whole acorns for several weeks. At some time and place it was discovered that previously ground acorn meal could be leached in a matter of minutes. The consequent increase in a convenient food supply would inevitably lead to a population explosion extending as far as the resource base was available.

Oak trees are abundant throughout California except for the southern deserts, the higher mountains, and the wettest parts of the northwest. They are also common in western Oregon except for patches and strips of well watered mountains. One strip without oaks extends generally along the divide between the Rogue and Umpqua River drainages, from the western rain forests to the high Cascades. This acorn gap is only 10 miles wide in places, but the rugged topography and limited resources may well have caused a long delay in spreading the acorn-based populations to the north. (One tributary of the Umpqua, Cow Creek, is south of this barrier and was inhabited by Takelma acorn users.) To the east, the Klamath Indians complete the acorn barrier as they instead have water lily seeds as an equivalent staple and did not adopt acorn usage. Still farther east the desert edge does not support oak trees.

In aboriginal times the middle of the Central Valley was not noted for oaks. Thus the appearance of acorn leaching among the central Wintun might not have been a local discovery, but instead it could have been an introduction from the Sierra foothills to the east. This would allow the innovation's source to be placed in these hills and would help account for the apparent lack of movement of the eastern boundary of the Wintun. Although it is not indicated in Figure 17, this boundary might instead have shifted somewhat to the west at this time. Granting this, it is also possible that the southern boundaries of Maidu and Miwok may owe their apparent shifts still farther south to this wave of acorn-based population explosion. Maidu territory then seems to be a good possibility for the place of origin of this innovation. This location for the source would bring leaching around both ends of the Central Valley to the Costanoan area at about the same time. Although little shows in terms of boundary shifts here, this may have brought about the final absorption of the pre-Costanoans, Hokan peoples, by the Penutians.

The sequence of boundary shifts shows that acorn leaching followed the great desiccation which had ended by 5,000 years ago. Other archaeological evidence points to a leaching date at about 2,000 B.P. because that is when such recognizable sites first appear in much of the Sierra foothills. The reasoning here is that acorn Indian sites are the easiest kind to recognize, both from the number of people involved and from the archaeological practice of working backward from known kinds of sites. Prior to the acorn revolution, native habits and habitations would expectably have been quite different and as yet undiscovered in these foothills.

What is missing from this reconstruction is an accounting for why some boundaries shifted and others did not. When a group adopts leaching, increases its food supply, and increases in population, the added people would continue to live in their own territory until it reaches its new carrying capacity. If

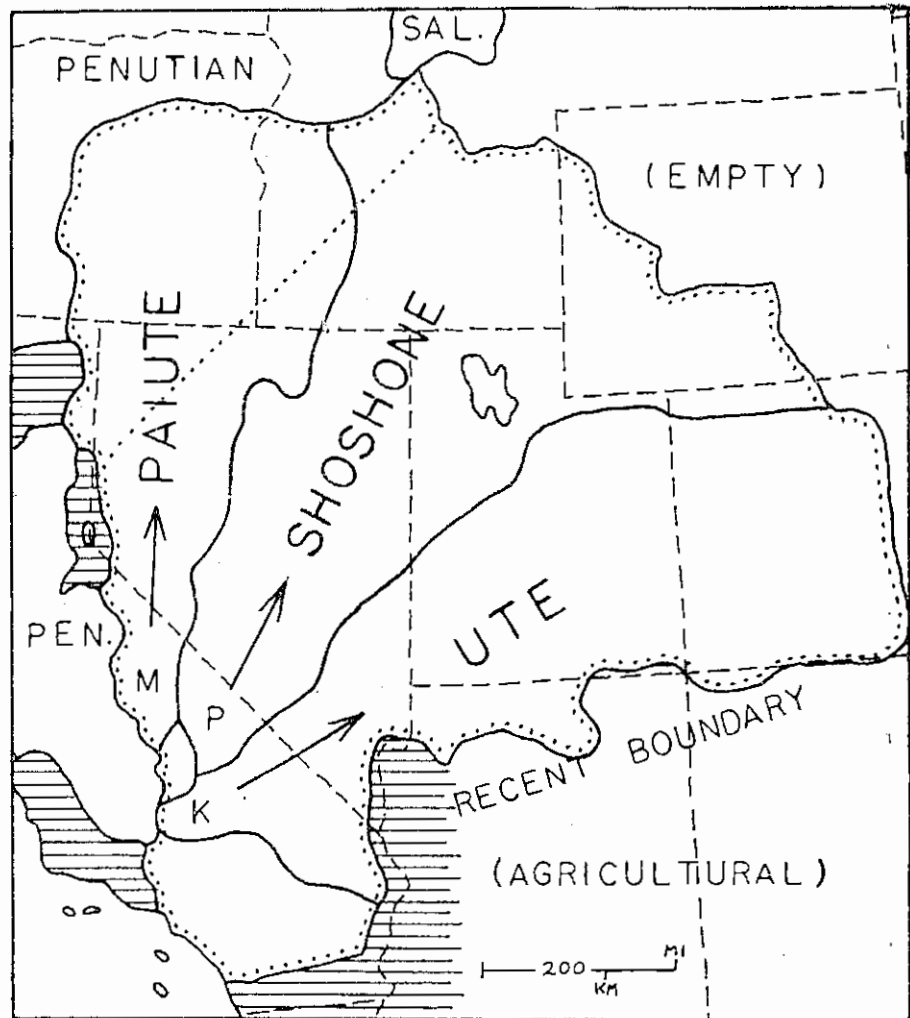
population control is not immediately adopted, the surplus will spread by infiltrating adjacent areas until they predominate there. The strength of this infiltration will depend on the overpopulation at the source. The infiltration will move into areas which also have acorns, and it will tend to follow routes that can be easily traveled. A more careful study of these factors should account for these movements in detail.

THE GREAT BASIN

Most of the Great Basin's inhabitants, the Numic speakers, form a cohesive linguistic unit consisting of Ute, Piute, and Shoshone languages. Each of these have representatives in eastern California (Mono, Panamint and Kawaiisu) and appear to fan out from there into the desert like gigantic pie slices (see Figure 18). It is commonly assumed that the three California groups are the parent sources which in fact spread themselves out over the desert areas about a thousand years ago (Lamb 1958). Given the shuffling effect which retards linguistic differentiation in sparsely inhabited regions, this expansion might be dated even somewhat earlier.

Most of the Numic area is here reconstructed as having been Utan speak-

Figure 18. Spread of Numic languages in the Great Basin. The source languages, Mono, Panamint, and Kawaiisu were evidently the first to regularly utilize pine nuts. This led to a population expansion which was able to overrun the very sparsely inhabited desert regions. In the northwest corner of the Basin the dotted line indicates the previous Penutian boundary. Numic speakers replaced (and absorbed) other Utan speakers in most of the Basin except for the northwest corner.



ing since the Altithermal, but two peculiarities of distribution require a special explanation. First, the Numic area extends too far to the northwest. Reoccupation of the Basin after the Altithermal cannot move the Utan line any farther to the northwest than to the southeastern corner of Oregon. Something else pushed it another 200 miles farther northwest. Second, the occurrence of the three distinct languages in California so closely placed, with each extending out into the desert, strongly suggests they are the source. Trying to argue for movement in the opposite direction would imply an extraordinary coincidence of their simultaneous arrival. A differentiation in place could have produced something like the Basin divisions, but this would not have separated the three California languages in their corresponding locations.

Further support for an actual population movement comes from basketry. The basket-making techniques of all the Numic speakers stand in sharp contrast with those of almost all their neighbors. Only in the California area are the basketry techniques similar to those of their non-Numic neighbors (Lawrence Dawson, personal communication). The gradation of techniques through California and the sharp boundary elsewhere on the Numic periphery can be accounted for only by a population expansion just as was postulated on linguistic grounds.

It would seem difficult, at first glance, for a population replacement to occur in this environment. The rule insisted on here is that any people will hold their own land against invasion unless they are greatly outnumbered by the intruders. Given the very low population of the Basin in recent times, it is difficult to imagine it's being inhabited earlier by only one-third as many people or less. Even a temporary depopulation would not work as this would have meant subsequent replacements from all directions, not just from the southwestern edge. Many archaeological sites suggest the area was more-or-less continuously occupied since the Altithermal.

By analogy with the California acorn situation, the solution would seem obviously to be the introduction of eating pine nuts (piñon) as a major food staple. If this practice began on the southwestern edge of the Basin, its spread would automatically take with it the three populations of the source area in just the geographic pattern that is seen. These people would have to outnumber the previous residents by at least two-to-one for their languages (or dialects) to prevail over the whole area. The area covered would be only that in which pine nuts were abundant and previous population very sparse.

The pre-Numic population would have been very sparse indeed, and largely occurring only in those favorable places where archaeological evidence is found. The incoming people would somehow have to occupy the larger areas of seemingly less favorable resources, and in such numbers as eventually to absorb their better situated, but less numerous, predecessors. The first people who were able to depend regularly on the pine nut as a staple food would have had just this advantage. It has been suggested it was a superior water-carrying container that enabled the newcomers to harvest the widespread piñon crop while those before them were restricted to the few places with natural water supplies. This becomes a matter for archaeology to settle, and thus moves out of the purview of this study. It is sufficient to say here that the recent Numic spread was real, that it was an overlay on a remarkably sparse population, and that it was probably based on piñon utilization.

The source area of the pine nut users suggests a stimulus diffusion

from the acorn users of California. If this were the case a beginning date of about 2,000 years ago is quite possible. This should not be in serious conflict with the suggested 1,000 year date of linguistics, especially as this should be regarded as the most recent possible time for the spread. The southern border of this area against the agriculturalists is quite uncertain, and its exact location would depend somewhat on the actual date.

THE SOUTHWEST

At this point the picture of linguistic distributions of the hunters of western America is essentially complete, leaving only the agricultural southwest to be dealt with. As noted above, the detailed rules of movement for farmers have yet to be worked out, although eventually these might be seen as clearly as those for hunters. It can be observed now that farming peoples generally follow roughly the same restrictions in relation to each other as do the hunters. A major exception is that they can often pass freely through areas occupied by other farmers. As long as these farmers operate in suitable environments they will have the advantages of numbers, seasonal freedom, and food supply with which they can advance their frontiers against hunters with relative ease. A seemingly wild variable is that some hunting groups may adopt agriculture while others will not. It would be useful to find some rule whereby this can be predicted instead of just recorded, especially for long vanished populations.

There is no compelling evidence in the southwest for introducing farming peoples from farther south in Mexico. Within the southwest the only clear linguistic evidence for agriculturally based movements is the northward and eastward extension of the Yuman-speaking Hokans up the Colorado River and its tributaries. Most of this territory could not have been held by them earlier, or else the Utan expansion through southern California would have been blocked. Similar northward advances by various Utan farmers and the New Mexico Hokans are also likely to have occurred but there is no obvious way this can be read from their modern distributions. Likewise, the retreat of a farming frontier may represent a physical withdrawal of the farmers themselves, or it may be an abandonment of the practice and an in-place return to hunting. Again, there are no linguistic distributions in the southwest that argue one way or the other without a consideration of archaeological data.

The various southwest farming peoples differ in the degree to which they are committed to, and integrated with, an agricultural way of life. The Zuni and Keresans are the most involved with it and presumably are direct descendants of the original farmers of the area; the Hopi are the least adapted and most recent; while Tanoans hold an intermediate position (Voegelin and Voegelin 1973). The length of time each of these peoples has been involved in agriculture appears to have directly affected their rates of linguistic change. This follows from increases in population and technological complexity which in turn tend to accelerate language modification.

The separate position of Hopi among the Shoshonean languages is to some degree an artifact of their recent way of life which should have affected their degree of language distinction. They may simply be an offshoot of the Piute with a separate history perhaps shorter than the 2,800 years (Hale 1958) that glottochronology would give them. The distinction of the Tanoans from other Utans is much greater, but a longer farming history and faster drift would have increased their relative differences. Instead of being a language

family in contact to the Uto-Aztecs, they may actually be a much later offshoot of the central branch within the Uto-Aztecs. Finally, the Zuni and Keresans, who have no clearly demonstrated linguistic relatives, need not have the degree of separate history this seems to imply--either could be originally a Utan or Hokan language with 4,000 to 6,000 years of independent agricultural history and rapid linguistic change.

The connection between Zuni and California Yokut claimed by S. Newman (1964) is not possible. While Zuni farmers might conceivably have moved a great distance to the west, an equivalent movement of nonfarming Yokuts to the east would require answers to all the questions of why, where, when, who, and how that were posed earlier.

Archaeological data do not contradict the above observations, but then, not much was said. The Mogollon culture corresponds closely to the proposed location of the New Mexico Hokans, and their modern descendants might include both Zuni and the Keresans. Hohokam culture is known from what is here reconstructed as Utan territory, but they are probably extinct because their language should have become totally distinct from any other in the area. The Patayan of the Colorado River are probably represented today by Yuman speakers. The more recent Anasazi to the north should have been Utan speakers, and a physical retreat of many of them would fit well with the Tanoans on the Rio Grande. (The above is deduced from applying the principles developed here to data from Jennings 1968, Spencer and Jennings 1965, Reed 1954 and Trager 1967.) Much more could be said about the sequence of human movements in the southwest, but it would have to be entirely from archaeological data which is difficult to correlate with languages. In any case, the reconstruction presented here is intended primarily to show how much can be learned only from recent distributions.

THE BOW AND ARROW

It was suggested earlier that some small Athapaskan interjections into what should have been Salish territory might be associated with the bow and arrow as a weapon of the hunt and of war. There are almost no other boundary shifts of hunting peoples against other hunters which might be explained by this device that have not already been accounted for. This is not surprising when it is realized that while the bow can make hunting easier in some cases it is not likely to increase the actual food supply. Its adoption will not lead to a significant increase in population which, in turn, could advance a language boundary.

Even the importance of the bow as a weapon of war is often minimized by the fact that it can be, and probably generally was, introduced to each new recipient in at least two major stages. The device can begin as a child's toy (and/or musical instrument) which then goes through a series of steps of improvement into a hunting weapon for small game and finally to the sinew-backed bow of great effectiveness. As the diffusion of this complex may progress through these same gradual stages, there might be no time when people from the source will have overwhelming and long-lasting power over their recipient neighbors. At any one time, two potentially opposing groups may have the same weapon, but merely in two different stages of development.

When the self bow is effective enough to be used for small-game hunting, it can be recognized archaeologically by the abrupt appearance of small projectile points. The development of the sinew backing allows the same power

with a much shorter bow (T.M. Hamilton 1972) and is archaeologically invisible except for the fact that it could not precede the appearance of small projectile points.

It is generally agreed that the bow entered America from the north. Hester and Heizer (1973) give dates for small projectile points which indicate it swept through western North America around A.D. 500. (Note the change in dating notation at this point.) At the same time the sinew-backed bow could well have come into action against the Salish in British Columbia and against the Algonkins in the High Plains. A modest penetration is indicated into the Salish area, but a comparable penetration in the Algonkin area would amount to a breakthrough into the almost empty plains. If these Athapaskan bowmen could operate successfully in the plains they would have had an open field for expansion almost to the Mexican border.

The timing of this Athapaskan breakthrough onto the plains, while based on an archaeological suggestion, is also consistent with their linguistic separation estimate as well as with subsequent events. The High Plains were occupied by some river-basin farmers and probably on a seasonal basis by hunting peoples based to the east and west. Nevertheless, a well-armed hunting population could have expanded down the western plains from a starting point near Edmonton, and at about the same rate of speed as the original immigrants did over 10,000 years earlier.

At A.D. 1,000 the Athapaskans would be in central Colorado, and by A.D. 1,200 they should have encountered agriculturalists in northeastern New Mexico. Somewhat earlier, bow-equipped Utans of the Great Basin would have been in contact with the farmers of Nevada and Utah. Foraging archers might be expected to have had a potentially devastating effect on small numbers of thinly-spread farmers, especially if large hunting areas are also available. The result of this contact was evidently a withdrawal of the more widespread agricultural periphery and even the devastation of a large part of the core area.

Much of the result of this contact can be read in the distribution of Apachean languages in the southwest. Even without archaeological information it could be surmised that Apache territory, excluding the plains, was all previously occupied by Hokan and Utan farmers. These settled peoples certainly could and did adopt the bow as a weapon. But without a drastic change in their way of life they could not easily match the proficiency and mobility of the Apache. Given greater numbers from a more complete utilization of the land areas, the inherent advantages of the farmer would certainly have reversed the tide of movement. But in the arid southwest this was not to be the case.

The European interjection put a stop to a still fluid situation. Apache advances may not have been at an end and many farming communities still retained their way of life. At the same time another process was well underway, the "Puebloization" of the invaders themselves. This was most pronounced among the Hopi who appear to have been part of the original intrusion from the Great Basin and who later adopted the essentials of their victim's agricultural way of life. With the exception of the Navajo, the Apachean part had made little progress in this direction.

Evidently the advent of the horse gave the Apache a new lease on their way of life: Their pressure on the eastern agricultural settlements should

have ended shortly after it did farther to the west. Actually, Apache raiding was at high tide concomitant with the Spanish penetration of the area. It has been noted that early Spanish records did not mention the Apache presence in the Pueblo area prior to A.D. 1525 (Gunnerson and Gunnerson 1971). I would simply suggest that the early records referred only to horse-based Apaches who were a serious threat to these Europeans as well. The previous foot hunters would not only be less powerful, but also had begun to settle in after their centuries' earlier disruption of the agricultural areas.

The manufacture and use of the bow would have spread to the Utans of northern Mexico, who in turn carried it on to temporary ascendancy over the civilizations of Mesoamerica. Equivalent phenomena probably occurred among the eastern U.S. farmers. Neither of these, however, is properly within the purview of this paper.

LINGUISTIC CONSIDERATIONS

The major linguistic distributions in western North America have here been accounted for with great accuracy by applying rules of animal geography. Still, certain of the usual affiliations are challenged, as are some of the generally accepted time depths of separations. The most notable of these disagreements relate to the Athapaskan coastal strip.

As reconstructed here the original coastal strip is now represented by the following families from north to south: Tlingit, Haida, Tsimshian, Kwakiutl, Nootka, Chimakuan, and Pacific coast Athapaskan. These can be linguistically treated in three sections. In the north, the Tlingit and Haida apparently relate distantly to each other and to the Canadian Athapaskans of the interior. For an 11,000 year separation this is acceptable, but surprising. The middle section includes Tsimshian (with Chinookan), Wakashan (Kwakiutl and Nootka), and Chimakuan, which are not demonstrably related to each other or to any other language group. After 11,000 years the distinction is entirely expectable. In the south, the Pacific coast Athapaskans from Willapa to Hupa are easily demonstrable relatives of the Canadian Athapaskans, with a commonly estimated date of separation on the order of 2,000 years. These people are the major problem as my reconstruction would have them separate 11,000 years ago, as did all of the others.

The rate of change, or linguistic drift, of these three distinct coastal sections would have to have been remarkably different. The northern section was just a little delayed, the middle drifted at a more normal rate, and the southern section was extraordinarily slow to change. These differences may actually turn out to be quite understandable. It is commonly assumed, especially by glottochronologists, that after two languages become distinct from one another their differences gradually increase over time. While this is true in general, there are other factors which are just as important to the rate of change as is the passage of time itself. These factors are population size, technological change and complexity, and linguistic influence in the form of pressure from other languages.

Population affects the rate of language change in an interesting manner: all else being equal, the rate of drift should be directly proportional to the square root of the number of speakers. This results from the relation between two factors, the frequency of linguistic innovations and the speed of their fixation. Innovations can originate with any individual in a speech

community, so the frequency of such innovations will be directly proportional to the total number of speakers.

Probability of fixation of an innovation is oppositely affected by population, but to a lesser degree. This conclusion follows from a model of how a typical innovation becomes fixed in an average language community. Each successful innovation can be treated as having originated in the center of a circular area of uniformly distributed speakers. It then spreads out at a uniform rate toward the periphery, an equal distance with each unit of time passed. While the linear movement of the innovation progresses directly with time, the area covered increases with the square of the number of time units passed. (This is simply because the area of a circle increases with the square of the increasing radius.) With a uniform distribution, the number of people adopting this innovation will be increasing with the square of the time passed. Turning this around, the radius of a circle is proportional to the square root of its area, so the time passed (radius) is proportional to the square root of the population (area). As larger populations are considered, the time required to fix an average successful innovation rises also, but not so rapidly. This, in a given length of time, the probability of fixation decreases with larger populations; this probability is inversely proportional to the square root of the population.

The rate of origin of linguistic innovations is directly proportional to population; the rate of fixation is inversely proportional to its square root. Combining these, linguistic drift becomes directly proportional to the square root of the population.

Translating this principle into real population numbers can be approximated for some of the peoples under consideration. It would make little difference whether one compares tribal dialects or full languages. As long as one is consistent, a ratio of one to four generally holds for the smallest to largest groups usually encountered at each level. Tribal or village dialects usually range from 500 to 2,000 speakers, while distinct languages usually range from 2,000 to 8,000 or so. As the square root of 1:4 is 1:2, it is to be expected that the smaller speech communities will change only half as fast as the larger ones. (For a number of aboriginal population estimates see Spencer and Jennings 1965, and Kroeber 1925.) For a rate of change ratio of 1:3 one would need population contrasts of 1:9. This extreme occurs occasionally, but is probably not common. For a variety of reasons, larger language groups in the modern world cannot be compared directly with these aboriginal Americans, but compared to each other, the modern languages appear to follow a similar rule of relative rates of change.

A good illustration of this, perhaps unexpected, phenomenon comes from two of the smallest languages of native California, Chimariko and Esselen. Both of these Hokan groups, numbering between 500 and 1,000 speakers, are compared with 8,000 for the more typical Pomo and Costanoan. Here we have the extreme 1:9 population ratio which reduces to an expected 1:3 ratio of linguistic drift. As Kroeber (1925) describes them, "It seems likely that Chimariko has preserved its words and constructions as near their original form as any Hokan language; better than Shasta which is more altered, or Pomo, which is worn down" (p. 109), and "In short, Esselen is free from the peculiarities of Chumash and Salinan, and is a generalized Hokan language" (p. 544). Kroeber did not seem to have noticed the implication of these observations.

The smaller groups of Pacific coast Athapaskans, like Willapa, Klats-

kanie, and Tolowa may also have numbered at or near 500 people, while others like Hupa and Umpqua were two or three times larger. In contrast, the Tlingit, Haida, Tsimshian, and Kwakiutl languages numbered 8,000 or more each. The expectable difference in rates of linguistic drift goes a long way toward accounting for the different amount of distinction each group shows from Canadian Athapaskan, the presumed ancestral language.

Culture change and increased technological complexity will lead to new terminology, new social status relationships, and inevitably to linguistic change. While no obvious numerical value can be placed on this effect, it should have greatly increased the rate of change in the classical Northwest Coast area. Specifically this would be the northern and middle sections of the coastal strip, as opposed to the southern section centering on western Oregon.

Extensive commercial and social contacts with large numbers of people of different language stocks has long been noted as a significant source of linguistic change. This effect is both in borrowing and in the triggering of internal changes, but again no obvious numerical value can be placed on it. The strongest influences of this kind would have been those of Coast Salish on Chimakuan and Wakashan, and of early Salish and later Penutian on the Tsimshian speakers. In contrast, Haida and Tlingit probably had much less contact until recent millenia, and the Pacific coast Athapaskans never had much contact with especially numerous or powerful tribes.

Adding up these likely influences on rates of linguistic change we find the three sections of the coastal strip were very differently affected. In all respects the southern section should have had the slowest rate of change. The northern section would have a high rate of change from the influences of large populations and complex technology. The central section would have the same factors as the north, and in addition, would have the effect of maximum foreign contacts, thus giving it the fastest linguistic drift of all.

One also gains the impression that Athapaskan languages have an unusual resistance to change. Only when sufficient influences are brought to bear will they "break" and commence to change at a more or less normal rate. This is just an impression which I have gained from all the above observations, and may or may not be important.

Most other languages in western North America do not present the same kinds of problems as do those of the coastal strip. Populations did not vary greatly in most instances, technological levels were not very different, and foreign contacts were generally not so intense. Still, some of the modest discrepancies between glottochronology and the present reconstruction might be looked at in view of the above considerations.

In the agricultural southwest, language change would have been much faster than in most areas, due to increased numbers, technology, and contacts. The Uto-Aztecan family spread out some 5,000 years ago after the great desiccation. The Tanoans are normally ranked as a separate, related family, and thus they supposedly separated sometime before. Actually, less than 4,000 years of farming would be sufficient to alter a Ute language to that extent. The Aztecs attained their prominence too recently for equivalent changes to have occurred.

Similarly, Zuni and Keresan, with over 4,000 years of farming adapta-

tions, have lost all easily recognizable signs of affiliation. The geographic setting suggests that Zuni is Utan, and Keresan is Hokan, but either one could belong to either phylum.

Another linguistic phenomenon worth mentioning is the shuffling effect which is characteristic of low density populations in the Great Basin and the northern forests and tundra. In these areas the high mobility of hunting bands tends to mix and spread both genes and speech habits thinly and widely. Tribal or macroband organization becomes less important and more flexible. Families and bands shift their higher social affiliations with ease, thus tending to prevent the formation of distinct language boundaries. Tragedies of small scale will frequently cause local depopulations and disperse stragglers in all directions. Repopulation of these spots can come from any or from several directions. Repeated events of this kind, overlapping each other, would be constantly stirring the linguistic soup and preventing the settling out of distinguishable parts. What differentiation does occur would mainly result from human concentrations in different environments.

The shuffling effect would have held together the vast tracts occupied by the Algonkin family for 11,000 years, permitting linguistic change for the whole, but retarding the tendency toward subdivision. There is no need to suppose a more recent expansion from a more limited region. The Canadian Athapaskans even still seem to function almost as a linguistic unit even though much of their area has been continuously occupied for more than 11,000 years. The Eskimo language need not date from the Thule of only 1,000 years ago, but rather may go back to the Dorset of several millenia earlier. The shuffling effect would hold it together through the major technological changes which diffused through at a later date.

Future linguistic research on the above subjects, and related ones, will require some new approaches and considerations. Most especially glottochronology is totally unrealistic as presently employed. There are detailed mathematical formulas used to translate cognate percentages into linguistic time depths (Sankoff 1973, Hymes 1960). The precision implied by these formulas is rather pointless in view of the following factors which are not generally considered.

(1) The likelihood of parallel change must be better quantified. The number and strength of intervening language boundaries between two languages directly affects the rate of linguistic divergence. Jorgenson (1969) made such a correction for Coast Salish relationships and found that it made more sense than not to consider it. Obviously, the fewer and/or weaker the intervening boundaries, the more likely two languages will feed innovations to each other.

(2) Some other kinds of rules should be adopted for cognate recognition. Where one worker may see two words as distinct cognates, another may see no similarity. The relationships an investigator wants to see can easily color his choices. A classic example of this is English "I" and German "ich" where not a single phoneme is the same or even close, yet the two words are commonly referred to as cognates. On surface examination this is absurd. By the same reasoning any word in any language can be called a cognate of any other, just so long as no common phonemes are required.

(3) The above example illustrates that a comparable depth of analysis must be used in glottochronological studies. Surface forms in two given languages will show a certain percentage of cognates (hopefully according to some

rules), while a comparison of underlying forms will reveal far more. Given a further analysis of sound shifts, still more cognates will be found. If one pair of languages is compared on surface forms, and another pair at some deeper level of analysis, then the finding of a similar percentage of cognates in each pair does not imply a similar degree of relationship. Formulas for translating cognate percentages into time depths are almost meaningless without taking this into consideration.

(4) As suggested here, the number of speakers of a language should have a significant effect on the rate of drift.

(5) Differences in technological complexity probably have an effect on drift that has been ignored or treated as though it were a constant.

(6) The effects of social interaction with speakers of other languages has not been quantified. A speaker with a foreign accent is more likely to trigger linguistic innovations than is an ordinary speaker in the language community.

(7) It should be established whether different types of languages have inherently different rates of drift. For instance, might it be possible that highly synthetic languages are slower to change than are most others? How about the possibility that one language phylum is more sensitive to population levels than are others, another phylum more affected by technological change, and still another more susceptible to foreign contacts?

If measurements of linguistic time depths by glottochronology are to have any usefulness, the above factors must be used and quantified or else clearly demonstrated to be unimportant. Some progress along this line could bring historical linguistics into a more meaningful relationship with archaeology.

SUMMARY OF THE RULES

The detailed locations of native American language phyla have been shown to follow directly from universally applicable principles. Foremost among these are the two basic rules of movement for hunting and gathering peoples.

The first (the rule of first occupancy) states that once an area has been occupied the inhabitants will tend to remain there indefinitely. While they may expand their holdings in other directions, they will never willingly abandon any territory that can support their people. Consequently, no other large group of people will ordinarily be able to move into or through such occupied territory. This tenacity follows from the fact that opposing sides in a contest are about equal and those defending their own territory have an automatic advantage. Thus any movement of hunting peoples is possible only into unoccupied or underutilized territory.

Application of this first rule shows that only a single wave of immigration was possible into North America. When the continent was initially filled, a period of stabilization was reached. Another, subsequent application was in the reoccupation of most of the arid west following the severe reduction in population of the Altithermal period. Again, the rule was whoever reached an area first would exclude any others.

The second rule (that of technological shifts) states that temporary advantages may cause regular movements of language boundaries. Only an over-

whelming superiority in population, wealth, prestige, or military power can cause such exceptions to the first rule. Among hunters this usually amounts to only modest movements before the responsible technology is diffused ahead of the advancing frontier. Frequently this leads to the domino effect where a series of shifts occur, all displaced in the same direction.

Several applications of the second rule have been described--the coastal shifts, the Chinook interjection, acorn leaching, pine nut eating, and the bow and arrow. Far more drastic dislocations were caused by agricultural practices, but these are only peripheral to the area under consideration.

The degrees of relationship among the various language groups, in terms of time of separation, have been determined here on purely geographical grounds. The occasional discrepancies between the reconstruction and the more usual linguistic interpretations have stimulated a few pertinent observations. Linguistic classifications are similar to the Linnaean classification of animals in that they are based on a hierarchy of morphological similarities. Both systems suggest phylogenies, but the actual histories are sometimes different. In particular, there are some biological lines that have evolved very rapidly, and others which have changed remarkably little over long periods of time. It is clearly indicated here that similar phenomena occur with certain American Indian languages. Three factors have been described which are largely responsible for such unusually rapid or slow rates of change.

The best quantified of these factors is that small language communities will undergo less linguistic drift than larger ones, all else being equal. It is observed that speed of drift will be in direct proportion to the square root of the population. The second factor is technological change which tends to increase the rate of drift. The third factor is that intensity of foreign contacts will affect the speed of language change.

CONCLUSIONS

One of the most interesting aspects of this whole investigation was the realization that most of the basic information was old data. In fact nothing really crucial to this study was discovered after the year 1930. This date is chosen mainly to follow the publication of Sapir's *Encyclopaedia Britannica* (1929) article tentatively combining many of the language families into major phyla. Since all of the geographical data used here was also reasonably well known by then, this reconstruction could have been devised by anyone during the last 45 years. The beginning date of 12,000 years ago and the evidence for disiccation were rather recently discovered, but these only put dates and reasons on a sequence of events that would have been postulated anyway.

Most other efforts by anthropologists to reconstruct past events are based on the most up-to-date information available. While this is admirable and useful, it also makes such reconstructions highly vulnerable. The more a theory depends on last year's data, the more likely it will be changed by next year's discoveries.

There is much room for improvement in this reconstruction of western North American prehistory. The reader will have noticed that the Northwest Coast and California are treated in greater detail than is the remainder of the area. This reflects the author's area of greatest familiarity. In order to offer a fairly consistent overall picture I decided to omit still more detail in the far west, and to make some wild guesses and vague generalizations

in areas well removed from there. The particulars in all of this are subject to possible revision, but in much of the interior of the continent the room for improvement is especially great.

In the area of historical linguistics there are several opportunities to check some of the relationships postulated here. If the extreme differences in rates of linguistic drift on the west coast do not prove to be correct, as postulated here, then the whole scheme will collapse. If the Ritwan languages of California really are Algonkin, instead of Penutian as I have claimed, then there is little hope for the rest of the theory. If the Mosan phylum has "genetic" reality, then my reconstruction does not. The supposed Penutian affiliation of Tsimshian and Chinook, as well as Zuni, cannot be correct without creating a whole new set of rules of movement.

Archaeological confirmation is more difficult. Dating the initial occupation of each area would be crucial, but this effort is often clouded by the headline-hunting tendencies of many workers who want to find dates older than anyone else's. In the long run there should emerge from the many dates, a pattern of immigration on which confirmation of my reconstruction will ultimately depend.

Recognizing separate linguistic groups from their cultural debris is a more difficult matter in lieu of ancient written documents. For example, the first inhabitants of Nevada are here derived from three well-separated routes of immigration which would have been linguistically distinct. An archaeological map of Nevada at 11,000 years ago would hopefully show three distinguishable artistic regions, but these might prove hard to identify, and anyway they would soon have merged into one another by trait diffusion. Other regions show the same problem--a combination of linguistic diversity with technological similarities.

As noted above, the archaeological visibility of the great depopulation is likely to be minimal at best. A more thorough investigation of the less favorable environments of a region would offer the best hope of recognizing a desiccation, but also the least hope of finding a large quantity of publishable data. More attention might be paid to finding evidence for the gradual overwhelming of cultures in choice locations by their more numerous neighbors after a major population turnover. Especially interesting might be the potential documentation of a sequence like that of the Tillamook region; sea-oriented Athapaskans, replaced by inland Penutians, replaced by canoe-using Salish.

Archaeological confirmation of this reconstruction from analyses of human skeletal remains is a remote prospect at present. Yet the general concept of a single act of immigration with subsequent physical differentiation has already been advanced with considerable documentation (M. Newman 1962). This is in refreshing contrast with previous studies which found "positive evidence" for at least four and perhaps six waves of immigration (Neuman 1949).

Throughout this paper I have systematically ignored the possibility of any human inhabitants in North America prior to 12,000 years ago. This is partly because there is no archaeological evidence that clearly calls for any such early people. This is also because the human distributions leave no room for any languages other than those derived from that single wave of immigration. If there were any earlier inhabitants they left no clear traces of themselves and did not impede the entry and dispersal of the Paleo-Indians in any discernible manner. Any such early people must have been very sparsely distributed,

culturally impoverished, and perhaps even without language. Still, one sometimes wonders about the deviant Yuki language and physical type, some bearded California Indians, and some of the pre-projectile point sites...

The complex distribution pattern of language groupings in western North America has previously defied systematic analysis. The procedure presented here easily accounts for these distributions. The essence of this procedure is the assumption that human movements are regular and predictable; that they occur for good and discoverable reasons. In all parts of this reconstruction the essential questions for any theory of migration are all dealt with by applying the same basic answer.

1. Why did the people move at all? Because there was unoccupied land for their population to expand into.
2. Why did they move where they did? Because that was where the unoccupied land was located.
3. Why did they move when they did? Because the unoccupied land was habitable and accessible at that time.
4. Why was it they who moved there? Because they were closer to the unoccupied land than any other people.
5. How was the move accomplished? By natural population expansion as long as the land was unoccupied.

After the initial filling of the continent discrepancies were noted between where the peoples "ought" to be and where they actually were. These discrepancies could then be described in terms of their regularities. To move from these to the probable causes was never a problem. Throughout, I have avoided the temptation to base any human movements on conscious decisions or any other "historical" reason. If this makes human movements sound mechanistic it also makes them natural.

To study unpredictable events is not to engage in science but in philosophy. While both pursuits are admirable exercises of the human intellect, the problem at hand proved amenable to a rigorously applied scientific method, and it was not necessary to employ the other.

Footnote--I wish to acknowledge the assistance of Gary Breschini, Raleigh Ferrell, Mark Fleisher, Frank Leonhardy, and Terry Rambo, without whose comments and criticisms this paper would have been written somewhat later and more poorly.

Table 1. These figures are a breakdown of all factors used to calculate the immigration rates shown in Figure 11 for the presumed Salish ancestors. With a beginning time of 12,000 B.P. at Edmonton, the date at each 100 mile mark is given according to procedures explained in the text. The doubling time of the frontier-feeding zone is affected by climate, fanning effects, and environmental delays.

Salish Route to Mouth of Columbia:

	Basic	Days	Winters	Fans	Delay	Total	Date
1.	25	15	15	--	--	55	11,945 B.P.
2.	25	15	15	--	--	55	11,890
3.	25	10	15	--	--	50	11,840
4.	25	10	15	--	--	50	11,790
5.	25	10	15	--	75	125	11,665
6.	25	10	15	100	--	150	11,515
7.	25	10	15	50	--	100	11,415
8.	25	10	10	90	--	135	11,280
9.	25	10	10	90	75	210	11,070
10.	25	5	5	--	--	35	11,035
11.	25	5	10	--	75	115	10,920
12.	25	5	5	35	75	145	10,775

Salish Route to Puget Sound:

10.	25	10	10	45	75	165	10,905
11.	25	10	5	80	75	195	10,710

Salish Branch to Fraser River:

9.	25	10	10	90	--	135	11,145
10.	25	10	10	45	--	90	11,055
11.	25	10	5	80	75	195	10,860

Salish Branch to Bella Coola:

9.	25	10	5	80	--	120	11,160
10.	25	10	5	40	--	80	11,080
11.	25	10	5	--	--	40	11,040
12.	25	15	15	--	--	55	10,985
13.	25	15	15	55	--	110	10,875

Salish Branch to Tsimshian Contact:

10.	25	10	5	40	--	80	11,080
11.	25	10	5	--	--	40	11,040
12.	25	15	15	--	--	55	10,985
13.	25	15	15	55	--	110	10,875
14.	25	15	15	--	--	55	10,820

Table 2. These figures are a breakdown of all factors used to calculate the immigration rates shown in Figure 11 for the presumed Penutian ancestors.

Penutian Route to Northern California:

(1. through 4. same as for Salish)

	Basic	Days	Winters	Fans	Delay	Total	Date
5.	25	10	10	--	--	45	11,745 B.P.
6.	25	5	15	--	--	45	11,700
7.	25	5	10	--	--	40	11,660
8.	25	5	15	--	--	45	11,615
9.	25	5	15	--	--	45	11,570
10.	25	5	20	50	75	175	11,395
11.	25	5	15	45	75	165	11,230
12.	25	5	10	--	--	40	11,190
13.	25	5	15	--	--	45	11,145
14.	25	5	20	--	--	50	11,095
15.	25	5	15	--	--	45	11,050
16.	25	5	15	--	--	45	11,005
17.	25	5	5	35	75	145	10,860

Penutian South Branch to Northern Sierras:

12.	25	5	15	--	--	45	11,185
13.	25	5	20	50	--	100	11,085
14.	25	5	15	--	--	45	11,040
15.	25	5	10	--	--	40	11,000
16.	25	5	15	--	75	120	10,880

Penutian South Branch to Central Nevada:

12.	25	5	15	--	--	45	11,185
13.	25	5	20	50	--	100	11,085
14.	25	5	15	--	--	45	11,045
15.	25	5	10	--	--	40	11,005

Penutian South Branch Around Lake:

13.	25	5	15	45	--	90	11,095
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Penutian North Branch Through Cascades:

14.	25	5	10	--	--	40	11,105
15.	25	5	15	--	--	45	11,060
16.	25	5	15	--	75	120	10,940
17.	25	5	5	35	75	145	10,795

Penutian North Branch to Columbia:

16.	25	5	10	--	--	40	11,020
17.	25	5	5	--	150	185	10,835

Table 3. These figures are a breakdown of all factors used to calculate the immigration rates shown in Figure 11 for the presumed Utan ancestors.

Utan Route to Arizona:

(1. through 10. same as for Penutians)

	Basic	Days	Winters	Fans	Delays	Total	Date
11.	25	5	15	45	--	90	11,305 B.P.
12.	25	5	10	--	--	40	11,265
13.	25	--	5	--	--	30	11,235
14.	25	--	10	--	--	35	11,200
15.	25	--	5	--	--	30	11,170

Utan Branch in Southern Nevada:

12.	25	5	15	--	--	45	11,260
13.	25	5	15	--	75	120	11,140
14.	25	--	5	--	--	30	11,110
15.	25	--	--	--	--	25	11,085

Utan Branch in Central Nevada:

13.	25	5	15	45	75	165	11,095
14.	25	5	15	--	--	45	11,050
15.	25	5	10	--	--	40	11,010

Utan Branch into New Mexico:

11.	25	5	15	--	--	45	11,350
12.	25	5	10	--	--	40	11,310
13.	25	--	5	30	--	60	11,250
14.	25	--	10	--	--	35	11,215

Table 4. These figures are a breakdown of all factors used to calculate the immigration rates shown in Figure 11 for the presumed Hokan ancestors.

Hokan Route Through California:

(1. through 7. same as for Utans)

	Basic	Days	Winters	Fans	Delay	Total	Date
8.	25	5	10	--	--	40	11,620 B.P.
9.	25	5	10	--	--	40	11,580
10.	25	5	10	--	--	40	11,540
11.	25	5	10	--	--	40	11,500
12.	25	5	10	--	--	40	11,460
13.	25	--	5	--	--	30	11,430
14.	25	--	5	--	--	30	11,400
15.	25	--	5	--	--	30	11,370
16.	25	--	--	--	--	25	11,345
17.	25	--	--	25	--	50	11,295
18.	25	--	--	--	--	25	11,270
19.	25	--	5	--	--	30	11,240
20.	25	--	--	--	--	25	11,215
21.	25	--	--	--	--	25	11,190
22.	25	--	--	25	--	50	11,140
23.	25	--	--	--	--	25	11,115
24.	25	--	--	--	--	25	11,090
25.	25	--	--	--	--	25	11,065
26.	25	--	5	--	--	30	11,035
27.	25	--	--	--	--	25	11,010
28.	25	5	--	--	--	30	10,980
29.	25	5	5	--	--	35	10,945
30.	25	5	10	--	75	115	10,830

Hokan Branch to Lake Tahoe:

26.	25	--	10	--	75	110	10,955
27.	25	5	10	--	--	40	10,915

Hokan Branch in Northern New Mexico:

14.	25	--	5	30	--	60	11,370
15.	25	--	5	--	75	105	11,265
16.	25	--	5	--	--	30	11,235

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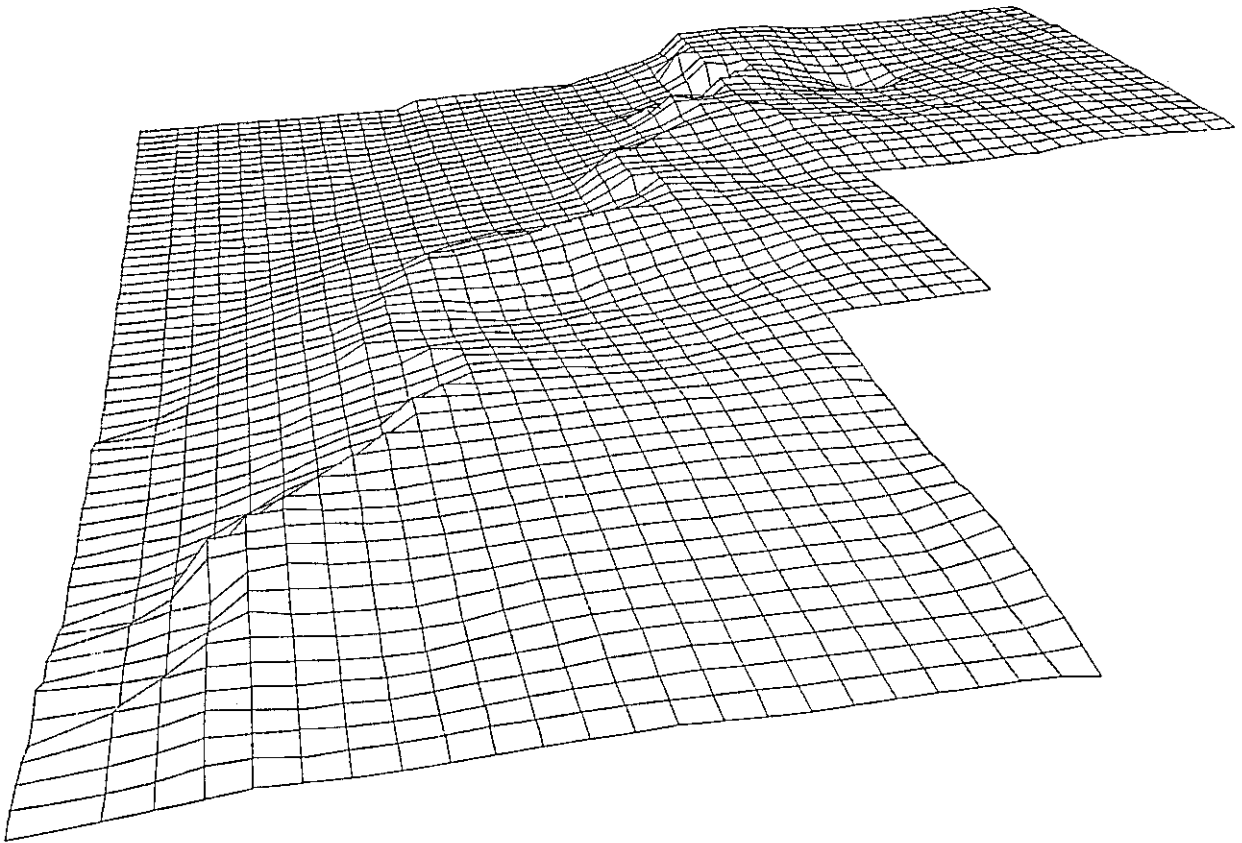
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INVESTIGATIONS INTO COMPUTER GRAPHICS:

ARCHAEOLOGICAL APPLICATIONS

by Stephan R. Samuels



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INTRODUCTION

The research for this paper developed from a need for more efficient methods for compiling, utilizing, and displaying information recorded during site excavation--specifically topographic, stratigraphic, and feature data. By their very nature archaeological excavations deal with three-dimensional relationships, but most reporting of stratigraphy is presented as "slices," two-dimensional sections, even when there have been large scale three-dimensional excavations. No efficient means of displaying three-dimensional pictures of stratigraphic units is accessible to most archaeologists. Attempts have been made to use hand-drawn obliques (Cressman 1960) to convey this kind of information, but this method has three basic disadvantages. First, it is extremely time consuming to accurately reproduce the measurements of the surfaces and features onto the isometric grid for inking. Second, isometric views are not as useful in representing the data as is perspective. Isometric views of spatial data distort the display, making objects in the background appear larger than they actually would appear visually. Perspective, single or double vanishing point, compensates for this distortion, but hand-drawn perspective views are more difficult to construct than isometrics. The third problem is the lack of versatility of hand-drawn representations. The angle of viewing is chosen at the beginning of the drafting procedure, and often features can overlap and otherwise obscure the presentation. In order to rectify such a situation, a new map would have to be drafted. This is inefficient, especially where many features are plotted and correct angle determination is difficult. The rise in the use of computers by archaeologists, along with increasing sophistication in computer technology, has resulted in interest in computer graphics as a possible solution to this display problem. A great deal of work has been done in the last 15 years in computer cartography, much of which may be applicable to archaeological requirements.

Geographers have experimented with data representations of several kinds. Peucker (1972) states that cartographers conceive of their data in terms of these features:

1. Point data--Zero-dimensional signs which either denote the location of content (name, value, etc.) or constitute a part of the other two features.
2. Line data--Consists of one or more line-segments which are straight lines (vectors) bounded by a point at each end.
3. Surface data--Can be determined in a numeric fashion by a grid of points, a series of lines, or by one of several mathematical functions.

For the archaeologist, an example of point data is artifact recording. Some of the information recorded about an artifact is zero-dimensional, such as type, material, color, condition, etc., while a three-dimensional coordinate system is often imposed on the site area to record the provenience information. Line and surface data can be found in archaeology in stratigraphy, topography,

features, and all other objects with significant three-dimensional extent. Peucker discusses several types of data display for points and surfaces. These can be examined in terms of the machinery needed (hardware), the programming (software), and the availability. Based on the hardware, two types of output formats exist.

The first, and most available output device, is the line printer. Maps can be produced by taking advantage of the varying grey levels of the different typewriter characters. Drawbacks are: 1) Low resolution. Ten characters per inch are printed across the page and 6 to 10 characters per inch vertically. This, along with the fading of carbon ribbons, which often vary the grey level from one page to the next, result in low resolution, and 2) This is still a representation of a two-dimensional area, and as such the disadvantages inherent--(i.e. lack of perspective viewing, inability to directly overlay several levels of information, etc.) apply.

One of the most versatile programs available to the archaeologist for producing maps on the line printer is SYMAP. As Jermann and Dunnell (1976) pointed out, however, there are limitations inherent in the use of this mapping system. The actual data represent continuously variable phenomena, rather than the stepped statistical surfaces produced by SYMAP, and determination of the steps, or class intervals delimiting the changes in grey levels, become critical for producing accurate maps. Placement of these contour intervals, or horizontal planes, through the three-dimensional surface of the continuous phenomena was varied, producing significant changes in the maps. Jermann and Dunnell found that the uncritical selection of these class intervals would drastically change the reliability of the maps in replication of the phenomena.

The second of the output devices to be discussed here is the line plotter. This can be used to produce three-dimensional isometric representations of the data, as contrasted to the two-dimensional SYMAP plots. Jermann and Dunnell (1976) state that these plots "are believed to be an accurate rendition of the site's underlying structure and can thus serve as the data models for further comparisons (with the SYMAP plots)" (p. 10). Given the disadvantages of the line printer output in production of accurate renditions of continuously variable phenomena, the question must be asked, why use SYMAP when isometric or perspective computer maps can be produced which overcome the weaknesses of the line printer? I believe the answer lies in the technology (both hardware and software) needed for the production of these maps.

Hardware for the second output format involves the use of the line plotter, and while both drum-type and flatbed type line plotters exist, they are not as common as the line printer. Therein lies one of the two main disadvantages of this format--availability. Drum-type plotters are the least expensive of the line plotters, and are occasionally available at university computer systems. Other line plotters may be available at governmental installations, as well as specialized engineering consultant firms, which utilize these machines in contour map production.

Line plotters are capable of very small incremental movement in both X and Y directions. Depending on the type and model, increments of 100-10,000 points per inch are possible. This allows high resolution of data, a significant advance over the line printer output. Programs are available which permit perspective or isometric representations of data arrays to be plotted. They generally require the data be presented as an array of points and input in order, as a "stream" of data points. The location of a point within the stream

reflects the specific horizontal location on the map (X-Y coordinates), while the value given to the point reflects the vertical elevation (Z coordinate) of the data point. The lack of efficient graphic means of converting archaeological data into this format (lack of software) is the second major disadvantage of the line plotter system. The focus of this research has been to develop an efficient means of implementing this conversion.

CARTOGRAPHIC FRAMEWORK

Most of the archaeological graphics for analysis and report production are the result of two methods of archaeological research. The first is intra-site excavations. Test excavations and/or complete excavation of large scale three-dimensional areas have similar data recording (input) and data display (output) needs. Three-dimensional provenience systems are being used to insure recording of artifact and stratigraphy/feature locations. Graphics have generally been limited to profile and plan views, or contour maps, all two-dimensional representations. The second method of archaeological research to be considered is inter-site survey. Since cultural management studies often take the form of regional site surveys, topographic, ecological, and archaeological data are collected, and if computer technology is to be used for storage and sorting, this data must be converted into forms compatible with computer use. Report graphics have generally been limited to two-dimensional maps of site localities with some topographic element, such as contour lines or river drainages superimposed.

Information gathered by these two research methods have similarities in a cartographic framework. First, large amounts of data are collected in both situations. In intra-site excavations, management of this data is often difficult, and computer storage and retrieval is sometimes used. In cartographic terms, both types of information display could take the form of perspective plotting of point and surface relationships. In excavations, the point information may represent artifact provenience, while the surfaces would represent the surrounding matrix, the stratigraphic context and the cultural features. In the regional survey situation, the points may represent site localities, while the surface represents the topographic and ecological setting. Here also, one may conceive of the surface as the matrix of the site locations.

In both types of archaeological investigation the point data (artifact or site locations) are relatively easily entered into the computer systems by converting this information using a three-dimensional grid system overlain onto the area, and entering the result tabularly into the computer. Automatic artifact registration techniques applied to intra-site excavations have significantly increased the speed and accuracy of provenience data recording (Newell and Vroomans 1972). Existing grid systems, shown on U.S.G.S. maps, provide a ready framework for site locality conversion in inter-site surveys.

Surface representations are not easily entered into computer systems. These surfaces can be formed into arrays, as previously discussed, and multiple sets of three-dimensional coordinates used to form a surface representation. Davis (1974) overlaid a grid system onto U.S.G.S. topographic section maps and determined the elevation at each grid intersection by hand and calculator interpolation from the map contours. He used a similar method for representing an archaeological surface and artifact assemblage, and these data were transmitted to the computer system in tabular form. While this technique suc-

cessfully converts the surface information, it has the disadvantage of being very tedious and time consuming. Photogrammetric technology exists for direct conversion of the same data from metric photographs to coordinate arrays, using controlled photogrammetry equipment (Gruber 1975), but this technique involves very expensive equipment and results in high costs for the archaeologist. While this advanced technology is useful for topographic surfaces, it is not feasible for intra-site stratigraphy, where excavation procedures often do not allow continuous exposure of stratigraphic surfaces. In such cases stratigraphic profile drawings and plan views must be depended upon for reconstructions. It is believed necessary to articulate the point and surface data on a single plot to adequately convey the complexities of the depositional environment and the artifact or site locational data.

My research has been directed towards developing a methodology which will allow rapid, precise, efficient and flexible entry of surface representations into computer systems. This will permit an articulation of point and surface data to be made producing advances for the archaeologist in analytical comprehension as well as increased information transfer in reports and presentations.

Several input devices are available which allow surface data to be converted to numerical arrays. These include:

Picture and map scanners, height scanners on aerial photographs, automatic line-followers and special pattern-recognition computers have been developed. However, they are seldom available to students of computer cartography. The only device which is frequently available in universities is an X-Y digitizer, similar to a drafting table with any one of several mechanisms to record the X,Y positions of a cursor or a magnetic pen (Peucker 1972: 10).

The available hardware greatly influences the choice of input devices, and in this research a sonic X-Y digitizer was available. This device, a Scientific Accessories Corporation (SAC) digitization system, consisted of a sonic slate, a control unit which converts the slate signals into digital coordinates, and a paper tape punch, for output of the results. Previous work (Irwin, Hurd, and LaJeunesse 1971) had used a smaller but similar slate with an interdata computer interface to produce rapid artifact measurements.

When research began, the SAC system had three major deficiencies. First, accuracy was impeded by environmental factors in the room used for slate digitization. The measurements are sonic, and the control unit measures the propagation delay between the initiation of the sound, by a spark at the stylus tip, and the arrival of the sound at the microphone strips (Birkenmeyer 1977). The atmospheric conditions greatly effect reliability, and changes in temperature and humidity had significant effects on performance. The second problem was that no software existed to convert data into arrays suitable for plotting. Also, the final output of the digitization system was on punched paper tape. This is utilizable with computer systems, but the large amount of data points necessary to record each map resulted in large amounts of paper tape, which quickly becomes unmanageable. The paper tape did not allow detection and correction of errors made during input, and the lack of interactive control between the slate and the computer system reduced the efficiency of this method.

The following solutions to these problems were implemented. First, the SAC system was moved into the stable environment provided at the computer room of the Hybrid Computer Center at Washington State University. Control of

the environment resulted in the reduction of erroneous coordinates in the digitization. A graduate student in the Electrical Engineering Department developed the programming necessary to form arrays of the digitized data. In this process the slate was added to the other hardware used by the Hybrid Center, allowing in-line interactive control of the digitization process to be initiated. Interactive control has proved to be an effective means of increasing accuracy and speed during map input. This system will be discussed in the next section. The use of interactive control also meant that paper tape was no longer necessary. Through the use of an interface, data points were directly entered into the active memory of the digital section of the Hybrid computer, an EAI 640 computer. This step also significantly increased efficiency. A machinery diagram of the resulting hardware components is included as Figure 2.

PERSPECTIVE PLOTTING METHOD

The plot production procedure consists of map digitization and plot production, and the entire procedure contains nine steps.

Map Digitization

The map digitization section consists of steps one through six. Contour maps are converted into numerical arrays, suitable for computer entry, and stored for further use by this section of the procedure.

Step 1--Map alignment onto the digitizing slate--Contour maps of a large variety of sizes and shapes can be entered. Map areas must be square or rectangular, and can be as small as 5 X 10 cm. or as large as the capacity of the slate, about 70 cm. in either dimension. Maps are taped to the slate surface so that the area to be digitized is parallel to the X and Y axes of the slate. This may be accomplished readily using the numerical display on the face of the digitizing slate control box.

Step 2--Range determination--The X-Y range of the map is determined by the computer, based on its capacity of 11,521 points and the size of the input area. The slate consists of a 4,000 X 4,000 point grid within the 76 cm. square slate size. Grid points are defined as the intersections of the slate matrix, the 4,000 X 4,000 grid. This results in an actual distance between points of 0.02 cm. The computer determines the maximum number of array points within its capacity while maintaining equal numbers of grid points between array points. Array points are defined as the intersections computed by the program. The numbers entered into the active computer memory (the array) are the elevations digitized within the array point input area. Figure 1 should clarify this concept. In this example, the number of grid points between array points is 10. This distance, in grid points, is known as the delta. Since the number of grid points on the slate is fixed (4,000 X 4,000) the map dimensions determine the delta.

Step 3--Contour entry--Contours are coded with the instruction and alpha-numeric coding strips permanently affixed to the slate. When coding an instruction, the spark gap has a one second delay after coding, allowing time to lift the stylus. When contours are entered, the computer accepts 80 sparks per second, allowing free movement of the stylus across the map surface. If the distance between entered points exceeds the delta, thus breaking the continuous line of a contour, a teletype bell rings, allowing the operator to retrace that part of the contour. If two or more contours are entered within a single array

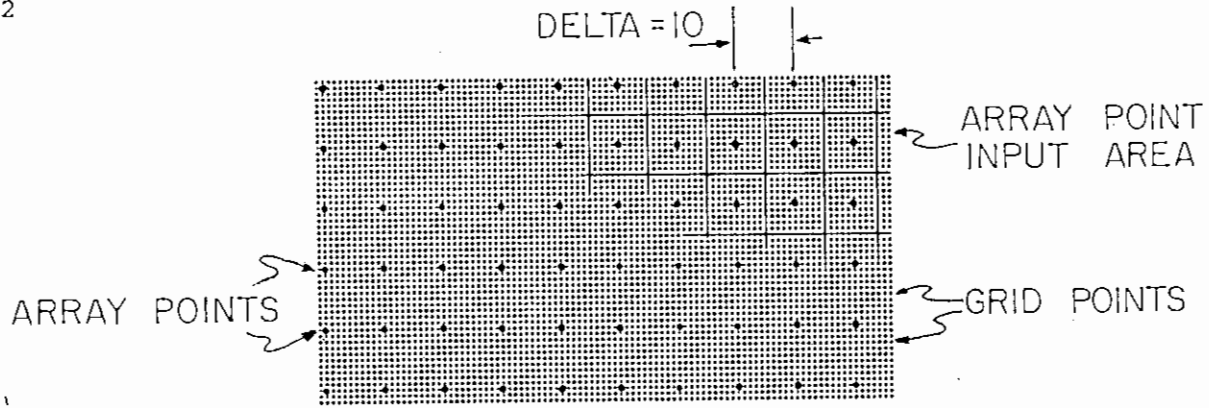


Figure 1. Array terminology.

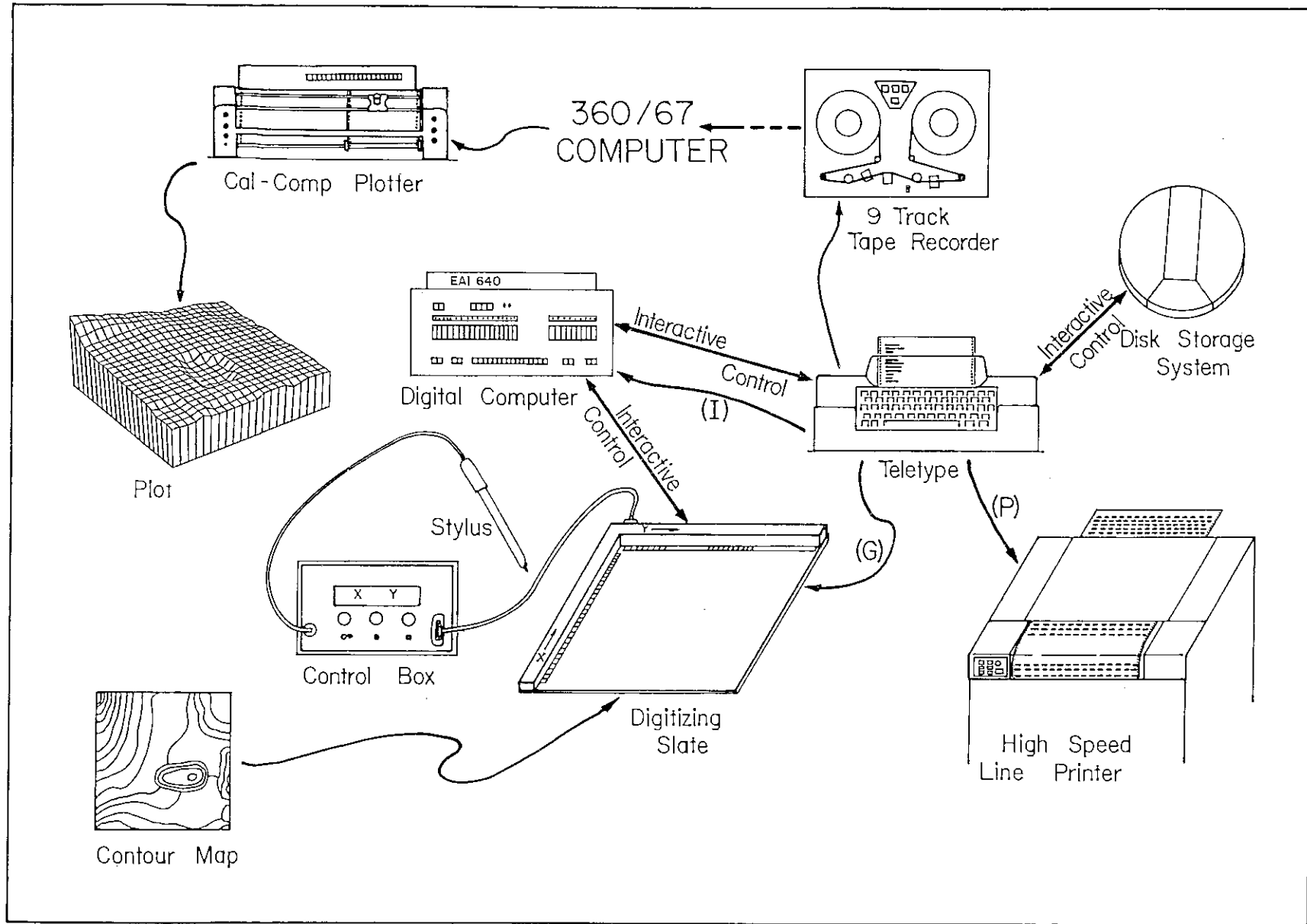
point input area, the final elevation entered overlays the previously entered elevation in the active computer memory. This may occur, especially when physically large maps with closely spaced contours are entered. Three classes of contours may be entered, background values, elevational contours, and boundary lines.

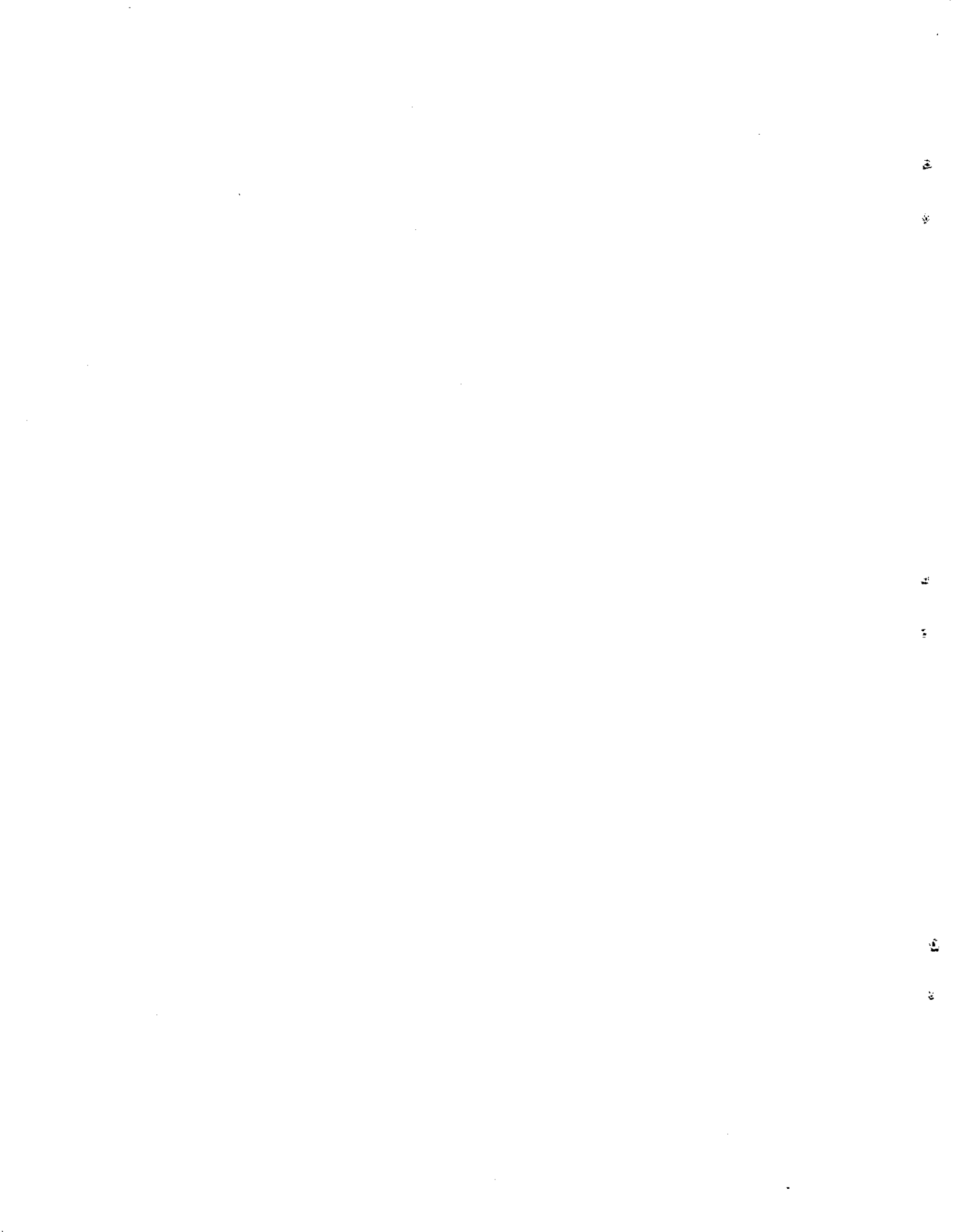
Background values--Often map areas are not square or rectangular. This is sometimes true with topographic maps, but more frequently this is the case with contour maps produced specifically for archaeological applications. Maps must still contain a square or rectangular total array, but it is possible to plot only portions of the total array, thereby allowing replication of original map sheets. This is done using an option in the plotting program which deletes chosen elevation points and prevents them from being plotted. The chosen elevations are coded first during contour entry, and these are termed background values. Values are chosen which do not appear in the range of values to be plotted. There is an inherent weakness in this system involving the conversion from array points to line segments delimited by those points at the plotter pen. The plotting routine deletes line segments which have the chosen value at either end. This results in an unfinished or "ragged" line where diagonal or curvilinear lines are the boundary with the deleted area, but line segments may be drawn in by hand completing the boundary. Where the deleted area forms a boundary parallel to the X or Y axes, however, the lines are completed by the plotter.

The background values should be entered so that several thicknesses of array points coded with that value occur at and directly adjacent to the boundary, in the direction of the deleted area. All array points in this area should be filled with the background value. If undefined points are left at this interface, they will be interpolated and produce incorrect values. This may be checked during the line printer verification procedure, before and after interpolation.

Elevational contours--Elevational contours should be entered after background values. They should be entered in numerical order, beginning with the lowest elevation and continuing towards the highest, generally. Because of the abil-

Figure 2. Digitization and plot production machinery diagram.





ity of the program to overlay several elevations in the same array point input area, the most extreme values must be entered last. Thus, where closed contours are found, the most divergent from the general surface--either the maximum in mound areas, or the minimum in depressions--should be entered last. It is therefore best to work from the outermost to the innermost area in digitizing maps of such topography. One must be consistent in the order of contour entry, as this will effect results. All occurrences of any entered contour may be deleted by specifying the elevation and the delete contour instruction.

Boundary lines--Elevational contours are line segments of specific horizontal placement (X-Y axes) representing a single elevation (Z axis). Boundary lines are lines of specific horizontal placement and varying elevation. It is assumed that the change in elevation along a straight line drawn between entered points is consistent. The elevations are known along boundary lines at points where elevational contours intersect, and at other points along the line, which can be specified. Because of the overlaying of array points, it is necessary to enter the boundary lines after the elevational contours which they intersect. Boundary lines are used to delimit points of change between generally horizontal and vertical surfaces. For example, they are used to mark the limit of excavation pits, where the vertical walls of the excavation meet the generally horizontal topographic surface and horizontal pit floors (see Fig. 4).

Step 4--Line printer verification--When map contours have been entered, control is returned to the teletype and the array may be printed on the high speed line printer. The line printer has the capacity of 30 columns (X dimension) and unlimited rows (Y dimension). It prints elevation values (Z dimension) in their X-Y location. Thus three to five strips are normally needed to completely print out the array. These can be placed together to give visual confirmation of the array accuracy. Verification is necessary to eliminate any mistakes made during contour entry, and it also provides a permanent record of the input data, so that if several surfaces must articulate adjustments may be made.

Control of the computer system using the teletype (Fig. 2) allows for digitization (G), line printer verification (P), or interpolation (I) to be chosen. At the conclusion of each step, control is returned to the teletype. Thus steps 3, 4, and 5 can be repeated as often as necessary to obtain an acceptable array, without disturbance to the previously recorded data.

Step 5--Interpolation--An eight-way interpolation is performed at each undefined point. Points defined by contour entry are flagged with a minus sign (-) as a prefix. The program first does a linear interpolation between defined points around the perimeter of the array. These defined points (the outermost row and columns) are flagged with a double minus sign (=) as a prefix. This provides the array with an outside border of "good" or defined points which are used in the general interpolation. The program then begins with the lower left hand corner of the array, as input, and searches for undefined points, moving along the first row (X dimension), up to the second row and along it, to the third row, and so forth, until the entire map is interpolated. At each undefined point, the program searches for the nearest "good" points, along the row, up and down the column, and both diagonals intersecting the point, a total of eight ways. The value and distance from the "good" points determine the elevation supplied to the undefined point. Only flagged points are used in the interpolation, not previously interpolated points. Interpolation of the complete array usually takes about one minute, and control is returned to the teletype after completion.

Step 6--Data storage--After the array has been interpolated, it must be stored

to be saved. The disk operating system (DOS) at the Hybrid center is used. The DOS program and the digitization program (Topo-Inputer) are located in the same place in the active computer memory to save space and thus maximize the array size. Thus, the DOS tape overlays the Topo-Inputer tape for Steps 6-8, without disturbance of the array data. The array is stored on a 2315 disk cartridge, which has a capacity of 1,171,200 bytes of storage, enough for over 500 arrays. Each array is coded using a six place alpha-numeric name. During this step the storage space required is transmitted by the operator, based on information provided by the computer, via the teletype, at the beginning of the digitization procedure.

Plot Production

The plot production section consists of steps seven through nine. Arrays are returned from storage, compacted, transferred to the main computer center, and plotted using this section of the procedure.

Step 7--Data compaction--Data compaction is necessary for several reasons, the primary one being that it is possible to reduce costs significantly by reducing the number of plotted points. In compaction, delta is the number of array points between points to be plotted in each direction, as shown below in Figure 3. The maximum concentration of points that can normally be plotted uses a delta of 2. This reduces the array size from about 11,000 points to about 2,800 points, cutting it by 75%. The computer costs (CPU) and plotting costs are presented below (Table 1) for different values of delta. As costs decrease (with increases in the delta) so does the resolution. Delta=3 produces little detail loss, with a substantial savings over delta=2. Delta=6 and above results in little savings of computer funds, but substantial loss of detail (see Fig. 3). Depending on available funds and the use of the plot, an appropriate delta can be chosen. When multiple surfaces are superimposed, one means of retaining clarity is to vary the density of lines being plotted. In this case, an increased delta may be preferred.

Step 8--Transfer to the IBM 360/67 computer system--Large amounts of temporary working space are required to solve the mathematical equations involved in plotting the perspective view. This requires a moderate sized computer facility, as when delta=2, 238,000 bytes of space are used. A nine-track incremental tape recorder is used to transfer the compacted array from the digiti-

TABLE 1

COST COMPARISON OF COMPACTED MAPS

Delta	Array points plotted	CPU costs	Plotting costs	Total costs
2	2809	\$3.74	\$9.97	\$13.71
3	1296	2.05	6.20	8.25
4	729	1.67	4.62	6.29
5	484	1.33	3.84	5.17
6	324	1.13	2.99	4.12
7	256	1.05	2.64	3.69
8	196	1.00	2.37	3.37
9	144	0.98	2.01	2.99

zation location to the IBM 360/67 computer. The data is then transferred to a disk storage system, allowing efficient access to the data for plotting.

Step 9--Plot production--SRFACE, a three-dimensional plotting program is used in plot production (Tangen n.d.). The deck and listing are both available from the Washington State University Computing Center Library, Pullman, Washington. Additional information is required in order to plot, and this is provided by teletype during compaction and data transfer. This required data is: X=, Y=, and Bytes=. X and Y are the number of lines to be drawn in these dimensions. Bytes is the amount of storage space necessary to contain the data array. Two bytes are used to record one data point, thus: number of bytes=X dimension times Y dimension times 2. Some options in plot production are discussed below.

ARCHAEOLOGICAL UTILIZATION

Work has been conducted utilizing the plot production procedure for data from two sites; 45-FR-5, The Miller Site--Strawberry Island Archaeological Project, and 45-CA-24, Ozette Archaeological Project. This preliminary work was done to test the utility of this procedure for the archaeologist, and some of the results are included below.

The Miller Site--45-FR-5

The concentration of semi-subterranean house pit remains on the north end of Upper Strawberry Island constitutes one of the last aboriginal villages extant in the Columbia-Snake River drainage, southeastern Washington (Cleveland 1976). The site is located 4.5 miles (7.5 km) upriver from the Columbia-Snake confluence, in the Snake River. The site was first examined in 1947, and test excavations were conducted in 1951 by Osborne and Crabtree (Osborne and Crabtree 1961). In 1976, exploratory excavations (Harkins, Brown, and Gray 1976) were followed by a field school. This was in response to increased public access and water fluctuations which threaten the site. An area containing three depressions was chosen for testing (D-96, D-117, and D-119).

A contour map was made of the topographic surface after brush clearing was completed. The area of the map was 34 meters by 27 meters, and a 5 cm. contour interval was used. Measurements were made using transit and stadia rod, after a grid system was superimposed. The datum was set equal to the annual mean sea level, which was calculated by use of U.S.G.S. survey markers and a 2 foot contour interval map of the island supplied by the U.S. Army Corps of Engineers. Two maps were inked. The first was of the topographic surface prior to excavation, and the second was similar to the first, but with the extent of the 1976 excavations superimposed (see Fig. 4).

The first map contained four main elements of surface information. These were: 1) The top surface of the island, marked by depressions, but generally horizontal, 2) The eroding bank edge, generally vertical, 3) The beach area, gently sloping away to the Snake River, and 4) The steep-sided relic collectors hole in D-96. In the second map (Fig. 4) the excavation areas were superimposed, replacing the relic collectors hole as the fourth element of cartographic information.

These maps were digitized using the procedure described above. It took about three hours to digitize both maps. Each plot consisted of a compacted array with a delta=2, yielding dimensions of X=51, Y=56, Bytes=5712 (Fig. 5).

It was necessary to square the corners and a portion of the area was unrecorded, so background values, boundary lines, and the SRFACE program options were used. Boundary lines also are used to mark the change from the top surface of the island to the eroded edge. On the prior to excavation map, the limit of the pit in D-96 was outlined with boundary lines, as were the excavation pits in the after excavation map. The additional elevations given along the length of the boundary lines assist in accurate digitization. The grid marks at the lower left and upper right of the maps are used in range determination.

These plots were found to be useful in visualization of the surface formed by the contour maps. Single plots did not, however, fully convey a complete view of the spatial relationships between these surface elements. The SRFACE plotting package allows ease of changing viewpoints, and this has been explored. Multiple views were plotted which appear to move the surface about its central axis, horizontally. The vertical dimension was kept constant through the rotational sequence in order to stabilize one dimension. This allows the viewer to better comprehend and compare surface features from one plot to another. The height selected for view was determined by trial and error. Plots drawn from very low angles resulted in lines merging and overblackening areas. Plots drawn from high angles reduced definition of surface topography. The line density (dependent on the delta) and pen point size of the plotter pen also must be counted as factors in height determination. An elevation was chosen which best satisfied these conditions, and a series of plots were made through about 140° of horizontal rotation (Fig. 5).

It is clear that these plots successfully transmit the spatial relationships involved in the surface elements. Viewing the same surface from several angles adds to the understanding of these relationships. It is also possible to separately digitize only a portion of the total map area, thus increasing the resolution of the fine detail in the original maps. Figure 7 shows the D-96 area of the previous maps.

Options in Surface Display

There are three steps in the plotting procedure where options exist which may be useful for display clarification--Step 3 (contour entry), Step 6 (compaction), and Step 9 (plot production).

Contour entry--Deletion of map areas by entering background values allows changes in the basic map shape. In the case of feature areas and stratigraphy, portions of one or more surfaces within the overall map can be deleted.

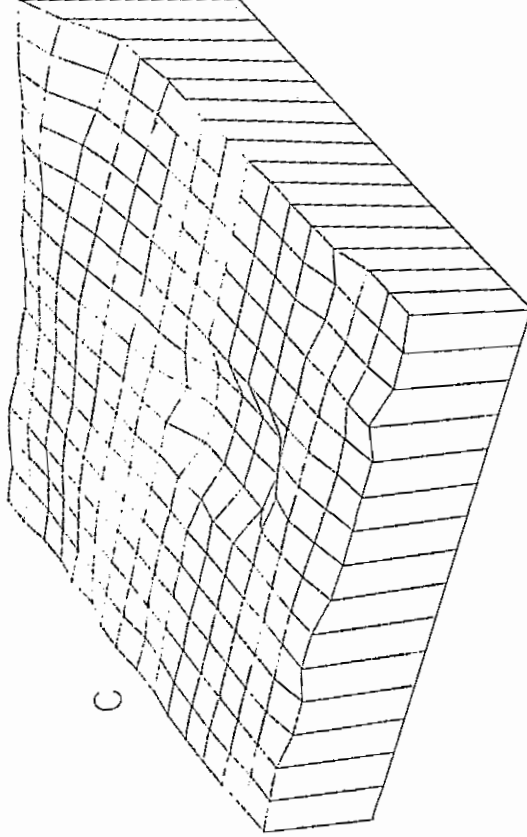
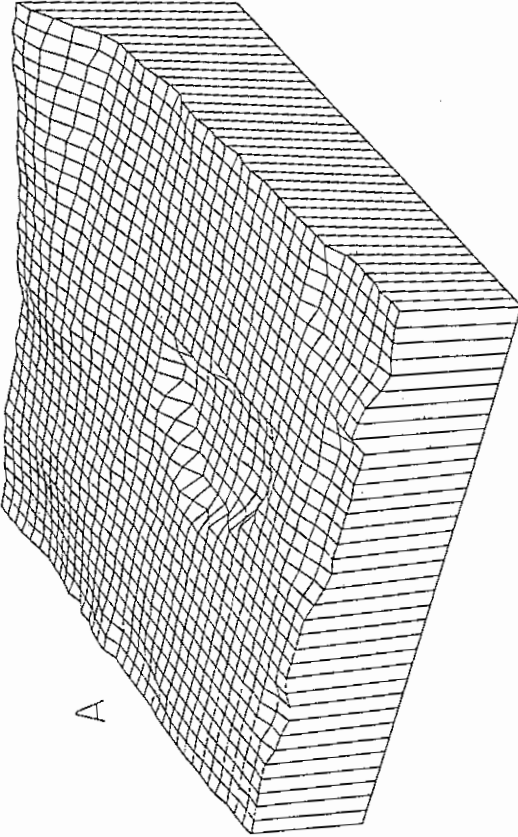
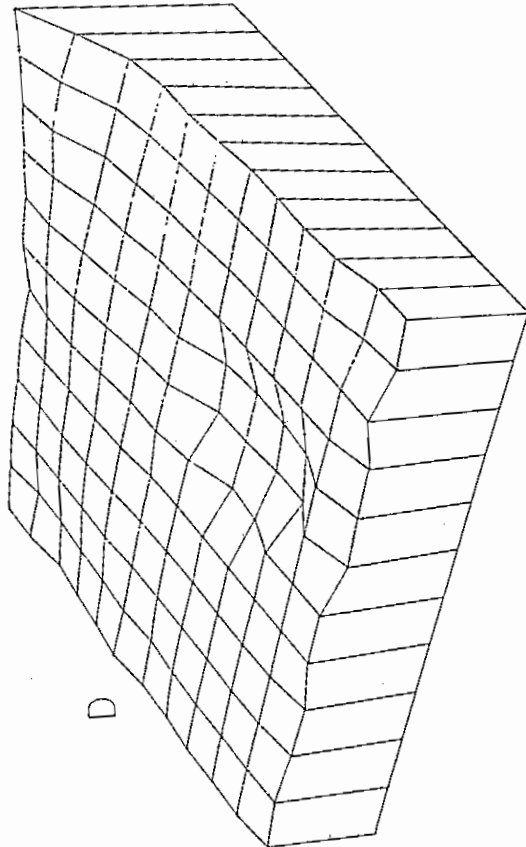
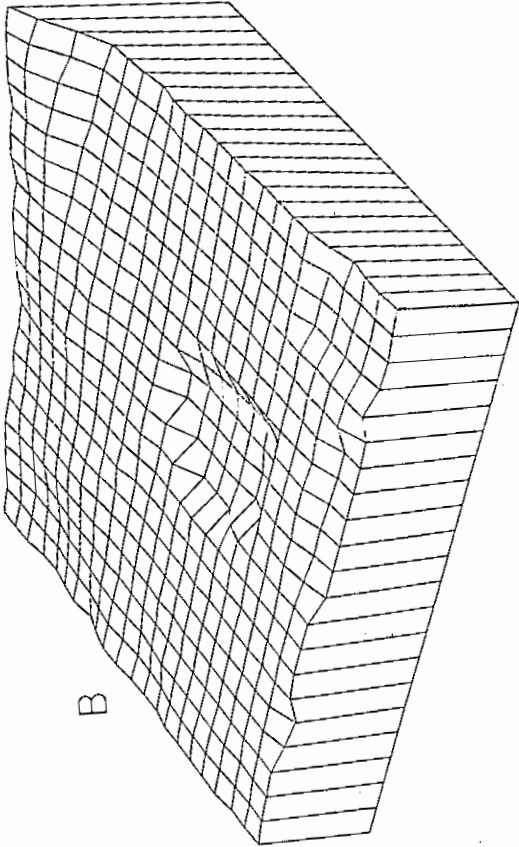
Compaction--Variation in the plot density is a major factor of surface differentiation. For example, an open grid (delta=6 or more) of thick lines would differentiate an upper surface from an underlying surface with a dense grid (delta=2 or 3) of thin lines.

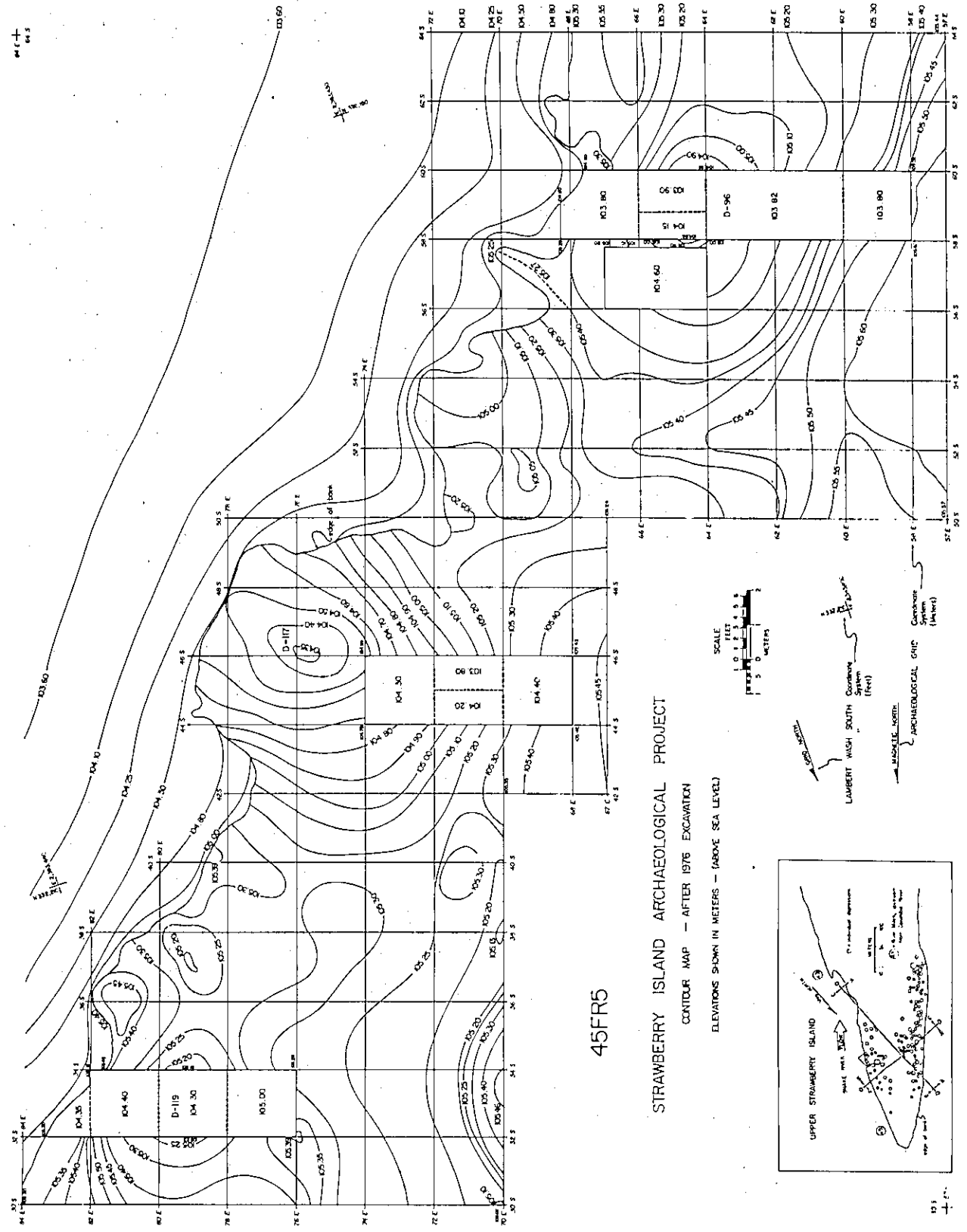
Two areas exist for changes within plot production--the line plotter itself, and within the SRFACE program.

Line plotter--The line thickness (pen point size) may be varied. This option

Figure 3. Plots with line density changes produced during compaction. A--Delta=3, B--Delta=5, C--Delta=7, D--Delta=9.

Figure 4. Contour map of 45-FR-5, showing 1976 excavation areas.

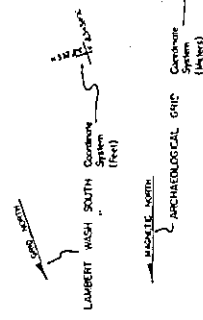
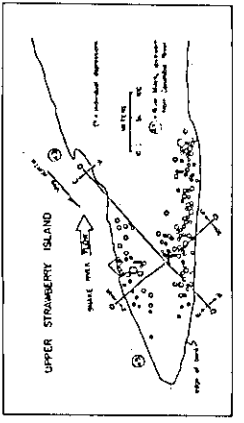




45FR5

STRAWBERRY ISLAND ARCHAEOLOGICAL PROJECT

CONTOUR MAP - AFTER 1976 EXCAVATION
 ELEVATIONS SHOWN IN METERS - (ABOVE SEA LEVEL)



10 1

can be used effectively with the plot density variations (see above) when a multiple pen plotter is used. Different color lines may be drawn as well as different point sizes. While the color option may be useful for plots drawn for analysis, its use will be avoided in report publication, due to the additional cost of color reproduction. It is believed that variations in pen point size will sufficiently differentiate elements in complex plots. Additional options involve paper type and size. All plots in this report were originally drawn on 11 inch wide non-lined chart paper. It is also possible to use 30 inch wide paper, and scale the points up in size.

SRFACE program--The SRFACE program contains several options which are useful. The 6 major options will be discussed here.

- 1) All X or all Y lines may be deleted, leaving lines in one direction.
- 2) The skirt may be added or deleted, as needed (this has been added to Figure 8).
- 3) Lines of constant elevation (contours) can be added to the surface plot. Value of these lines may be left to the program (default option), or set by the operator. These can be useful for highlighting changes in surface topography.
- 4) Deletion of map areas from the plot. This option is useful when combined with background values producing changes in the plot area.
- 5) Drawing of stereo-pair plots. This approach has value for analytical purposes, but for report production, the special equipment needed (stereoscope) limits its usefulness.
- 6) Variations are possible in the relative scale of any dimension. This is most useful when applied to the vertical dimension, as vertical exaggeration can emphasize surface features.

Use of these options and multiple-view data display should provide the means for differentiating complex surface and point data relationships. These in turn should prove a valuable tool for archaeological analysis and reporting.

CONCLUSIONS

The problem approached in this project was to develop more efficient methods for compiling, utilizing, and displaying spatial relationships between topographic, stratigraphic, feature, and artifact archaeological data. Use of computer systems was necessitated by the large volume of data collected. Computer cartographic methods were evaluated, and a plotting method was developed which fulfills these goals. The plots produced convey the spatial relationships more clearly than two-dimensional maps, and can be produced at a moderate cost in time and money. As report production tools, they are successful in transmitting topographic information, especially to readers not familiar with the locality plotted. As an analytic tool, the utility of this method has yet to be confirmed. Preliminary work clearly shows that computer representations are only as accurate as the data they represent. Where data is inaccurate or incomplete no amount of technology can overcome this lack. As a consequence, the spatial relationships involved in a complex sequence must be understood before any digitization can be done. In this case, perspective plots only act to transmit information to others, not to acquire it for the researcher. Where point data is interrelated with surface data, this method may act as a research

tool, and new information acquired using the perspective plots.

Several other research tools could be utilized with this method.

- 1) Remote sensing technology can provide rapid and accurate data acquisition without disturbance to the site areas (Lyons 1976). Contour maps may be produced by specialized engineering firms from aerial photographs, and these may be digitized to produce plots. Site localities (as point data) or vegetation zones (as multiple color lines) could be plotted with topographic maps (surfaces). Several map scales could be utilized from the same topographic map sections.
- 2) Normal spatial statistic operations may be used to quantify the point data relationships. Standard statistical packages are available (such as SPSS) for use with the IBM computer system. It is possible to develop programming which would allow computation of three-dimensional measurements, such as volume and surface area, directly from the digitized data.
- 3) A system of photographic provenience recording using the digitizing slate could also be developed. A program exists, used in Biomechanics, which translates point data into a three-dimensional coordinate system, by digitization of photographs (Bergemann 1974). This system could be adapted specifically for archaeological needs. This system would not need metric cameras or standardized camera location, two of the more troublesome requirements for most photographic digitization procedures. (See Gruber 1975 for a metric camera system for the archaeologist.) Low oblique photographs could also be used. Probably the only requirement would be to know the three-dimensional provenience of several points in each photograph, a requirement usually met by corner stakes in an archaeological excavation. This system could be useful for obtaining comprehensive records of excavation, while reducing field write-up time.

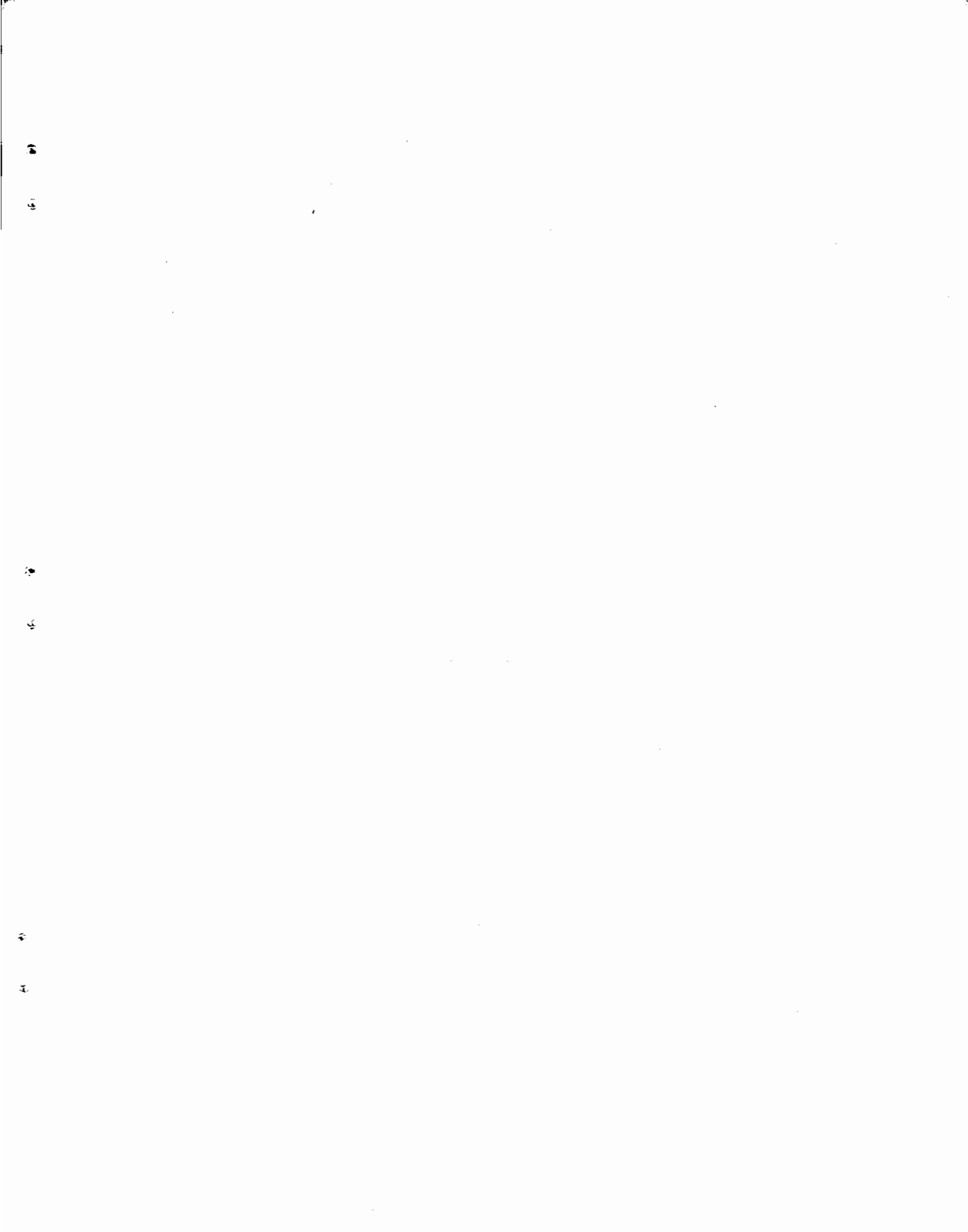
One minor difficulty with the digitization method has been found. The reader may have already noticed the walls of the excavation pits are not vertical. The digitization procedure does not allow production of vertical lines within a surface plot. When the array is formed a single vertical elevation (Z coordinate) can be recorded for any given horizontal point (X-Y coordinates). A vertical line segment is described by two vertical elevations at the same horizontal point. Thus it is necessary to move one space for one end of the line segment, producing the sloped lines for the excavation walls. This is a minor disturbance, but if necessary the vertical lines could be hand-drawn after deletion of the sloping lines.

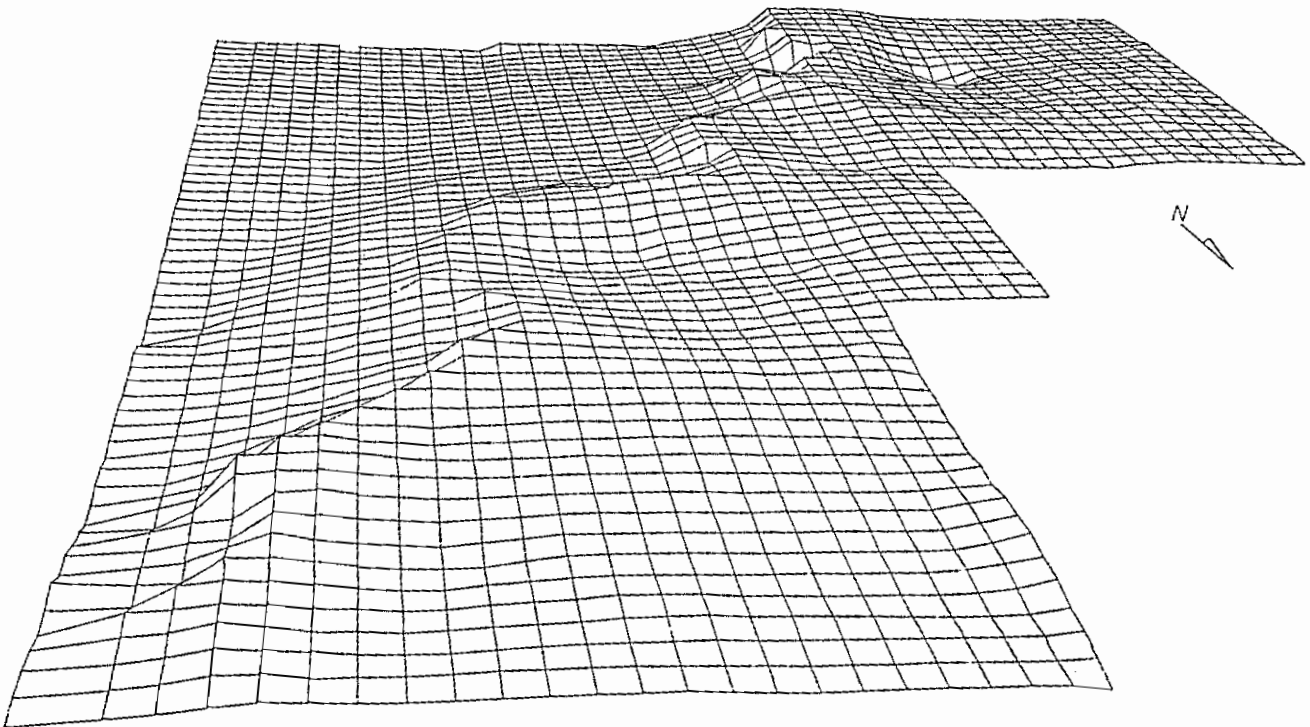
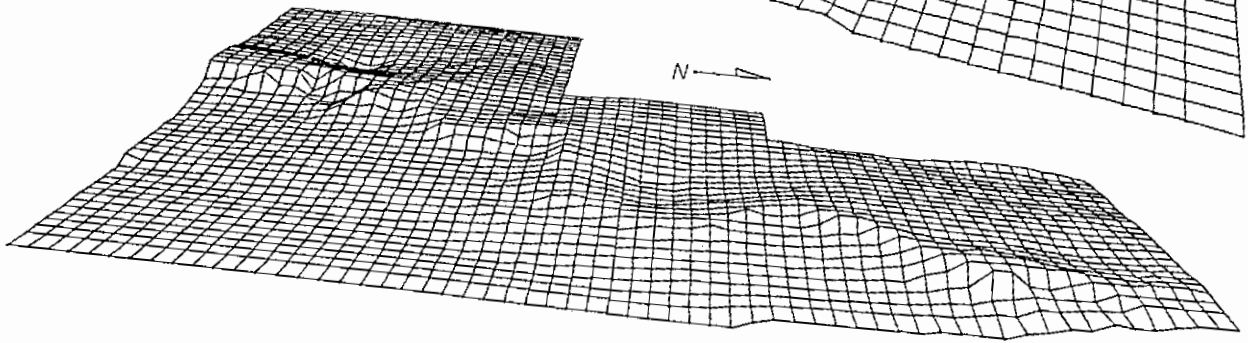
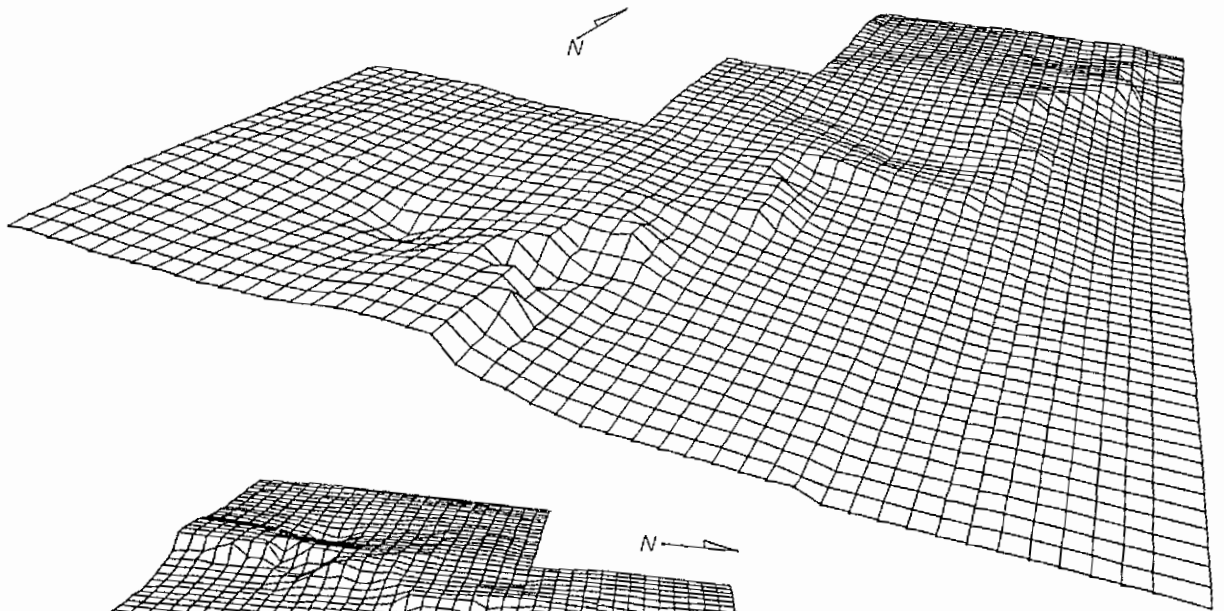
Presently, the plotting program used limits display to a single surface representation on a plot. Research is continuing to develop a plotting program which will "stack" surfaces, allowing multiple surface representations to be overlain on the same plot. The addition of this capacity will allow complex stratigraphic relationships to be plotted.

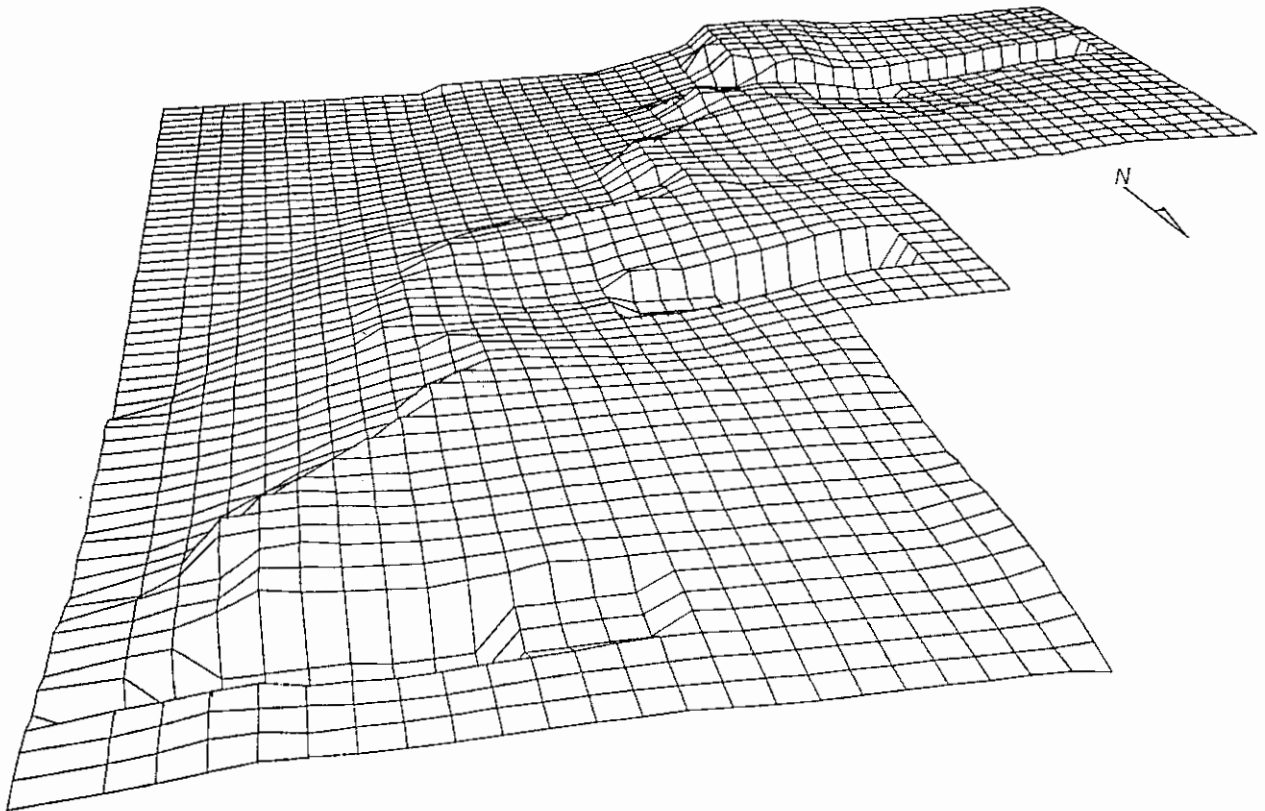
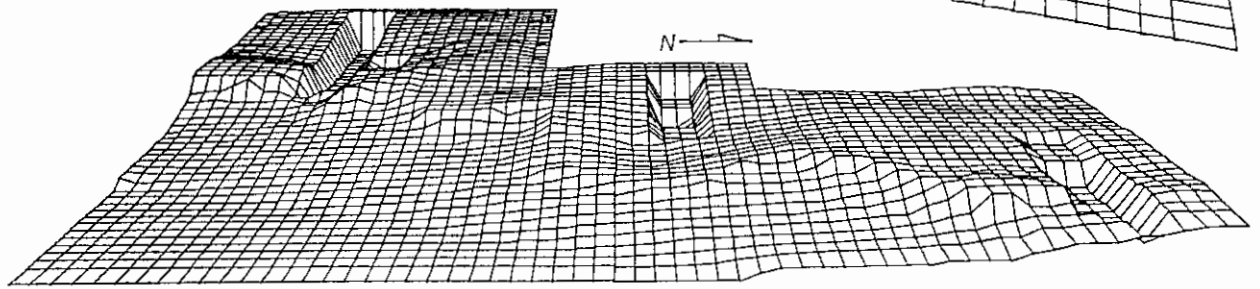
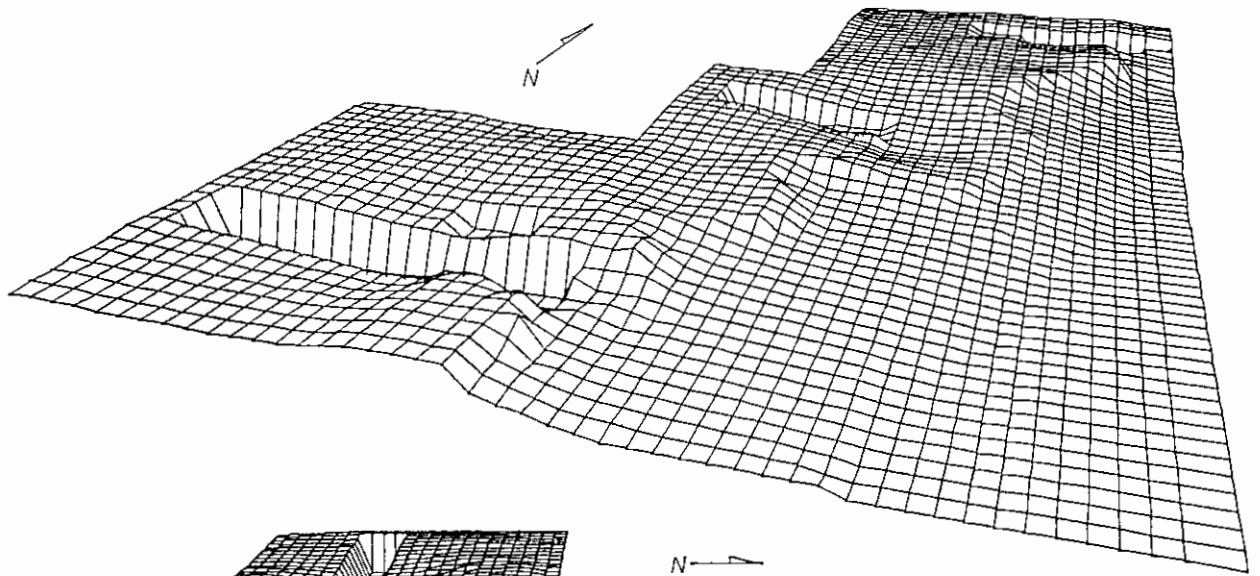
Figure 5. Multiple views of 45-FR-5 prior to 1976 excavation.

Figure 6. Multiple views of 45-FR-5 after 1976 excavation.

Figure 7. Plots of D-96 area. A) Prior to excavation. B) After 1976 excavation.







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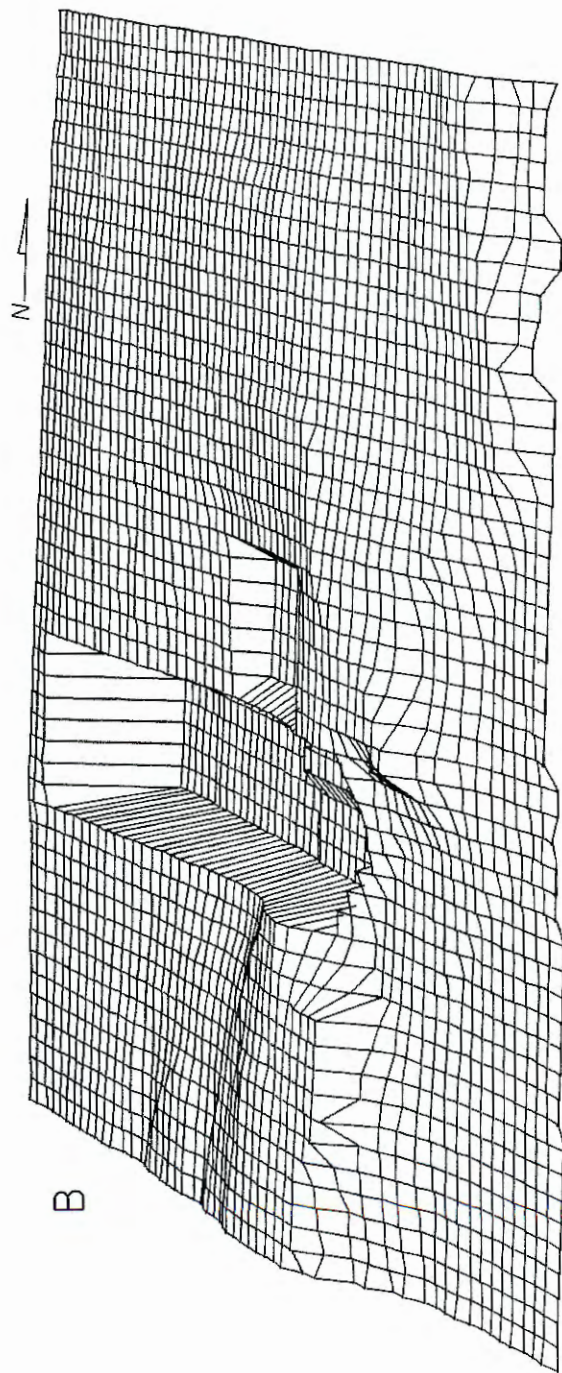
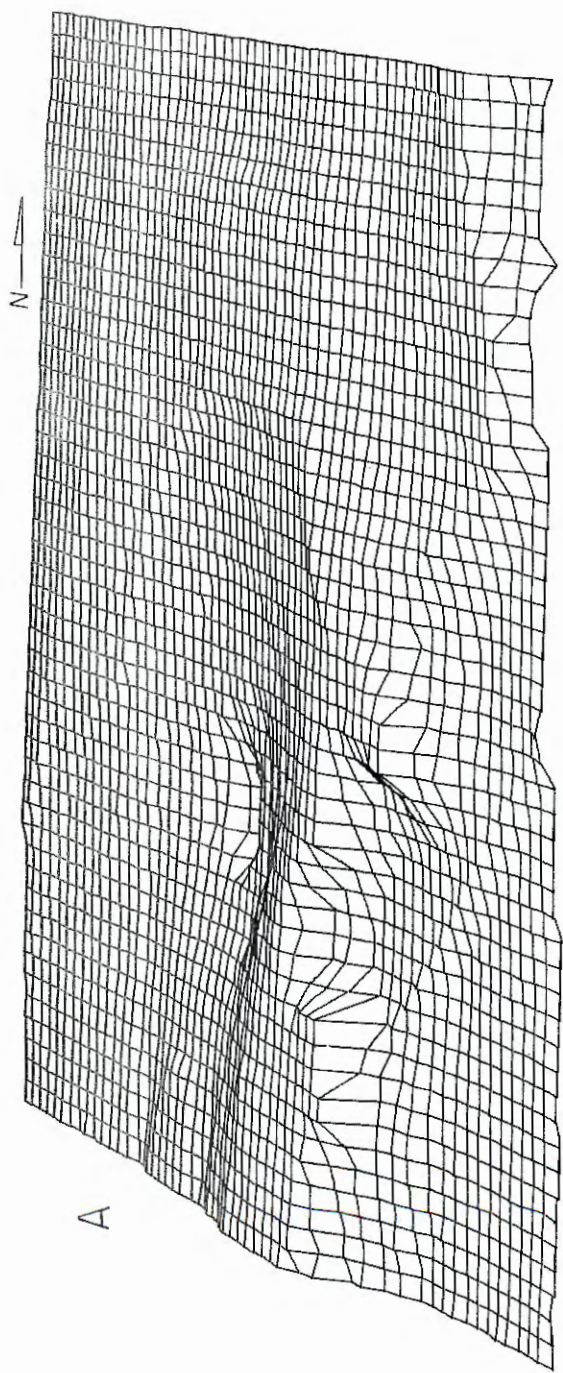
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The figures in this paper were reduced and reproduced by use of metal plates. Paper plates or xerox-type reproduction would not preserve the detail and clarity of the originals.

It is hoped that this research stimulates further cartographic experimentation by archaeologists. Computer plots are an effective way of transmitting spatial information to other archaeologists, as well as the general public. As well, these plots may be useful as a research tool. Computerization of surface data can lead to quantitative evaluation. Remote sensing and photographic provenience systems can be interfaced with computer systems using the method described in this paper. It is hoped that this research may produce further development of the computer as a tool for the archaeological community.

Further information on this process may be obtained from the author at the following address:

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Washington State University
Pullman, WA 99164

Figure 8. Multiple view plots of sample point and surface data.

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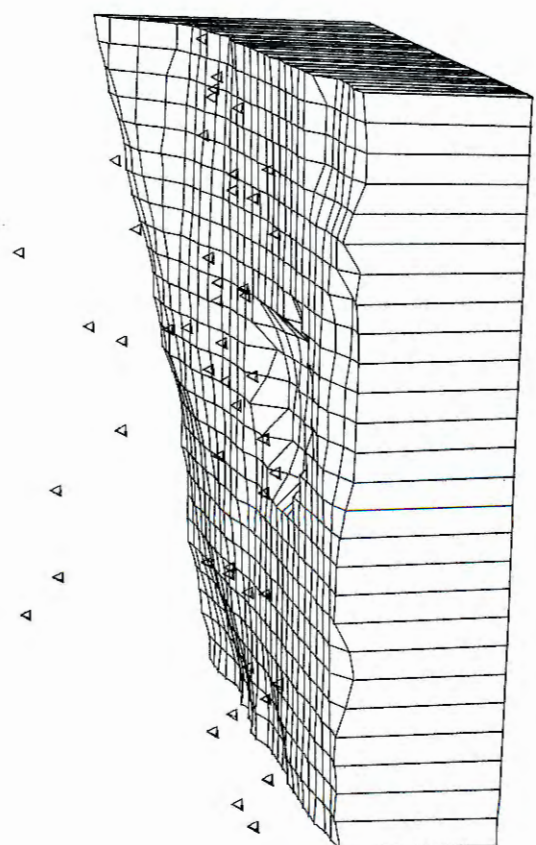
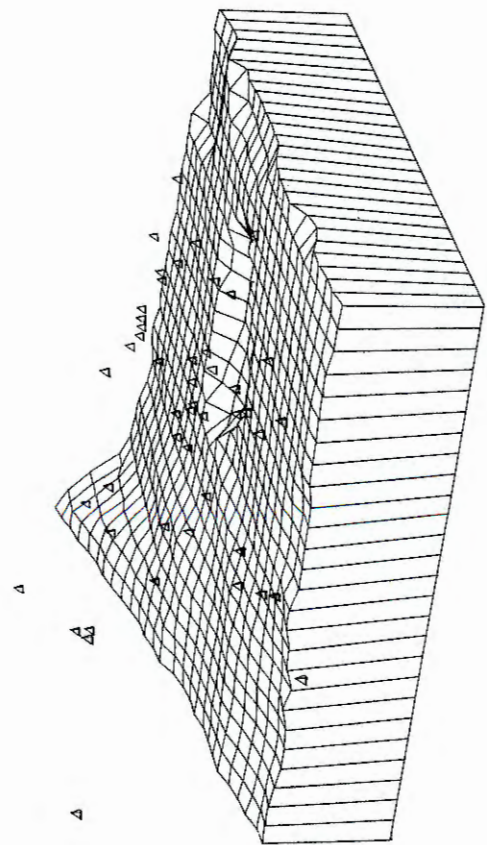
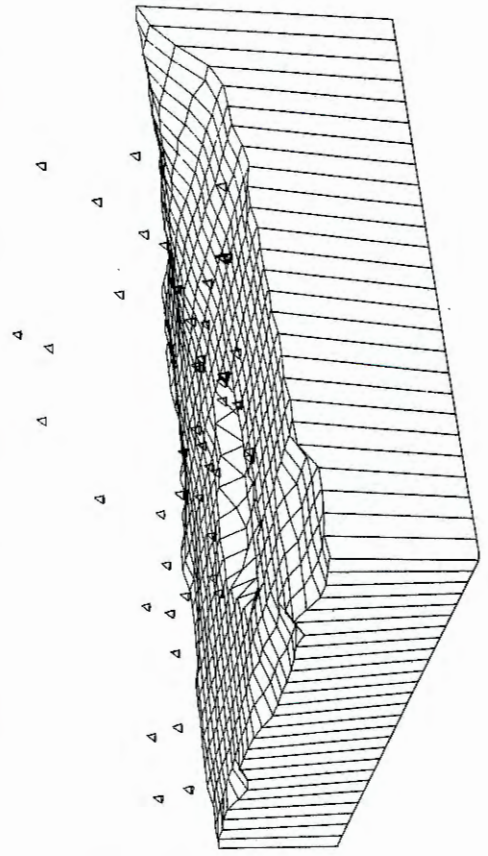
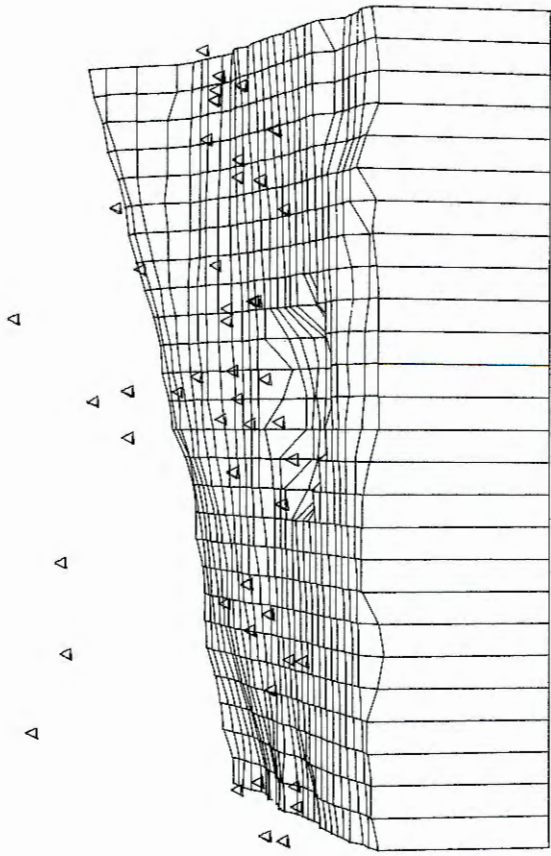
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