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ABSTRACT

MODELS OF PRE-DORSET CULTURE:
TOWARDS AN EXPLICIT METHODOLOGY

by

Albert Arch Dekin, Jr.

Past archaeological methods and techniques have not resolved several long-standing problems in Eastern Arctic prehistory. Moreover, the recent resurgence of once-discarded models leads to the conclusion that our research has not resulted in significant improvements in our understanding of processes of Pre-Dorset behavioral variation in space and time. Archaeological research has been conducted under implicit paradigms with imprecise methods and techniques. Variations in archaeological interpretations of the Arctic Small Tool tradition have resulted from the lack of a generally accepted paradigm and from variations in archaeological data, techniques, and methods. Such interpretive problems are characteristic of our study of Pre-Dorset structures, the Pre-Dorset migration into the Eastern Arctic, and the subsequent processes of formation of regional variants of Pre-Dorset culture.

The explanation of behavioral change in archaeologically-known populations requires a precise chronological framework. Using available radiocarbon dates from the Arctic Small Tool tradition, making implicit possible sources of variation and adjustments in individual

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dates, these data are treated precisely and a provisional chronology is established. Techniques for the evaluation of significance of difference between individual dates are used to establish the probability of contemporaneity of several components of the Closure site (KdDq 11).

Because human behavioral systems are our major means of adaptation to our environment, any changes in the environment of prehistoric Arctic populations are potential sources of variation in human behaviors. Data on prehistoric environmental change during the time of Pre-Dorset occupation (4050-2750 B.P.) are synthesized to establish trends in environmental change and a marked shift in climate at ca. 3500-3600 B.P..

Precise field techniques and data depiction from the excavation of the Closure site (KdDq 11) resulted in data on rock and artifact distributions manipulated using an elliptical data structure and statistical tests of significance to test hypothesized structural attributes. These methods produced a hypothetical model of Pre-Dorset tent structures at the Closure site, which is suggested for further testing on comparable Pre-Dorset data sets. The value of precise and explicit field techniques and analytical methods is demonstrated.

A model of the migration of Arctic Small Tool tradition peoples as a diverging horizon is derived deductively from theoretical dispersal processes in other species in which there is increasing behavioral variegation with

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dispersal into an unoccupied ecological niche. This model is tested on available data from the Arctic Small Tool horizon, using artifact variation in burins, microblades and end blades. In spite of the limited data available on variety in these artifacts, the data on morphological and dimensional variegations are in accordance with the prediction of the model and it is suggested for further testing.

An elaborate model of the relationship between environmental change, social scale, and technological change is derived from other studies of environmental change, social change, and economic development. This model is tested with data on the development of regional variants of the Eastern Arctic Small Tool tradition. Portions of the model accurately predict the macro-fragmentation of the Arctic Small Tool horizon following the demonstrated climatic change _ca_. 3500-3600 B.P..

This study demonstrates the impact which implicit assumptions and imprecise methods and techniques have had on our understanding of prehistoric behavioral processes in the Eastern Arctic. The advantage of precise field techniques for the testing of hypotheses is demonstrated.

The use of explicit and precise processes of model building and testing leads to a greater methodological sophistication and to the increased potential for significant theoretical contributions. Models of Pre-Dorset structures, of the Arctic Small Tool horizon, and of the impact of environmental change on cultural systems may be developed and

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tested. Greater technical precision and methodological explicitness are necessary if we are to benefit from the time depth and ecological dimensions of archaeological data to contribute to the more general explanation of human behavioral processes.

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MODELS OF PRE-DORSET CULTURE: TOWARDS AN EXPLICIT METHODOLOGY

by

Albert Arch Dekin, Jr.

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

Department of Anthropology

1975

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ACKNOWLEDGMENTS

If the research presented herein contains elements of lasting value, they stem largely from the mentors, colleagues, and students who have influenced me. The shortcomings result from my failure to follow their advice or to fulfill their expectations.

Above all, Moreau Maxwell has provided me guidance, knowledge, opportunity and freedom to pursue what some may see as high risk research. More than anyone, he has fostered a respect for the variety in artifacts which form the basis for archaeological research. Charles Cleland, Charles Hughes, and Iwao Ishino have continually forced me to seek the larger picture of human behaviors and have kept me from seeing burins as self-replicating populations. Douglas Byers taught me the advantage of precise archaeological field techniques and my students taught me that such precision should suit the problem being investigated. Elmer Harp, Jr., kindled my original interest in the Arctic and has fanned its flames ever since.

The institutional support and cooperation of the National Science Foundation, Michigan State University, The State University of New York, College at Potsdam, and the National Museum of Man, National Museums of Canada are gratefully ackno

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I owe a special debt to my family, especially to my wife Ruth, whose tolerance and efforts through the evolution of this dissertation are much appreciated.

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INTRODUCTION

It is a difficult task to assess the present state of our understanding of Eastern Arctic prehistory and to evaluate an ever increasing number of alternate hypotheses. The last decade has demonstrated that we are learning more and more about less and less, as increasingly sophisticated methods of analysis are applied to smaller and smaller data sets. We are faced with increasingly diverse interpretations of the same data and a corresponding inability to formulate research strategies which would reduce the interpretive chaos. There is a general lack of explicitness and precision in the conduct of archaeological research in the Eastern Arctic and it is difficult to determine the sources of variation in archaeological interpretations.

The lack of explicitness and precision has led to the selection of data and analytical techniques based on implicit assumptions and biases which are not made available for evaluation or discussion. Thus, there is great variation in: excavation techniques; criteria for data recording, depiction and description; amounts of data published in support of conclusions; methods of analysis; criteria for the evaluation of radiocarbon dates; and data on the environmental setting. Since the criteria on which these

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selections are based have not been made explicit, differential selection is a significant source of variance. This lack of methodological specificity has plagued much of archaeology, but we cannot continue to use implicit and imprecise methods if we wish to make non-trivial contributions to our understanding of human behavior.

This research has two goals: 1) to demonstrate sources of variance in Arctic archaeological interpretation resulting from differences in the data, their collection and analysis. These differences often result from a lack of precision and explicitness. 2) to model Pre-Dorset behaviors at three levels of abstraction, using explicit and precise methods of data collection and analysis.

My interest in this problem stems from recent research and historical studies which contributed to my conclusion that we had not increased our understanding of the Eastern Arctic prehistory during the last decade, and that we were beginning to learn more about less. The recent revival of concepts once thought to be of limited utility was further indication that our studies were not building a body of understanding on which to base my own (and future) research.

It became apparent that one reason why we lacked sophisticated understandings of these data was that we lacked any rigorous treatment of data, method and theory.

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and theories on the present state of our knowledge, a plea for greater explicitness and precision in archaeological research, and a demonstration of such methodologies in the formulation and testing of models of human behavior at several levels of analysis. By making the treatment of these data as explicit as possible, others can see where and how the analysis proceeds, and why. While such a procedure may make this study more open to criticism, it should pave the way for fruitful discussion of archaeological methodology and place archaeological explanation in the Eastern Arctic on a more firm foundation.

PART I

THE INTELLECTUAL BACKGROUND

Chapter 1. The Study of Pre-Dorset Culture and the Arctic Small Tool Tradition

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Chapter 1

The Study of Pre-Dorset Culture and the Arctic Small Tool Tradition

Three Concepts

The study of the early prehistory of the Eastern American Arctic is dominated by three concepts and associated models: 1) Paleo-Eskimo; 2) Pre-Dorset; and 3) Arctic Small Tool tradition. These concepts have been generally used and accepted as heuristic devises for ordering the widely distributed and often little known cultures across the American Arctic during the period 5000 to 1000 B.P.

The concept of Paleo-Eskimo was suggested by Steensby (1917) to differentiate between two cultural strata of Eskimos. The Paleo-Eskimo were adapted to land and sea-ice hunting and lived in snow houses, originating from the inland Indian cultures in the Central Arctic and spreading both East and West. In Alaska, this earliest Eskimo population was subjected to the influences of Pacific cultures, ostensibly including Japan, leading to the Neo-Eskimo development of open sea kayak hunting and to the use of the umiak. Their descendants were considered to be the historic Eskimos.

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In 1950, Helge Larsen made this concept explicitly archaeological, relating it to technology: "A Paleo-Eskimo culture would thus be a culture in which chipped flint implements are preferably used, and a Neo-Eskimo culture one in which ground slate implements are predominant" (1950:186). In the Eastern Arctic, Paleo-Eskimo and Neo-Eskimo have served as a useful distinction between early coastal-tundra cultures whose technology included an extensive reliance on ground slate. The distinction is both temporal and cultural, with considerable spatial overlap of both stages.

Paleo-Eskimo cultures in the Eastern Arctic have been defined by reference to specific distinctive cultures in Greenland (Sarqaq--see Meldgaard 1962; Independence I and II--see Knuth 1967) or as developmental to later cultures (Pre-Dorset--see Collins 1954b). Recently, the inconsistent application of these concepts as unifying ideas lead McGhee to revitalize the concept of Paleo-Eskimo to refer to those early cultures in the Eastern Arctic which stemmed from an early migration, or migrations, and which developed into several distinctive adaptations to differing environments before the development and migrations of the Thule culture of ca. A.D. 1000 (McGhee 1973). Taylor had recognized the strain on the concepts of Pre-Dorset and Sarqaq in 1968, suggesting that we substitute the term "Carlsberg culture" to encompass those Canadian-Greenland cultures previously called Independence, Sarqaq, and Pre-Dorset (

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and Pre-Dorset (Taylor 1968:85). Whatever the title, it has become apparent that a new model is necessary to describe the relations among these early cultures.

The Arctic Small Tool tradition was first defined in Alaska as a result of studies of core and blade technologies. The studies of MacNeish (1954:252) in the Yukon and interior Alaska demonstrated the presence of a series of boreal sites containing polyhedral and tongue-shaped cores and blades struck from them, several varieties of scrapers, choppers, and large projectile points. "This early interior northwest North American cultural pattern seems to be distantly related to another early pattern along the Arctic coast, often called the Paleo-Eskimo. . . . Both of these patterns may have developed from some as yet undefined early (Mesolithic) Paleo-Siberian cultural complex" (MacNeish 1954:252).

This contrast in stone technologies was also observed by Irving, who noted that "the boreal forest sites discussed here seem to hang together, and lack the types associated with early man which appear in the Denbigh Flint complex. They seem to belong to a line of development different from that found thus far in the Eskimo area" (1955:382).

Irving formalized the similarities in tundra and coastal burin-blade industries into the "arctic small-tool tradition" (Irving 1957:47) and contrasted it with MacNeish's boreal forest sites at Pointed Mountain and the

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Campus site. Irving's prominent reference to "early man" and to the Denbigh Flint complex reinforces the interpretation that this tradition was believed to have great time depth as well as extensive geographic range, being representative of an "extensive continuum through space and time" (Irving 1953:71).

Pre-Dorset was first defined as a cultural entity with reference to those cultures in Eastern Canada which were believed to develop into Dorset culture, representing a migration of peoples closely related to the Denbigh Flint complex in Alaska (Collins 1954b:304). This definition resulted from the discovery of sites earlier than classic Dorset sites which contained burins, burin spalls, microblades and other artifacts similar to those believed to be early in Alaska. Thus, the first application of the term Pre-Dorset was as if it were a residual category produced by the timing and extent of previous archaeological investigations in the Eastern Arctic. It is the developmental aspect of the term that has lead to its criticism (Noble 1971; McGhee 1973) and to its rejection as a widely useful term for comparative studies.

It is significant to emphasize the points of reference for the development of these concepts. 1) Paleo-Eskimo. Steensby's attempt was essentially ethno-historic in approach, attempting to explain the observed diversity in historic Eskimo adaptations as a result of the movements of people with different technical knowledge and subsistence patt

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subsistence patterns. The application of this model to archaeology was a fortuitous result of the attempt to fit archaeological data in support of his considerations of historic Eskimo migrations.

2) Arctic Small Tool Tradition. The Arctic Small Tool tradition was formulated by specific reference to the Denbigh Flint complex, then believed to represent early man in Alaska (Giddings 1951, 1954, 1955). The long time depth associated with the idea of "tradition" resulted not from extensive chronological inference, but from the association of basal fluting with precision flaking and a burin-core-blade technology, all presumed to be indicative of early migrations to the New World. Thus, the initial time depth to the Arctic Small Tool tradition came by deduction from association and assumption.

3) Pre-Dorset. The Pre-Dorset was a by-product of the research into Dorset origins, when it became obvious that earlier cultures in the Eastern Arctic lacked many of the traits of "classic" Dorset, but were apparently ancestral to it and were technologically related to the early cultures of the Denbigh Flint complex in Alaska. Again, this concept was formulated to describe ill-defined intermediary developmental stages between relatively well-known cultures (Denbigh and Dorset). Evidence from Meldgaard's excavations at Igloolik (1962), Maxwell's analysis of Dorset development (Maxwell 1967), Noble's investigations of the Canadian Tundra tradition (1971) and from McGhee's

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reconnaissance of Devon and Dundas Islands (McGhee 1973) suggests that much of what we have considered Pre-Dorset is not developmental into Dorset and thus the term has been too widely applied and has misleading implications. The developmental model does not apply to all that are frequently included under its wing.

It is apparent that the conceptual schemes and models for studying the early cultures of the Eastern Arctic are in a state of flux, lacking general acceptance or utility. The use of several of these is anachronistic and the models which they represent no longer fit with the available data. One of the purposes of this analysis is to develop models which will reflect the present state of our knowledge and which will be useful in the generation of testable hypotheses regarding cultural processes.

An Historical Perspective

Because the substantive data on which this study is based are from the Pre-Dorset of the Eastern Canadian Arctic, the following discussion of the historic background to this study will regard Pre-Dorset as a distinctive variant of an Eastern extension of the Arctic Small Tool tradition, reserving until later the evaluation of the appropriateness of these concepts.

The study of Pre-Dorset culture was spared the early stages of the development of Arctic archaeology (see Dekin 1973a:15-21). The earliest published finds of what were later recog

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were later recognized as Sarqaq artifacts and Independence houses in Greenland were conspicuous by the relative lack of attention that they attracted. Solberg's early attempt to establish a stone age culture in West Greenland (1907) lay largely unaccepted and generally regarded as iconoclastic because it conflicted with the views of the Mathiassen establishment. Thus, his contribution lay largely ignored for almost fifty years. Thostrup published one of the first series of mid-passage houses from Northeast Greenland (1911:194, 195), but they were accorded no great age. Pre-Thule culture in Greenland was unacceptable to the general understanding of Greenlandic prehistory and thus almost a forbidden topic for half a century. The link between these early studies and the later efflorescence of research is tenuous at best.

Arctic archaeology prior to 1950 could be described as "Boasian" in that the emphasis was on the collection of data and the interpretations were built by "letting the data speak". This emphasis on empiricism and on an inductive approach to analysis has a long history in the Arctic, persisting even to the present day. The major analytical tool used for comparisons was the trait list supposedly defining trait complexes, which formed, through time, traditions (as trait complexes with time depth). Perhaps the greatest conflict during this period was between the migrationists and the diffusionists, although neither group formed a cohesive faction. We must keep in and that this p

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mind that this pre-1950 period in Arctic archaeology was before radiocarbon dating and that culture history was built on developmental sequences of technology with little regard for theoretical discussions regarding the nature of ethnic groups or cultural systems. "Culture" was almost synonymous with "trait complex" and the literature is conspicuous by the imprecise use of the concept "culture."

While this terminological conservatism kept the archaeologists close to their artifacts, several were quite liberal with their use of artifacts and artifact types as guide fossils, lending significance to the presence or absence of certain apparently significant artifacts (such as microblades) from collections of minute size (often as low as 30 fragmentary artifacts created as tools).

Credit for initiating the modern stage of our studies of Pre-Dorset cultures must rest with J. Louis Giddings, whose excavations of the Iyatayet site at Cape Denbigh led to the establishment of the Denbigh Flint Complex (Giddings 1949, 1950, 1951). The presence of almost exquisitely flaked side and end blades with presumed "Old World" forms of artifacts (burins and microblades) and several projectile points with basal thinning reminiscent of "fluting" then presumed to be of great antiquity in North America, led Giddings to suggest great antiquity for the Denbigh artifacts, believing them to be the products of "Early Man" in the Arctic. His early papers make obvious that he

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obvious that he regarded this complex as much earlier than even developmental Eskimo culture and his initial rejection of radiocarbon dates of three to five thousand years before present (Giddings 1955) confirmed his intent to search for evidence to establish such antiquity.

Almost immediately, other sites with similar artifacts were found in interior Alaska in Anaktuvuk Pass (Solecki 1951; Solecki and Hackman 1951; and Irving 1951) all of which were considered representatives of an apparently early complex.

The Eastern Arctic was also shaken from its state of complacency by the finds of Hans Mosegaard from Sarqaq in West Greenland. These were reported by Jorgan Meldgaard in 1952 who suggested that

> . . .the Sarqaq material can be interpreted as evidence of an Eskimo culture, closely related to the earliest Eskimo cultures in Alaska, which appeared in West Greenland after wanderings without lengthy stops; i.e., without development of local types in the eastern areas, contrary to what happened to the Dorset culture (Meldgaard 1952:299).

Meldgaard saw the Sarqaq artifacts as the remains of an Eskimo culture related to the Denbigh Flint Complex and later cultures in Alaska, but not directly related to the then fairly well known Dorset culture, which was believed by Meldgaard to be at least partially contemporary with Sarqaq (1952:229).

Dorset culture sites had been found across a wide expanse of the Eastern Arctic from Newfoundland (Harp 1953) to Northeast Gre

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to Northeast Greenland (Knuth 1952) and these were seen by some archaeologists as being related also to both the Sarqaq and Denbigh sites, although the exact nature of this relationship was unclear.

Henry Collins summarized "Archaeological Research in the North American Arctic" (1954b) in a paper which has stood the test of time and is remarkable for the degree to which the conclusions remain generally accepted. His remarks on the relationships between Denbigh, Dorset, and what he called pre-Dorset bear quoting.

> On the other hand, there are indications of a cultural connection, despite a great time gap, between the Denbigh Flint Complex, pre-Dorset and Dorset-like cultures in Canada and Greenland, and the typical Dorset cultures of these regions (Collins, 1951, 1953 a,b, 1954a; Knuth, 1952; Meldgaard, 1952; Harp, 1953). There are also significant resemblances between some of the Denbigh implements and those of the much later Ipiutak culture (Giddings, 1951; Collins, 1951, 1953b, 1954a; Harp, 1953). It appears, therefore, that the Denbigh Flint Complex was one of the sources, perhaps the principal source, from which Eskimo culture developed. Though the Denbigh Complex and later culture stages related to it seem to have extended from Bering Sea to Greenland, it was not entirely, or perhaps even primarily an American phenomenon. Recent reportings by Russian archaeologists have described Mesolithic sites in Siberia containing burins, lamellar flakes, and other stone implements like those found at Denbigh. These Siberian sites do not stand in isolation; rather, they are part of the Eurasiatic Upper Paleolithic-Mesolithic continuum. This suggests that the pre-Eskimo Denbigh Flint Complex as known in Alaska may eventually be revealed as an easterly extension, on American soil, of a widespread Eurasiatic culture of Mesolithic age from which the earliest forms of Eskimo culture were derived (Collins 1954b:298-99).

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similarities among artifacts across the American Arctic at a level of culture prior to the Thule horizon, but the relation of these cultures to later Eskimo cultures was unknown.

The researches in the interior of Alaska and the Yukon by MacNeish and Irving led to the distinction between two types of core and blade technologies, one apparently tied to the boreal forest and the other to the tundra and the sea. As a result of Irving's researches in the Brooks Range (1954, 1955) he

> pointed out some of the differences between early industries of the boreal forest (e.g. the Campus site and Pointed Mountain, N.W.T.) and what he proposes now to call the "arctic small-tool tradition", represented at the Denbigh type site and sites in the Brooks Range (Giddings, 1951; Irving, 1953, 1954) (Irving 1957:47).

However, Irving did not follow through and specify the defining characteristics of this tradition, and it is clear that its definition to Irving was as contrasted with other Western Arctic core and blade sites. The initial categorization of the Arctic Small Tool tradition was the lumping of Denbigh-like sites in Alaska, and did not include any non-Alaskan sites.

Giddings acquired a small collection from the Thyazzi site in northern Manitoba, which he saw as relating directly to his own Denbigh Flint complex in the West, and to sites in Greenland, Alaska, and the Siberian Neolithic (Giddings 1956:266). Giddings was apparently cautioned by negative evidence from creating any larger categories of sites, finding

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sites, finding it ". . .useful to speak of sites as 'burin sites,' 'microblade sites,' and 'side blade sites,' with respect to emphasis on (not merely the presence of) one or more of these distinctive technics" (Giddings 1956:266). At any rate, Giddings demonstrated the extremely widespread distribution of sites with similar artifacts, and hinted at the possibility of tracing diffusion of technical traits through time and space.

MacNeish also filled in a gap in the distribution of these sites by his excavations in the northern Yukon at the Engigstciak site (MacNeish 1956), where his New Mountain complex was seen as related rather closely to Denbigh and similar sites in Alaska as well as to previously discovered sites in the Canadian Arctic and Greenland. "Thus it may well be that Early New Mountain and Irving's Brooks Range material are ancestral to other Arctic micro-tool cultures" (MacNeish 1956:100).

Meldgaard's report on Mosegaard's collections from Sarqaq precipitated a renewed interest in Greenlandic archaeology. Knuth initiated a series of excavations in Northeast Greenland where he at first believed he had found a variant of Dorset culture. His subsequent expeditions produced evidence for two cultures believed distinct from others found in the Eastern Arctic, Independence I and Independence II, with the remains dated to four thousand and three thousand years before present (1958:570). The larger size of the Independence I lithic artifacts, their lack of g

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their lack of grinding, the unusual mid-passage houses, the reliance on musk-ox hunting, and the presence of microblades all indicated differences from other Arctic cultures, while the lack of slate, grinding, and stone lamps differentiated Independence II from Dorset (1958:572-3). Knuth made it clear that he characterized ". . .Independence II--which may have several phases in the northern area--as: pre-Dorset" (Knuth 1958:573).

Larsen and Meldgaard (1958) and Mathiassen (1958) conducted excavations in West Greenland, confirming the existence of the distinctive Sarqaq culture as well as the later Dorset culture. Using microblades as a horizon marker for Dorset, they forced the distinction between Dorset and Sarqaq at numerous sites in Disko Bay, where stratification at Sermermiut was used to confirm this distinction. Radiocarbon dates on these two cultures indicated an age of approximately 3000 years for Sarqaq (Larsen and Meldgaard 1958:40) with Dorset cross-dated by stratigraphy and similarity to other dated sequences at just before 500 A.D. (1958:24).

Harp conducted a survey of the Coronation Gulf littoral in the Central Arctic, where his Dismal 2 complex near Dismal Lake was recognized as relating to both the western Denbigh-like sites and to those of Eastern Canada and Greenland, where Harp saw Dorset developing from a long and complicated cultural continuum of microlithic technology (Harp 1958:247).

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By the late 1950's, the increasing amounts of data from sites believed to have some relationship with the Denbigh Flint complex extended across the entire American Arctic and the stage was set for some attempt at the synthesis of a somewhat more general picture of cultural relationships. While Irving had made an attempt in this direction with his Arctic Small-Tool tradition, it remained for MacNeish to spring into the void with his "A Speculative Framework of Northern North American Prehistory as of April 1959" (1959). While warning the reader ". . .not to believe as facts all that you read herein" (1959:1), MacNeish went on to carve the disparate data from the Arctic into a number of complexes and traditions. Of interest to us is his conception of the Arctic Small Tool tradition, as it marks the first formal statement of its characteristics.

> During the latter part of the development of the Northwest Micro-blade tradition, a new tradition appears on the Arctic coast, called here the Arctic Small Tool tradition (Irving, 1957, page 47, footnote 4). Characteristic of this tradition are burins with chipped surfaces, burin spall tools, cuboid (and conical and tabular) polyhedral cores, micro-blades (usually not retouched), ripple-flaked lenticular, lanceolate and triangular end-blades for arrows (or harpoons), antler foreshafts for arrows, delicate, small neatly chipped half-moon side-blades often with ripple flaking, ovoid, semi-subterranean houses with specialized central fire place (often outlined by boulders), and an economy based on caribou hunting but supplemented by a little sea-mammal hunting. The earliest manifestation of this tradition is the Denbigh Flint complex (Giddings, 1951) at the Iyatayet site on the Seward Peninsula of Alaska. Carbon-14 dates indicate that this is not younger than 4,000

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years ago (Rainey and Ralph, 1959) while sea level datings hint that it probably also is not older than 5,500 years ago (Giddings, personal communication). Recently Giddings found on an old high beach level in the Kotzebue Sound area another manifestation of this culture. It also appeared in the Brooks Range where Solecki (Solecki, 1951), J. Campbell, and Irving (Irving, 1953) have found similar remains. These remains from the Brooks Range I am calling the Itivlik phase. Actually some of these sites found by various archaeologists may likely represent different stages of this single tradition. However, no one has worked this out as yet. On the Firth River, the New Mountain phase (MacNeish, 1956 and 1959), estimated to be about 4,000 years old on the basis of Carbon-14 (Rainey and Ralph, 1959) represents another part of this tradition. Here the Firth River stage with fabric impressed and cord-marked pottery, and the Buckland stage with dentate stamped, grooved and cord-marked pottery, represent still later phases of this tradition (MacNeish 1956 and 1959). In the Coronation Gulf region the Dismal II component (Harp, [1958]) are of the same tradition. At the Alarnerk site near Igloolik, the two earliest stages which might be called Alarnerk I and II (Meldgaard, 1955) dated (Rainey and Ralph, [1959]) as between 3,900 and 3,000 years ago, represent a development within this tradition as do the Independence I (Knuth, 1958) and the Sarqaq remains (Larsen and Meldgaard, 1958; Mathiassen, 1958) of Greenland. The latter has been dated as from 3,500 to 2,500 years ago (Larsen and Meldgaard, 1958). A few artifacts from the Button Point site in the Franklin District (Mathiassen, 1927) and from the Nuvuk site in the Ungava Peninsula (Taylor, personal communication) hint that this tradition also occurred in these regions. The neolithic-type burins, side-blades and projectile points (and ceramics) from the middle Lena (Okladnikov, 1955) and the Yakitikiveem site (Krader, 1952) from the interior of north-eastern Siberia suggest (if the Russian dating is correct) that some of the elements of this tradition were derived from the interior of north-east Asia. The micro-blade industry may have come from the North-west (interior) Micro-blade tradition already in North America, as might the Yuma chipping technique. The tools adapted to

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> marine subsistence may have derived ultimately from the North Pacific tradition, which we will speak of presently. Here again is a case where a series of elements seem to have piled up in North America to form a new cultural tradition, and then moved as a unit across the entire Arctic and persisted in time (MacNeish 1959:8-9).

However rough the boards of MacNeish's construction, he made clear that he was talking in generalities, even with regard to his use of the concept of tradition.

> By tradition I mean a distinct way of life as it is distinguished by different complexes of artifacts or diagnostic traits that persist in time and space. The exact process by which these traditions originate, spread, change, persist, and finally disappear, cannot be determined by the present evidence. Some of these traditions may reflect actual migrations of distinct people with distinct cultures; others may result from the readaptation of a way of life from one ecological zone to another; still other traditions may derive by combination of all the above-mentioned processes as well as many others not mentioned here. Be that as it may, the origin, spread, persistence and disappearance of traditions seem to be a complicated process. However, in spite of this, the concept of tradition seems useful in delineating cultural relationships in time and space in the north (MacNeish 1959:2,4).

MacNeish's use of the tradition concept is first and foremost as a technological tradition which, as he points out, may be characteristic of one or several groups of people. Perhaps his greatest deviation from the concept of tradition developed by the 1955 Seminars in Archaeology (Wauchope 1956) is in his failure to restrict the spatial dimension and emphasize the temporal dimension (Wauchope 1956:38-39).

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distributions of artifacts and sets of artifacts (complexes) assuming that similar artifacts are indicative of similar "cultures" or similar behaviors was characteristic of most of Arctic archaeology during the 1950's and even through the next couple decades. There was a tendency to drift easily from the discussion of specific artifact distributions to the extent of complexes to the migrations of peoples and to the spread of cultures. MacNeish's 1959 paper demonstrates this tendency rather well, as he discusses specific characteristic artifacts, sub-stages of a culture, complexes, phases, cultures, elements, horizons and traditions as well as a culture complex. This terminological imprecision was perhaps indicative of the rather sketchy and widely scattered shreds out of which he was trying to weave his synthesis, but it is characteristic of the times that the major concern of Arctic archaeologists was the construction of a space-time framework using what we might call site-occurrences as the data on the frame. Radiocarbon dating was a newly found tool, and one whose use in scattered areas served as props for cultural chronologies across the Arctic. The "type fossil" or "guide fossil" approach became the accepted technique, where an artifact type dated in one locale was assumed to date at a similar time wherever it occurred. Traditions were formed without much evidence of time depth, and migrations (and rarely diffusions) occurred with some alacrity (Larsen and Meldgaard 1958:71).

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This pattern of thinking in Arctic archaeology has set the groundwork for research undertaken even in the present day. Archaeological problems were still largely confined to the development of a space-time framework for artifacts or artifact sets (however named or described). Chronology building was an adequate research goal.

Perhaps the greatest shift in the use of concepts was in the greater use of a more precise concept of culture, as we became more aware that artifacts were made by people, and that these people formed groups and had characteristic ideas regarding technology and tool making. Even so, much of the research conducted during the last several decades is as if we were studying entities which were spread by life processes and which were perpetuated by genetics (see Dekin 1973a:41 for further explication of this point).

It is almost as if conceptual precision has been deemed unnecessary and luxurious. Fortunately, the renewed concern for theory and method that has been racking North American Archaeology in general is beginning to reach the Arctic, as the recent discussions at a School of American Research Advanced Seminar on Pre-Dorset--Dorset Problems indicated.

Shortly after MacNeish's pioneering attempt to bring a semblance of synthetic order out of prehistoric chaos, two symposia were held and collected papers were edited by Campbell (1962b) and Hadleigh-West (1963). These collections are significant watersheds in the development of our thinking in that they caused the widespread sharing of information and allowed the presentation of up-to-date ideas to a wider audience than existed in the informal communications systems.

Meldgaard reported on his excavations at Igloolik where an extensive series of raised beaches were used in conjunction with a program of radiocarbon dating to produce a chronology of cultural changes in Pre-Dorset culture leading to a marked chang <u>ca</u>. 1000 B.C. when Dorset culture apparently replaced Pre-Dorset (Meldgaard 1962: 95). Meldgaard suggested that this change was caused by the local disappearance of Pre-Dorset people and the migration of new Dorset people stemming from somewhere south of James Bay (1962:95).

Taylor reported the results of excavations at Ivugivik in northern Quebec where three small sites represented a single stage of Pre-Dorset culture (1962:81). Two ground stone artifacts and several flakes of slate were regarded as possible intrusions from a later Dorset occupation (in which similar artifacts were prevalent), once again following a "guide fossil" type approach (Taylor 1962:88). Taylor considered these finds as indicative of placement early in the Pre-Dorset continuum and he remarked on the near-identity in burin forms between his Ivugivik specimens and those of the Denbigh Flint complex as reflecting the extent ". . .of cohesion within the Arctic Small-Tool tradition" (Taylor 1962:89).

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Campbell summarized his extensive excavations at Anaktuvuk Pass in the Brooks Range of Alaska where his Natvakruak complex represented ". . .an inland Denbigh Flint complex manifestation" (1962a:44) agreeing with the earlier interpretations of Solecki and Hackman from limited samples of the same sites. Campbell believed the alternation of several disparate complexes through time in the Anaktuvuk Pass region to ". . .represent separate hunting societies, often having quite separate cultural origins, who gradually expanded their territorial boundaries into the region. . .and who, in each instance, were content to settle in the higher reaches of the range and to exploit its resources, quite probably for generations" (Campbell 1962a:54).

Harp described the results of a survey of the Baker Lake and Thelon River areas of Keewatin where his Phase 2 was a ". . .Pre-Dorset Eskimo culture, derived from the central Arctic" (Harp 1962:72) which Harp suggested dated ca. 1000 B.C. by cross-dating of artifact similarities with Igloolik. There was a notable absence of burins which he suggested resulted from the difficulty in working the predominant material--coarse-grained quartzite. Harp also suggested that

> . . .we cannot yet rule out the possibility that Archaic Indian culture may have contributed something to the Dorset Eskimos through this area. Such diffusion may have developed through the medium of Pre-Dorset culture there, and then have been transmitted to Dorset people who apparently adhered more

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closely to the coast of Hudson Bay (Harp 1962:75).

Irving compared what was then known of Alaskan and Asian stone industries dealing ". . .for the most part, with highly specialized types and modes (Rouse, 1953; 1960), industries, complexes of types, and traditions. At the present stage of work in this area it is seldom profitable to give much attention to whole cultures" (Irving 1962:55). He provided a list of traits characteristic of the Arctic Small Tool tradition.

> Large numbers of microblades struck from conical cores;
> Burins with extensive retouch on one or both faces and prepared for hafting ("tanged burins") of several types;
> Burin spalls retouched for use as minute engraving tools;
> Many, very small, bifacially retouched, inset side blades, less than 4 cm. long, with distinctive crescentic (not rectangular) shapes;
> Many, very small, biface points without stems or notches, but of specialized forms;
> Medium size (4-10 cm. long) biface points and knife blades, without stems or notches;
> Scarcity or absence of implements made by grinding or polishing, and of large implements;
> At most sites, absence of pottery;
> A unique style and technique of tine workmanship, which at most sites appear on most of the implements (Irving 1962:56).

While this list differs slightly from that of MacNeish (see above), there is no doubt that they are referring to the same manifestations and to the same series of sites.

Campbell, in this same volume, mentioned the presence of distinctive societies with distinct cultural traditions at Anaktuvuk Pass, thus attempting to speak of groups of perp

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people with sets of behaviors other than technological. Irving, however, continued the traditionalist approach of sticking with technological studies only rarely considering the implications of these concepts for groups of people.

> A tradition, as the term will be used here, is an aggregate of type complexes which, by virtue of their sharing distinctive artifact types and other distinctive features such as styles of decoration and geographic distributions, give the appearance of having been derived from a common predecessor. Persistence and historical continuity over long periods of time are implied. A tradition is only part of a culture, and it is not necessarily co-terminal in time or in space with a culture. Cultures may exist in which more than one tradition is represented; there may be others which cannot be classified or analyzed in terms of traditions in the present state of knowledge. In this event, it may be possible nevertheless to speak of complexes, that is, of aggregates of types found to recur in a reasonably consistent pattern in several sites of about the same age. "Complex" has much in common with "tradition", but it lacks great time depth and is a smaller taxonomic unit. Industry is understood to mean a specialized manufacturing technique together with implement types and other diagnostic traits associated with it. It may have considerable time depth (Irving 1962:55).

It is obvious that this conceptual framework is designed for the study of artifacts and sets of artifacts and that this conceptual system does <u>not</u> nest within any larger conceptual system designed for the study of people or behavior or of culture. Irving's is a paradigm for the study of <u>artifacts</u>.

The reliance on a set of characteristic traits (guide fossils) for the description and categorization of Arctic

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Small Tool tradition sites led to the establishment of a fairly homogeneous series of sites, especially since unusual artifacts from sites believed on typological grounds to be within the Arctic Small Tool tradition were frequently considered intrusive from other cultures if they did not conform to the trait list (Irving 1964:143-148,294; Taylor 1962:88; Larsen and Meldgaard 1958:40; Campbell 1962a:44; Harp 1958:227).

While most Arctic archaeologists recognized that not all sites which they would include within the Arctic Small Tool tradition would fit exactly the trait lists of MacNeish or Irving, they were reluctant to include within their sample any artifacts that occurred in small numbers and were different from the trait list. Ground stone artifacts in Pre-Dorset, microblades in Sarqaq, adzes in Denbigh at Iyatayet, polished burins and more crudely made end blades at Punyik Point, and large crude quartzite bifaces in Dismal-2 are all examples of problematic interpretations that were once typologically eliminated from the complexes under discussion but which now seem as if they could be included as easily as a priori excluded--perhaps more easily.

The tendency to compare the distributions of specific artifacts is nowhere more obvious than in Hadleigh-West's symposium volume in the Anthropological Papers of the University of Alaska. While the Arctic Small Tool tradition should not be expected to rate much coverage in a volume devo

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volume devoted to "Early Man in the Western American Arctic", several papers discuss it in passing. Bandi discusses burins in the Eskimo area, using data from Arctic Small Tool tradition sites across the Arctic, but his observations have proved of little use and merit little attention (Bandi 1963). His definitions of ordinary, retouched and pseudo burins are virtually useless in understanding the role of burins in Arctic technologies.

Gidding's paper on Arctic spear points continues to compare specific artifacts of similar attributes from wide ranging and probably otherwise unconnected cultural complexes, in particular with those of the Great Plains (1963: 11). He justifies this approach by stating: "Yet if we are going to compare sites of the Arctic with those of distant and warmer parts of the world, we shall have to do so on the basis of a few wide-ranging styles, rather than whole complexes of culture, for the Eskimos of the tundras were never the Sioux of the Plains" (1963:1). Harp's warning regarding the use of attributes and typologies developed for ". . .the analysis of ancient complexes far to the south" (1958:242) in the Arctic was unfortunately not heeded.

Lowther filled a major gap in our knowledge of the distribution of Pre-Dorset sites with his excavations at Cape Sparbo on Devon Island (1962). The majority of the artifacts were "typically" Pre-Dorset with several possibly intrusive from a later Dorset occupation in the area, and

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the entire collection was very different from that excavated by Knuth in northern Greenland. Lowther concluded: "Thus the material from. . .Cape Sparbo is principally pre-Dorset, Arctic Small Tool, with some that may be of a Dorset character" (1962:14).

Maxwell surveyed several areas southeast of Lake Harbour on southern Baffin Island, finding eight sites all of which he categorized as part of the Arctic Small Tool tradition (1962:36). The sites appeared to ". . .demonstrate an unbroken cultural continuum from a Cape Denbigh-like pre-Dorset period to the beginning of Dorset culture" (1962:39), linking Dorset culture to the Arctic Small Tool tradition.

Rousselière reported the results of a brief reconnaissance in the vicinity of the Pelly Bay Mission in 1964 where his Kugarjuk IV and St. Mary's Hill sites were considered to be traces of a Paleo-Eskimo occupation older than Dorset, and fitting ". . .generally within the Arctic Small Tool tradition" (Rousselière 1964:181). While he observed two different types of houses at these sites he was unable to relate these differences to other data. Subsequent work at St. Mary's Hill revealed a significant distribution of artifacts in relation to the house plan, but this enigmatic end note has never been expanded in print.

Irving completed a major analysis of Arctic Small Tool tradition complexes at Punyik Point near Howard Pass in the Brooks Range in 1964, where he elaborated on the conception of the Arctic

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of the Arctic Small Tool tradition (1964:290). Given his normative approach and the lack of distinctive stratification at Punyik, it is not surprising that he saw much evidence for mixing of artifacts produced by several complexes, most of which were related to the Arctic Small Tool tradition.

> There are also many cultures yet to be found in this area, indicated now by only an occasional specimen typologically different from anything in the complexes described above. In particular, several varieties of microblade and large-blade-and-burin-industry are definitely present, but have not yet been isolated in sites (1964:22).
>
> . . .the Punyik complex as a whole is enough like the Denbigh Flint Complex at Cape Krusenstern and Cape Denbigh so that we may assume that they are closely comparable in age, that is, both date from around 3000 B.C. But a few rare types in the Punyik complex, such as ground burins and small, triangular points, compare closely with implements characteristic of the 2nd and 1st millennia B.C. in the Central Area and Greenland. There probably is a simple explanation for this, but as indicated in Chapter VII it is not now readily apparent (1964:320).

Irving's suggestion that many of the finely worked artifacts are the result of the work of a group of craftsmen specializing in stoneworking (1964:325) is one of the first attempts in the study of this tradition to infer cultural characteristics other than technological, and his further suggestion of extensive trade networks involving both raw materials and finished artifacts ". . .provides a partial explanation of the high degree of similarity throughout Alaskan sites of the Arctic Small Tool tradition; it also suggests that change within the tradition of stone implement manufacture may well have happened somewhat independent of

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independent of change in other aspects of culture, and further that there may have been regional cultures whose distinctive character is partially obscured by uniformity imposed on the only preserved relics by a sub-culture of artisans" (1964:327).

However, it is also possible that the methods of analysis employed by Irving and others may also have obscured the distinctive character of regional cultures by imposing uniformity that resulted from a normative approach to the study of this tradition. It is indeed unfortunate that more students of Arctic cultures have not followed Irving's lead to test hypotheses regarding other behaviors of people characterized by the Arctic Small Tool tradition. It is also unfortunate that the development of these ideas has never been published in a more accessible form.

Dumond conducted an extremely extensive series of excavations across the base of the Alaska Peninsula, where his Brooks River Gravels Phase dates ca. 1900-1000 B.C. and contains a small biface and burin-microblade industry with ". . .an obvious affiliation with the Arctic Small Tool tradition, the nearest exemplar of which is the Denbigh Flint complex" (Dumond 1971:40). Ground stone burin-like tools and adzes are found, and while

> . . .the B.R. Gravels assemblage includes many more snub-nosed end scrapers and many fewer burins and microblades than the type collection of Cape Denbigh, the resemblance between the collections is unmistakable. These people may be considered the first speakers of Eskimoan to enter the region (see Dumond 1965, 1969a, with additional references) (Dumond 1971:40).

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These sites mark the southwestern-most extension of the Arctic Small Tool tradition, which apparently laid the cultural horizon for the heritage of subsequent "Eskimo" cultural development, as Dumond saw continuities between B.R. Gravels and subsequent Norton-like phases (1971:40-41).

Dumond has suggested quite convincingly that the combination of ethnographic, linguistic and archaeologic evidence supports the hypothesis that the people who produced the Arctic Small Tool tradition spoke Eskimoan dialects (Dumond 1965) and were thus the first Eskimo culture across a large expanse of the American Arctic. If this hypothesis is appropriate, and it has received general acceptance and support, then it lends support to the use of ethnographic data on contemporary and historic Eskimo hunters as analogues for the development of models and hypotheses regarding the behaviors of the people of the Arctic Small Tool tradition.

The similarities between his Brooks River Gravels assemblage and that from the Closure site are truly remarkable, considering the geographic distance which separates them (nearly three thousand miles as the crow flies, but much more as the Eskimo walks, paddles, or whatever).

Alexander excavated three chipping stations and a campsite in the Atigun Valley 70 miles west of Anaktuvuk Pass in the Brooks Range of Alaska, lumping them into the Itivlik Phase (following MacNeish 1959:14) of the Arctic Small Tool tradition (Alexander 1969:51). While the greatest simi

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greatest similarities were with four chipping stations in the Shubelik Mountain region northwest of the Atigun Valley (excavated by Solecki; Solecki et al. 1973) because of the lack of side grinding on burins, he estimated the age of the Itivlik Phase as coeval with the Onion Portage Denbigh Flint Complex occupation of ca. 2200 B.C. (see below; Alexander 1969:51-52). His sample size of 26 artifacts and 187 flakes underscores the tentative nature of his conclusions.

The Utukok-Denbigh Complex was excavated by Humphrey on the upper reaches of the Utukok River north of the Brooks Range in Alaska where two sites (56 artifacts) are typically Arctic Small Tool, except that they are found fully on the tundra and they include no microblades (Humphrey 1970:120). Humphrey points out that the location of the sites almost

> . . .assuredly reflects an inland big-game hunting subsistence for Arctic Small Tool peoples in this area, for neither fishing nor gathering would provide sufficient food. Perhaps this different ecological situation could provide a functional explanation for the lack of microblades in the assemblage (1970:120).

Giddings has probably excavated or collected from more sites of the Arctic Small Tool tradition than any other Arctic archaeologist, beginning with the Iyatayet site at Cape Denbigh (the "type" site; see above) and continuing at Cape Prince of Wales and Cape Espenberg (unpublished; 1966) and Cape Krusenstern (1961, 1966) where his beach ridge chronology wa

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chronology was generally corroborated by his subsequent and highly successful excavations at Onion Portage, on the Kobuk River (1962, 1965, 1966, 1967).

Gidding's 1964 report on the Iyatayet excavations is the most complete and lavishly reproduced report we have received on an Arctic archaeological site, summarizing the data for several occupations including the Denbigh Flint complex. However, it is a disappointment in that it does not contain a single reference to the Arctic Small Tool tradition or to Paleo-Eskimo as generalizing concepts. It is apparent that these idiosyncracies on the part of Giddings reflect his attitudes and maybe even his disappointment in finding that the Denbigh Flint Complex was not accepted as resulting from "Early Man." While his data on chipped stone artifacts are baselines for comparison as are his excellent illustrations (both line drawings and photographs), his failure to consider adequately the extent of grinding on burins and adzes has reduced the utility of his studies. Again, his use of a normative approach to his data has resulted in the exclusion of infrequent traits at Iyatayet from the careful analysis that characterizes the remainder of his research.

> The Denbigh Flint complex may be summarized speculatively, then, as representing people who visited Iyatayet only seasonally for sealing and caribou hunting, bringing with them the raw flints needed for temporary manufactures, foregoing the heavy work that one might expect around more permanent camps, and moving into the forest for the winter season. Skilled enough at boating to procure seal in quantity and to live along a

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> very wide stretch of the seacoast of western Alaska, they may also have possessed the excellent snowshoes without which life in the dry, wind-free forests would be unthinkable, and they probably concentrated on caribou as a principal food source throughout most of the year. As to whether or not they were Eskimos, we can answer only by defining "Eskimo" more closely than we are accustomed to do. There is scarcely a Denbigh object in the same form used by Eskimos of Thule or later cultures, yet some continuities into the Ipiutak period of about 2000 years ago are quite direct. Regardless of how we designate them, these Denbigh people appear to be in a direct line of cultural continuity with Eskimos.
>
> Technically, the Denbigh Flint complex is more closely related to the European Paleolithic (in burin techniques and variety, certain scrapers, and miscellaneous rare forms), the Mesolithic of Europe, and the early "Neolithic" of Siberia (in microblade industry and inset side-blading) than to early temperate America; yet fluted and diagonally flaked points and expert bifacing of small flints are old in America. The complex was unique in the world, however, in the meticulous skill employed in flint flaking, and probably in the origination of the burin spall artifact, the most minuscule of the widely used flint implements (Giddings 1964:242-243).

Gidding's treatment of the Eastern Arctic is indicative of his unwillingness to see Denbigh as part of any larger cultural system.

> Thus, moving from the Bering Strait region around the coast of Alaska and Canada to Greenland, one finds clear-cut evidence of a contiuity of the Denbigh Flint complex--in a "small tool" horizon--the entire distance, though with a time lag of 1000 or 2000 years between sites of Denbigh Flint complex of western Alaska and Independence I in Greenland. Whether or not there proves to be a still earlier Greenland manifestation, the derivation of Independence I from a Denbigh-like base is clear. As I interpret the data, a Sarqaq-like cast of culture in the east resembles the Choris and pre-Choris casts in the west enough to suggest a second and following continuity across the Arctic (Giddings 1964:261).

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Giddings has thus provided much substantive data from his researches, but he has occasionally (in this instance) neglected to relate them to hypotheses and concepts produced by other researchers, especially when they are in conflict with his own interpretations.

In a footnote to his 1966 summation of the cultural sequences at Cape Krusenstern and Onion Portage, Giddings explained some of his reservations regarding the concept of Arctic Small Tool tradition.

> I do not subscribe to the term "Arctic small tool tradition". . .for these reasons. First, I prefer to use the term "tradition" in a more limited geographical sense to describe transmission upward through time. Second, we must not yet rule out the possibility of a rapid spread of Denbigh culture in essentially a horizon. And, third, the use of "small tool tradition" by several authors has been confused. . . . This does not mean, however, that Sarqaq and Dorset cultures of the eastern part of the continent have not descended directly from the older, Denbigh-like base in the same area. Recent excavations by H. B. Collins, W. E. Taylor, J. Meldgaard, E. Harp, M. Maxwell, G. M. Rousseliere, T. Mathiassen, H. Larsen, and others clearly show that they have (Giddings 1966:Footnote 7, p. 135).

As I will elaborate below, I find the data supportive of a horizon stretching from Alaska to Greenland ca. 2000 B.C., but the differences in assemblages support the idea that Denbigh is but one variant of a somewhat larger system of related assemblages and peoples, which we can usefully call the Arctic Small Tool horizon and tradition.

It is indeed unfortunate that Giddings was unable to bring his project at Onion Portage to complete fruition, as his death

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as his death in 1964 cut short an exemplary career. Fortunately, his researches have been continued by Elizabeth Giddings and Douglas Anderson, whose own contributions to our understanding of the Denbigh Flint complex development at Onion Portage are vital to the development of this thesis. The stratification at Onion Portage revealed a developmental sequence leading from the somewhat larger and cruder Proto-Denbigh assemblage to a Classic Denbigh assemblage with the finer workmanship and edge serration characteristic of Denbigh at Iyatayet and other sites (Anderson 1968a, 1970).

The dates on the Denbigh Flint complex levels at Onion Portage range from _ca_. 1700 B.C. to _ca_. 2000 B.C. and the Proto-Denbigh levels, while not dated satisfactorily, are bracketed by these dates and those from Band 5 below which average _ca_. 2350 B.C., thus Anderson's approximation of _ca_. 2300 B.C. for Proto-Denbigh seems appropriate (Anderson 1970:10), if not slightly old.

Anderson's listing of traits defining the Denbigh and Proto-Denbigh periods is the most recent assessment of the nature of these assemblages, and bears quoting extensively for comparison with previous trait-lists (see Irving, above, for example) and as a statement of the technological changes occurring at Onion Portage and elsewhere.

Proto-Denbigh

1. Wider microblades* mainly with wide rounded distal ends (mean: 7.7-8.3)*(Fig. 4:13-22).
2. Irregular flaking of insets* (Fig. 4:3-5,7-9).

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3. Few end blade insets relative to side blade insets.
4. No fine serration.
5. Shouldered steep-edged unifacial flake knives with spatulate distal ends (Anderson 1968:29, Fig. III-H).
6. Tanged end scrapers, with asymmetrical lateral beaks (Fig. 4:6).
7. Side blade insets relatively wide and short (Fig. 4:7,10).
8. Elongate semi-lunar biface knife blades (Fig. 4:26).
9. Thicker spear points (Anderson 1968:29, Fig. III-B).
10. Large (10 cm + in length) massive knife bifaces present.
11. Burins are all unpolished (Fig. 4:1,2).
12. Notched net sinkers.*
13. Large utilized flakes and thin uniface flake knives abundant (Fig. 4:23).
14. End-of-blade end scrapers present.
15. Oval-platformed microscores,* in addition to the flat-faceted cuboid microcores.
16. Whetstone or needle sharpener of soft pumice (Fig. 4:24).
17. Rectangular house plan with rounded corners (1 example).

(*means also present in Lower Bench Denbigh at Cape Krusenstern--Anderson 1970:11)

<u>Classic</u> <u>Denbigh</u> (Giddings 1964; Anderson 1968a, 1968b)

1. Narrower microblades, often with pointed distal ends (means: O.P. 6.8-7.3, C.K. 6.3-6.8, Iy. 6.9).
2. Oblique parallel flaking common on insets.
3. More end blade insets than side blade insets.
4. Fine serration of end blade insets common.
5. Shouldered steep-edged flake knives with pointed distal ends.
6. No tanged end scrapers.
7. Side blade insets longer and narrower.
8. Elongate semi-lunar bifaces absent.
9. Thinner spear points.
10. Massive knife bifaces absent.
11. A small percentage of polished burins.
12. No stone net sinkers.
13. Similar flake knives rare.
14. End-of-blade end scrapers absent (not to be confused with end-of-microblade end scrapers).
15. Flat-faceted cuboid and "wedge-shaped" microscores.

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16. Needle sharpeners of visicular basalt.
17. Round house plans (5 examples).
    (Anderson 1970:11)

The trends seem to be in the diminution of artifact size, in the increased technical competence of the artisan (see Irving's Arctic Small tool technique, 1964) (as evidenced by parallel flaking, edge serration, thinner spear points, smaller microblades, and longer and narrower side blade insets in Classic Denbigh), and in the development of polish on burins (Anderson 1970:11). As we will see below, the trajectory of these trends is important to our understanding of diversity and change within the Arctic Small Tool tradition.

Anderson cautions us not to minimize the similarities between Proto-Denbigh and Classic Denbigh which make these complexes more like each other than like any other finds in the Arctic region, which is what we would expect from a sequence of developmental stages within the same complex and tradition.

Anderson's listing of the significant similarities is as follows.

1. Double-tanged end blade insets (Anderson 1968: 29, Fig. III-C).
2. Presence of end and side blade insets (Giddings 1964).
3. Extensive use of gray chert.
4. Cuboid microcores (Giddings 1966:Fig. 2a).
5. Burins and burin spall artifacts of same style (Giddings 1964).
6. Steep-edged flake knives (Giddings 1964: pl. 69).
7. Thumbnail end scrapers (Giddings 1964: pl. 70b, 4).

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8. Triangular end scrapers with ventral retouching in bulbar region of ventral face (Giddings 1964).

9. Rejuvenation of faceted end by a longitudinal blow rare.
10. Microblades with same range of wear patterns. (Anderson 1970:11)

While Anderson demonstrates no reluctance to utilize the concept of Arctic Small Tool tradition as a tradition in which to include the several developmental stages of the Denbigh Flint complex, he has not utilized the insights of his researches at Onion Portage and elsewhere (see Anderson 1972) for a reevaluation of the utility of more general concepts. He concluded that:

> In addition, until our excavations of Proto-Denbigh at Onion Portage, it seemed that the Denbigh Flint complex appeared suddenly in the New World. We can now demonstrate that there are earlier phases of the complex in America, and they probably precede the last major sea level stillstand.
>
> On the other hand, some major gaps remain to be filled. Because of the rising sea level before 4500 B.P., much evidence of the early beginnings of the coastal and tundra aspects of Denbigh is now under water. There are, however, non-Campus-type microblade and core assemblages in the Brooks Range which are likely later than the American Paleo-Arctic tradition, yet earlier than the Arctic Small Tool tradition periods. Thus the wide temporal gap between the traditions may be more apparent than real. Despite the great differences between specific techniques in microblade and core manufacture of Denbigh and the American Paleo-Arctic tradition, the traditions may represent two different periods of an uninterrupted succession of microblade-using peoples from 10,000 years ago to about 1500 years ago. If so, then we have evidence of a line of cultural continuity in the American Arctic that spans the last 10,000 years (Anderson 1970:15).

While the "suddenness" of the appearance of the Denbigh Flint complex as a part of the Arctic Small Tool tradition may be a relative phenomenon, I would argue that the approximate

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approximately four hundred year time span of the Arctic Small Tool horizon (spreading from the Alaska Peninsula to Greenland) is a sudden phenomenon when viewed in the light of the at least eleven thousand year span of Arctic prehistory. Further, I would view with caution any attempt to make meaningful cultural units based on microblades and cores as it seems obvious that they are restricted neither spatially nor temporally in northern regions of both Asia and America. Any fabric of cultural affinities built on such tenuous threads is likely to be of little use, even as a heuristic device.

Anderson also reported an important series of surveys conducted during four field seasons along the Noatak River in the southwestern Brooks Range of Alaska (Anderson 1972). Notably, his NR-1 site has only 1 microblade from a small sample of 28, but the Denbigh Flint complex sites appear to be late in the developmental sequence for northern Alaska, and to relate to a developmental transition between Classic Denbigh and Chrois.

> A fourth occupation, represented by NR-1, combines both Denbigh Flint and Choris elements. Included in this assemblage are burins, both polished and unpolished, flakes with burin blows, tiny side blade insets, a regularly flaked thin biface point with a straight or perhaps a single shouldered base, an adze blade, linear stamped pottery, a whetstone and hammerstone, and utilized flakes. No microblades, with one possible exception, were present. This assemblage may represent a transition phase between Denbigh Flint and Choris complexes and if so, would probably date to between 1500 and 1200 B.C. (Anderson 1972:99).

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This sequence is corroborated by the mixed samples at Engigstciak, Irving's Punyik Point, possibly the Trail Creek Caves (Larsen 1968:78), and Onion Portage.

Excavations by Dennis Stanford at Point Barrow have revealed a similar late stage of Denbigh with pottery, which Stanford has called Walakpa Denbigh. According to McGhee (1971:491) "The Walakpa Denbigh inventory includes cord-marked pottery, ground slate, burins, microblades and other typical Denbigh forms" and is dated by two dates: 3400±520 years (ca. 1450 B.C.--Gak-2299) and 2260±300 years (ca. 310 B.C.--Gak-2300) of which Stanford prefers the earlier (McGhee 1971a:491). Note that the difference between these two dates would occur by chance from the same population less than once in twenty samples (p=ca. 0.05 by chance), thus either the occupation was for a long duration, the samples are in poor association, or there is possible contamination of one or both dated samples. Other sites were found in the same locale, including several Classic Denbigh sites. These Denbigh sites on the northern coast should provide important evidence for the adaptive diversity of Denbigh, as they are rare along the Alaskan coast. Presumably, their coastal location is suggestive of a rather late date in the continuum of the Denbigh Flint complex.

Hall conducted an extensive series of excavations and surveys at Tukuto Lake, approximately 140 miles west of Anaktuvuk Pass in the central Brooks Range. His ca. 200

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artifacts from Arctic Small Tool tradition sites are as yet unreported except for his preliminary report (1970). Hopefully the precision with which his report indicates that he excavated will be reflected in the final report on these materials. McGhee's report on Arctic research also indicates that some of these artifacts were associated with "thin, hard-fired pottery" (McGhee 1971a:491), thus we may expect additional information on the later phases or the Denbigh Flint complex from these researches.

In 1961, Ralph Solecki conducted an archaeological survey of two areas North of the Brooks Range in the far northeast corner of Alaska. Here, four sites contained materials relating to the Arctic Small Tool tradition, but their extremely small sample size forces reliance on "index fossils" such as chipped burins, burin spalls, and the use of the Arctic Small Tool technique of parallel flaking (Irving 1964:325) (note that Solecki relates the diagonal parallel flaking to Angostura techniques, citing a personal confirmation by MacNeish--Solecki et al. 1973:88). None of the burins is mentioned as having grinding facets, although the entire sample from four sites is 9 burins and 9 burin spalls. Solecki infers that this sample of sites ". . .is simply the representative record of a seasonal hunting ground, or a part culture" (Solecki et al. 1973:88).

Since its inception in the early 1950's the Arctic Small Tool tradition and its component sites have not been subjected to a major synthesis, although Irving's 1964 paper is an

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paper is an approximation. Almost everyone doing archaeological survey work in Alaska has found some Arctic Small Tool tradition sites, but they have not received priority attention as they did not suit the problems that these researchers were pursuing. Even as this thesis is an attempt to review and revise our conceptions on the eastern extension of the Arctic Small Tool tradition, there is a need for such an intensive study of the Alaskan representatives. There is a further need for those with small Arctic Small Tool collections to publish them, or at least make them available to other researchers, as there are really a lot of sites located, tested and even excavated, but the amount of information in print is woefully inadequate for anything but a cursory analysis. The data are there for analyses going beyond the study of technology to the study of the human behaviors of these tool-makers, but this will require a major effort by some researcher and great cooperation from Arctic archaeologists.

Turning to the Eastern Arctic, Taylor reported on his excavations of the Roberts and Arnapik sites on Mansel Island in northeastern Hudson Bay where his nearly 1900 specimens (1791 artifacts) make Arnapik one of the largest Pre-Dorset samples reported in print (Taylor 1968a:15) (see Taylor 1968b for a photograph of some Roberts artifacts). The site comprised some 120 find spots predominantly characterized by a scatter of somewhat larger rocks than the usual surface scatter with artifacts found on the sur-

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face and within the rock matrix. These rock and artifact clusters extended over approximately 1200 yards along a headland at ca. 90 feet above present sea level (Taylor 1968a:12-14). Samples were recorded and kept separate from each cluster, but analysis revealed no consistent differentiation by area. Because of its elevation and relations with other radiocarbon dated Eastern Arctic sites (comparing frequency of polish on burins, popularity of burins and burin spalls, and specific artifact forms), Taylor estimates the occupation as dating between 1500 and 1000 B.C. (1968a:43). While specific comparisons with the Closure site and other Pre-Dorset sites in the Lake Harbour region will be made in other portions of this thesis, it is interesting to note at this time that no soapstone was found at the site and that the proportion of burin spalls is among the smallest from any burin producing site in the Arctic. This anomaly may have resulted from a combination of natural and technical factors, as the freeze-thaw cycle in the Arctic can lead to differential sorting of lithics by size, with smaller sizes descending and larger ones rising (see Corte 1963:499), thus imbedding the tiny burin spalls deeper in the surface gravels under investigation here. Additionally, Taylor explains that "The crew was small and generally unaware of the nature of archaeological field work. Nor were we blessed with good weather and an abundance of time. Consequently I restricted our activities on the site to surface collecting and recording"

Table 1

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Alexander	1
Humphrey	1
Giddings	1
Anderson	1
Stanford	U
Hall	U
Solecki et al.	1
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Table 1 Summary of ASTt Field Research: Western Arctic

Author	Publication Dates	Map Key	Location or Sites Excavated
Giddings	1950,51,64	1	Cape Denbigh, Alaska Iyatayet
Giddings	Unpublished	2	Cape Espenberg, Alaska
Giddings	Unpublished	3	Cape Prince of Wales, Alaska
Giddings	1960,61,66	4	Cape Krusenstern, Alaska
Solecki	1951	5	Anaktuvuk Pass, Alaska Natvakruak
Solecki and Hackman	1951	5	Anaktuvuk Pass, Alaska Natvakruak
Irving	1951,53,55	5	Anaktuvuk Pass, Alaska Imaigenik
Campbell	1962a	5	Anaktuvuk Pass, Alaska
Irving	1951	6	Susitna Valley, Alaska
MacNeish	1956,59,62	7	Firth River, Yukon Engigstciak
Irving	1962,64	8	Itivlik Lake, Alaska Punyik Point
Dumond	1964,65,69,71	9	Alaska Peninsula, Naknek River. Brooks River Gravels
Alexander	1969	10	Atigun Valley, Alaska Itivlik Complex
Humphrey	1970	11	Utukok River, Alaska Utukok Denbigh Complex
Giddings	1965,66	12	Onion Portage, Kobuk River Denbigh Flint Complex
Anderson	1968a,70	12	Onion Portage, Kobuk River Denbigh Flint Complex Proto-Denbigh
Stanford	Unpublished	13	Point Barrow, Alaska Walakpa Denbigh
Hall	Unpublished	14	Tukuto Lake, Alaska Arctic Small Tool tradition
Solecki et al.	1973	15	Sadlerochit-Shubelik Mtns. Denbigh Flint Complex
Solecki et al.	1973	16	Franklin Bluffs Denbigh Flint Complex
Anderson	1972	17	Noatak River, Alaska Denbigh Flint Complex

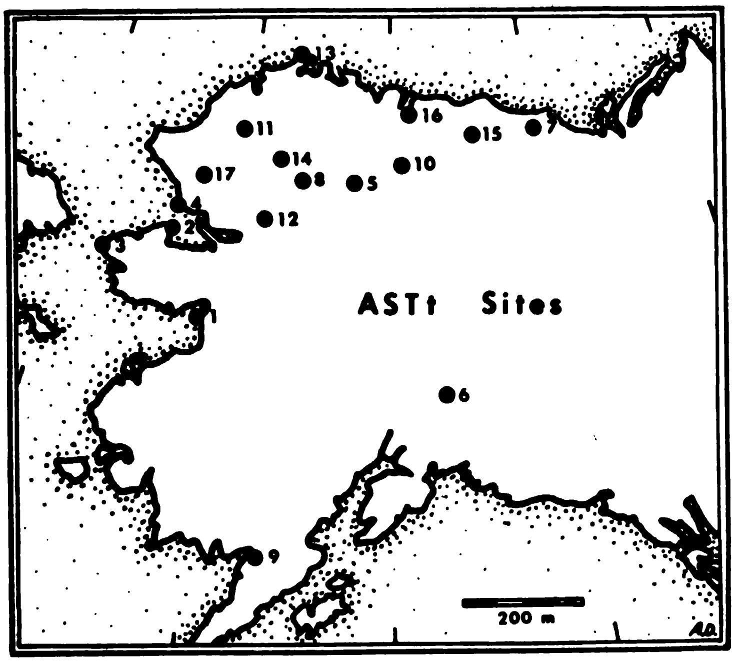

Figure 1 ASTt Sites from Table 1

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(Taylor 1968a:15). The sampling techniques and the environment may have combined to produce fewer burin spalls than we might expect from this site.

Taylor's research here, and at the Tyara site reported in the same volume, virtually nailed the lid on the argument that Dorset developed rather directly from the Pre-Dorset in an area which included the Hudson Straits region of the Eastern Arctic, and his summation of research through 1966 is extremely useful as a guide to the literature. In addition, he seriated the Sarqaq and Dorset sites reported by Larsen and Meldgaard (1958) from Disko Bay, West Greenland, where he suggested there had been a period ". . .of more rapid cultural change near 500 B.C." (Taylor 1968a:91) but that the ". . .Disko Bay Dorset developed in large part from the preceding Pre-Dorset Sarqaq stage without any appreciable break in the occupation of the region at that time. Since the late Sarqaq of Disko Bay seems contemporary with the earliest recognized Dorset occupations in the Canadian Arctic one may suggest for consideration that, as the Sarqaq-Dorset transition occurred slightly later around Disko Bay, the stimulus or causes for the change derived in some measure from the Canadian Arctic" (Taylor 1968a:93). This hypothesis is almost diametrically opposed to the previous interpretations by the Danish archaeologists of a migration of Dorset people into a virtually depopulated area (see above for their earlier discussions of migrations into Greenland).

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Knuth summarized his interpretations of the results of his extensive series of excavations in northern Greenland and adjacent Ellesmere Island, where his Independence I and II occupations followed the "musk-ox way" (Knuth 1967). The general picture of these high Arctic peoples is fairly clear, with a reliance on musk-ox and seal hunting, supplemented with other land mammals. Caribou and walrus were conspicuous by their absence. Their technology was most like the Denbigh Flint complex, than like other assemblages in Greenland and the Eastern Arctic, characterized in particular by a lack of grinding on any sort of stone tool, larger burins, double tapered end blades, microblades, and no soapstone. While specific comparisons will be made below, the early radiocarbon dates have led some to regard these as the earliest migrants into the Eastern Arctic, possibly coeval with or earlier than Classic Denbigh in Alaska. Knuth approximates dates of ca. 2000 B.C. for Independence I with a stage transition to Independence II at about 1440 B.C. and Independence II at ca. 640 B.C. (Knuth 1967:26). Knuth emphasized that cultural changes involving Pre-Dorset and Dorset cultures in other areas may only have weakly reached these remote areas, where the people ". . .were forced to maintain certain basic principles in their high-arctic economy" (Knuth 1967:40).

However, there are also some aspects of Knuth's methodology which may have contributed to his considerations and conclusions. The image that Knuth has portrayed and published is one of an Independence I house form involving central passages (the mid-passage hearth), but his report indicates that this feature was found in only 30% of the cases (47 ruins) of which 40 were excavated which is a much higher portion than was excavated of the ruins without mid-passages. The more extensive sample of mid-passage ruins may have biased the sample in favor of representing whatever factors lead to the creation of the different house types, so that the picture which Knuth paints may be biased towards the unique features of the Independence culture. His conclusions are somewhat difficult to substantiate from the data presented, as the "big picture" is well documented, but the artifacts and their distributions are slighted, even to the extent that his single figure of stone artifacts has appeared in print before, and so we are in the dark as to the diversity and characterization of his lithic artifacts--the ones which have the greatest affinity with those from Alaska.

However, his series of well-conducted and truly remarkable excavations under extreme environmental conditions are only to be lauded, as is his use of corroborative data from other scientific researches in this region. Whether his Independence cultures represent regional ecological variants of the Arctic Small Tool tradition, or evidence for the earliest of several migrations across the Eastern Arctic remains a question for later in this paper.

Maxwell continued his researches near Frobisher Bay and Lake Ha

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and Lake Harbour on southern Baffin Island where he established a long sequence of continuous occupations stretching from the Pre-Dorset Closure site (ca. 2000 B.C.) through a transitional period to Dorset culture (1100-700 B.C.) and fully developed Dorset lasting at least through 500-600 A.D., (Maxwell 1973a:287-88,351; 1967). His excavations are among the most extensive (and intensive) in the Eastern Arctic and his combination of trait seriation, sea level, and radiocarbon chronologies of development of Pre-Dorset--Dorset is the most completely documented and published in the Eastern Arctic (Maxwell 1973a). Maxwell's conclusions emphasized the technological continuity (with relatively slow technological evolution and stylistic variation) throughout the sequence of occupations, emphasizing the evidence for the in situ evolution of Dorset culture within an area of which the Lake Harbour sites were a part (1967).

> A second general point of interpretation to be derived from the Lake Harbour sequence (and augmented by information from the whole pre-Dorset-Dorset geographic range) is that this was essentially, if not completely, a closed cultural system. Once the Arctic Small Tool Tradition arrived on the coast of the Eastern Arctic it became the tool inventory base for all subsequent developments. At least in the Lake Harbour region, there is no development in material culture (and as yet no discernible development in non-material culture) throughout the continuum that cannot be accounted for in the indigenous system. Each specific tool can be traced through a developmental sequence to its prototype in the earliest sites (Maxwell 1973a:343).

Maxwell has long been impressed by the implications of this almost th

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almost three-thousand year continuum of steady-state development (personal communications) as it is manifest of an unusually conservative technological system.

> The interpretation this evidence leads me to is that from the very beginning the average tool kit of the average man functioned sufficiently well to allow him to maintain an efficient position of equilibrium within his ecosystem. Over time there were minor attempts to modify the forms of certain tools, presumably in the direction of greater efficiency. But there does not seem to be any compulsion toward change, and in fact the very changelessness of the tool kit suggests ideational sanctions against major change. Such minor stylistic drift as does occur, more often than not returns to the original starting point, reminding one of Kroeber's famous study of oscillating hemlines on ladies' dresses. In ecological terms, in this particular continuum, the cultural, or extra-corporeal adjunctive aspects of man's adaptive equipment became a constant--comparable to the fangs and claws of the polar bear -- so that he remained at a climax state in a condition of dynamic equilibrium as the top carnivore in a virtually unchanging ecosystem. In other words, seeing culture as an adaptive mechanism, the adaptation once evolved did not have to be modified, and there appear to have been selective pressures to maintain a changeless technology. It seems to me that this provides a subtle but significant difference in the way that culture is often viewed. Perhaps it is because culture history has provided few examples of a steady state maintained over several millenia, or because, from a platform of ethnocentric progression, we see the adaptive aspects of culture as providing increasingly efficient ways of processing resources. But certainly in the technological aspects of culture we are more apt to think in terms of progression or regression rather than in terms of steady states. The parallel here, in evolutionary terms, is the specialized rather than generalized animal, although this comparison lacks aptness from the very fact that this average tool kit could be used to exploit a finite but varied complex of land and sea animals. It is of this type of steady-state adaptation that Charles Cleland (Cleland:1967 [1966]) is speaking when

> he refers to adaptation characterizing societies with focal, as distinct from diffuse, economies.
>
> This ecological equilibrium is even more impressive when we consider it in the matrix of climatic change. The three millenia continuum we are considering here bridges a number of climatic shifts which have had major importance elsewhere in the world. To date, insufficient research has been carried on in the Arctic to suggest the impact of these changes in northern latitudes, but there is little reason to believe that climate has been at a constant state in this region through this period. . . .Presumably, however these shifts in temperature were not of such magnitude to cause dramatic shifts in the ecosystem (Maxwell 1973a:341-343).

These conclusions come from data which include the presence of ground slate adzes and burin-like tools in the earliest Pre-Dorset and true-burins in the latest Dorset (to name only two). An alternative explanation for the extent of cultural homogeneity through <u>both</u> space and time is that there was considerable mixing of component samples by the re-occupation of sites and by the use of cut-sod from adjacent sites (both older and younger) for house construction which effectively mixed components from vastly different time periods. However, few would argue that this would account for mixing of the very earliest and the very latest components, and mixing would most likely occur in adjacent sites, which Maxwell's radiocarbon dating program suggests are usually also the closest in time (see sites in the Tanfield Valley). Additionally, this continuity may be the result of one of the most complete diachronous samples from a single locale in the Eastern Arctic. Discontinuity elsewhere may be because the archaeological sample is

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discontinuous--the opposite of the situation near Lake Harbour.

While Maxwell's analytic approach is essentially typological (see 1973a: Chapter II) with regard to artifacts, he was largely unable to discover any set of horizons in artifact style, material, or function. To a great degree, the completeness of the artifactual sample through time (note that this does not suggest that any complete cultural system has been sampled, but only that a piece of geography through time has had its human behaviors sampled) made the usual normative approach (which Maxwell had used effectively before, and with which he began this series of excavations) of less use after the inferred culture history had been established, and Maxwell embarked on a discussion of cultural processes. This discussion could only come after his previous analyses had resulted in a regional chronology and culture history. In so doing, he was breaking new ground in discussions of the Arctic Small Tool tradition, again largely because his data were so complete.

In attempting to go beyond the study of artifacts to the study of the relations of other systems (cultural, social, technological, etc.) with their environment, Maxwell found that his interpretations, if formulated as hypotheses as they could easily be, were not capable of easy testing in other locales in the Eastern Arctic because the data were either not available in print or not complete or not collected in a manner as to make them comparable.

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While he did not articulate these problems with his interpretations, these complications with the data available for the testing of sophisticated hypotheses are endemic to Arctic archaeology and result, in part, from the operations of what passes for a paradigm among archaeologists (see above for a discussion of paradigms). The techniques and methods were adequate for the examination of the spatio-temporal distributions of artifacts, sets of artifacts, and attributes of artifacts, and as long as this was seen as the major goal of research, then our discipline was adequate for the task. However, after this had been accomplished in a locale (such as the Lake Harbour region, or even southern Baffin Island?), Maxwell was unable to proceed with certainty, as previous methods and techniques had not provided adequate data for the consideration of his hypotheses. This situation has contributed to my present concern for archaeological techniques and methods and data from the Eastern Arctic (see Dekin 1973a:41-42, 1974--in preparation and in press).

Maxwell reiterated his inclusion of these assemblages within the Arctic Small Tool tradition agreeing with Irving that the tradition may contain ". . .distinctive regional variations, but the commonality of specific complexes of types is more impressive than the variants" (Maxwell 1973a: 346). He went on to make the following suggestions ad modifications of Irving's core of characteristics of the Arctic Small Tool tradition.

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> This list includes large numbers of microblades struck from conical cores. I would change this to read "prepared" cores, as a more general term to include a wide variety of core shapes throughout the distribution of the complex. Continuing, he adds burins of several types, and burin spalls retouched for use as minute engraving tools. This last phrase should be modified since it is now apparent that burin spalls were used in many activities in addition to engraving. The description of side blades should be less specific, since, in the Eastern Arctic particularly, there appears to be a wide variety of side blades, but his description of small biface points, or end blades, I would make more specific to refer to two distinctive forms -- the small triangular form and the constricting, tapered based form. To this list I would add a distinctive complex of scraping tools which include edge-expanded end scrapers, and side scrapers with oblique and concave edges. Grinding and polishing of stone tools now appears to be more common in the tradition, and the unique style and technique of fine workmanship which Irving refers to appears to be much more diagnostic of the western than of the eastern part of the range.
>
> Presumably the well-spring from which this tradition departs for the east is the Denbigh Flint Complex (Giddings:1964). While this complex contains features distinctive from early traits at Lake Harbour, there seems to be little doubt that the assemblages from the two regions represent a common technology for adapting to common ecosystems (Maxwell 1973a:346-347).

These revisions were suggested apparently to make the trait list of the Arctic Small Tool tradition more descriptive of the Eastern extension of this tradition as the original characterization was based largely on Western data.

Much of the substantive data on which this thesis is based was collected under the aegis of Maxwell, and relevant portions of the data presented in his 1973 report will be re-examined in conjunction with the previously unpublished data from subsequent field seasons and from the

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Closure site. Many of the ideas presented here have their stimulation from Maxwell's work and thinking, both published and unpublished, as my selections from his work may indicate.

Taylor has reported on his 1963 reconnaissance of the Central Arctic coast between Cape Parry and Cambridge Bay (1964, 1972) and on his continuation of this work in 1965 (1967) on Banks and Victoria Islands. The Buchanan site on the south bank of the Ekalluk River on Victoria Island contained the only Pre-Dorset component encountered in 1963, where its assemblage included

> burins, side blades, microblades, end blades, and other small chert tools (Plate II) along with a considerable number of large coarsely chipped biface blades or choppers and worked quartzite flakes (Plate III). Of the very few nonlithic artifacts, five of antler and ivory are surely arrowheads. They have rounded cross sections, conical or scarfed tangs, and, in one case, a barb. Although seal and fish remains were recorded in the bone refuse from the three test trenches in the Pre-Dorset midden, caribou bones were again predominant. The age of this component leads to indulgent speculation; my speculation that it was occupied about the period 2500 B.C. to 3000 B.C. may well be a very bad guess (Taylor 1964:54-55).

This site was significant because of the association of large crude quartzite tools *in situ* with the smaller Pre-Dorset chert tools all within a closed stratum--a buried soil horizon with sod separation from a higher surface scatter of Dorset artifacts. However, subsequent work (in 1965) cast doubt on the association of microblades with the Pre-Dorset artifacts (1972:64) but confirmed the associati

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association of the chert and quartzite artifacts found also in Harp's Dismal-2 and Kamut Lake assemblages ca. 300 miles to the southwest (see Harp 1958, and above). Both the location and the predominance of caribou bones (both raw and made into artifacts) indicate the heavy dependence on caribou with virtually no sea mammal bones.

Taylor's 1965 work on Banks and Victoria Islands confirmed his previous work at Buchanan (with the exception of microblades--see above) and resulted in the discovery of several additional Pre-Dorset sites. A small copper fragment was also found in the Pre-Dorset component of Buchanan and the association of larger quartzite tools continued in the Wellington Bay site, which also had a more maritime orientation deduced from the majority of seal bones from the refuse (Taylor 1967:225). The Menez site also was a Pre-Dorset site with artifacts similar to those described above from Buchanan. Several radiocarbon dates surprised Taylor by their lateness, with dates from Buchanan, Wellington Bay and Menez ranging 200 years each side of 3000 B.P. (ca. 1000 B.C.) (see below for date list) indicating an occupation late in the Pre-Dorset period (Taylor 1967:229).

On Banks Island, the Umingmak site and the Shoran Lake site echoed the general picture of Pre-Dorset that emerged from the Victoria Island excavations, but with a reliance on musk ox for subsistence (Taylor 1967:227). The artifact sample from Umingmak was somewhat larger (N=400) and it included rare examples of polish on burins and microblades

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with somewhat lower frequencies of large quartzite bifaces. Taylor suggested ". . .that the large, ovate or semi-lunate quartzite bifaces represent a low frequency Arctic trait that, by stimulus diffusion from Archaic complexes to the south, was proliferated in the Pre-Dorset of Victoria, and to a lesser degree Banks, Island" (Taylor 1967:228). Taylor's age assessment of Umingmak (as earlier than the Ekalluk River sites on Victoria Island) was borne out by radiocarbon dates of approximately 1500 B.C. (Wilmeth 1971: 80).

McGhee continued the National Museum of Man's interest in Victoria Island in 1966, where he found two Arctic Small Tool tradition components with small quantities of artifacts (OhPo-4 N=41 including 19 retouched and utilized flakes; OdPq-4 N=15 including 6 retouched and utilized flakes) (McGhee 1971b:160-163). The large-tool quartzite industry from the Victoria and Banks Islands sites excavated by Taylor is not prominent, although several flakes and an end blade were found. The use of basalt and schist at OdPq-4 was similar to Harp's Dismal-2 site (Harp 1958), and McGhee saw the affinities of these sites as being with southern Arctic Small Tool sites rather than with the northern ones on Banks and Victoria Islands, with their ages as early (OhPo-5) and relatively late (OdPq-4) in Pre-Dorset time (McGhee 1971b:162-3). Noteworthy is the presense of a contracting stem end blade of quartzite and a heavily ground burin (OdPq-4). It seems reasonable to hypothes

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hypothesize that these sites pre-date the acquisition of the large quartzite bifaces in this region, although seasonal behaviors provide an alternative explanation, given the scanty nature of the comparable data available.

McGhee excavated an Arctic Small Tool tradition component of the Bloody Falls Site in 1968, which is now one of the best described and dated collections from northwestern Canada (McGhee 1970a). The 250 artifacts (and 2955 detritus flakes) contained 30 burins (3 with surface polish), 84 burin spalls, 15 quartz crystal microblades, end and side scrapers, end blades (several with ground facial facets), and several rare artifacts of possible diagnostic significance -- two ground slate end blades, a chipped and ground adze, and three "pins" of native copper (1970:54-59). Wood charcoal from a hearth was dated _ca._ 3300 ± 90 B.P. (S-463) which McGhee views as consistent with the artifact assemblage. It is perhaps surprising that the large biface industry from Victoria and Banks Islands is not represented in this collection, which dates several centuries more recent in time, and McGhee suggests that such an industry had not diffused this far in this direction (1970:59).

Noble conducted an extensive series of site surveys in the Central District of Mackenzie from 1966 through 1969. Noble used the concepts of tradition and complex to define representatives of three archaeological traditions and nineteen complexes (phases). In so doing, he followed

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MacNeish and Wright in their use of the concept of tradition:

> a tradition is a distinct way of life reflected in the diagnostic material culture of a series of generically related complexes, which persist through appreciable time and across space. In specific cases, the tradition may conceivably develop from a fusion of several traditions, and it may also give rise to a number of closely related historic groups (Noble 1971:104).

Noble's Canadian Tundra Tradition contained ". . .four late small tool complexes which may be considered part of a regional tradition within an overall generalized Arctic Small Tool co-tradition. Seriation and radiocarbon dating suggests a 1000-year period of development for the four complexes, between 1200-200 B.C." (Noble 1971:107). The finding of these complexes extending ". . .up to 150 miles within the present tree line" (107) suggested ". . .a cautious and slow movement inland of taiga-tundra adapted peoples" (Noble 1971:107). The earliest complex, Rocknest Lake dated ca. 1200-900 B.C., contained triangular end blades, microblades and rare burins (including a polished burin) and polished adzes, but the most distinctive aspect was the presence of oval bifaces of quartzite which tie this complex rather directly to Taylor's southern Victoria Island sites (see above). "Rocknest Lake, therefore, is considered to be a late contemporary or a derivation of the more northerly Ekalluk River sites mentioned above" (Noble 1971:108).

Subsequent complexes in this tradition continue and increase the frequency of polished burins while the quartzite bifaces,

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bifaces, microblades (some of quartz crystal), and small concave-base points continue and bear resemblances to Irving's Keewatin sites and Nash's Twin Lakes assemblage in northern Manitoba (see below, Nash 1969). Noble discussed the entire tradition, concluding that it probably arose from the more northerly Buchanan complex of Taylor and pushed ". . .eastward into southern Keewatin and probably gave rise to Nash's (1969) local expression of Twin Lakes" (Noble 1971:110). Noble found the appellation "Pre-Dorset" ambiguous when applied to these complexes, especially since it would be difficult to see many of the "southern" traits as ancestral to Dorset. Noble saw the Canadian Tundra tradition as resulting from Arctic Small Tool tradition cultures but not developing into Dorset, yet the final phases, with their inland orientation and forest adaptation (Noble 1971:110) have no clear continuity into subsequent traditions. With their ethnic identity uncertain, the Canadian Tundra peoples remain an enigma which is difficult to solve, given the nature of the data with which we are forced to deal and of the paradigm of assumptions, concepts and methods we have applied.

Gordon discovered 61 sites (remarkably, seven were stratified) on the Upper Thelon River where extensive excavations were conducted at KjNb-6 and 7, including several levels of Arctic Small Tool occupation (Gordon 1972).

> Arctic Small Tool Tradition Variant Level, dated at 890$\pm$95 (I-5975 and 1210$\pm$95 (I-5978) B.C., bears close resemblances to Arctic Small Tool Tradition

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> (ASTT) materials from the Buchanan, Menez and Uminga [sic] on Victoria and Banks Islands. Fewer affinities exist between the Thelon sites and the ASTT sites of Noble and Harp in the western barrenlands (Gordon 1972:3).

The presence of copper ulus and the large quartzite industry document the widespread extent of this regional variant of the Arctic Small Tool tradition at about 1000 B.C. It is noteworthy that the botanical analysis is indicative of at least partial forestation during this period (Gordon 1972:3), although the specific dates are not yet published.

It is apparent that the tundra-boreal forest ecotone has played a major role in the cultural dynamics of the Districts of Mackenzie and Keewatin and that we must be cautious before concluding anything about the relations among the varying Arctic Small Tool assemblages in this region.

Irving conducted additional surveys of northern Manitoba and southern Keewatin in 1960, 1963 and 1964 finding additional sites of the Arctic Small Tool tradition (Irving 1968:36,46), although a full report has not been published. Irving did discover the Twin Lakes site near Churchill (see below), concluding that both relatively early and relatively late Pre-Dorset occupations had occurred: "The forms of very distinctive implements, including burins, microblades, and very small, bifacially chipped, side-shafted knife blades and weapon points, compare closely with Pre-Dorset (Carlsberg) cultures of the central Arctic, which probably date between 1,500 and 2,500

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B.C. . ." (Irving 1968:46). "Thus, here, as elsewhere in the Canadian north, the Arctic Small Tool tradition spread to its farthest geographic limits (Churchill and the tree-line) very quickly and remained in possession for several centuries at least" (1968:46).

Irving, recognizing the implications of the use of the concept Pre-Dorset in these areas, also used Taylor's term Carlsberg culture to refer to these Canadian Arctic Small Tool tradition peoples who may have only indirectly influenced the evolution of Dorset culture. In this article, Irving points out clearly that these early Eskimos were not the first people in the barren grounds and that subsequent populations in the interior were not derived from this tradition (note that this confirms Harp's general conclusions, 1961).

Irving's work at the Twin Lake site was followed up by Mayer-Oakes in 1964 and by Nash in 1965. Nash conducted extensive research into the Pre-Dorset occupation of northern Manitoba, reinvestigating Giddings's Thyazzi site, and working at Seahorse Gully, near Churchill. Nash summarized his work in 1969 relating the Twin Lakes assemblage to the late Pre-Dorset of eastern Canada, in particular to the Davidee site which Maxwell has categorized as possibly having mixed components (Maxwell 1973a:317-318). The predominance of a notched and ground burin industry strengthens Nash's conclusions. Seahorse Gully was seen as somewhat younger than Twin Lakes, and its large tools of

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chert, granite and pyroclastic stone (Nash 1969:143) were thought to reflect the possible availability of wood or contact with Indians in the adjacent forests (1969:143). It is significant that these large stone tools were not seen by Nash as being either directly or indirectly related to the large stone industry of Noble (Canadian Tundra tradition) or Taylor (Banks and Victoria Island Pre-Dorset) (see above). Subsequent analysis by Meyer (1970) of the Seahorse Gully Pre-Dorset component has confirmed the general conclusions of Nash, with the additional evidence from the analysis of internal divisions of the site and of the radiocarbon date on seal bones of ca. 3000 years B.P. (S-251, 2900±100 B.P., ca. 945 B.C.) which has pushed it back further in time (Meyer 1970:167). The presence of the large tool inventory makes these assemblages as distinctive within the Arctic Small Tool tradition as any other and provide more data on diversity within continuity.

Nash's 1969 volume also contains a relatively complete depiction of the then state of our knowledge of the Arctic Small Tool tradition complexes and affinities and his statement of problems includes the role of the tree line in limiting the southern extension of these early Eskimos (Nash 1969:155). Recent survey has revealed additional Arctic Small Tool sites further eastward along the shore of Hudson Bay within the present limit of trees, so we must await further refinement of paleo-botanical sequences in this area (as along the Thelon, see Gordon above) to find eviden

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evidence to test these ideas.

On the East Coast of Hudson Bay, Plumet discovered a Pre-Dorset site near Great Whale River radiocarbon dated to ca. 1300 B.C. (personal communication) which seems appropriate for the burins, burin spalls, microblades, and end scrapers that I have seen through his courtesy. Structural remnants are rectangular (Gosselin et al. 1974) and there are no large tools from his present sample. We must await further research and publication in this area by Plumet and Harp.

Fitzhugh has conducted an extensive series of excavations around Hamilton Inlet in Labrador and further north in the vicinity of Nain, where a few Pre-Dorset sites were found, but none were found further south. The artifacts were notable for the usual dominance of burins and burin spalls, the inclusion of a ground celt and an ulu fragment, a paucity of microblades and a variety of end blades, including contracting stemmed bifaces (Fitzhugh 1970; 1973). This Thalia Point site was radiocarbon dated to 1710$\pm$140 B.C. and seemingly represents the earliest Pre-Dorset in this region. Fitzhugh has also reported a significant find in a Maritime Archaic context from Hamilton Inlet. The Rattlers Bight site contained a single Pre-Dorset burin made on Ramah Chert which is the only evidence for such contact in the area and is far to the south of the known distribution of Pre-Dorset along the coast. Because the Rattlers Bight site has dates of ca. 1900 B.C. and 2600 B.C. (Fitzhugh

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1973) and contains no burin industry, this is evidence for both contact of some nature between these diverse peoples and for the early presence of Pre-Dorset within (or at least in contact with) the range of Maritime Archaic. This is partially substantiated by the presence of several flakes of Ramah Chert (apparently found only in northern Labrador--see Fitzhugh 1972) in the debitage from the Closure site (see below) (I am indebted to Fitzhugh for the personal identification of these flakes).

As Nash concluded from his analysis of the Pre-Dorset extension southwards along the west coast of Hudson Bay it is remarkable that these people carrying the Arctic Small Tool tradition spread along the tundra coast to its limits, either as defined by the treeline (or other biological limits) or by the presence of indigenous peoples.

Tuck excavated at Saglek Bay, northern Labrador between 1969 and 1971, and has reported Site Q, dated 1880$\pm$115 B.C. (Tuck 1973). The Pre-Dorset site K contains a small tool assemblage dominated by burins, burin spalls, and microblades (totalling 67% of the assemblage, after Tuck 1973) with grinding present on a single burin and lacking steatite lamps or bowls (the sample is small--N=150). There is a distinctive complex of end blades and knives, characterized by fine workmanship, edge serration, and thin cross-sections. The small, thin contracting stem end blades are similar to those found by McGhee in the high Arctic (see below) and rarely at other Pre-Dorset sites in the Eas

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the Eastern Arctic, while other artifacts (burin-like tools and triangular end blades, for example) were typically Pre-Dorset. However, Tuck saw Pre-Dorset affinities as less than those with McGhee's northern Independence I complex, which possibly evolved into his Site K complex (Tuck 1973) (see McGhee below). Tuck saw no continuity between his Site K Pre-Dorset and later occupations, thus seeing two migrations into northern Labrador. In short, the link that Tuck and McGhee see between Independence I and the northern Labrador Pre-Dorset is predominantly in end-blade form, and we will examine this proposition in some detail later in this thesis.

Rousselière continued his archaeological research in the vicinity of Pont Inlet, northeast Baffin Island, where his Oqalik site was thought to be early in the Pre-Dorset continuum (based on the lack of polish on 17 burins--Rousselière 1968:40,43). His Mittimatalik site (PeFr-2), containing a similar assemblage, was radiocarbon dated as one of the earliest dates in the Eastern Arctic (S-589, 4385±155 B.P., ca. 2435 B.C.) but the seal bones dated may cause the sample to be somewhat older than the actual date. The Pre-Dorset material, while present, is scanty and its preliminary nature does not allow conclusions (Rousselière 1973) other than the fact that an early Pre-Dorset occupation comparable to those from Igloolik and southern Baffin Island was present in this region.

McGhee conducted an extremely important archaeological reconn

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reconnaissance of the south coast of Grinnell Peninsula of Devon Island and nearby Dundas Island in the summer of 1972 and his Independence I and Pre-Dorset assemblages are important data for the development of this thesis. McGhee's Independence I sites had apparently both summer and winter dwellings and were categorized by the presence of stone outlines and slab mid-passages (see Knuth 1967 and above). The artifacts included unpolished burins, frequent microblades, many end-blades (including thin triangular and bipointed forms reminiscent of those from Tuck's Saglek Bay site K, see above) with often deep edge serration, and a non-toggling form of harpoon. McGhee infers coastal sealing as the major subsistence, from the prevalence of seal bones, while caribou and musk ox also occur. The moderate size of the sample (N=267) and lack of complete excavation suggest that negative evidence may again be of relatively little value (McGhee 1973). McGhee dates this occupation as coeval with that excavated by Knuth from Ellesmere Island and northern Greenland, probably _ca_. 2600 B.C. The settlement pattern comprises strings of adjacent houses along raised beaches.

McGhee's Pre-Dorset assemblages are somewhat larger (N=481) and their house forms less-descript without evidence for internal structures or clearly marked walls. As expected, burins, burin spalls, and microblades comprise almost two-thirds of the assemblage and only two burins have polishing. The raw materials are different from the

Independence I artifacts, and the Pre-Dorset appear somewhat cruder, lacking edge serration and lacking the thin triangular and bi-pointed end blades. Their subsistence patterns appear to be similar to the Independence occupation, but their settlement pattern is not, being house clusters rather than alignments. The somewhat lower average elevation of the Pre-Dorset is taken as an indication of somewhat later age, and McGhee suggests possibly 1700-1500 B.C. (McGhee 1973).

The finding of these two distinct complexes in situations where their settlements overlap on the same beaches was not expected in this area, as our previous thinking suggested a clinal relationship between regional variants of the Arctic Small Tool tradition in the Eastern Arctic. McGhee, emphasizing the distinctive characteristics of each assemblage, suggests that they represent two distinct migrations from the Western Arctic, with the Independence I being the earliest occupants of the far North, followed by migrants of Pre-Dorset people from the core area (McGhee 1973, 1974). McGhee also suggests that this earlier Independence I migration was not limited to the high Arctic littoral, but may have spread over a large area of the Eastern Canadian Arctic, where their sites have gone undiscovered because of their linear settlement pattern (McGee 1973, 1974). Within the core area, these Independence I peoples were displaced or acculturated by a second migration, this time of Pre-Dorset people.

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McGhee applied concepts derived from the study of Historic Eskimos to attempt an understanding of the social dynamics that might make such technological and social changes comprehensible. He grouped bands of *ca*. 50-100 people into larger "maximal breeding units" of perhaps 500-1000 people with distinctive cultural systems setting them apart from similar groups in adjoining areas. He called these larger groups "tribes" and believed them to categorize the diverse regional archaeological assemblages that we have observed in the prehistoric record, following an analogy from the study of the relations between regional groups during the Historic period (McGhee 1973).

Aside from the importance of McGhee's archaeological data from the Port Refuge area of Devon Island, his 1973 discussions with regard to the population dynamics of the *people* who made these artifacts mark an important shift in the nature of our discussions of the Arctic Small Tool tradition and of Eastern Canadian Arctic archaeology. The further discussion of these matters will be deferred until later in this thesis, but McGhee's ideas make a fitting close to this preliminary history of the study of the Arctic Small Tool tradition.

As a result of the above assessment of our past researches in this area, I see Pre-Dorset as an Eastern extension of the Arctic Small Tool tradition, in which the dispersal of the population marked by the Arctic Small Tool horizon resulted in a diverging and elaborating horizon and tradi

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tradition (Wauchope 1956:43), with complex internal dynamics and interrelations among its constituent populations.

These dynamics were influenced by the diversity of environments in which the horizon extended and by changes in these environments through the time of the tradition. The understanding of changes in Pre-Dorset culture which emerges from the survey of the literature, is one of great complexity. We should not, therefore, expect to reach such understandings through the use of a small number of simple models. Because I anticipate the necessity for a number of complementary and complex models, the methodology and theoretical framework in which such studies must occur demand further attention.

The last several years have seen an increasing concern among Arctic archaeologists for archaeological method and theory (see Dekin 1973a:41-42). In part, this is because Arctic archaeology has been a classic example of "The more we know, the less we know" because as we accumulated more knowledge of the diversity of evidence for behaviors, we became less sure of what we had known before, re-opening old questions with new data (McGhee 1973 is a case in point).

Several of us have tried to make our methodologies more openly explicit, so that others could examine them (see Nash 1973, Fitzhugh 1973, Dekin 1972a, 1972b, 1973b), and the recent Seminar on Pre-Dorset -- Dorset Problems at the School of American Research was notable for the concern

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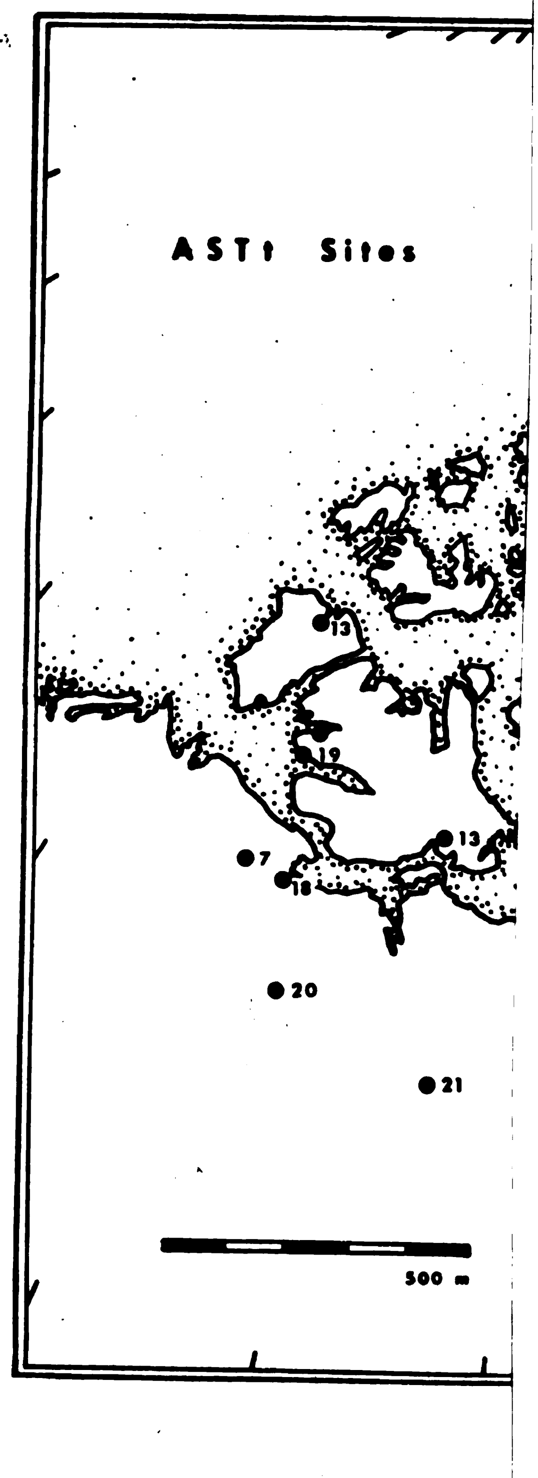
Table 2 Summary of ASTt Field Research: Eastern Arctic

Author	Publication Dates	Map Key	Location or Sites Excavated
Solberg	1907	1	West Greenland
Thostrup	1911	2	Northeast Greenland
Meldgaard	1952	3	Disko Bay, West Greenland
Larsen and Meldgaard	1958	3	Disko Bay, West Greenland Sermermiut, Sarqaq
Mathiassen	1958	3	Disko Bay, West Greenland
Knuth	1958	4	Independence Fiord, Northeast Greenland
Giddings	1956	5	Northern Manitoba Thyazzi
Meldgaard	1960,62	6	Igloolik, N.W.T.
Harp	1958	7	Coronation Gulf, N.W.T. Dismal-2
Harp	1959,61,62	8	Thelon River, N.W.T. Complex A/Phase 2
Lowther	1962	9	Cape Sparbo, N.W.T.
Taylor	1962,65,68a	10	West Hudson Straits, N.W.T. Ivugivik, Arnapik
Rousselière	1964		Pelly Bay, N.W.T. St. Mary's Hill
Knuth	1967	4	Ellesmere Island and Northeast Greenland Independence I and II
Maxwell	1962,67,73a	12	Frobisher Bay, N.W.T. Lake Harbour, N.W.T. Pre-Dorset
Taylor	1964,67,72	13	Banks and Victoria Islands N.W.T. Pre-Dorset
Irving	1968	14	Southern Keewatin
Nash	1969	15	Northern Manitoba, Twin Lakes, Seahorse Gully
Fitzhugh	1970	16	Labrador Coast, Thalia Point
Plumet	unpublished	17	Great Whale River, Quebec
McGhee	1970a	18	Bloody Falls, N.W.T.
McGhee	1971b	19	Victoria Island, N.W.T.
Noble	1971	20	Central District of MacKenzie Canadian Tundra Tradition
Meyer	1970	15	Northern Manitoba Seahorse Gully
Gordon	1972	21	Thelon River, N.W.T.
Tuck	1973	22	Labrador Coast, Saglek Bay Site K
McGhee	1973	23	Devon Island, N.W.T. Port Refuge Independence I & II
Rousselière	1968,73	24	Pond Inlet, Baffin Island, N.W.T. Oqalik, Mittimatalik



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expressed and discussed for method and theory. Seemingly, we were striving to go beyond the study of technology through time and space, and we were more than occasionally snagged by inadequate or unavailable data.

Attempts to utilize perspectives, techniques, and methods of ecological systems research were frustrated by the unavailability of good quantifiable data on the subsistence of Arctic peoples, although a number of suggestions were made on a somewhat more general level of abstraction (see Fitzhugh 1973, Dekin 1973b). General systems theory has also been used in an attempt to generate hypotheses for testing with regard to prehistoric cultural systems (Dekin 1972a) and as a framework for the consideration and display of data (Nash 1973 used a more inductive approach linking his generalizations with the data, while Dekin's hypotheses could be tested on the archaeological data as they were developed independently). The utility of these approaches remains to be seen, as does the level of their acceptance by other Arctic archaeologists.

Recent studies have also demonstrated the interest of Arctic archaeologists in utilizing the implications of functionalist anthropology for archaeology (see Leach 1973: 762; Binford 1973) where Maxwell has attempted to assess the activities at the Morrison site (1973b) and Dekin has attempted to define patterns of structural design and internal division into activity areas at the Closure site (Dekin 1973b and this thesis). However, these attempts at analyzing

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analyzing the internal behavioral dynamics of social groups which produce sites have been the rare exception, and are themselves still in the developmental stage.

The discipline remains dominated by a culture-historical traditionalist paradigm (if one exists; see above) in which the unit of analysis is the site-assemblage of artifacts. There is, however, increasing concern with ecological relationships, with sampling techniques, with representativeness of samples, and with data preserved, presented and published. This thesis is a further step in this direction of methodological change, in that a major concern is in the behaviors of individuals within archaeologically-known cultures--an area of interest in which some have argued we would make no contribution, and thus collected andpublished their data in a manner which occasionally precludes and usually hampers such study.

Recapitulation

The state of the art of producing generalizing concepts is unclear, as we have gone for some time accepting and using concepts differentially in our analysis and it is often clear that the meanings are not shared by all of those working in the field.

Paleo-Eskimo has been used as a catch-all to include almost all Arctic cultures which were seen as developmental from a Denbigh Flint Complex-like base and not yet developed into Thule (See Mathiassen 1958:3). As a chipped-

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stone complex, this also is too general to be of much use and it is difficult to see what utility this concept has, in that it does not define human behaviors accurately (or precisely) enough for operationalizing research. In the Eastern Arctic, it may still possibly be useful to refer generally to pre-Thule Eskimo cultures, but only on the most general level.

In the Western Arctic, the diverse and dynamic cultures which would be included in this category would be better served by greater specificity, and it is interesting to note that the concept has not received much use or attention in the West when compared to its rather intermittent use in the East. In short, its recent use in the Eastern Arctic does not seem appropriate, and I will suggest some alternatives later in this thesis.

The Arctic Small Tool tradition has undergone subtle changes in its conception and definition since its first development. It now appears that the homogeneity inherent in the concept of tradition may not apply to as long as time depth as initially perceived. As I will suggest below, the data are supportive of an Arctic Small Tool horizon, spreading rapidly across the entire North American Arctic between 2100 and 1900 B.C., which should subsequently be considered possibly as both a diverging and an elaborating tradition (see Wauchope 1956:43) with particular attention to the development of diversity within the tradition, which, toward the upper limit of its existence, has lost all implicatio

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It is apparent that the concept of Pre-Dorset would be most useful when applied to a restricted area in which Dorset could be seen to develop from it, in this case the core area of Foxe Basin and Hudson Straits. To do this, I will develop categories to delineate sub-traditions co-eval with Pre-Dorset, but which were not directly ancestral to Dorset culture. Noble's Canadian Tundra tradition may qualify for such a tradition.

Taylor's Carlsberg culture may provide a useful distinction between the Western developments from the Denbigh Flint Complex and the Eastern developments, but it requires subdivision to categorize the cultural diversity present in Greenland, the high Arctic, the core area, and southern and western peripheries, and we will look carefully at such schema to develop concepts which will be inductively sound and operationally useful.

It is apparent from the above discussion that while the concepts used to generalize about Eastern Arctic prehistory have been used with relative explicitness, they lack precision in both their definition and use. As the corpus of data to which these concepts refer has grown, the concepts have not been re-defined but largely reified, and we are faced with a number of different concepts none of which seems to fit the data well. There is a need for greater explicitness and precision in the use of the general conceptual framework in which we wish to work towards a better un

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better understanding of prehistoric cultural dynamics in the Eastern Arctic.

Nowhere is this need for a precise conceptual framework more obvious than in the consideration of the temporal and environmental contexts in which the human occupations of the Eastern Arctic occurred. The development of explanatory models of human behavior must be based on a precise and accepted time frame, especially when we are dealing with complex sets of ecological relations (Dekin 1973a:41). It is essential that our treatment of temporal and environmental data be precise and explicit, since differences in this basic framework are a considerable source of variation in interpretation. Part II will develop a precise and explicit depiction of the chronological framework of archaeological data and of the changing environmental setting between 4000 and 2500 B.P. as a basis for the development of more complex models of human ecological relations (see Part III).

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Chapter 2

Dating the Arctic Small Tool Tradition

The first formulation of the Arctic Small Tool tradition was based largely on the assumption of time depth, even though there were then no absolute dates for any members of this tradition and the only relative dating available was that for the type site of the Denbigh Flint complex at Iyatayet where Denbigh was stratigraphically separable and overlain by Norton and Nukleet strata.

However, the thinking on the development of this tradition has evolved hand in hand with the increased knowledge of radiocarbon dating, thus most of our interpretations have developed with radiocarbon dates available for many of the assemblages in the tradition. Wherever possible, our chronology for the tradition has been based largely on radiocarbon dates.

In specific locations, these have been supplemented with other techniques of relative dating, most of which involve cross-dating from chronological sequences derived from other sciences. The geology of the Arctic has provided a powerful tool for cross dating because of the isostatic rebound of the land rising after the ice sheets had melted.

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This possibility was first used in the Eastern Arctic by Mathiassen (1925:206) when he attributed considerable age to house ruins found at higher elevations and also further distant from the shore. On a rather simplistic basis, Arctic archaeologists have often used relative height above present sea level as a measure of relative age, especially in the absence of other relevant criteria. This practice is ubiquitous, in spite of the cautions voiced by Collins (1962:128-129) who considered the method useful ". . .and generally valid when no complicating factors are involved" (1962:128) (see Taylor 1968a:98-99).

Sequences of beach ridges raised in elevation above the present sea level have been used effectively in both the Eastern and Western Arctic (Meldgaard 1962; Giddings 1960, 1961, 1966) but this is useful in those relatively rare occasions where distinct beaches with definable occupations were preserved.

It is also obvious that these data are relevant only in those areas in which the relative sea level has varied and only along the coast. Strictly speaking, age estimations of this sort can only be minimum ages of the occupations of the raised area, as has been effectively pointed out by Andrews et al. (1971) in their compilation of uplift isobases and relations with elevations of archaeological sites.

Formal attempts at the seriation of archaeological data with the goal of establishing relative sequences of asse

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assemblages are virtually non-existent. In part, this may reflect the general decline in the use of formal seriation methods in North American archaeology that occurred after the advent of radiocarbon dating and before the increased utilization of computer technology to speed the data processing. Taylor's 1968 seriation of the West Greenland Sarqaq and Dorset sites reported in Larsen and Meldgaard, 1958, is perhaps the most formal attempt and even he acknowledged the apparent bias in favor of cultural continuity inherent in the method (Taylor 1968a:91).

Informal seriations of data have involved:

1. the establishment of synchronous horizons of artifact style or type (attribute or attribute set); and

2. the establishment of trends in artifact frequency through time.

Thus, Dorset was seen as a horizon of the following set of artifacts:

1. burin-like tools;
2. microblades;
3. steatite lamps and pots;
4. isosceles triangular end blades;
5. rectangular houses;
6. ground-slate projectile points and knife-forms;
7. side-notched flint end blades; etc.

Yet, this horizon did not mark a clear cut separation from earlier artifact forms and sets, as Meldgaard's data from Igloolik clearly indicate (Meldgaard 1962:Plate 5). Harp's categorization of traits of Dorset culture was a statement of primary traits that might appear in some locales as horizons, but which appeared in his Newfoundland sites as a fully-

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The issue of whether the development of Dorset culture was revolutionary or evolutionary apparently depended in part on the nature of the microscope (level of abstraction) the analyst was using, as the relative speed and pervasiveness of cultural changes were subject to the judgment of the archaeologist.

The following trends were perceived in the Eastern Arctic Small Tool tradition:

1. decline in the frequency of true burins;
2. increase in the frequency of side notching on artifacts, including end blads and burins;
3. increase in the frequency of grinding and polishing as construction techniques;
4. increase in the use of quartz crystal as a raw material;
5. increase in the use of steatite for lamps and pots; etc.

It is interesting to note that those archaeologists who saw the major cultural changes in the Eastern Arctic as resulting principally from migrations were prone to utilize a guide-fossil approach (Dorset as a horizon) (see Mathiassen 1985; Larsen and Meldgaard 1958; Meldgaard 1952; Knuth 1967) while those archaeologists who saw a sequence of development from Pre-Dorset through Dorset were more concerned with "trends" rather than type fossils (see Taylor 1962; Maxwell 1962; Taylor 1968a). In a real sense, the techniques of serial dating utilized in the Eastern Arctic reflected the theoretical biases of the investigators, in most cases only little affected by what limited radiocarbon dating had been done at the time.

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However, rather little effort was expended on the development of formal statements of horizon markers or trends, in large measure because there were few locations where extensive series of sites demonstrated great variance through either space or time. Most Arctic Small Tool tradition sites listed in Tables 1 and 2 are small isolated finds not directly related to a regional developmental sequence. The notable exceptions in the Eastern Arctic are the researches of Knuth in northern Greenland and Ellesmere Island, Meldgaard in the Igloolik area and Maxwell in the Lake Harbour region. Of these, Maxwell has provided the best published examples of a regional sequence, while Meldgaard has provided a preliminary report and some conclusions and Knuth has treated his material as representative of relatively static stages of cultural development (compare Maxwell 1973a; Meldgaard 1960, 1962; and Knuth 1967). Meldgaard stressed the breaks in his sequence of occupations while Knuth stressed their homogeneity utilizing a general model of cultural replacement by migration and Maxwell utilized a model of general cultural evolution *in situ*. Thus, Meldgaard used a trait list of characteristics, Knuth stressed those characteristics that differentiated his regional occupations, and Maxwell discussed more-or-less cyclical variations about modes of stylistic variation. Maxwell has come the closest to providing data for the establishment of horizon markers, yet his emphasis on cultural continuity stresses the lack of clearly defined

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horizon markers of cultural change and he further emphasizes that there are few instances where several items of change seem to occur coevally to mark a period of greater change and a horizon.

The study of Arctic Small Tool tradition assemblages in the Western Arctic has been a history of "splitters" (Dumond 1974b). While some have seen cultural continuity, with changes, through the Denbigh Flint complex, Choris, Norton and Ipiutak and on to proto-Eskimo and even Eskimo traditions, none has presented a synthesis emphasizing the cultural continuity and demonstrating the existence of horizon markers or any evidence for similar changes across a large geographic expanse. Further, the greatest interest in Arctic Small Tool tradition sites in the Western Arctic has been expressed by Anderson with his developmental sequence at Onion Portage from Proto-Denbigh to Denbigh. In the last ten years, very few Denbigh (or Arctic Small Tool) sites of significance have been excavated or fully reported, largely because other problems have dominated the research interests of Western Arctic archaeologists, and Arctic Small Tool sites have been by-products of other research programs and reports (see research by Stanford, Humphrey, and Alexander: McGhee 1971a; Humphrey 1970; and Alexander 1969). Thus, the Western Arctic has been dominated by distinctive cultures and changes were seen as large enough to indicate breaks in sequences of cultural development. Chronologies were developed from beach ridge sequences on the coast

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(Giddings 1960, 1961, 1966) and, since 1963, from stratified sites in the interior (Giddings 1966; Anderson 1968a, 1970) relying on artifact similarities and radiocarbon dating for cross-dating. Perhaps the greatest change in the Arctic Small Tool tradition in the Western Arctic was the addition of pottery, presumably from Asia, contributing to Choris culture. The continuties between Denbigh and Choris were recognized, but the cultural assemblages and their respective cultures were seen as being different, and not simply as a continuation of Denbigh with pottery. The Western Arctic, being adjacent to four contiguous regions with distinctive cultural traditions (The Aleutians, Southwest Alaska, Interior Alaska, and the Asiatic Coast), was influenced by ideas stemming from migrations and other movements of people apparently more frequently than was the Eastern Arctic, which may be seen as a relatively closed system during the Pre-Dorset -- Dorset period (Maxwell 1967, 1973a).

The prehistory of the Western Arctic has been marked by a number of distinctive migrations with clear horizon markers, stratigraphic separation, and only general threads of cumulative sequential development. In such circumstances, radiocarbon dates have been the major source of absolute chronology, with cross-dating from regional sequences as the major comparative tool.

This reliance on radiocarbon dating for the establishment of a regional chronology has resulted in rather spotty chron

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chronological control, as the well-dated sequences are widely spaced and can only be linked by cross-dating using a guide-fossil approach.

The general approach to radiocarbon dating has involved the following analytical steps, after the receipt of the date from the laboratory.

1. Convert from conventional radiocarbon years before present (B.P., 1950) by subtracting 1950 years.

2. Accept or reject date based on previous finds and prior speculation or conclusions (a priori).

3. Draw rationale for acceptance or rejection of date (including bad association, contamination by more recent materials, fractionation by natural processes, differences in the radiocarbon reservoir, etc.).

4. Operationally, consider the measure of central tendency of the date as if it were representative of a year in real time, ignoring the standard deviations and the probabilities associated with them, even to the extent of referring to the date by its measure of central tendency alone.

This approach has produced a chronology which is relative and which is generally accepted as a fairly close approximation to real time, even though it has been known for at least fifteen years that the ratio of available C^{14} to C^{12} had changed through time, and that there would have to be additional conversions to obtain dates in actual years (real time) from radiocarbon years. Suess has led in this

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attempt to date tree rings (of known age) to define the relationship between radiocarbon years and actual years (which is based on the fluctuations in the C^{14} / C^{12} ratio through time) and his curves have been used in attempts to reconcile apparent problems in regional chronologies (Renfrew 1971). However, it is important to note that these problems in European prehistory came as a result of two presumably absolute systems of time measurement: written history and radiocarbon dating. At certain times, these two systems did not mesh well and only after Renfrew made extensive conversions based on the Suess curve did some semblance of order result from the previous disagreements. However, it is important that we recognize that this situation was a special case resulting from the availability of historic records in the area. The relative sequence was largely unchanged and merely shifted somewhat. We have no reason to expect great changes in our regional chronologies based on radiocarbon sequences as a result of the application of a Suess chronology to convert to apparent real time, as we shall see below.

There continue to be other sources of discrepancies in radiocarbon datings, resulting from differences in the use of half-lives (5568 vs. 5730), in the application of asssessments of fractionation (C^{14}, C^{13}, C^{12}), in the use of 1950 as an artificial "present", as well as the inherent variation in sample collection, pre-treatment, and the material itself. It is difficult to control all of these variables, espe

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especially since the users of these data (archaeologists) are not conscientious in their reporting of what has been done to their dates (half-life, fractionation assays, etc.).

Recently, McGhee and Tuck (1973) have attempted to develop a regional chronology for the Eastern Arctic by converting dates in conventional radiocarbon years to years AD/BC using a Suess curve from Stuiver and Suess (1966) and Renfrew (1971). They attempted to reduce systematic variation by applying a fractionation correction of -400 years to sea mammal dates, but concluded by eliminating dates on any materials other than indigenous charcoal from the establishment of a chronology. In part, this was necessitated by the spotty availability of information on fractionation corrections that have already been made before release of the dates, and by our lack of precise information on the nature of variations in materials from different life zones.

The major impact on a series of dates from conversion to "real time" using a Suess conversion curve is the change in clusters of dates through time, as certain periods of time are "stretched" and others are "shrunk" vis-à-vis dates in conventional radiocarbon years from these periods. Thus, in a radiocarbon chronology, time passes more slowly at certain times than others, clustering dates from more widely in time. In general, conversion has little effect on relative positions in time, especially if the numbers of dates are relatively small and thus relatively dispersed throu

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through time. Perhaps the greatest contribution of such conversion is in the increased alternatives to certain dates which might otherwise appear anomalous. However, this new variance is often less than the original variance of the date itself (standard deviations).

Tables 3 and 4 are summaries of dates associated with cultural materials from the Arctic Small Tool tradition in both Eastern and Western Arctic as of January 1974. The data presented include the original date expressed in conventional radiocarbon years, other characteristics of the date and its material, a conversion to "AD/BC" by subtracting 1950 years, and a conversion to Suess-curve dates and the alternatives.

With regard to interpreting these data, I would emphasize that the relative order is only rarely threatened by alternatives and that the range of alternatives is by and large less than the two standard deviations range of the original date (of the 85 dates in this sample, the ranges of 76 are less than two standard deviations, the ranges of 8 are between two and four standard deviations, and the range of only 1 is greater than four standard deviations) which supports the following conclusion: the ranges of the alternatives created by conversion to the Suess curve demonstrate no more variability and dispersion than the variability and dispersion of the original date determination itself. Conversion thus does not change the variability (and therefore imprecision) inherent in the dating

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process. This point is significant in light of the difficulty which some archaeologists have with regard to interpreting such imprecision, as they perceive it. One drawback to the use of the Suess curve, is the reluctance of archaeologists to reduce voluntarily the apparent precision of their dates by recognizing that there may be several alternative choices. In most cases, I believe that this is indicative of a general failure of archaeologists to deal adequately with the dispersion of the original date in radiocarbon years. We have not generally coped with the stochastic processes inherent in radiocarbon dating (Polach and Golson 1966).

Spaulding pointed out in 1958 that statistical techniques were available to allow the testing of the probability that two radiocarbon dates dated the same event (were coeval) or dated different events. Because this testing is not usually applied and represents an important contribution to our understanding and assessment of radiocarbon dates, I will reproduce this method here in some detail.

The essence of the technique is the assessment of whether or not the two dates under consideration could be samples drawn from the same population. The statistic "t" is the expression of the difference between the two measures of central tendency (means of the "samples") as the number of standard errors of the difference between these two dates. The standard error is computed in the foll

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Date 1: $\bar{m}_1 \pm sd_1$ $\bar{m}$ is the mean

Date 2: $\bar{m}_2 \pm sd_2$ sd is the standard deviation

Standard error of difference: $\sqrt{(sd_1)^2 + (sd_2)^2}$

$$t = \frac{|\bar{m}_1 - \bar{m}_2|}{\text{Std. Error}}$$

The probability of such a value of t occuring by chance in two samples drawn from the same population is obtained by reference to the curve reproduced in Figure 4 (Hodgman 1958). This procedure allows us to test whether two dates are coeval, and to assess this probability.

As an example, Dumond assesses the evidence for the beginnings of his Brooks River Gravels phase, rejecting a previously held valid date of 2022 B.C. ± 440 (Y-930) on the basis of association, using the date of 1950 B.C. ± 130 (I-1629) as the earliest date of the Brooks River Gravels Phase, which he assesses as 1900 B.C. ± 100 (Dumond 1971:9). As part of his evaluative process, it would have been appropriate to assess whether or not the two dates above could have been coeval.

The standard error is: $\sqrt{(440)^2 + (130)^2} = 458.8$

$$t = \frac{|2022 - 1950|}{458.8} = \frac{72}{458.8} = 0.1569$$

From Figure 4, we can see that such a value of t would occur *ca*. 87 times out of 100 by chance in two samples from the same population and the probability that the dates are coeval is p = 0.87.

Table 3 Radiocarbon Dates--Western Arctic ASTt

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Years BP	Lab. #	Site	Citation	Assemblage	Map Key

Table 4 Radiocarbon Dates--Eastern Arctic ASTt

Table 4 Radiocarbon Dates--Eastern Arctic ASTt

Years BP	Lab. #	Site	Citation	Assemblage	Map Key

Table 4 (Cont'd)

Table 4 (Cont'd)

Lab. #	Material Dated	-1950 Yrs.	Suess Tree Ring Calibration (B.C.)

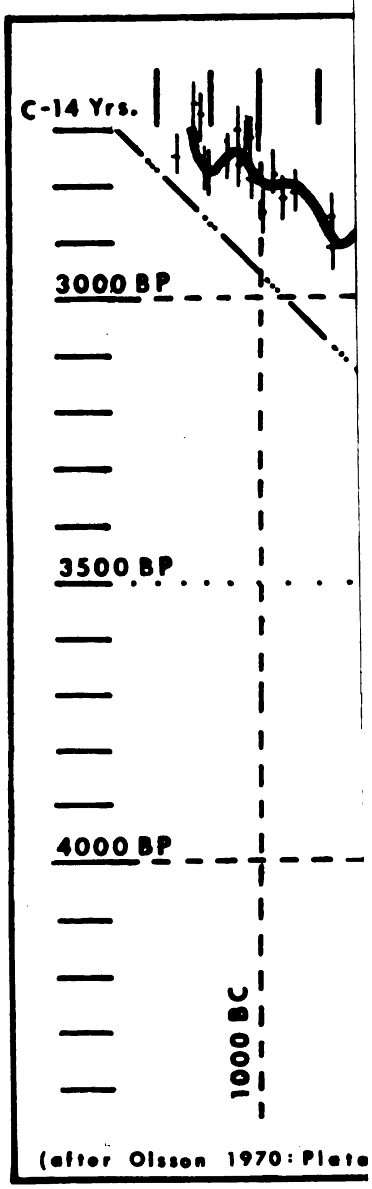

Figure 3 Portions of the Suess Curve for Tree Ring Calibrations

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The radiocarbon dates from Pre-Dorset sites analyzed in this thesis are listed in Table 5, as conventional radiocarbon years. However, their evaluation is a rather complex phenomenon.

These dates are all on what are believed to be cinders of charred animal fat and sand produced by the burning of oil lamps, which are usually (historically in this region and today) thought to burn sea mammal fat (probably seal; the species is uncertain and is probably unknowable in the forseeable future). Dating of this material has proved somewhat different from other materials, and several sources of variability are possible.

1. Reservoir exchange--the marine ecosystem may contain carbon somewhat older than the terrestrial ecosystem because of a time lag in the exchange of C 14 produced in the atmosphere with the dissolved carbon in the sea. Within limits, this difference is probably almost a constant, although some differences could be caused by changes in circulation of atmosphere, storm tracks, open water, etc. The adjustment of these dates 400 years more recent in time as recommended by I. Olsson could be seen as an attempt to compensate for this delay in reservoir exchange (McGhee and Tuck 1973).

2. Fractionation--certain metabolic processes cause the proportions of the various isotopes of carbon to be accumulated differentially and to be deposited in living material in a proportion different from that present in

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the biosphere (atmosphere or water). This difference should be proportional to the amount of carbon taken in and to the different proportions of the isotopes present in the organism's environment. This is not a time-delay phenomenon, but a constant ratio of the carbon composition of the environment to the carbon composition of the organism. Several figures have been suggested varying from 10% to 20% reductions in conventional radiocarbon years to compensate for this source of variance.

3. <u>Variegation in the Marine Reservoir</u>--this is the "old water" phenomenon, in which it is recognized that deep ocean currents receive C^{14} produced in the atmosphere in exchange with surface waters, and then go on about their underwater business keeping well beneath any subsequent addition of new C^{14} until they upwell on a distant shore or sea rise possibly as much as several centuries later. Obviously, the C^{14} content of that old upwelling water will have been reduced by the decay processes and any animals or plants in the food chain produced by that water and its resources will contain a diminished proportion of C^{14}, thus dating somewhat older than their death. Migratory seals are especially prone to this sort of variation, and while they are not as numerous in the Lake Harbour region as other marine mammals, this possibility cannot be ignored. Harp's adjustment of his Port au Choix radiocarbon dates 200 years more recent in time to compensate for this effect was based on changes recommended by this difference

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4. Variegation in the Marine Reservoir--an additional source of variation is in the presence of substantial amounts of old carbonate in coastal waters, especially in areas where carbonate rocks form the shore line. The weathering process may produce amounts of this old carbonate which may enter the ecosystem in the form of marine shell or other forms. It seems unlikely that such variation would influence the above dates, but they are a factor in the geological dating of raised strand lines and other coastal phenomena.

Table 6 is another listing of these dates and possible adjustments to them, based on the above discussion. Please note that the chronological order of the dates remains relatively unchanged. Perhaps the most important implication of the Suess adjustment of the dates is the *ca*. 33% increase in the time span covered by the dates. While this span may be relatively unimportant in the present study, any attempt at analyzing the rate of cultural change through time would have to take these changes in presumed time into consideration.

Table 7 is a table of values of t for the possible contemporaneity of all of the dates from Tables 5 and 6. While the probability that KdDq 11-6 and KdDq 11-8 are coeval is .55 and thus almost an even chance, the probability that Shaymark (KkDn 2) and Closure (KdDq 11) are coeval rises to

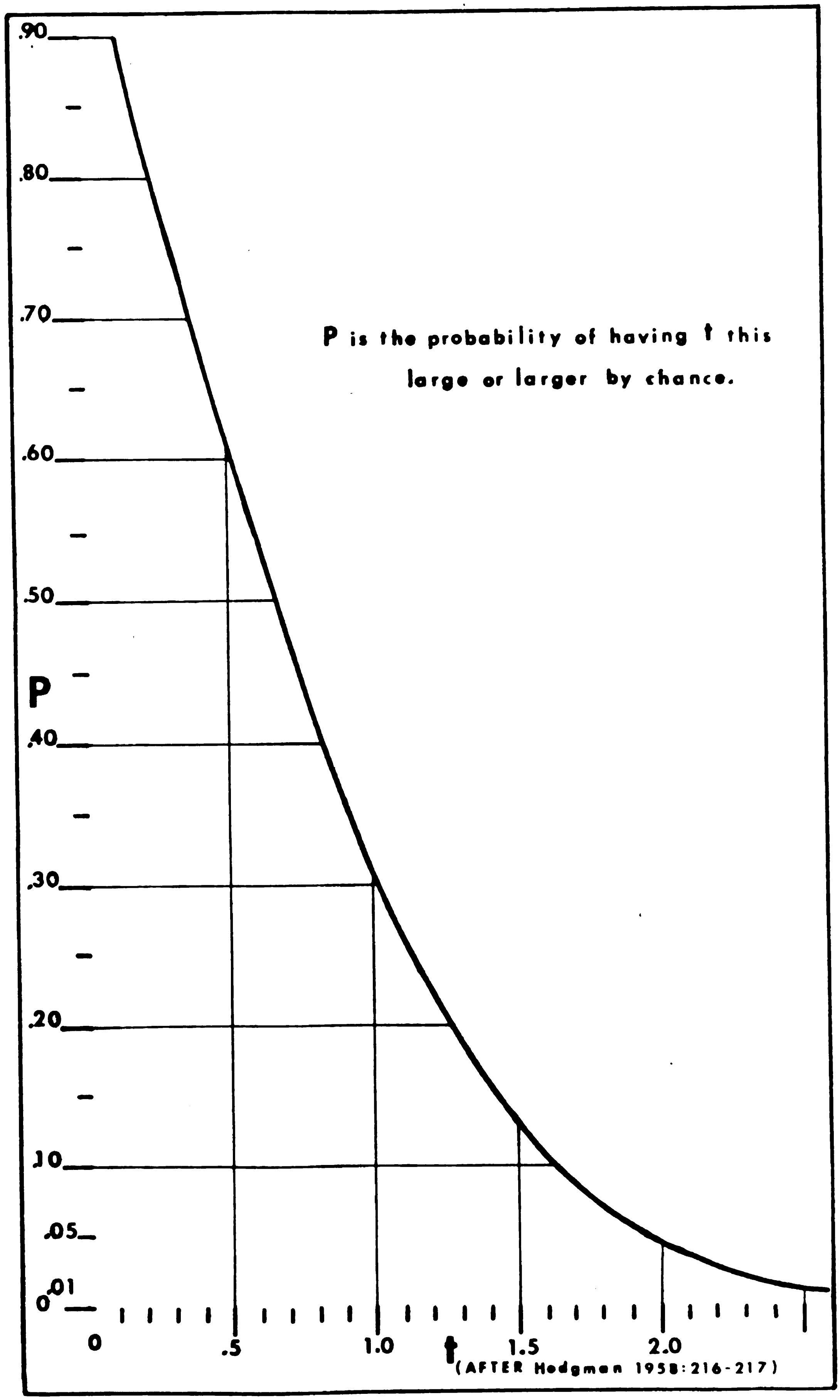

Figure 4 Probabilities for Values of "T"

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Table 5 Radiocarbon Dates from Pre-Dorset Sites Analyzed in this Thesis

Conventional Radiocarbon Years BP	Lab. #	Site	Material Dated
4690 ± 380 BP	GSC-1382	KdDq 11-8	Charred fat cinder (?)
4460 ± 100 BP	Gak-1281	KdDl 11-6	Charred fat cinder (?)
4140 ± 130 BP	GSC-849	KkDn 2	Charred fat cinder (?)
4080 ± 66 BP	P-707	KdDq 11	Charred fat cinder (?)
3814 ± 69 BP	P-708	KeDr 1	Charred fat cinder (?)
3577 ± 69 BP	P-710	KdDq 10	Charred fat cinder (?)
3480 ± 200 BP	M-1531	KdDq 13	Charred fat cinder (?)
3390 ± 210 BP	GSC-1051	KdDq 23	Charred fat cinder (?)

Tab

Lab.#

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Gak-12

GSC-8

P-701

P-708

P-710

M-153

GSC-

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Table 6 South Baffin Island Pre-Dorset Radiocarbon Dates, With Adjustments

Lab.#	Years BP	-1950 AD/BC	Suess AD/BC	-400 Yrs. BP	-10% Yrs. BP
GSC-1382	4690±380	2740±380	3420/ 3510 BC	4290±380	4221±380
Gak-1281	4460±100	2510±100	3380 BC	4060±100	4014±100
GSC-849	4140±130	2190±130	2610/ 2750/ 2770/ 2925 BC	3740±130	3726±130
P-701	4080±66	2130±66	2520/ 2680/ 2740/ 2790/ 2830/ 2900 BC	3680±66	3672±66
P-708	3814±69	1864±69	2175/ 2300/ 2340 BC	3414±69	3433±69
P-710	3577±69	1627±69	2075 BC	3177±69	3219±69
M-1531	3480±200	1530±200	1715/ 1780/ 2020 BC	3080±200	3132±200
GSC-1051	3390±210	1440±200	1680 BC	2990±210	3051±210

Note these time spans: 1300 yrs. 1740-1830 yrs.

Table 7. Tables of "t" and Probabilities of Significant Difference Between Radiocarbon Dates of Table 5

Table 7 Tables of "t" and Probabilities of Significant Difference Between Radiocarbon Dates of Table 5

	KdDq 11-6 Gak-1281 4460±100	KkDn 2 GSC-849 4140±130	KdDq 11 P-707 4080±66	KeDr 1 P-708 3814±69	KdDq 10 P-710 3577±69	KdDq 13 M-1531 3480±200	KdDq 23 GSC-1051 3390±210
KdDq 11-8 GSC-1382 4690±380	t= .59 p= .55	t=1.37 p= .17	t=1.58 p= .11	t=2.27 p= .02	t=2.88 p= .01	t=2.82 p= .01	t=2.99 p= .01
KdDq 11-6 Gak-1281 4460±100		t=1.95 p= .05	t=3.17 p= .01	t=5.32 p= .01	t=7.27 p= .01	t=4.38 p= .01	t=4.60 p= .01
KkDn 2 GSC-849 4140±130			t= .41 p= .67	t=2.21 p= .01	t=3.82 p= .01	t=2.76 p= .01	t=3.04 p= .01
KdDq 11 P-707 4080±66				t=2.78 p= .01	t=5.26 p= .01	t=2.85 p= .01	t=3.13 p= .01
KeDr 1 P-708 3814±69					t=2.42 p= .01	t=1.58 p= .11	t=1.92 p= .05
KdDq 10 P-710 3577±69						t= .46 p= .64	t= .84 p= .39
KdDq 13 M-1531 3480±200							t= .31 p= .75

p is the probability that the dates are coeval.

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.67, making the odds two to one; the probability that KdDq 10 (Loon) and KdDq 13 are coeval is .64, again almost two to one odds; and the probability that KdDq 13 and KdDq 23 are coeval has risen to .75, which is three to one odds. This analysis may be interpreted as suggesting that several of the dates are statistically indistinguishable from each other, in spite of the up to 90 year difference in their measures of central tendency, which should be a caution to those prone to accept radiocarbon dates uncritically.

I have not averaged this series of dates from the Closure site because I have no evidence that we are dealing with a single occupation of contemporaneous structures. Averaging would not reduce the initial variability of the dates unless we assume all of the dates to date the same event (a discrete event in time) for which there should be only one year in real time. This would be like trying to establish an average date for World War Two (for example) which would yield a false sense of increased precision and contribute another source of bias in our assessment of the dating of these occupations. Averaging of radiocarbon dates is not always appropriate and should be used <u>only</u> in those circumstances where a case may be made for the precise contemporaneity of the samples dated. Otherwise, we are reducing variability based on a priori assumptions which may not be valid. If averaging is used, then the case for contemporaneity should be made explicit and should

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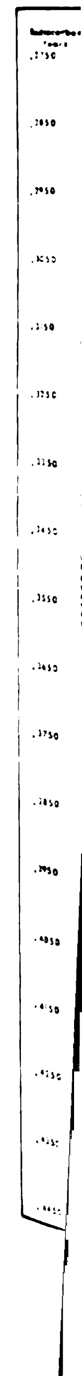
be based on data which are distinct from the radiocarbon dates themselves. In the absence of data indicative of contemporaneous occupations, averaging is not justified.

Figure 5 is a summation of the chronology for the Arctic Small Tool tradition, plotting all radiocarbon dates with one standard deviation and placing undated sites in approximate temporal positions. Figure 6 is a recasting of the same data as Figure 5, with a selective adjustment of 10% more recent in time (reduction of 10% in conventional radiocarbon years) to compensate partially for the apparent greater age of those samples based on sea mammals. Those dates adjusted in this manner, have had an arrowpoint added to the more recent end of their deviation plotted in the figure. In addition, dates on driftwood, which may also date several centuries earlier in time than samples from indigenous growth, have been labeled with a "D" and dates obtained on antler, which is also thought to yield anomalously early dates, have been marked by an "A".

It should be obvious that the manipulation of these series of dates has almost infinite possibilities, with really very little data to validate some of the manipulative possibilities. I have attempted to cover some of the more relevant sources of variance, but it is apparent that one may cook these data until the consistency provides a good fit with whatever predelictions one has. We have failed to deal deliberately and appropriately with the variability inherent in the dating processes which we have



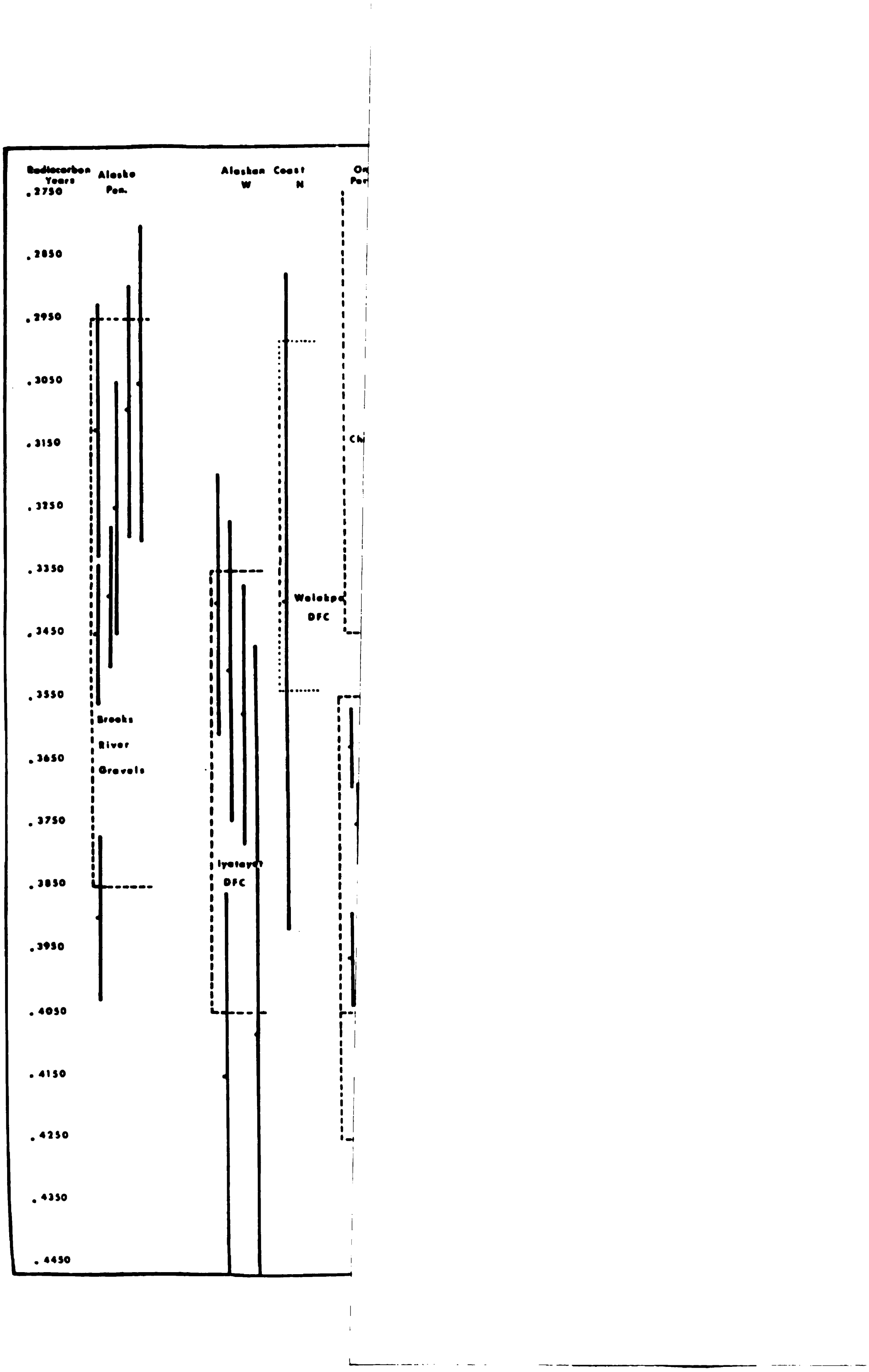
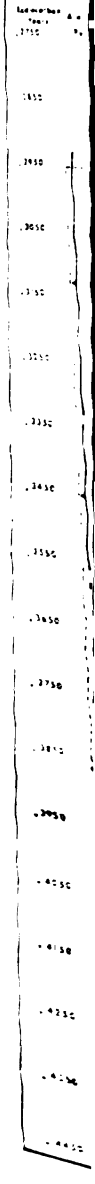


Figure 5 A Provisional Chronology of the Arctic Small Tool Tradition



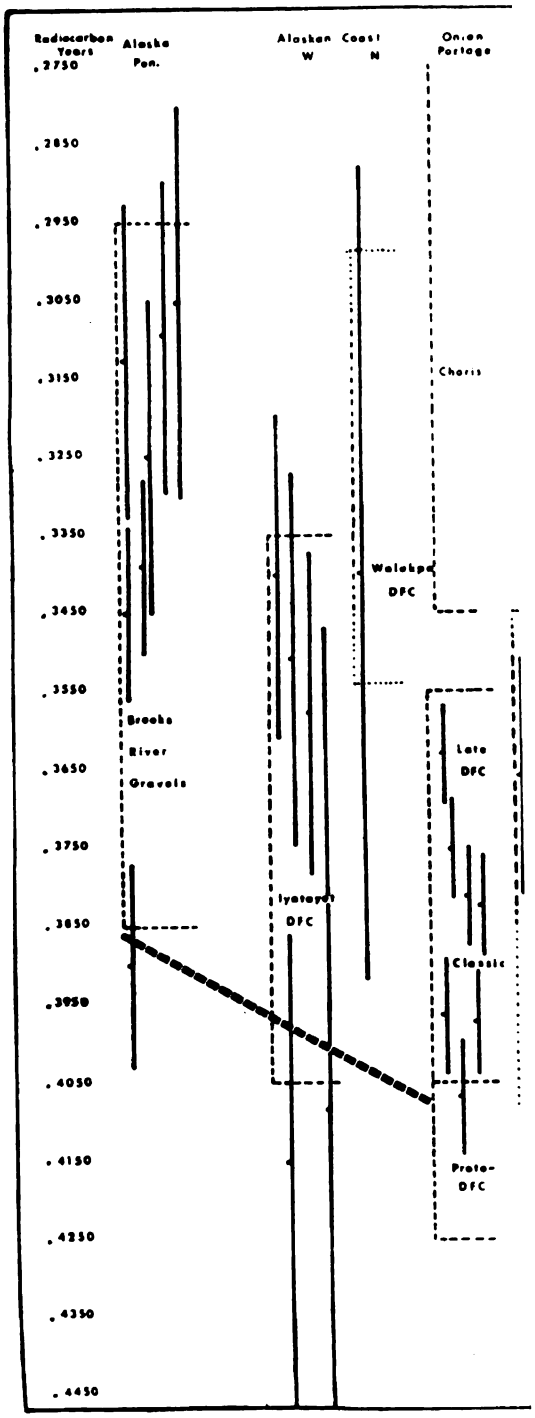


Figure 6 An Adjusted Chronology of the Arctic Small Tool Tradition

used, and such failures have contributed to the variation in archaeological interpretations. It is essential that we learn to understand the dating processes and deal with the statistical probabilities which they involve. We must make explicit the adjustments which we make in the data for dating, whether this be pre-treatment of the sample for radiocarbon dating or adjustment of the statistical descriptors of the dates which we receive. We can no longer ignore the statistical variations in our dates, and must face the statistical imprecisions as part of a more precise strategy of chronology building.

I justified the 10% reduction in radiocarbon years for dates on charred fat (seal?) by noting the logical consistency and noting that such a reduction removes a number of anomalies in the dating (the great antiquity of the Baffin Island Pre-Dorset, when compared to presumed earlier complexes in the Western Arctic for example) while causing no apparent anomalies of its own. While this reduction of 10% may not be a precise estimate of what such an adjustment should be, we would be fooling ourselves to believe that we can be more precise, given the present data.

I have not used (and will not use) the Suess Tree Ring Calibrations as a basis for comparison nor as a framework for the depiction of data, especially in light of the growing doubt that the "kinks" of his curve will be substantiated by additional data and that these kinks are applicable for all samples. Wendland and Donley have prepared a smoothed curve of the relationship between radiocarbon years and calendar years (1971), but this does little more than adjust the "spread" of distributions already established by the use of radiocarbon years alone, or converted to "AD/BC" by the subtraction of 1950 years.

Perhaps we would benefit by responding to a plea for greater explicitness and precision in our attempts at establishing a chronological framework, which parallels a recent plea aimed at students of Early Man in the Americas:

> I wish to end this critical review with a friendly plea to my fellow specialists for rationality in our search for Early Man: (1) we should be more cautious in the use of dating techniques, geomorphological context, and presumed associations and correlations; (2) we should rely less upon preconceptions about technological and typological progression; (3) we should broaden our sense of historical perspective, being wary of former mistakes with quarry and surface sites; and (4) we should stop arguing so confidently where the evidence is weak. Perhaps, if we stop looking so hard for Early Man, we shall someday find him (Lynch 1974:375).

If we are to understand the sources of variation in Pre-Dorset behaviors, we must make our chronological reconstructions as explicit and precise as possible so that variations in our understandings do not result from imprecise chronologies. Any attempts at modeling behavioral change are dependent upon a chronological framework, and the models presented in Part III follow from this discussion.

Chapter 3

The Changing Environmental Setting

Introduction

Any attempt at paleoenvironmental reconstruction is based on the postulation of a uniformitarian principle--that the present environmental processes were operating in the past through geological time (Barry and Perry 1973:349). The validity and reliability of this postulate are supported by recent studies which have considered historical data and the extension of trajectories of change both forward and backward in time from periods in which historical data are available (Lamb 1964, 1973 etc.; Le Roy Ladurie 1971; Johnsen et al. 1970; Dzerdzeevskii and Sergin 1972). This use of the present and recent past as explicit and implicit models for depiction of past environments is widespread, and forms the basis for this and other studies.

The general structure of climatic and environmental changes in the North American Arctic is known, and there was apparently relatively little change after the tundra biota had been established following the withdrawal of the continental ice sheets (Bryson et al. 1969). During the periods of occupation by the Arctic Small Tool tradition, there was little general change in the Arctic ecosystem, and what changes there were were subtle when compared to the major successional changes in southern North America, and are thus somewhat more difficult to discover and to interpret. Major changes in the Arctic ecosystem are documented by ecotonal changes, such as the location of the tree line, the variance in peat communities, the changing distribution of marine fauna, etc. (see, for example, Nichols 1967a, b, c; Noble 1974; Fredskild 1973; Sorenson and Knox 1974; Matthews 1967a). However, as Bryson et al. have pointed out, ". . .there must be many climatic-biotic core areas with very little change" (1970:72).

Thus, we may be able to specify the nature of the general climatic changes which affect the entire Arctic ecosystem as part of synchronous global changes (see Bryson et al. 1970; Dekin 1972a, 1972b), with greater reliability in ecotonal areas. The negative evidence for changes in some areas is somewhat more difficult to interpret.

Where limits to distributions of biota are not clearly defined and are subject to great local variation (Polunin 1948:3), we have not been able to specify the precise nature of environmental changes on these distributions, and negative evidence favors the interpretation of environmental complacency. The ecological relationships among environmental variables, while generally known, are not precisely defined, especially with regard to time-delay factors associated with related changes (attempts have been made to relate these changes and to model the delay in responses--Bryson et al. 1970: Fig. 14; Nichols 1967a: Figs. 4,5; McGhee 1972a:54; Miller 1973:574-5). Barry and Perry have suggested that vegetation responses to climatic change may take from 10 to 100 years to respond, while major ice caps and ice sheets may take thousands of years (1973:350).

Miller has demonstrated the general relationships of glacial response to climatic change, with particular attention to the problem of dating these relationships.

> Whereas glacier activity is determined by the magnitude and duration of climatic change, the apparent age of a moraine from which we infer glacier activity is a function of the response of the glacier to climate and the dating technique used. Arctic glaciers are generally more sensitive to small climatic shifts than are more southerly glaciers, but have longer response times, which in turn effects the apparent age of the moraine. . . . A climatic deterioration of greater magnitude is required to affect southerly glaciers than for Arctic glaciers, but the response of low-latitude glaciers is more rapid due to the high mass turnover involved (Miller 1973:574-575).

The timing of this delay is difficult to ascertain, because the errors associated with radiocarbon dates (see above) are larger than the apparent time-delay factor for vegetation, and we cannot really eliminate this imprecision. The technique used by me in previous papers (1969, 1970, 1972b) was to observe plots of dates of logically and ecologically related environmental events, drawing a "best-fit" distinction between sets of dates on either side of the observed boundary. A more sophisticated and less judgemental technique was used by Bryson et al. to establish a global sequence of climatic changes, in which sample deviations were minimized and regionally significant changes were suppressed in favor of more widely significant changes (1970). However, the Eastern Arctic and North Atlantic regions have been subject to greater climatic fluctuations in historic time than have other areas of the northern hemisphere (Lamb 1966:58,94,171,201) and may be expected to demonstrate environmental changes which were not synchronous with other parts of the world in light of this greater sensitivity. We would expect the environmental episodes of the Eastern Arctic to be synchronous with major changes of global extent and to contain additional variations of more local significance, thus presenting a complex sequence of environmental changes.

While the general character and distribution of Arctic biota are known, specific population densities, distributions, and fluctuations are rather poorly known. The available data vary from the rather extensive study of Vibe (1967) of the utilization of Greenlandic fauna (albeit in a managed economy) to Boas's brief description of the availability of walrus and caribou (1964:53-54). Studies by the Fisheries Research Board of Canada have described the historic distribution of pinnipeds (Mansfield 1959, 1964) and other sea mammals, while recent studies of the Canadian Wildlife Service have considered muskoxen and caribou (Tener 1965; Kelsall 1968), although neither is complete for any period of history and a precise paleo-geography is not

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not yet possible. Correspondence with Arthur Mansfield indicates that we must generate the prehistoric distributional data ourselves, and cannot expect independent data from their studies.

Within the limits of reliability apparent from these scattered studies, we still obtain a generally reliable picture of the distribution of Arctic biota which is useful as a general model of the biotic environment to which prehistoric Arctic human populations adapted. These data will be used below in an attempt to specify the spatial variations in relevant biotic distributions and to look at possible changes in these distributions and characteristics through time.

Space--Variations on an Arctic Theme

The general picture that emerges from the study of the last five thousand years of Arctic occupations is one of human populations with subsistence adaptations flexible enough to adapt to almost any regional or temporal variation of the Arctic tundra and coast. Attempts to dichotomize adaptations into coastal and inland adaptations have proved far too simple even as heuristic devices because the totality of knowledge and adaptive experience of any of these cultures under study was adequate for the maintenance of an efficient subsistence base (see Taylor 1966). No matter how we categorize the regions of Arctic Small Tool occupation, it is clear that these people had a successful adaptation which allowed their movement to the fullest extent of this Arctic coastal-tundra environment.

In general, the Arctic Small Tool tradition was confined to the Eskimoan Biotic Province (Dice 1943) with sites located on the tundra (exceptions include river valleys in Alaska where sites may have intruded into the boreal forest) (see Figure 7a) and on the coasts. Coastal sites appear to have been limited in their distribution to those areas where winter sea ice was prevalent (see Figure 7b) (see Dumond 1969, etc.). The coincidence of cultural tradition, Arctic climate, and biotic province is striking and we should not lose sight of these generalities when focusing on specific variances. The following discussion will focus on what is known of the most important resources available in this region, while slighting much of the lower trophic levels on which the food web is based. In part this is necessitated by the lack of specific data on many biota from this region and by the rather obvious point that some species were more important to historic and prehistoric Eskimos than were others. These twin constraints of data availability and relevance focus our attention on land mammals (caribou, musk ox), sea mammals (seals of various species and walrus), and fish. While most of the species in the above categories are distributed across the North American Arctic, there are significant differences in their frequency and availability, and these will form the basis for the following discussion.

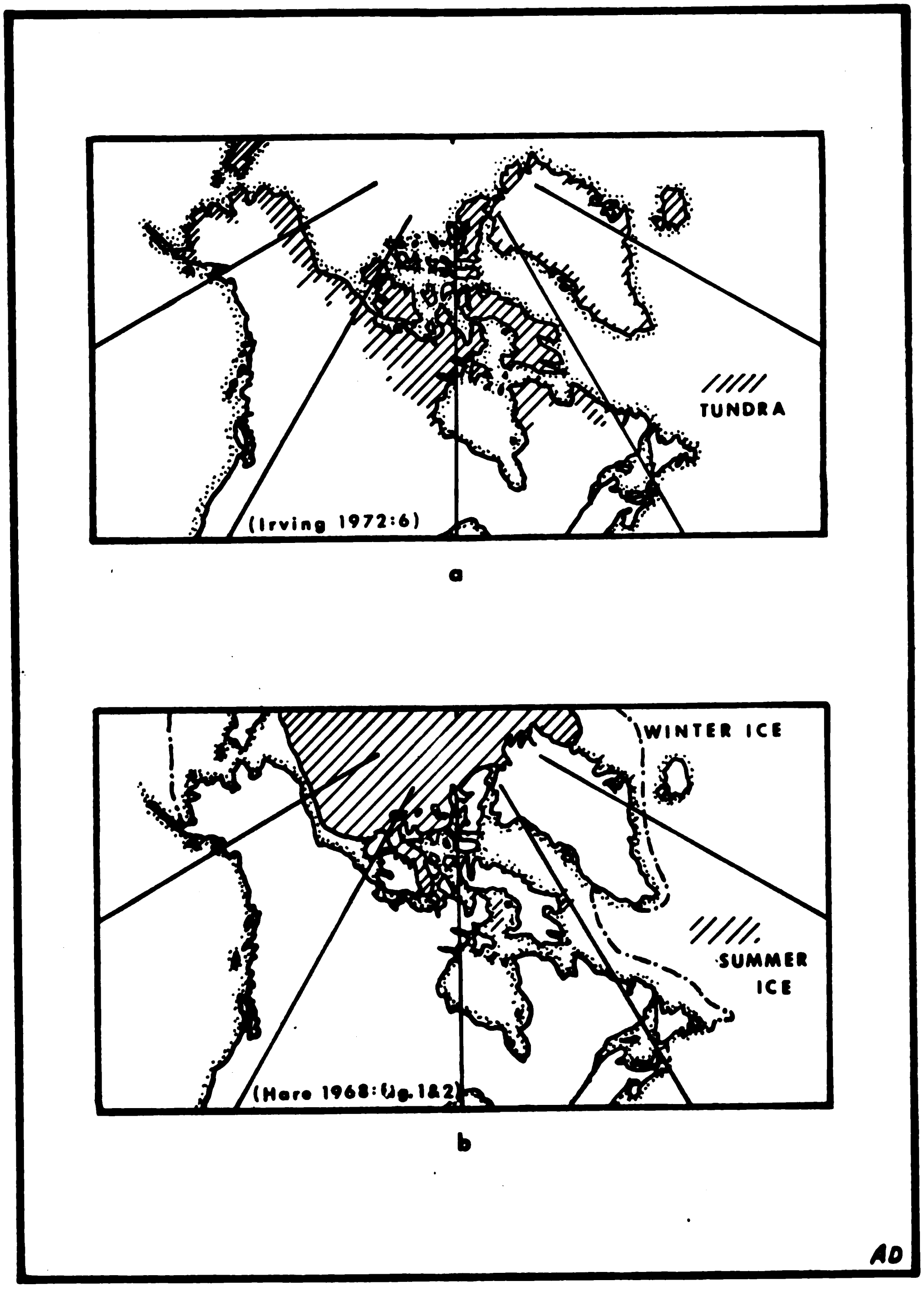

Figure 7 North American Arctic--
Tundra and Sea Ice

The researches of Vibe (1967, 1970) have demonstrated the drastic limitations that weather can force on the distributions of Arctic fauna. The long term patterns of weather which we call climate also have long term limits. The circulation pattern in the Eastern Arctic is dominated by the southward flow of air from the Arctic Ocean and High Arctic Islands (Bryson 1966), with occasional depression tracks penetrating the region from the south and west. Southern Greenland is more affected by such storm systems, especially since it juts southward into the North Atlantic, penetrating the extensive low pressure system near Iceland. Figure 8 is the mean contours of the 500-mb surface (with elevations in km) for January and July (from Hare 1968) which demonstrates the similarity in tropospheric conditions over the Eastern Arctic in both summer and winter. Of particular interest is the prominent trough extending from Ellesmere Island to Foxe Basin.

The surface pressure means in this region document the prevalence of low pressure systems crossing the general triangle bounded by Davis Strait, northern Labrador, and Southampton Island (see Barry and Chorley 1971:140-141) in the summer and their relative complacency in winter, then dominated by more or less permanent highs. The prevalent tropospheric trough over Foxe Basin is significant in influencing long-term meteorological continuity in this locale.

Frequencies of storms, wind directions, form of

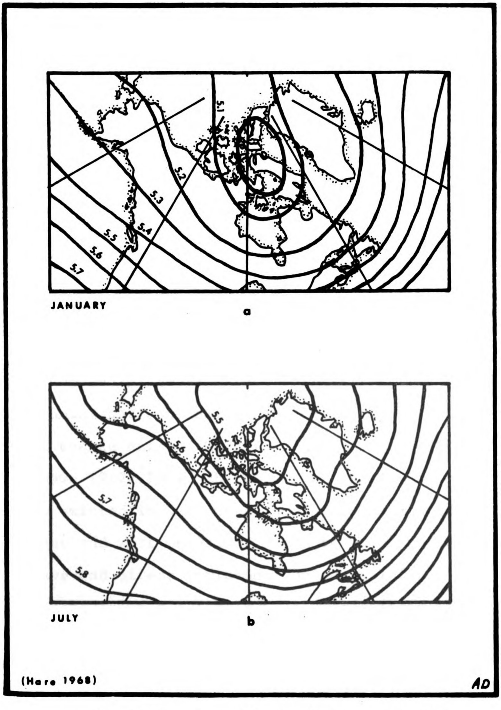

Figure 8 Mean 500-mb Contours (in Km)

precipitation and temperatures all have a direct impact on the environment, particularly on surface phenomena such as snow or ice cover, wave characteristics, water temperature and current direction and force (Foote and Greer-Wootten 1966; Breverton and Lee 1965; Nelson 1969:34-53; Vibe 1967). The marine environment is directly influenced by the above variations in meteorology, and there is considerable evidence for the variation in current strengths off West Greenland in changing the marine conditions resulting in the historic increase in the availability in cod (and the decline in seals and arctic fauna) (Vibe 1967; Jensen 1939; Dunbar 1951). Variations in the relative strength of Arctic currents have been suggested as explanations for changes in marine fauna and in ocean bottom sediments (Alverson and Wilimovsky 1966:855; Vilks 1970:108-109), yet the general pattern seems to have been one of relatively little change during the period under consideration here (Matthews 1967a:192). Perhaps the greatest potential for change existed in West Greenland, where the strength of the Irminger and West Greenland currents may have been variable and have had direct influences on coastal fauna. These have been the most variable of Eastern Arctic currents (Vibe 1967; Dunbar 1951; Breverton and Lee 1965), and would have had their greatest impact on the environment of West Greenland. At the opposite extreme is the area of Foxe Basin, Hudson Bay and straits, where the currents have probably not been subject to major changes (Dunbar 1968:49), with water flowing from the Arctic Ocean dominating a southward flow through Foxe Basin (see Figure 9; the area marked "CORE AREA" is between 70 and 90 degrees West Longitude and 60 and 70 degrees North Latitude, which approximates an area of Pre-Dorset and Dorset cultural continuity and efflorescence), mixing the freshwater from the southern rivers in Hudson Bay, and flowing out into the Atlantic through Hudson straits. The major zone of mixing of Atlantic (Sub-Arctic) and Arctic waters occurs in Hudson Straits where a westward moving current along the South coast of Baffin Island curves southward into the Straits off Big Island mixing with the eastward moving waters from Hudson Bay and Foxe Basin. The strength of this westward current may have varied in intensity, causing increased Atlantic water further West in Hudson Straits, but the coastal configuration near Big Island has meant that considerable mixing in this area was a consistent oceanographic feature, even though additional mixing may have occurred further to the West along this coast. This mixing of water produced increased amounts of marine growth at all trophic levels (see Dunbar 1968). Thus, the Foxe Basin--Hudson Straits region has probably not been subject to drastic marine changes during the time period under study here, and the marine waters near Lake Harbour have supported a somewhat richer fauna than other portions of the Hudson Straits (Dunbar 1951; Soper 1928). The coincidence of relative complacency in the meteorological and oceanographic systems over Foxe

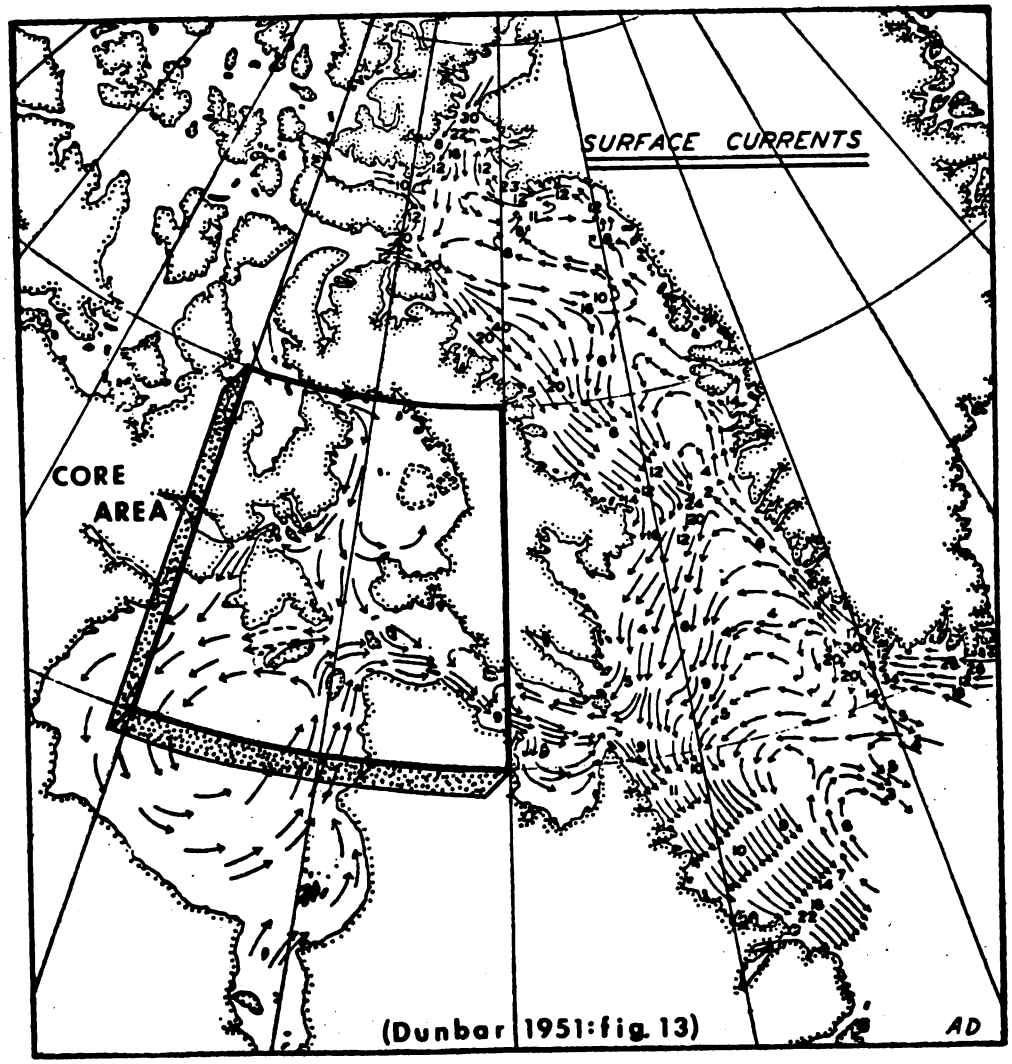

Figure 9 Surface Currents of the Eastern Arctic

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Foxe Basin and Hudson Straits will be of great importance in the definition of an ecological core area in the Eastern Arctic and in the relation of such an environmental core to regional culture history (see below).

In recent times, the economy of the Eastern Arctic has rested mainly on the seal hunt, and it is for this reason that the distribution of seals is best known among Arctic animals. Very little is known about the specific timing and local availability of birds, which we will have to assume as being of rather minimal influence on the distribution of Arctic peoples. This follows from their position in the subsistence pattern as being of supplementary use when available (Usher 1970:80; Bissett 1970:102; Villiers 1970:70; Higgins 1968:172).

Unfortunately, we will have to relegate fish to the same position, as most of the larger rivers in the Eastern Arctic had runs of Arctic char and most coastal lakes of any size had char in some numbers (Scott and Crossman 1973: 203). However, there is no evidence that the distribution of either these rivers or lakes with their fish resources is not coincident with the Eskimoan biotic province discussed above (Scott and Crossman 1973:203).

While there are a number of species of fish that may have achieved significance in isolated locations and on rare occasions when other resources may have failed, there were only a half dozen or so which could have been reliable subsistence bases for any number of people for any length of t

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of time. These include the Arctic Char, the Lake Trout, the Arctic Cisco, the Lake Whitefish, the Arctic Grayling, and the Longnose Sucker, all of which have been taken by Arctic residents in historic times (Scott and Crossman 1973). Other resources may not have been used because of technological limitations, ignorance, forbidden behaviors (taboos), or the availability of more desirable (efficient?) alternatives.

Of those mentioned above, the Arctic Char has been the most significant in recent years across the Canadian Arctic, while commercial fisheries of char and whitefish have developed and grayling and lake trout have been of interest to sport fishermen. The Longnose Sucker seems to have been widely used as dog food (Scott and Crossman 1973:535) and only slightly as a commercial fishery.

Figure 10a is the distribution of Arctic Char (Salvelinus alpinus) at present, which covers all of the northern coastal regions, except that ". . .arctic char does not usually range far inland except in the larger rivers" (Scott and Crossman 1973:203). Most char make their way between inland lakes and the sea several times in their lifetime, but some populations are landlocked. The limitation to coastal rivers and lakes is apparently because "Char cannot leap like Atlantic salmon and depend on moving in with the tide to surmount obstacles" (Scott and Crossman 1973:204). The isostasy of Arctic land forms has meant that landlocked char populations are quite frequent.

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The Lake Trout (Salvelinus namaycush) is a land locked "char" which is extremely intolerant of salinity, and is generally confined to lakes (see Figure 10b) (Scott and Crossman 1973:223), where it is most frequently caught through the ice or netted (see Villiers 1970:55).

The Arctic Cisco (Covegonus autumnalis) is an anadromous species spending much of its time in marine coastal waters, but running up Arctic coastal rivers in some numbers to spawn, where it has been caught by native peoples (Scott and Crossman 1973:245-246) particularly in the Mackenzie River. While its distribution in North America is limited (see Figure 11a), it is of potential interest along the Western Arctic coast.

The Lake Whitefish (Covegonus clupeaformis) is widely distributed in the lower Arctic (Figure 11b) where it has become one of the most valuable commercial freshwater fish in Canada. It has been netted in historic times and can be caught by hook and line, but their preference for deeper water (at least in southern lakes) suggests that they may not have been readily accessible to Arctic fishermen.

The Arctic Grayling (Thymallus araticus) (Figure 12a) has been used by both Indians and Eskimos in recent times, most frequently for dog food when trout or whitefish were scarce (Scott and Crossman 1973:304). They are found in lakes, large rivers, and rocky streams. As surface feeders, they may have been more easily accessible to fishermen with a limited technological ability, and thus a significant alte

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alternate resource when more efficient ones were scarce.

The Longnose Sucker (Catostomus catostomus) while not usually mentioned as a significant resource, was taken by native peoples in varying amounts, and it is ". . .used everywhere as food for dogs, but even they prefer lake whitefish" (Scott and Crossman 1973:535). In southern areas, it is more prevalent than trout.

It is pertinent to note that I know of no instance where the lack of fish resources in any particular locale has been noted as an adaptive problem to either historic or prehistoric inhabitants of the Eastern Arctic (for example: Rostlund 1952; Birket-Smith 1928; Boas 1964; Soper 1928: 116; Graburn 1969:22; Balikci 1970:28; Knuth 1967:31; Graburn and Strong 1973:147). The relative insignificance of fishing to the historic and modern Eskimo economies (Usher 1970:81; Bissett 1968:96; Villiers 1970:70; Higgins 1968:172) has resulted in a dearth of specific information regarding the spatial distributions and frequencies of fish resources.

Land animals of significance include the musk ox and caribou, supplemented with rabbits and small carnivores.

The musk ox (Ovibos moschatus) distribution is not continuous within the Eskimoan Biotic province, being restricted to the Arctic Islands and adjacent mainland (see Figure 13; MacPherson 1965:Fig. 11). While the data on both historic and prehistoric were never present in significant numbers on Baffin and Southampton Islands, nor were present

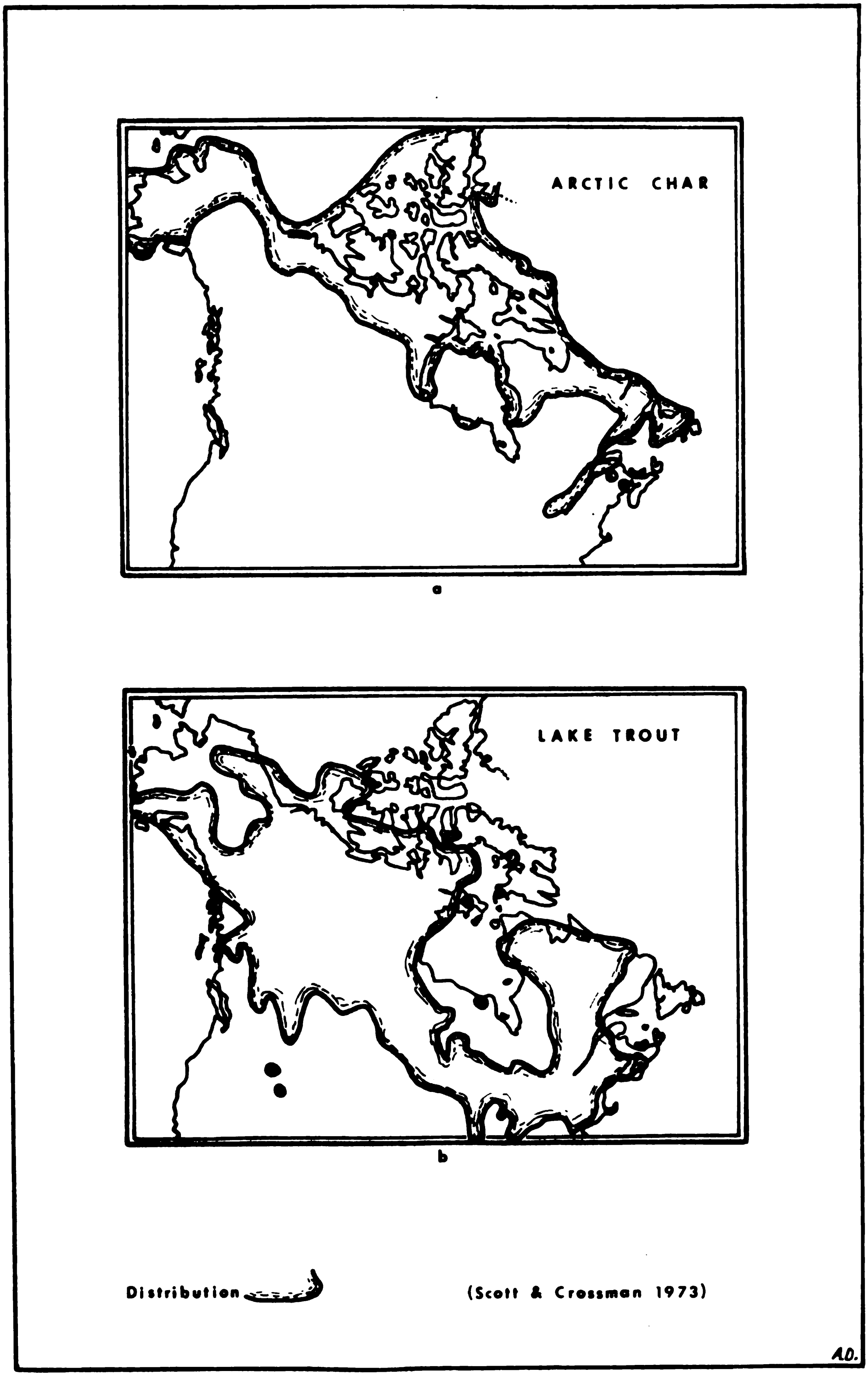


Figure 10 Distribution of Arctic Char and Lake Trout

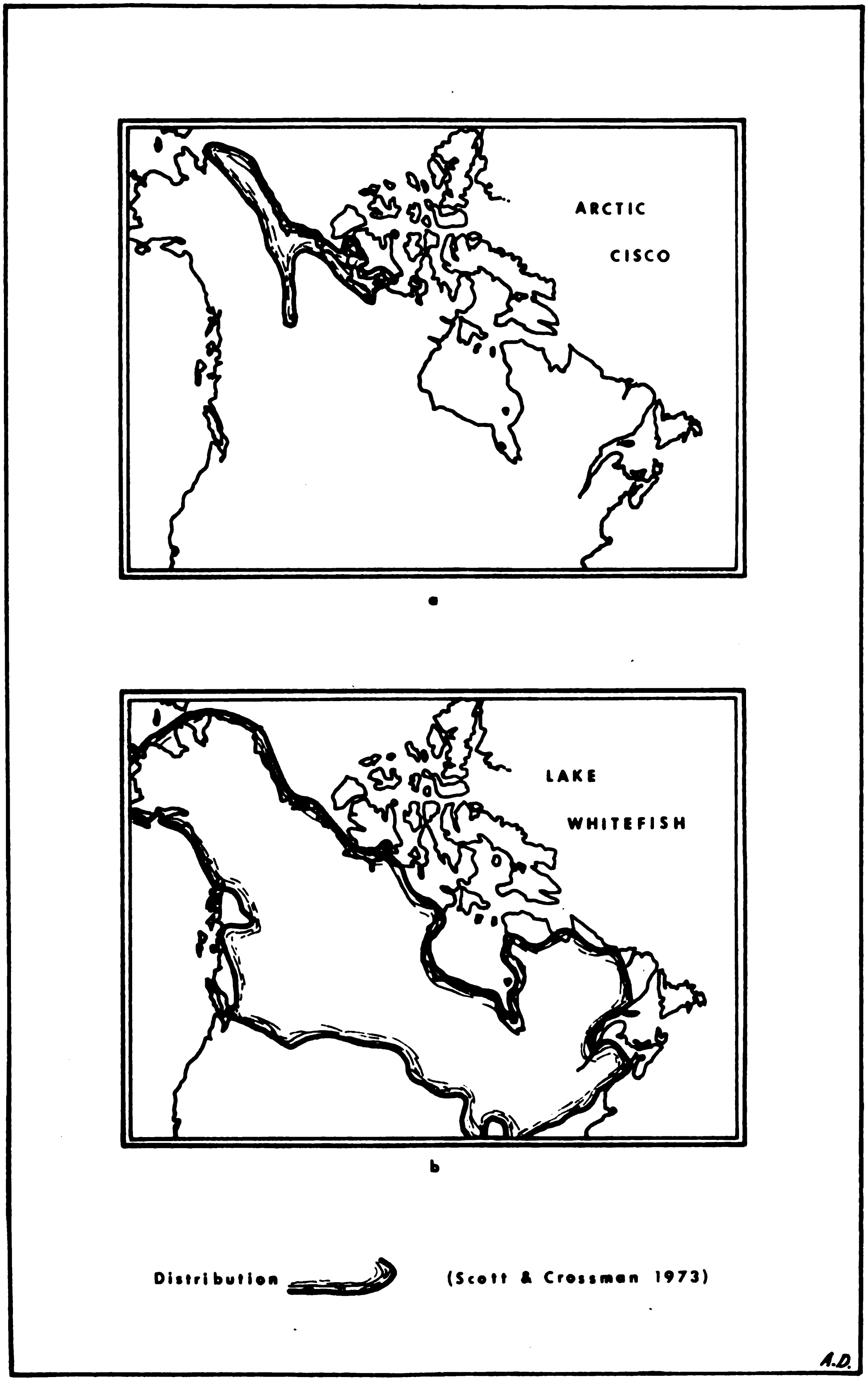


Figure 11 Distribution of Arctic Cisco and Lake Whitefish

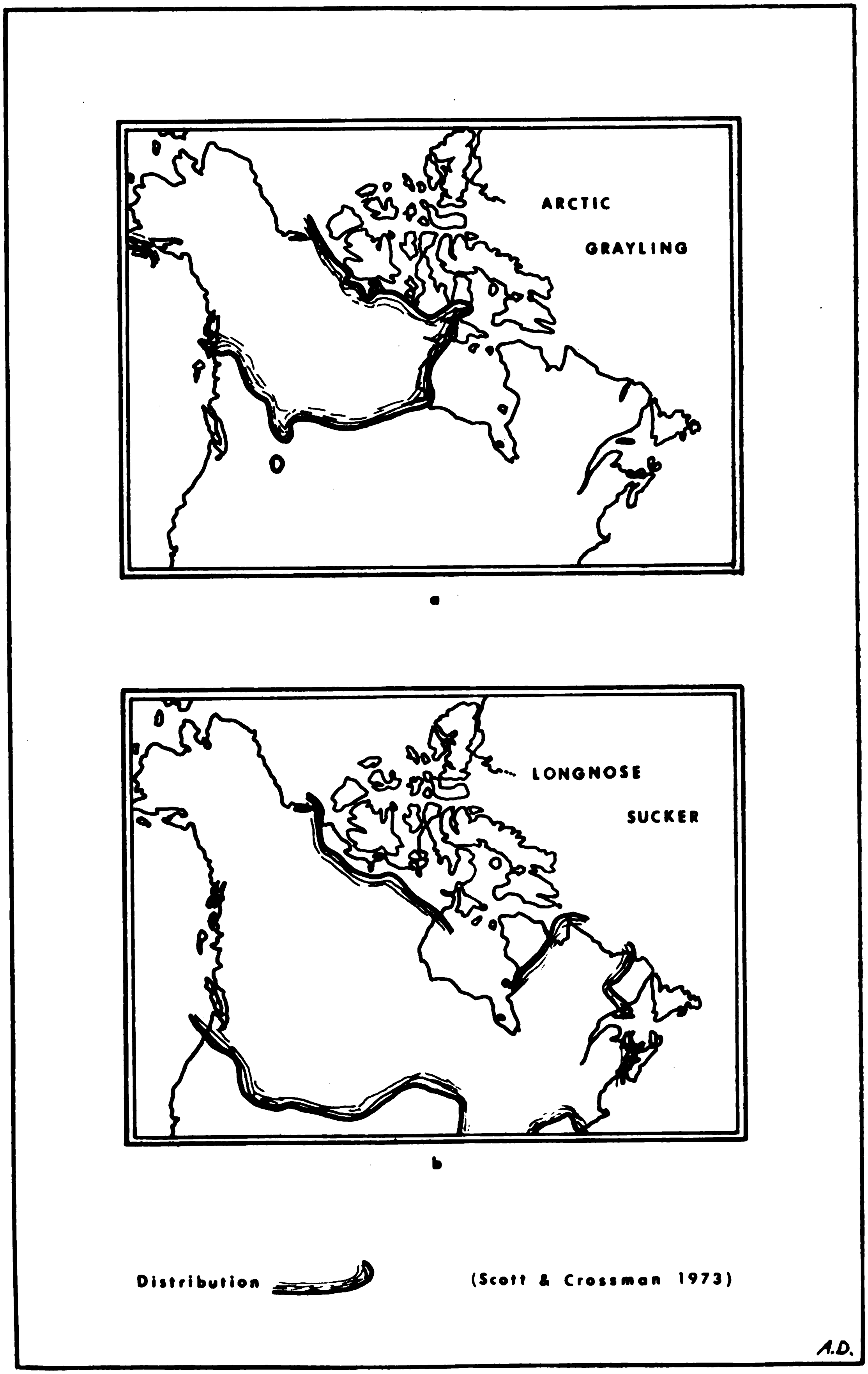


Figure 12 Distribution of Arctic Grayling and Longnose Sucker

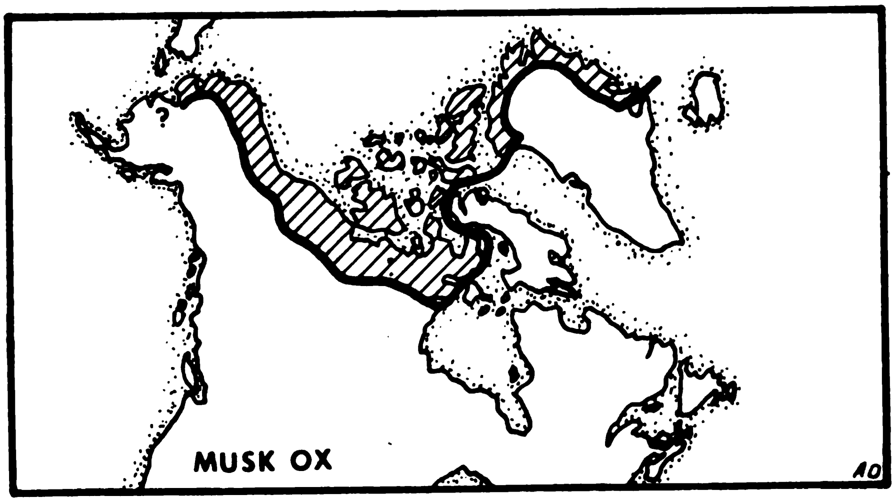


Figure 13 Distribution of Musk Ox

in any numbers far from the Arctic coast (these conclusions and the data for Figure 13 were compiled from: Bee and Hall 1956:253-54; Boas 1964:7,42; Flerow 1967:278; Irving 1972:84; Harington 1970:6; Manning and MacPherson 1958:67; Pruitt 1966:527; Soper 1928:28; Tener 1965:16; and Vibe 1967:181-192). Vibe and others have demonstrated that climatic changes and hunting have contributed to local extinctions of populations within the historic period, but there is no evidence to indicate that these changes have caused any drastic alteration of the range of this species, nor that such changes could explain the absence from Baffin Island, for example.

The ease with which musk ox herds were deplentished by hunting and climatic phenomena (Vibe 1967), suggests that while they may have been of supplemental value to human subsistence patterns, they were not of sufficient reliability to support an economic pattern focused on them. Even Knuth's "Musk Ox Way" (1967) did not demonstrate such a long-term focus, as the bone remains suggest a balanced use of fish, land fauna, and shore birds (1967:30-32).

The most important land mammal was the caribou (Rangifer tarandus), whose distribution filled the tundra (MacPherson 1965:Fig. 9), providing one of the most important sources of food and raw materials for both prehistoric and historic Eskimos. While the distribution of these animals on the tundra is uneven, irregular and unpredictable, they nevertheless provide an essential resource on which any successful hunting adaptation must depend (Taylor 1966; Soper 1928:63). While the habits of these animals vary significantly from locale to locale, all populations seem to have seasonal movements whose specifics are influenced by local environmental conditions and geography. Thus, the extensive migrations of the Central and Western Arctic mainland (Kelsall 1968; Irving 1972:85-87; Manning and MacPherson 1958:65; Manning 1960:7-10) are also reflected in the seasonal movements on the islands of the Eastern Arctic, with the exception that the herds were not as large nor were their movements as "coordinated" (Soper 1928:63-72; Manning and MacPherson 1958:65; Vibe 1967: 174-178). Several areas of the Eastern Arctic have received specific mention as containing unusually large numbers of caribou, especially during the summer, and these include Central District of Mackenzie (Kelsall 1968:46,47, Maps 11-24), northern Keewatin (Boas 1964:54), and the western plains of Baffin Island (Boas 1964:54; Soper 1928: 63-72). In historic times, the movements of caribou were frequently erratic and undependable, leading to several well known periods of hardship among Eskimos and local extinctions of caribou populations (Irving 1972:85; Vibe 1967: 163-180; Manning 1960:9; Kelsall 1968:17-18). However, throughout their range, Caribou provided one essential link in human subsistence and it is doubtful that any human occupation could exist well for any length of time without them. While the stochastics of their availability are not known, subsistence strategies that took this uncertainty into account were apparently successful.

The element that best captures the stereotype of what is "Eskimo" is perhaps its maritime economy based on the hunting of sea mammals. Even though Taylor has drawn attention to the omnivorous character of Eskimo subsistence (1966:119), it is still its perspective on the sea and its fauna that is a part of almost everyone's definition of Eskimo. Among marine mammals, the ringed seal (Phoca hispida) stands out as "the one great and unfailing standby of the Eskimos the year round" (Soper 1928:63), being found throughout the range of historic Eskimos wherever there is fast ice for breeding (Mansfield 1964:17) (see Figure 14). Variations in the distribution of ringed seal stem from variegations in the complexity of coastline and in the stability of sea ice, with highly convoluted coasts with stable sea ice being the most productive of ringed seal populations (McLaren 1961; Smith 1973:50). These highly productive areas have also functioned as sources of animals spreading to other regions where hunting pressures or general lower productivity had caused a depression in population below that which the food chain could support (Smith 1973:50).

The bearded seal (Erignathus barbatus) also shares the widespread distribution of the ringed seal (see Figure 15), but is much larger and does not compete in the same niche of the wood web (Mansfield 1964:19,23). As a comparison of

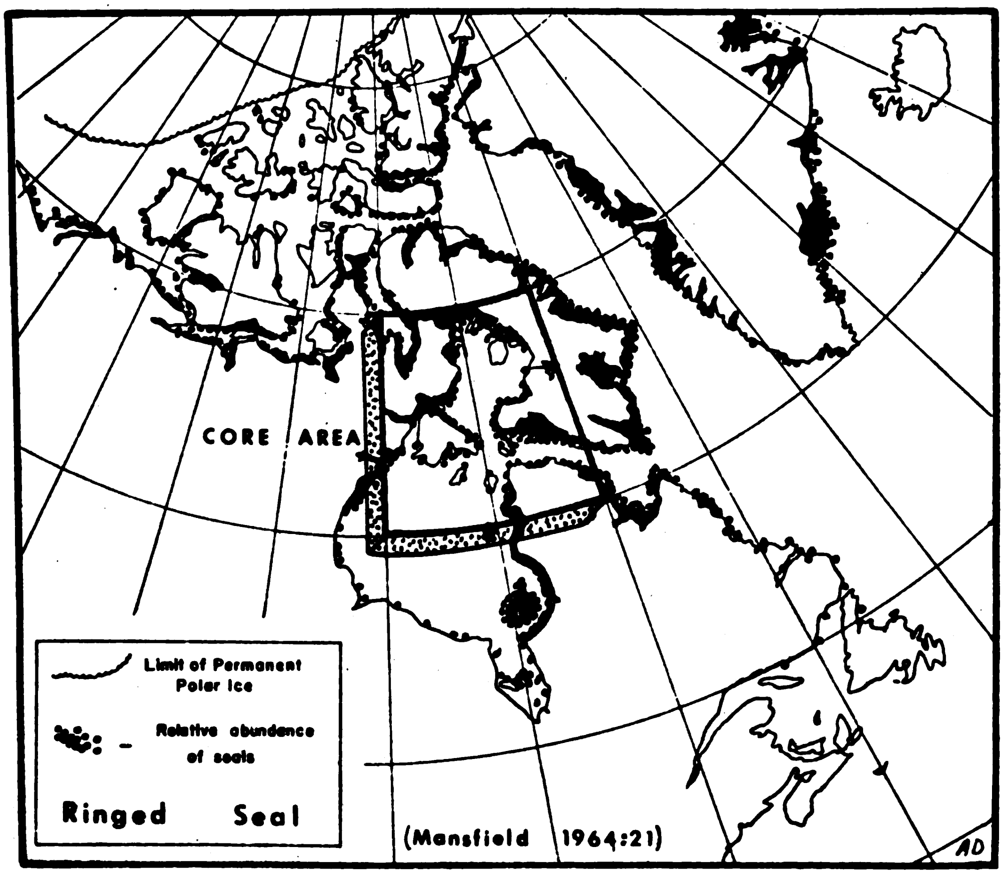


Figure 14 Distribution of Ringed Seal

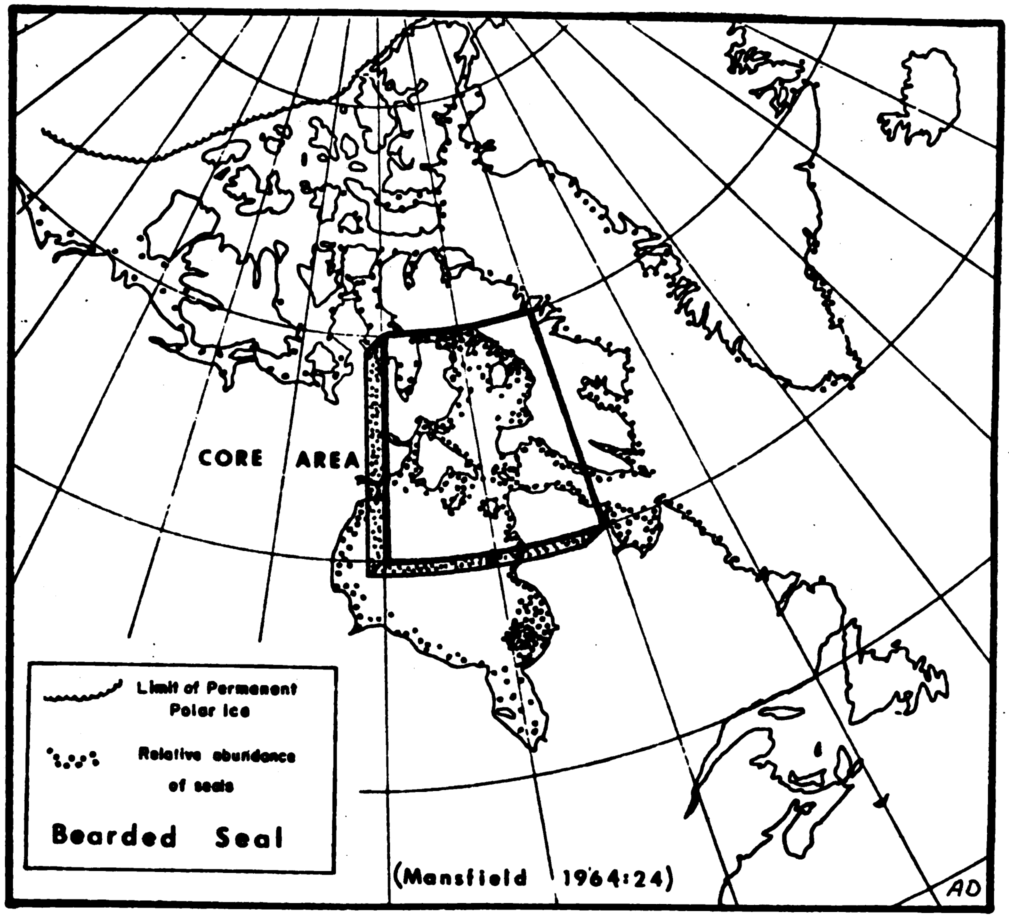


Figure 15 Distribution of Bearded Seal

Figures 14 and 15 may suggest, the bearded seal is not as prevalent in the Eastern Arctic as the ringed seal, but the toughness of its pelt has made it highly desirable for special purposes such as boot soles and line.

The harbour seal (Phoca vitulina) is a fish eater widely scattered throughout the Eastern Arctic (see Figure 16) occurring in numbers in areas where local conditions create small areas of open water throughout the year (Mansfield 1964:4). Again, its numbers are much less than the previously mentioned seals, but its distribution seems to approach theirs.

The harp seal (Phoca groenlandica) is a summer resident of the Arctic, migrating north from breeding areas near Newfoundland (see Figure 17). Their breeding pattern and food habits serve to differentiate their habitats and locations from other seals, as they seem to swim in offshore schools, and have been most easily taken by nets (Mansfield 1964:11-13). While they may be of economic interest in a very few locations in the Arctic, their importance is overshadowed by the year-round seals and by the fact that summers are times when other resources are also available to Arctic hunters.

The general pattern of Arctic seal distribution seems to be one of widespread distribution of fairly even populations with occasional dense populations in areas particularly attractive to the habits of the particular species. It is perhaps not inappropriate to compare the distribution

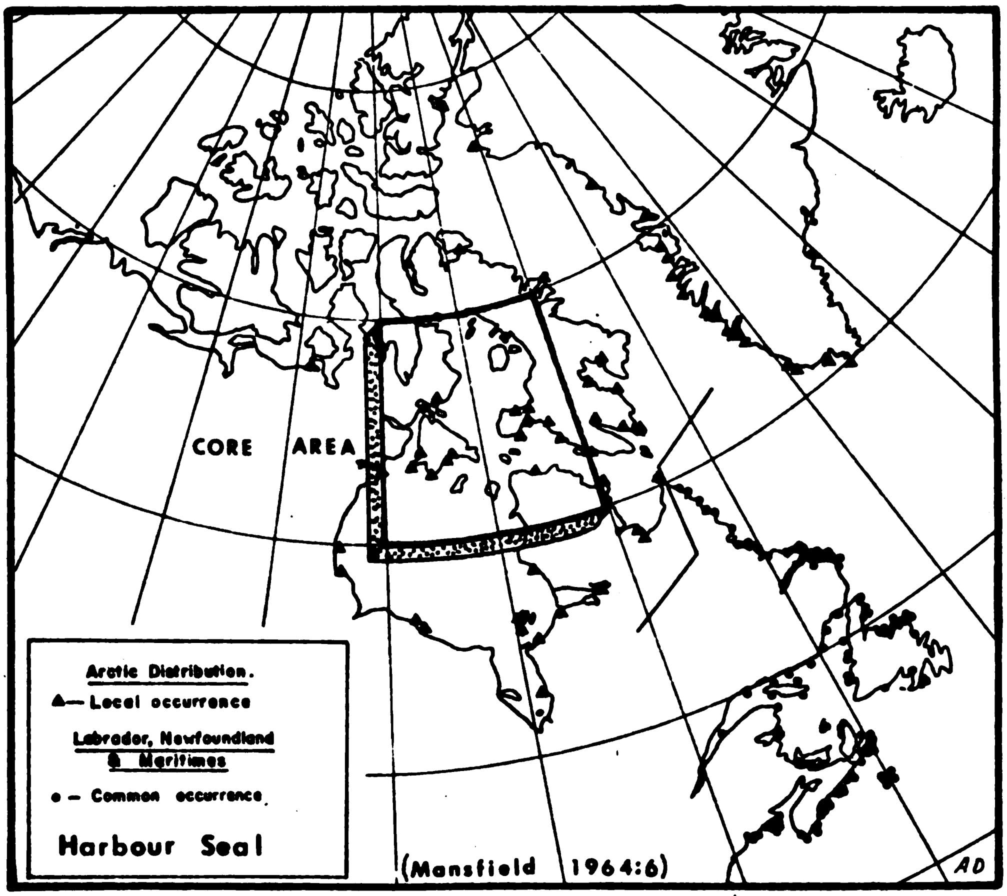


Figure 16 Distribution of Harbour Seal

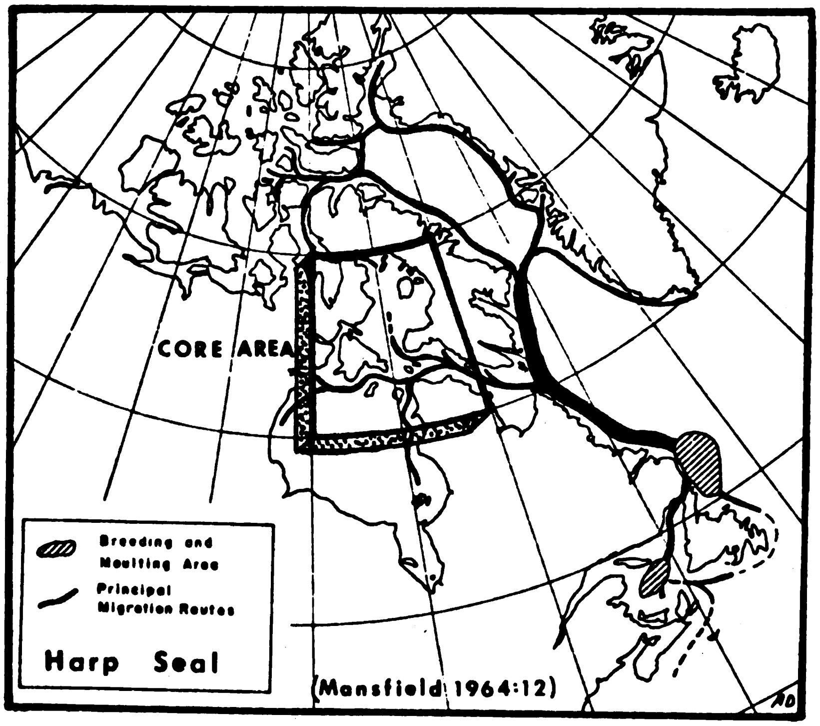


Figure 17 Distribution of Harp Seal

of seals to that of caribou, even though the caribou demonstrate high seasonal variation in availability, because both seal and caribou are ubiquitous throughout the Arctic.

One sea mammal which has been of significant economic importance to Eskimos is a notable exception to the above generally even distribution of sea mammals. The walrus (Odobenus rosmarus) is found occasionally in a rather wide range which approaches that of the ringed seal (see Figure 18), but it is highly gregarious with large populations in the relatively few areas of the Eastern Arctic where the shallow inshore areas are free of fast ice in winter (Mansfield 1964:25). When compared with the distribution of seals, walrus concentrations are restricted to the Foxe Basin--Hudson Bay area and northwest and west Greenland (Soper 1928:48-49) (Figure 18). Walrus are rare in the Central Arctic and in the northern and western Canadian Arctic archipelago and do not extend north beyond Kane Basin (Mansfield 1959:Fig. 1). Their distribution was once more extensive to the south, where eighteenth century hunting extinguished their populations (Mansfield 1964:28; 1966:89). Thus, the walrus has the most varied availability of the Arctic sea mammals which have been of major significance to historic and prehistoric Eskimo economies.

In spite of the limitations of the available data caused by lack of knowledge or by uncertainty regarding the impact of historic hunting on faunal ranges and local extinctions, a general pattern emerges. Musk ox and walrus

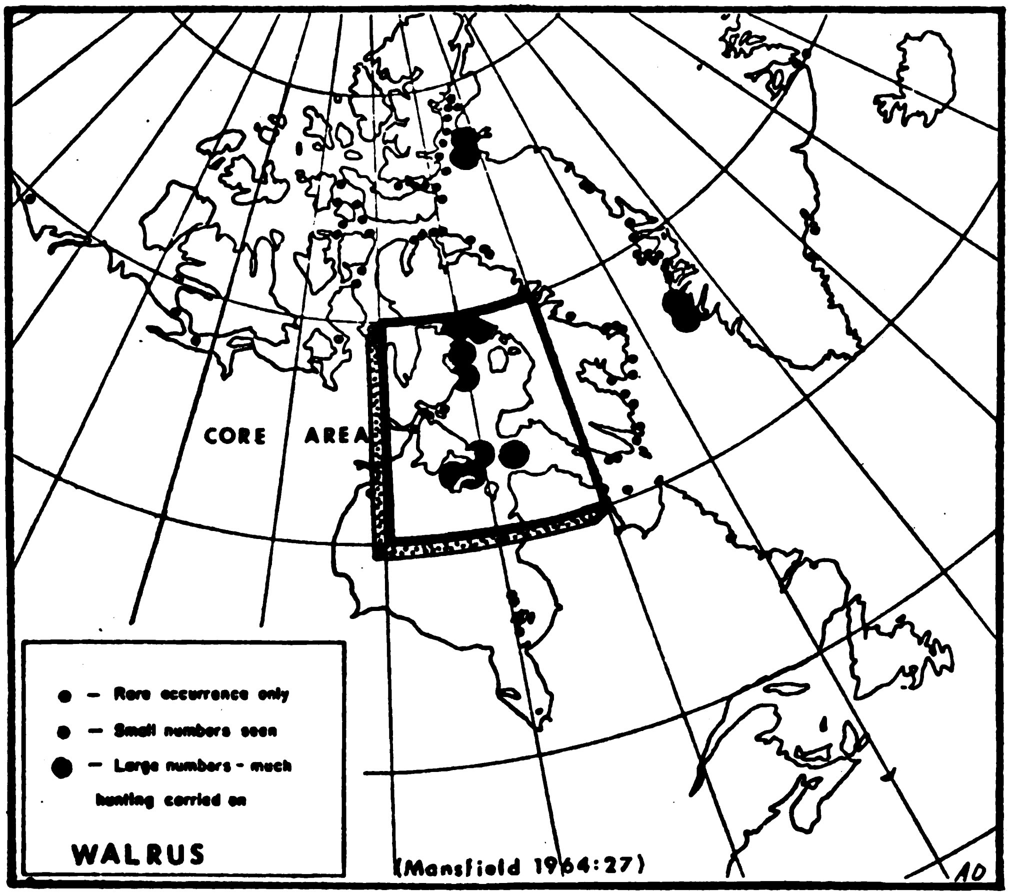


Figure 18 Distribution of Walrus

have variable distributions and are scarce or absent in large areas. Caribou, ringed seal and bearded seal are found throughout the Arctic, and provide ecological and adaptive continuity through space. The mainland migratory caribou and harp seal have significant seasonal movements which alter their regional availability, such changes being most significant in the southern portions of the Arctic areas. It is also remarkable that those areas of walrus concentration do not coincide with areas of musk ox availability, thus their distributions are complementary (compare Figures 13 and 18).

In summation, the biogeography of the Eastern Arctic consists of variations on a theme, the theme being the cold desert of the tundra -- a land of low productivity with severely disruptive environmental processes and variations which have precluded the evolution of stable climax communities (Johnson et al. 1966:279; Dunbar 1968:73,74; Margalef 1968:33; Irving 1972:15). Perhaps the most striking point, first expressed by Polunin (1948:3), is that within this general continuity there is extreme variability from place to place in which combinations of geological, topographical, meteorological and vegetational processes have produced micro-anomalies of environment and biota. Fortunately for our purposes, the biota of direct interest to us and to Arctic peoples have had sufficient mobility to adapt to locales or regions and are not restricted to microenvironments of limited distribution

(Flannery 1968:67). In the marine environment, ecological continuity is achieved through the influences of extensive currents, with confluences marked by an increased productivity, and of the generally more stable marine environment.

However, the variations on the above theme are of significance to Arctic hunters, as some areas have access to more frequent and reliable resources than do others. In general, the region surrounding Foxe Basin has access to an unusually diverse and reliable set of resources, both in the sea (walrus, seals, beluga, etc.) and on land (caribou, musk ox, etc.). The opportunity for utilization of a more complex (and hence adaptively stable) food chain in this region is unusual when compared to the remainder of the Eastern Arctic. At the other end of the complexity and reliability spectrum, both northern and southwestern Greenland have fewer alternative resources and demonstrably less reliability in their subsistence possibilities.

For our purposes, we may find an operational level between the macro-similarity and the micro-complexity, but the specification of such a level of analysis awaits the consideration of the problems with which we wish to deal.

Time--Continuity and Change in Eastern Arctic Environment

From the previous discussion of chronology, we are interested in the period from approximately 4150 radiocarbon years to 2750 radiocarbon years (see above and Figure 6) which is approximately the period of Pre-Dorset occupation of the Eastern Arctic. From my previous work on these matters (Dekin 1968, 1969, 1970, 1972a, 1972b) it is apparent that the Eastern Arctic has evidence of somewhat greater climatic variability than most parts of the world and that this period under discussion here was not climatically nor environmentally complacent.

Unfortunately, there are a number of problems with the data and with their interpretation that cannot be readily resolved either by me for present purposes or by those more intimately involved with the study of biotic evidence for climatic change. One of the major problems that continues to resist solution is that of time. The data for chronology and timing of climatic events are not subject to refinement beyond the limitations inherent in the radiocarbon dating process already discussed above. The period of response of some floral conditions to environmental changes exceeds the standard deviations of the dates for the climatic change, thus the response of various elements of the environment cannot be calibrated more precisely than plus or minus a century or two, and are virtually impossible to sequence inductively at the present time. It might be possible to interrelate the various variables in an Arctic ecosystem as I attempted in 1972 (1972a:Part II, Figure 1) and to devise an extensive series of studies to evaluate the responses of various components of the ecosystem to a well-dated and accepted climatic change, but it seems more appropriate in the long run for us to learn to deal with the built-in imprecision resulting from the dating processes (see above) than to put off consideration of precise relationships until a hoped-for chronology is fulfilled.

The present series of data are inadequate for the timing of events or processes of a duration of less than at least a century, unless the impact of these processes lasted that long, or longer. An unusual ice storm in midwinter that prevented herbivores from access to plants for a month or more might lead to their extinction from a particular area, while the vegetation itself may not have suffered any permanent damage. Several summers in a row having weather patterns producing more severe conditions than usual may prevent the production of spores in sphagnum or other marshy plants, but unless this pattern continues for a long time, such a short term change may be unreflected in the depositional record of micro or macro flora in that locale.

It is apparent that we are forced to deal not with events *per se*, but with patterns of events through time. Even though seasons and storms are discrete entities at a precise level of analysis, we can only study patterns in these data through time, thus the approach which we utilize may itself structure the data and influence the patterns which we derive from such studies. Even the terms used in such studies express the predelictions which we take to them: cycles; oscillations; stades; fluctuations; stages; periods; etc.

As an example, Vibe's analysis of Greenlandic climatic change suggested a three stade sequence in 1967 which he subsequently modified to a sequence of alternations between two stades (compare Vibe 1967 and 1970), but he is still focused on the alternation between two periods of stable climate interspersed with periods of relatively rapid climatic change in two directions, similarities of which he virtually ignores because of the thrust of his research. His model utilizing stades is not designed to deal with the process of change and he virtually ignores data which suggest that the times of transition also have periods of similar environmental conditions, perhaps of shorter duration than his stades.

Rather than dwell unduly on problems of the data and their analysis, I will describe some of the relevant data for paleoenvironmental reconstruction, recognizing that micro-environmental changes may not reflect changes of the entire system and that there probably exist core areas of the system which were relatively complacent, even in the face of major systemic changes which drastically altered the environmental conditions of ecotones and transitional areas (see Dekin 1972b:13; 1972a:Part II;8).

It is also apparent that within any particular system of interest (global, continental, hemispheric, etc.) a climatic change may have produced vastly different environmental changes in any two locales, so that changes in the movements of air masses, for example, may have resulted in increased precipitation and temperature in one locale within a region and just the opposite in another locale, thus an apparent paradox in which synchronous changes may produce very different results within the same ecosystem (Dekin 1972b:13). Bryson et al. have used these data to their advantage in an attempt to determine synchronous global changes even though they recognized that their sequences reflected only widespread changes while dampening those of regional or local significance (Bryson et al. 1970). Thus, between 4150 and 2750 radiocarbon years they suggest a single major change at 2890 ± 510 marking the transition between their Sub-Boreal and Sub-Atlantic stages with tentative sub-episodes of the Sub-Boreal at ca. 3970 and 3480 radiocarbon years (Bryson et al. 1970:56,63). While it is probable that their methods have categorized those periods of transition between stable stages of world-wide occurrence, I have elsewhere summarized evidence to suggest that the North American Arctic has been subjected to changes that did not occur elsewhere (with such frequence or magnitude or duration) and that we should not deduce Arctic changes from schemes derived elsewhere but need extensive inductive categorizations of the paleoclimatic changes for which we can find evidence (Dekin 1972b:11).

I have previously categorized this evidence as follows:

1) geomorphological--uplift phenomena, fossil strand lines, stratigraphic sequences, eustatic changes in sea level, glacial features, frozen-ground phenomena, etc.;

2) botanical--faunal distributions, floral distributions, biotic stratigraphic sequences, (bogs), pollen profiles, etc.;

3) geophysical--isotopic studies of variations of isotopes of oxygen or carbon in the Greenland ice sheet; and

4) oceanographic--sedimentation, ice formation and distribution, timing and sequencing of sea-ice development (distribution and break-up), temperature and salinity variations, driftwood distributions, etc. (Dekin 1972a:Part II:2).

Perhaps the most widely available data on relevant geomorphological processes result from the study of the dynamics of glacial fronts, yet there is not universal agreement on time and sequencing of glacial events from Ellesmere Island (Lyons and Mielke 1973), Baffin Island (Andrews and Ives 1972), and Greenland (Weidick 1968; Malaurie et al. 1972). Moreover, the relationship between glacial events and climatic changes is by no means precise (Miller 1973:Fig. 9). Malaurie has commented on this state of the art, suggesting that the relationship between glacial characteristics, movements and environmental phenomena requires further study.

> It appears that, here [Thule] as elsewhere in Greenland, taking into consideration the dimensions of the glacial mass, for the well known causes of glacial dynamics and inertia, advances and retreats of glacial tongues have not been synchronous with general climatic phenomena (Malaurie et al. 1972:112).

However, Weidick suggests that "The subsequent period since 6,000 years B.P. reflects, in the glacier variations, a period of relative stability when the glaciers must be presumed to have closely followed the variations of climate, locally as well as in general trends" (Weidick 1972:197). The problem is perhaps one of the dating, when trying to reconcile dates obtained from gytta, peat, lichenometry, shells, and radioisotopes of oxygen, plus having to attempt to deal with time-delay phenomena within the data (Bryson and Wendland 1967a,b).

Thus, in spite of the fact that several periods of Greenlandic readvance dated by lichenometry coincide rather well with dates from other phenomena believed to be related (Dekin 1972a:Part III:5,7; Weidick 1972:196), it may be best to avoid the problem of selective selection of dates that fit from series of dates available, by holding the data in abeyance. The general picture of glacial events is clear and does correspond with interpretations of general climatic change inferred from other evidence, but the specifics are not yet subject to a clear regional synthesis.

During the time period in which we are interested, Miller reports lichenometric dates averaging 3200 ± 600 B.P. for the ending of a Neoglacial advance of presently unknown beginning and duration from Cumberland Peninsula in Eastern Baffin Island (Miller 1973:577). This episode may be related to a climatic deterioration dated in the same locale by plant material overlain by gravel stream deposits, but the evidence is inconclusive. However, the date appears to be of the appropriate magnitude and its context suggests that the relationship to climatic change is sound (3570 ± 140 B.P., GSC-1507; Miller 1973:577).

Oceanographic data are available from the Eastern high Arctic regarding the availability of driftwood in this region which is interpreted as relating to the amounts of open water on these coasts allowing driftwood to accumulate. Blake has accumulated evidence for the greater accumulation in the Queen Elizabeth Islands between 6500 and 4500 B.P. with an abrupt decrease after 4500 B.P. (Blake 1972:77) indicating the onset of more severe sea ice conditions. These data are in accordance with his survey of pumice distributions (including pumice from the Closure site) which was also widely distributed before 4500 B.P. (Blake 1970). Fredskild pointed out that dates on driftwood from Peary Land are mostly between 4000 and 3600 B.P., suggesting that open water may have lasted longer here than in other areas (Fredskild 1973:221).

These data are not indicative of major climatic shifts during the period under consideration (4150-2750 B. P.) with the possible exception of an ending to the presence of more open coasts in North Greenland at about 3600 B.P. (Fredskild 1969:580; 1973:221). The apparent trend of climate from these data is one of increased cooling with increased shore ice of longer duration and greater extent than had been the case in this region before 4500 B.P..

Matthews has interpreted distributional data on "warmth indicator" species of shells from Hudson Strait and Frobisher Bay as indicating a hydroclimatic optimum of warmer water (with associated warmth indicator fauna) dating ca. 3900 and 5200 B.P. (Sugluk Bay and Deception Bay in northern Ungava) and 6400 B.P. (at Frobisher Bay) (Matthews 1967a, 1967b). Matthews recognizes the problems of adequate samples of these locales and time periods, but believes the data to suggest a considerable period of warmer marine conditions, without evidence of marine environmental fluctuations during this period.

Andrews has summarized available data for variations in the growth rates of marine bivalves from raised beaches in the Eastern Arctic using these rates as an indicator of the marine conditions present during growth. From 8000 to 2500 B.P., several species of marine bivalve extended their range further north along eastern Baffin Island and along the entire mainland coast, but they retreated west and south following 2500 B.P. (Andrews 1972:157). Growth rates increased from 8500 to 3500 B.P. in Hudson Bay and eastern Baffin Island, but declined after 3500 B.P. (Andrews 1972: 157). Andrews concluded that warmer marine conditions prevailed between 8500 and 2500 B.P. with a marine optimum ca. 3500 B.P. across much of the Eastern Arctic (note that these data do relate to ecotonal changes but also to the bulk of the marine system as a whole).

Of special interest to this thesis is the timing of a shift in marine conditions from cold to warm in the Hudson Bay--Foxe Basin area at approximately 4000 B.P. (Andrews 1972:Figure 8) with additional information suggesting a climatic optimum in this region and through Hudson Straits and in eastern Baffin Island at approximately 4000-3000 B.P. or ca. 3500 B.P. (Andrews 1972:172).

While the onset of this warmer period is relatively clearly marked (Andrews 1972:Figure 8), the deterioration is less well-defined, possibly as a result of inadequate data or poor correlations among the various regions sampled, but they range from ca. 3000 to 2000 B.P.. By this time, marine conditions (and therefore productivity) had declined throughout the Eastern Arctic. Andrews underscored his finding that the environmental changes in the marine systems, while similar to those from terrestrial systems, had lagged behind as much as 1500 years (1972:174). It is important to note that these changes are not limited to ecotonal situations nor do they result from shifts in circulation networks or currents. Andrews's data from widely scattered areas of the Eastern Arctic suggest that the entire system was changing and that evidence for ecotonal fluctuations must be superposed on these more general changes in the entire marine system.

The coincidence of ca. 3500-3600 B.P. as a change in the growth rates of marine bivalves and as a change in the amounts of open water in the high Arctic (see above) should be underscored as an indicator of the basic agreement on the timing of major oceanographic changes in both the high and low Arctic.

The geophysical data presently available consist of variations in the ratio of isotopes of oxygen and carbon, available from ice cores of the Greenland Ice Sheet and from studies of tree rings from several parts of the world. The oxygen isotope studies have the advantage that they are directly indicative of climatic phenomena and are from the Eastern Arctic. Their disadvantage is that the chronology of the variations stems from the application of a theoretical model for ice flow in Greenland. Variations in carbon isotopes, on the other hand, have excellent chronological control (being from annual growth rings sequenced from the present to beyond 5-6000 years), but their relation to climatic change is not as clear nor as convincing.

In glacial ice, the ratio of 0^{16} to 0^{18} is relative to the temperature of formation of the precipitation, with higher proportions of 0^{18} at lower temperatures. The relative proportions of the sample are believed indicative of the year's precipitation, and it is thus an annual sample. Long range trends in the averages of these data are directly indicative of climatic trends and changes (Langway 1970:43-44). While it is possible to categorize the more recent layers into seasonal indicators (Langway 1970:47-51, Fig. 8), the thickness of seasonal layers decreases with time depth, and such precision is impossible at lower layers. Cross-checks of modern variations with historic climatic and temperature records indicate the validity of the approach (compare Ahlmann 1953 with Johnsen et al. 1970). The data published by Dansgaard and his colleagues

(Dansgaard et al. 1969; Dansgaard et al. 1971) are indicative of a climatic "optimum" from 8000 B.P. to 4100 B.P. (It is vital that we understand that this use of B.P. is apparently not subject to the kinds of fluctuations which have caused some to express radiocarbon dates as "radiocarbon years B.P.", and thus may be directly convertible to years B.C., but is indicative of actual years -- Dansgaard et al. 1970:338-341) with colder periods ca. 3600 B.P. and 28-2900 B.P. bracketing a warmer period just before 3000 B.P. (from Dansgaard et al. 1971:Plate 3,344) (For an additional discussion of these data, see Dekin 1972a). I must emphasize that these data categorize annual temperature variations, and thus may not relate directly to other phenomena more directly linked to the variations of a single season (Miller 1973:579).

The major source of variation in these temperatures is thought to be fluctuations in solar radiation (Dansgaard et al. 1969:378; Dansgaard et al. 1971; Dekin 1970), which may also contribute to the variations in other isotopes, particularly carbon (Dansgaard et al. 1971:46; Dansgaard et al. 1970:343; Suess 1970:Fig. 2,599). If this is the case, then we may be able to infer variations in solar radiation from variations in the C^{14}/C^{12} ratio.

> Explanations for the observed variations of the C^{14} level in atmospheric carbon dioxide may be sought in: (1) changes in the C^{14}-production rate due to changes in the intensity of the geomagnetic field, (2) changes in the production rate due to the modulation of the cosmic-ray flux by solar activity, and (3) changes in the

> geochemical radiocarbon reservoirs and in the rates of carbon transfer between them (Suess 1970:595).

While the correlations between climatic change and the ratio of C^{14} in the atmosphere are generally accepted and rather clearly delineated for recent times, there remains the question of the mechanism relating these phenomena. The data presently available seem to suggest that the sun has a simultaneous influence both on climate and on C^{14} production, rather than the sun causing climatic changes which in turn influence the C^{14} reservoir (Suess 1970; Denton and Karlen 1973:201) (see Dekin 1970 for a somewhat more detailed discussion of the details of these relationships).

The general curve of C^{14} deviations corresponds approximately to changes explicable by changes in the earth's geomagnetic field (Suess 1970:Fig. 1), but there is an apparent systematic variation in the pattern of deviations from this curve, which we can reasonably infer to have been caused by solar activity (only limited data are applicable to the details of this problem, and these are compatible with the solar activity explanation). While these data are suggestive, it is presently impossible to state with any certainty that all of the deviations from Suess's curve result from solar activity. Within these limits of certainty, it is of interest to examine the curve assuming that fluctuations in C^{14} deviations are linked to climatic changes, to ascertain if there are correlates with our other climatic data.

Suess's curve of C^{14} variations (1970:Fig. 2) demonstrates decreased C^{14} peaking approximately 2000 B.C. (note: tree ring B.C. date!) with an increase shifting to "normal" by about 1500 B.C. and continuing this marked increase to 1300 B.C., when a rebound to "normal" and just beyond can be seen, followed by a sharp increase in C^{14} at about 800 B.C.. If we can be so bold as to translate these data into solar variation and thus into climate, those times of increasing C^{14} proportions are times of reduced solar radiation and thus of cold or cooling climate (see Dekin 1970; Suess 1970:602). Such periods occurred about 1700-1300 B.C. and 800 B.C. which these data suggest were colder periods (in the sense of having reduced solar radiation). Times of decreasing or decreased C^{14} production were either side of 2000 B.C. (which was one of the most sustained periods of increase in the 7000 years covered by these data) and ca. 1000 B.C., and these are suggested as warmer periods (increased solar radiation and decreased C^{14} production).

It is difficult to reconcile these dates on tree-rings with the chronology available from radiocarbon dates (see previous discussion on chronology), as we are in grave danger of making the dating process circular. However, we can look at that portion of Suess's 1970 curve reproduced as my Figure 3 and see a different portrayal of the above data. On this curve, those portions of the curve which trend vertically towards the top margin away from the diagonal are periods of increased C^{14} production and possibly colder periods (observe that portion of the curve after 1700 B.C.) while those portions of the curve which trend horizontally towards the diagonal (either directly or at an acute angle of intersection) are periods of decreased C^{14} production and possibly warmer periods (observe that portion of the curve between 2000 B.C. and 1700 B.C.). It is this decreased C^{14} production which produces tree rings (and other organic materials in a readily mixed reservoir) this year with the same, or less, proportion of radioactive carbon as last year's tree rings.

The congruence of these data from the isotopes of oxygen and carbon is striking, and lends credence to the inferred climatic changes presented previously by me (1969, 1970, 1972a, 1972b) and by others.

The botanical evidence for climatic change is perhaps the most important, complete, and directly relevant data we have available. These data are of three major kinds: spatial--tree lines and soils change; vertical--changes in the growth pattern of sphagnum bogs; and depositional--changes in the composition of the rain of pollen through space and time.

The forest-tundra ecotone has varied in location during much of post glacial time (Bryson et al. 1965; Nichols 1967a, 1967b, 1967c; Jungerius 1969; Sorenson et al. 1971; Gordon 1972; Noble 1971; Ritchie 1972; Dekin 1972b) and its location is apparently related directly to other climatic variables (Barry 1967; Bryson 1966; Bryson and Wendland 1967b; Bryson et al. 1970; Larsen 1971; Hansell et al. 1971).

While the locations of the tree line have been affected by phenomena not directly related to climatic change (for example fires: Nichols 1967a:188-189, 1972:324,339; Noble 1971:106; Noble 1974; Fredskild 1967:45), the chronology of changes in tree line location is sufficient to relate such changes to other evidence for climatic change.

There is agreement that the tree line in the Central Arctic reached a northern maximum sometime before 4000 B.P. and suffered a well-marked decline about 3600-3500 B.P. (Nichols 1967a:186-187; Sorenson et al. 1971:471; Sorenson and Knox 1974; Nichols 1970:52; Ritchie and Hare 1971:337) after which the ecotone has not advanced to its previous limits in this area. Apparently, the minimum was reached ca. 3000 B.P. after which there was a slight readvance of the tree line North, with possible fluctuations, lasting until another major southward movement ca. 2600-2400 B.P. (Nichols 1970:54; Sorenson and Knox 1974:Fig. 5; Sorenson et al. 1971:471). The shift southwards at ca. 3500 B.P. is apparently indicative of a major shift in weather patterns and climate influencing the general circulations of the atmosphere in the Eastern Arctic at that time (Bryson 1966; Bryson et al. 1970:59; Nichols 1967a:185).

Thus, it is not surprising to find that the evidence from lake and bog deposits in Arctic Canada confirms these general relations and the timing of these changes. While there may be microenvironmental factors of local significance which do not accurately coincide with weather and climatic changes, in general the patterns of growth changes in peat bogs coincide with changes in climate. Peat growth at Sugluk on the South Coast of the Hudson Straits is indicative of a warmer period _ca_. 4000 B.P. and 2800 B.P. (Bartley and Matthews 1969:45) while Fredskild's data from West Greenland is indicative of marked sphagnum growth changes _ca_. 800 B.C. (_ca_. 2700 B.P.) (Fredskild 1967:39, Plate 6b) with changes in charcoal and exotic pollen deposition _ca_. 2350 B.P..

At Ennadai Lake, Nichols reported marked increases in sphagnum growth after 3650 B.P. (Nichols 1967a:187) with a corresponding change at Lynn Lake. Changes in bogs and sphagnum growth are frequent throughout the sub-Arctic regions in both North America and Europe, but the more common data from the Arctic are changes in the pollen deposited in lacustrine muds, bogs, archaeological sites, and raised beaches. Fredskild has summarized the response to the question of when a change in the pollen components indicates a change in climate (1972:277), pointing out that in closed plant communities, deteriorations are easier to infer than are ameliorations, in part because of the differences in response-times, and because climatic change cannot directly cause plants to migrate but it can cause their demise.

Perhaps the best and most complete depiction of the data from pollen analyses in Arctic Canada is found in the work of Nichols (especially 1967a and 1972), which has become a standard of reference in Arctic paleoclimatology. Pelly Lake profiles suggest a southerly movement of the Arctic front and decreasing arboreal pollen of pine and spruce _ca_. 3360 B.P. (Nichols 1970:49; 1972:316; 1967a:188). While the _ca_. 3500 B.P. destruction of the advanced tree line at Ennadai Lake is well known and dated by macrofossils (3430±110, 3550±120, 3650±100, and 3450±110--Nichols 1967a:188), the pollen diagrams from Ennadai and Lynn Lakes suggest a period of environmental fluctuations beginning after _ca_. 3650 B.P. (Nichols 1967a:191) and lasting until _ca_. 2670 B.P., when there was a marked climatic deterioration evidenced by a retreat of spruce forest, slow peat growth with oxidation, and spreading tundra around Ennadai Lake (Nichols 1967a:191). Nichols points out the concurrence of these changes with other changes in Greenland and Europe.

The general picture of climatic change which has emerged from these studies is as follows. Using temperatures from present locations 200km south of Ennadai today to represent the temperatures at Ennadai when the treeline was 200km north of Ennadai, Nichols reconstructed the following sequence relevant to our present investigations. The estimated changes in mean July temperatures from 4150 to 2750 B.P. were: 4150-3650 B.P. = +6 degrees F; _ca_. 3650 B.P. = drop of 4 1/2 degrees F to +1 1/2 degrees F; 3650 to ca. 3000 B.P. = fluctuations between +1 1/2 to +2 1/2 degrees F; ca. 3000 B.P. to ca. 2500 B.P. = more stable temperatures at +2 1/2 degrees F above present; and ca. 2500 B.P. a marked abrupt drop in temperatures to below the recent standard used in the research (Nichols 1967a:187, Figure 5). To summarize, there was a period of stable warmer climate followed by a marked decline and a period of temperature variation followed by another shorter stable period of moderate climate and another marked decline. The timing of these declines at ca. 3650 and 2500 B.P. is noteworthy.

The general acceptance of these interpretations is suggested by their subsequent utility (Terasmae 1973:Figure 9; Miller 1973:Figure 11), and by their congruity with other data. The convergence of data on the nature and timing of environmental or climatic changes in the North American Arctic between 4150 and 2750 B.P. is remarkable, and should give us confidence in our reconstruction of paleoenvironments. These data continue to substantiate the nature and timing of the paleoclimatic sequence which I have presented in previous papers.

Summary

For our purposes, the period before and shortly after 4000 B.P. was characterized by generally warmer environmental conditions in both the marine land biospheres than was the case before or after that time. While there may have been periods prior to that time when the land was somewhat warmer, this time marked the coincidence of unusually optimal conditions in both the marine and terrestrial environments. Both of these systems apparently underwent significant changes synchronously at ca. 3500-3600 B.P.. This contemporaneity (at least in so far as we can measure it) is suggested by a large number of dates ranging from ca. 3480 to 3650 B.P., each with a standard deviation of about a century. Even the date which Ritchie and Hare use to establish a southward movement of the tree line (which we might expect would be contemporaneous with other similar changes in the Central Arctic) is compatible with this dating, even though they use it to establish a date of 4000 B.P. (3630$\pm$140 B.P./GSC-1338--Ritchie and Hare 1971:Figure 2). When we consider the diversity of materials and events dated, the clustering of dates is remarkable, and we are probably correct in recognizing this general time as one of significant global climatic change (Bryson et al. 1970: Table 2,56).

There was more open water in the Eastern Arctic prior to this time (ca. 3600 B.P.), glaciers were generally receding, summers were generally warmer (for a longer period), atmospheric circulation was zonal, some marine growth was more vigorous, less carbon fourteen was being produced as time went on, trees were advancing North, etc.. These environmental processes were apparently abruptly reversed at 3600 B.P.. There was a period of fluctuating but

Table 8 Summary of Eastern Arctic Environmental Changes, 4000-2500 B.P.

COLDER	C^{14} Increase Tree Line South
2750-2500 B.P. (ca. 800 B.C.)...........................	
COOLER/FLUCTUATING	Marine Ice Increase Marine Fauna Growth Less Tree Line South C^{18} Decrease C^{14} Increase
2500-3600 B.P. (1650 B.C.)..........	Marine Optimum......
GENERALLY WARMER	Open Coasts in North Greenland Warming Marine Conditions
4000 B.P. (ca. 2050 B.C.)	

moderate climate, and then a further decline at about 2750-2500 B.P., again represented by a breadth and diversity of climatically significant events.

The impact of these changes on Arctic flora and fauna is difficult to assess, especially since it is rare to find direct evidence of faunal change (Andrews 1972 is an exception). Increased ice cover would cause a reduction in the availability of walrus in the high Arctic, as they are limited to areas of year-round open water. However, ringed seal and bearded seal would have been minimally affected. If the historic period of weather patterns is a useful model, the increased strength of the Arctic Front would have been associated with meridional circulation, more northward storm tracks, a greater frequency of blocking high pressure cells in Greenland, with more frequent mid-winter storms with ice in northern Greenland and adjacent islands, possibly leading to decreased caribou in these regions (see Vibe 1967:169-172; Dekin 1970, 1972a, 1972b) but increased caribou in southwest Greenland. The decline in open water in the high Arctic may have reduced the lushness of the vegetation on which musk ox depend, leading to a decline in their numbers after ca. 3500 B.P.. Fish resources were probably largely unaffected.

It is interesting to note that the evidence of major changes in Arctic environment comes from ecotonal situations or from peripheral areas. These include changes in the location of the treeline in mainland Canada, in the character and extent of inland glaciers, and in the amount of open water in the high Arctic. Major environmental changes did not seem to be evidenced from the central regions of the Eastern Arctic, in particular that area in the vicinity of Foxe Basin, marked as CORE AREA on most of the Figures of faunal distribution. This area was discussed as a core of Arctic cultures during the Pre-Dorset and Dorset periods at a School of American Research Advanced Seminar on Pre-Dorset -- Dorset Problems held in 1973. Interestingly, there has been little evidence for climatic or environmental change obtained from this region, with the exception of Matthews (1967a) and Andrews (1972). In addition, the distributional data suggest that most animal species of limited distribution (or species with variations in availability) are accessible from the periphery of the Foxe Basin and are included in at least portions of the Core Area. Moreover, the weather patterns in the historic period are indicative of less variation (with generally lower rainfall and lower temperatures) in this area.

Thus, the core of cultural continuity which was perceived by a group of investigators of culture history in this region may also be seen as an environmental core, in which there was ecological continuity through time, and in which human hunters of moderate mobility would have been assured of access to whatever resources the Eastern Arctic had to offer. In spite of this general picture of continuity, climatic changes may have influenced the environment and its human inhabitants, in part directly by altering the biotic conditions of even the core area (albeit perhaps only slightly) and in part indirectly by drastically altering the conditions in the peripheral areas and increasing the movement of animals (including people) to and from these areas. Because of the importance of the relationship of changing environment to the behavioral responses of hunters, we will elaborate on these questions later in this thesis, in an attempt to develop an explanatory model for change in the behavior of Pre-Dorset people which relates environmental change to human behavioral change.

It is essential that <u>all</u> data relevant for the modeling of prehistoric environmental conditions be considered and evaluated. In the past, archaeologists have sometimes chosen those data which fit with their predilections and have ignored or minimized seemingly conflicting data and interpretations. Further, in our zeal to utilize data from other disciplines, we have often disregarded their cautionary or preliminary conclusions. I have attempted to cover all possibly relevant data presently available and to be as explicit as possible regarding my treatment of them and their shortcomings. In my opinion, the broad outlines as presented in Table 8 are generally reliable, while the specifics (of both time and space) will require additional work.

To summarize, at a general level the Eastern Arctic presents an environment for man which includes ringed seal, caribou and Arctic char in relative abundance, and which, with local exceptions, has probably done so throughout the time period under discussion (4150 to 2750 B.P.). There were variegations in these and other resources through space and time, some of which may be important variations for human adaptations. The restricted spatial distribution of walrus and musk ox and the environmental changes at *ca*. 3500-3600 B.P. and *ca*. 2700 B.P. may be of major significance to our understanding of human adaptations in this region.

The historic period of hunting adaptations provides a model of general adaptation to genera and regions, rather than to a spatially restricted microenvironment (Flannery 1968:67), and thus man is like other animals in this system. It is this spatial flexibility which is worthy of mention, because it will be an important constraint in the following discussion of levels of analysis, methodology, and evaluations of explanatory models of Pre-Dorset behavioral change.

This assessment of the changing environmental setting of the Pre-Dorset is important as a general background to any study, but specifically important to the development of models of Pre-Dorset behavior which attempt to relate changes in such behavior to the changing environment. These dynamics will receive further attention in Part III.

PART III

MODELING THE PRE-DORSET

Chapter 4

The Data -- Their Collection and Analysis

Artifacts, and in particular stone tools, are not produced and deposited by random human behavior, but are the results of purposeful behaviors subject to a number of environmental constraints. The artisan was influenced by the constraints of production and use, while the morphological and spatial characteristics of his artifacts were subjected to constraints of deposition and preservation. The archaeologist can exercise influence only with regard to data-gathering constraints (see Figure 19). Any student of human behavior attempting to utilize archaeological data in his analyses should be sensitized to the relations between processes of artifact production, deposition, preservation, excavation, and analysis, because differences in data and analytical conclusions may stem from differences in these processes. It is desirable to study these processes in some detail before attempting to synthesize what in many cases are extremely disparate data from across the Arctic.

As students of human behavior, we are initially limited by the fact that some ideas and behaviors do not result in archaeologically available data (see Munton 1973: 686), however it would be wise to continue to evaluate this constraint as recent investigations suggest that it may be possible to investigate behaviors once thought impossible (Deetz 1965; Hill 1970; Longacre 1970).

Some archaeologists have assumed that the artifacts found on a site represent activities carried on there, but while this is generally true, it may be difficult to specify precisely what activities led to the deposition of particular artifacts. In this sense, storage of artifacts may be defined as an "activity", but the use to which stored artifacts may be put may cause different interpretations of the "activities" carried on at that location (see Binford and Binford 1966, 1969; Isaac 1972:177). While this approach may be sound and productive, there is grave danger of oversimplification. For example, the presence of harpoons in a site does not by itself imply that the site was the base of harpooning activity, especially if the subsistence data do not support such an inference and if the storage and settlement pattern are compatible with a site inventory of tools curated for use in other locales and other seasons (Taylor 1967:223).

However, this approach does focus on the adaptive relationship between technology and the behaviors occurring at a site, and is thus an important dimension of the analysis of technological systems and of the ecological relations of human behaviors and technological systems.

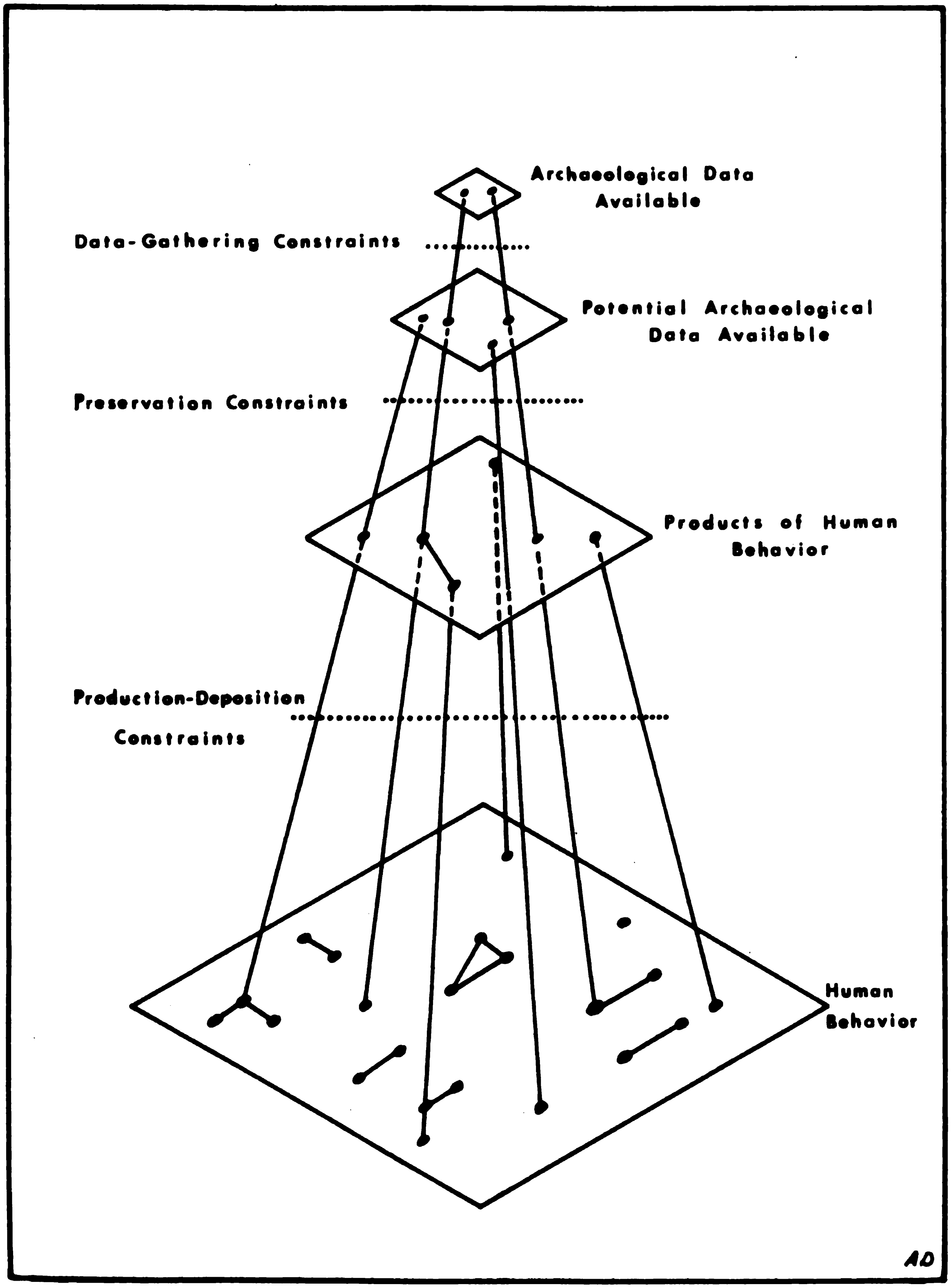


Figure 19 Constraints on Archaeological Data for the Study of Human Behavior

Students of artifact morphology have approached artifacts as if artifact patterns represent some sort of mental set (mental template) of the people who made them (Deetz 1967, 1968). This approach has assumed the existence of a normative system for artifact morphology, treating the data as if processes of artifact production are dominated by adherence to such a set of norms. Artifact form is seen largely as a result of processes which focus on the "replication of uniformity" (Wallace 1961:26) and production is seen as a series of choices by the artisan (Deetz 1967, 1968). However, as Isaac has pointed out so clearly, this aspect of traditional patterning is but one constraint on the form of a particular artifact (Isaac 1972:177). Perhaps this approach is best suited to those circumstances in which other sources of variance (physical, geographical, economic, functional, etc. -- see Isaac 1972:Fig. 4.1; Clarke 1968:Fig. 17) may be controlled (Cole and Kleindienst 1974:353-354).

Recent efforts to study technological systems as systems have suggested several useful categories of tool use which may influence their production and deposition. Tools designed and produced to be used for a specific task are apparently treated differently from those tools designed for several possible uses. While technological evolution has been defined in part by the increase in single-use tools, those instances in which ethnographic data have been analyzed from this point of view are extremely rare.

Maxwell has suggested that several artifact forms from the Eastern Arctic were functionally specific tools designed and used for a specific task (Maxwell 1973a:321,344 for example) and he has gone on to suggest that the Pre-Dorset and Dorset technological systems were characterized by a high degree of functional specificity in virtually all of their lithic tools (Maxwell 1973a:345). By attributing a majority of variation to artifact function, Maxwell has emphasized the similarities in artifact morphology across large areas of the Eastern Arctic and the extreme conservatism of the technological system (and other behavioral systems) through the Pre-Dorset--Dorset continuum. Additional studies of tool use among contemporary peoples are needed in order to test the relevance of this categorization to technological systems in other areas and in other times (see Arutiunov and Sergeev 1973:4,5).

The recent studies of Binford of contemporary Alaskan hunters have suggested the utility of a distinction between curated and expedient tools. Curated tools are those produced and preserved for use at appropriate times and possibly other locales, while expedient tools are those "tools of the moment" which are produced for imminent use and then put aside (see Binford 1972:133; 1973:242). Obviously, there is a more direct relationship between expedient tools found on a site and the activities carried on there than there is between curated tools found on a site and the activities carried on there. Curated tools may be lost in transit on sites in which they were neither produced nor used, while expedient tools, by definition, were made and used on the site. Manufactured artifacts (and possibly some types of raw materials) which are traded are also curated tools, and may be found on sites where they were deposited "in transit" and therefore may not reflect subsistence or other activities carried on there. Because of their rarity, sentimental value, or uniqueness, some artifacts may be curated (almost as in a museum) even though they have no relation to the customary behaviors of the people occupying the site (see Fitzhugh 1973 for a discussion of a Ramah chert Pre-Dorset burin from the Rattler Bight site in a Maritime archaic site dated _ca_. 1900-2600 B.C.).

Binford has suggested that the variation among artifact assemblages ". . .should vary inversely with the degree that tools were multifunctional, and/or curated in anticipation of future tasks" (1972:133), thus the homogeneity which Taylor and Maxwell have demonstrated would be in accordance with a high proportion of curated tools and/or a high degree of multifunctional tools. Inasmuch as Maxwell has made a case for a high degree of functional specificity, we should re-assess this possibility in conjunction with an examination of the utility of the categorization of curated and expedient tools from the Pre-Dorset.

From the above considerations, it is also apparent that all tools are not of equivalent diagnostic and analytic significance. Broken artifacts are not the equivalent of unbroken artifacts, and rare artifacts are not the equivalent of prevalent ones. We have come face to face with what is perhaps the real revolution in archaeology today. This revolution is the shift from the study of artifacts to the study of the relations among artifacts. It is this shift which has made systems theory so popular, because it makes explicit the study of not only the entities within the system (artifacts) but of the relations among these entities. If we take an intellectual historical perspective, such a shift has characterized a number of diverse sciences, from anatomy (physiology) to ethnology (functionalism) and it is this parallel in the development of disciplines which has influenced contemporary discussions of scientific paradigms (Kuhn 1970; Fitting 1973; Leach 1973).

In the Arctic, the most underrepresented aspect of archaeological studies is of the relationships among artifacts, in spite of the habit we have fostered of talking of technological systems. It is when we make an explicit effort to study both the artifacts themselves and the relationships among them (spatial, temporal, contextual, coincidence, etc.) that previous analytical paradigms become limited in their utility, and we are forced to re-think our basic assumptions regarding archaeological data.

Processes of artifact production have been summarized above and include dimensions of the physical environment (physical geography and properties of materials), the learned normative tradition (mental templates), the ecological uses to which the artifact will be put, and the relations among these dimensions (economy and subsistence patterns, functions of artifacts, etc.). It is important to emphasize that while processes of production are related to processes of use (especially with a feedback mode involving assessments of efficiency, etc.), the spatial relations of production and of use are not necessarily similar, and we must be careful not to believe that these structured spatial relations are isomorphic. Processes of repair are perhaps a third set, whose spatial relations may be analyzed in relation to those of production and of use.

While processes of tool production have been analyzed by Deetz and others, and processes of tool use have been analyzed by numerous "experimental archaeologists", relatively little attention has been paid to diversity in the processes of artifact deposition (Binford 1972, 1973; Schiffer 1972:156; Daniels 1972:203). While tools may be subject to stochastically variable losses, it is doubtful whether these are of a random nature (of equiprobability) in either space or time. In essence, tools may be purposely deposited, discarded, or lost. Artifacts may be discarded because they are no longer useful, broken, or otherwise not salvageable. Many broken fragments of artifacts from archaeological sites result from this sort of discarding. Other artifacts may be still functional, but are discarded because they are not worth retaining, or moving to a different location (those of us who have moved household goods know this only too well). These are Binford's expedient tools, which are "easier" to replace than to retain. In the Pre-Dorset, certain burins, burin spalls, and microblades may be of this type.

We should also be aware that portions of broken artifacts may be treated differently, depending on the nature of their characteristics (size, shape, etc.), and some may be converted to either curated or expedient tools, thus broken parts of the same artifact may be subject to different depositional processes--some discarded, some re-hafted, some re-worked, and some retained.

Lost artifacts are essentially those which would have been retained (curated) had they not been left behind or misplaced. While some artifacts may be lost or misplaced on a day-to-day basis, it seems likely that more would be lost when left behind during a movement of the household (a change in settlement location). Even here, this distinction is not clear cut, because some artifacts may be cached (stored for later use) and not recovered. Among the set of curated artifacts, some are of greater value than others and are less likely to be lost. This value may be proportional to the replacement cost in terms of energy (polished stone tools), rarity of the raw material (steatite or slate), inability of replacement (traded artifact or tabooed raw material or work process), and aesthetic or sentimental value to the user or to others. Pride of workmanship may also lead to a particular artifact being curated when similar artifacts are expedient.

Artifacts may also be purposely deposited, perhaps most frequently in a ritual context, as in a burial, under house posts, etc. However, the unusual nature of these contexts and their concentration in space facilitates the discovery of such purposeful deposits.

The operations of these different processes of deposition leads to a marked disparity in the frequency of curated and expedient artifacts in archaeological collections. The rarity of extremely well made and still-functional artifacts from certain Pre-Dorset sites (the Closure site, for example) and the application of a guide fossil approach to interpreting the data, base chronological development and comparisons on relatively rare artifacts, and possibly on those which we might expect to adhere most closely to some sort of mental template. In historical perspective, this conjunction seems fortuitous. Earlier attempts to focus on the presence or absence of expedient artifacts (such as Giddings's use of microblades and burins; 1956: 266, Fig. 81) have met with little acceptance, because of their general lack of specificity and because they tend to lump assemblages which are quite diverse in their other characteristics (especially among curated artifacts).

Among those factors affecting the frequency of lost tools are: artifact size; ease of replacement (size of

labor investment, etc.); frequency of use (now and in the near future); importance to other behaviors (participation in a tool kit, for example); location of use (inside a structure, outside, on the sea ice, etc.); location of storage (pouched, on sleeping platform, along tent wall, cached, in entrance feature, etc.); identity of curator (owner, user, disinterested party, etc.); and number of possible uses (functionally specific or multifunctional). While it may be impossible to control all of these possibilities for comparative purposes, the above list may serve to sensitize us to alternative influences on the processes of tool deposition, and thus alternative sources of variation in artifact assemblages (Schiffer 1972:163).

At first glance, the Closure site artifacts are dominated by what may be expedient artifacts with more than eighty per cent of the artifact sample composed of worked chert and quartz, unifacially retouched flakes, microblades, burins and burin spalls. Unbroken artifacts of rare types are highly unusual in the sample. Illustrated collections from other areas seem to have a much higher proportion of unusual artifacts (unusual for the Closure site) and of still functional artifacts (large numbers of complete end blades, for example). Seemingly, some sites have a larger proportion of curated artifacts deposited than others, and closer attention to such variation seems appropriate.

What kinds of processes would cause the deposition of a larger number of curated artifacts (artifacts which by definition would be abandoned only in unusual circumstances)? Sites which were abandoned abruptly or in which the occupants were killed or died would cause their entire technological inventory to be deposited, leaving a higher proportion of curated artifacts than would otherwise be the case. An abrupt subsistence shift of a more-or-less permanent nature would cause the abandonment of artifacts no longer useful (or less useful) in the projected tasks, thus curated artifacts became expedient ones with such a subsistence shift. A change in settlement pattern interrupting what had been planned for a seasonal round, might lead to the cacheing of artifacts for a future use which never came.

The unplanned abandonment of a site due to changing ice conditions or weather may also result in increased deposition of curated artifacts, as might any number of shifts in circumstances (taboos, social ostracism, murder) probably incapable of precise measurement in archaeological data. I would hypothesize that sites in marginal areas (of uncertain resources or environment) would include a higher frequency of curated tools (when compared to locales of more stable environment and resources) as a result of their adaptive instability. To be specific, sites within the "Core Area" (see above) would be expected to contain a somewhat higher proportion of expedient tools and lower frequencies of curated artifacts, when compared to those sites in fringe areas. Unretrieved caches of potentially useful artifacts would be somewhat more frequent in fringe

areas. The general character of assemblages from locales in fringe areas would be distinctive, in part because of the greater possibility of finding otherwise curated artifacts.

Thus, depositional processes cannot be simply categorized, and are much more complicated than simply the operation of activities or of learned norms. When we intend to investigate human behaviors which influence the production and deposition of artifacts, we are initiating a long and complicated study only dimly related to those of yesteryear in which we ignored or assumed the answers to many of these questions of diversity in site artifact assemblages.

Once artifacts are deposited, we are faced with a second series of constraints which influence the survival of the archaeological data. We can call these preservation constraints, largely related to organic materials. In the Lake Harbour region, it is not until after the colder period ca. 700 B.C. (Dekin 1972b:21) that Maxwell's Dorset sites contain significant organic preservation, and Maxwell has pointed out that early Pre-Dorset bone preservation is found only north of 69° North Latitude (Maxwell 1973a:300). Extrapolating from subsequent Dorset sites and from our knowledge of Historic Eskimo technology, this preservation constraint on our knowledge severely restricts our information to a potentially tiny portion of the technological system -- lithic artifacts, and their distributions.

Once artifacts are deposited on the surface of a site (or within it), they are subject to a number of soil processes which may change their interrelationships (Ascher 1968). Since I have pointed out above that this dimension of technological systems (that of the interrelations among artifacts) has come to be of increasing significance in archaeology, it is significant that in the Arctic this dimension may be subject to a number of changes, including movement in both horizontal and vertical directions. Alternate freezing and thawing may cause vertical sorting by artifact size, with larger artifacts moving upwards and smaller artifacts moving downwards (Corte 1963:499). If there is any slope to the deposit, solifluction may cause soil creep and the convolution of even clearly stratified layers (Iyatayet, Cape Denbigh and Engigstciak, Firth River for examples). On the Closure site, the disintegration of the convoluted bedrock outcrops which bound areas containing artifacts has led to a more or less small but continual depositing of small grains from this decomposed metamorphic bedrock onto the surface of these sites. In some areas, this has led to sterile sandy layers just under the active sod layer. Both wind and water are the agents which move these particles downslope and to leeward of their origin, but these agents do not seem to have influenced the location of artifacts previously deposited, except for the possibility that the vertical extent of the site has been increased due to this deposition, thus increasing the vertical distribution of freeze-sorted particles and artifacts. In general, the Closure site has not been subjected to processes which have noticeably altered the distribution of the artifacts found, as no systematic distribution by size or elevation is evident. The major change in the data since the time of their deposition is apparently the loss of organic materials.

Data gathering constraints are the last set to be considered here, and they are the constraints most susceptible to our analysis and control. It is thus surprising to note that relatively little formal attention has been devoted to describing, explaining, or even justifying the technical and methodological choices which archaeologists have made regarding their field work and analysis, particularly in the Arctic.

Archaeological research in the Arctic differs markedly from any number of norms of North American archaeology, in particular with regard to the necessity for self-sufficiency of the party and the large amount of time and energy which must be devoted to "housekeeping" and logistics. Most survey operations have been restricted by transportation problems and by the direct impact of weather and other environmental influences (see Solecki et al. 1973:11-18; Maxwell 1962:20-21). These restrictions have made such research expensive in terms of both time and money, limiting the amount that could be done in the time allocated. The relative brevity of the summer season and the frequent necessity to wait for the ground to thaw in order to allow excavation are also unusual constraints. Further, field workers in the Arctic have been an unusual combination of untrained local workers (Eskimos) and a small number of trained archaeologists brought up from the south, leading to some variation in the abilities of field parties to conduct precise controlled excavation (Taylor 1968:9).

Equipment problems have made adequate surveying equipment an increasing rarity, yet the quality of published maps has been good. A number of substitutes for field equipment have been utilized, such as the substitution of aluminum "pegs" for wooden stakes or of plastic bags for paper bags. Several archaeologists have used triangulation techniques for the horizontal control of the data to facilitate the recording techniques (Gordon 1972; Hall 1970, 1973), especially in circumstances where an extensive grid system would have been inappropriate.

Archaeological research and coverage of regions in the entire Arctic has been influenced by the existing transportation networks and by recent economic development, especially in the case of salvage or reconnaissance work (oil company reconnaissance and highway and pipeline salvage; location of existing facilities--airfields, radar stations, settlements, etc.). Our knowledge is by no means the result of any random sample, and perceived boundaries between archaeological regions may in part be the product of the structure of the sample taken. Our archaeological sample of the Arctic is extremely spotty, but this is partially a result of the vastness of the region to be covered and by the relatively limited number of researchers who have been working in this area.

There are few areas in which we may have a sample representative of the complete human utilization of a locale through prehistoric time or of the complete set of activities of any particular village or society at any particular point in time. The most complete samples within the Arctic Small Tool tradition are at Igloolik, Lake Harbour, Northeast Greenland and Kotzebue Sound, but even these may sample the similar utilization of a locale through time, and not be representative of the complete yearly activities of any group of people at any point in time. To my knowledge, no one has yet the temerity to suggest that his sample included a yearly range of activities, thus seasonal variation in behavior is yet unstudied and uncontrolled for any locale or time period. Even the attention to the problem of seasonal variation is as yet nascent among sites where faunal preservation holds hope of establishing the season of occupation, so we should not be surprised to find a lack of ability to control this variable where faunal materials are absent.

The recent sensitization of archaeologists to the constraints of sampling techniques has raised questions of sample bias and sample adequacy, for which we have only unsatisfactory answers. To my knowledge, no one has extracted a truly random sample from any artifactual population in the Arctic, nor have they been careful to specify the nature of possible biases or rationales for many of the samples taken. Perhaps the major accepted sampling technique has been to excavate test pits (of varying sizes) in "likely-looking" locations, expanding into adjacent areas when the results warranted further excavation. The two sources of judgment here are what is a likely looking location and what results warranted further excavation. Perhaps most frequently, artifact density is the major criterion for the second judgment, excavating areas of highest artifact yield. This bias may be against those areas where significant activities were carried on but which used a relatively small number of stone tools (albeit significant ones, or curated ones), thus a sample in which artifact yield was the major choice criterion for areas to be excavated contains a significant bias against certain behaviors being represented in the archaeological sample.

The judgment as to what is a likely-looking location is also a source of bias, depending on the nature of the surface features used as criteria. On Cape Tanfield, we found that there was no discernible surface feature that would allow the prediction of the density of Pre-Dorset artifacts (from none to high), thus we tested every depression in bedrock containing soil and sod, to make the reconnaissance as complete as possible and to minimize sample bias. While I too was prone to excavate initially those areas where test pits had the highest artifact yield, we did excavate KdDq 11-B and KdDq 23 which had extremely low artifact yield, compared to other areas. While I cannot describe our sample as random, I can state my belief that it is representative of the population of Closure site artifacts and that it suffers from no systematic bias that I can determine.

However, some of the samples which will be used for comparison with my data have been systematically biased by data collection techniques and strategies. The Arnapik site consisted of 120 find spots all of which were surface collected <u>only</u> (Taylor 1968a:15). McGhee's researches on Independence I and Pre-Dorset ruins on Grinnell Peninsula were largely surface collected with very little excavation (McGhee 1973; personal communication). Knuth's excavations of houses in Northeast Greenland were biased in that a significantly higher proportion of those houses with mid-passage features were excavated than those which did not have this feature (Knuth 1967:28,47) which is compounded by the fact that the data are not presented in a manner that allows the comparison of house contents from these two types of houses. Other sources of bias would be the selection of the best-preserved houses for excavation, the excavation of only those areas with surface indications, etc.

Differences in field techniques are a great source of sample bias, especially in sites where the most frequent artifact may be minute (i.e., burin spalls, the largest artifact category from the Closure site at 40%). Additional excavations at sites previously sampled by Maxwell's field parties on earlier reconnaissances had their sample proportion of some artifacts (burin spalls in particular) significantly increased when greater precision and the sacrifice of efficiency for completeness characterized subsequent excavations (Maxwell, personal communication). We were fortunate to be able to devote considerable time to the excavation of the Closure site and the freedom from the constraint of time allowed the use of more precise and time consuming excavation and recording techniques.

During the excavation process, it is impractical to record all potentially significant archaeologically useful data. It is also impossible to record everything and we are forced to make judgments on what data should be preserved. We plotted all artifacts identifiable in the field and saved all probable and possible lithic artifacts. All detritus was bagged by excavation unit. All rocks greater than fist size were plotted and mapped as were those instances of homogeneous "rotted" granite where rocks had been. However, a search of the literature indicates that this combination, the plotting of all rocks and artifacts, has not been the rule and that data useful to compare the relations between these distribution patterns are virtually nonexistent. While some might argue that hindsight is unfair, this neglect of potentially useful data (see Chapter 5) is no longer professionally acceptable.

The analysis and presentation of archaeological data are also subject to bias which may influence the conclusions of the investigator and the interpretations of those interested in the data. One of the major sources of such differences in data is in the categorization process. It is essential that categories of data be strictly defined and any lumping of data (from excavation units, different areas of the site, different sites, etc.) be made explicit (Taylor's is an admirable example of such explicitness--1968a:Chapter 3). Sub-sets of artifacts from possibly relevant units of sites should be kept separate and described separately, even after preliminary analysis has been interpreted that no definable differences among the sets exists, to allow others to make their own conclusions, and to allow a reassessment in the future when we are sensitized to different sources of potential variation. Such sensitization is another source of bias in interpretation and presentation, because if the investigator is not aware of the potential comparative significance of an attribute or artifact, it may be neither recorded, considered, nor presented (polish on burins and burin spalls, burin spall artifacts, and the use of pumice are examples) (French 1973:105; Munton 1973:686).

The categorization of the data for publication is another source of difference in data, as the lack of standardization in typologies and descriptive criteria is a problem (compare Taylor 1968a, Nash 1969 and Maxwell 1973a on descriptions of burins). Standardization of measurements of artifacts is also not yet the rule, and our confidence in the measurement techniques of others is based largely on faith, in the absence of replicability studies. The treatment of stone detritus is also not standardized, with some investigators including such a category within the sample size and using the resultant number (N) for percentage computation, while others cast these data aside (Giddings 1964).

While some archaeologists are "lumpers" with data presented in large categories, others are "splitters" with a great many categories (See Dumond 1974b). In an operational sense, the splitters prepare their data for more facile use by other researchers, and should be emulated in this practice.

The selection of illustrative data is also prone to bias, especially when only "ideal type" artifacts are presented. This "replication of uniformity" approach uses selected artifacts and conveys a picture of uniformity within the categories used by the archaeologist. When this approach is combined with a limited number of illustrations (Knuth 1967), the data are severely limited and biased by the selector. An "organization of diversity" approach (see Wallace 1961:26-27) includes data on the variation within artifact categories, as well as more "typical" artifacts, providing data of much greater utility to other investigators. Perhaps the selection of an artifact sample for illustration should receive as much attention as the excavation process itself, in order to insure the representativeness of the illustrated sample.

Parametric data are also prone to bias in their selection and depiction, although these are more likely to be the result of "sins" of omission than of commission. A normative approach to measured data involves the depiction of certain parameters related to "measures of central tendency", such as means, modes, and medians without the inclusion of data indicative of the actual dispersion of the variables (ranges, standard deviations, measures of skewness or kurtosis, etc.). Perhaps this is because some of these parameters require additional and often extensive manipulation of the raw data, but the advent of sophisticated computers and inexpensive calculators will alleviate the laborious and imprecise aspects of such depiction. The addition of such a fundamental value as the standard deviation allows the testing of significance of difference between two sample means to determine the probability that such a difference could have occurred by chance in two random samples drawn from the same population. Such an estimate is essential before we can attribute significance to such a difference (see Anderson 1970:13). Thus, even parametric data have been subject to a selection biased in the direction of behavioral norms, reducing the presented evidence for behavioral variety. Greater precision and explicitness in the depiction of variation in parametric data will result in a reduction of bias in data depiction, and will allow greater precision in the comparison of data among artifact assemblages.

Perhaps the biggest problem of comparison of archaeological data is related to the representativeness of the sample to the population from which it was drawn. Virtually everyone has talked about sample inadequacy, but few are doing anything about it. A concern for sampling techniques is relatively recent in archaeology, yet we have all at one time or another wondered about negative evidence and whether the absence of a particular trait from a sample was significant. In this problem, sample size frequently seems to be a major problem, particularly in the Arctic where some sites have thousands of artifacts and others several dozen.

Statisticians have lead in the consideration of sampling problems (for example, Cochran 1953) and geographers have followed, from whom we could learn a great deal. Berry presented a clear and succinct analysis of sampling problems and spatial dimensions in 1962 which was elaborated and summarized by Haggett (1966). My Figure 20 is based on data and formulas presented in Berry (1962), Haggett (1966), Cochran (1953:Chapter 4), and Tate and Clelland (1957:149-152). It depicts in graphic form probability curves for a random sample having <u>none</u> of a certain category of items found in a given proportion of a population from which the sample was drawn. It is thus

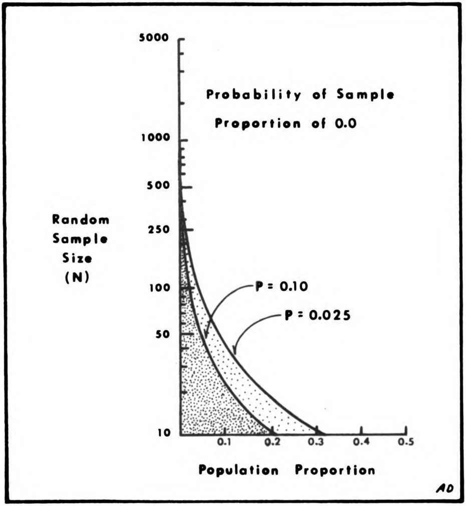


Figure 20 Probability Curves for the Absence of Artifacts With Specific Random Sample Sizes and Population Proportions

useful in the evaluation of negative evidence (absence of traits), because it allows the statement of the probability that such an absence would occur by chance, if we can specify the size of the random sample (from an infinite population) and the proportion which we might expect in the population. For example, in a random sample of 50 artifacts from a large site, there is one chance in ten (P=0.10) that an artifact type comprising four percent (0.04) of the population would not be found in the sample (see Table 9). In a sample of 250 artifacts (which is approximately the median sample size of assemblages available for comparison in this thesis) there is one chance in ten that the sample would not include items present in the population in a proportion of slightly less than two percent (0.02).

A more precise example may be the presence or absence of steatite (or graphite) vessel fragments, of which we found 3 at the Closure site (among 1323 artifacts). If we can assume that the Closure site sample is representative of Pre-Dorset assemblages on the South Coast of Baffin Island (which we will assume here only for the purpose of illustration), then the population proportion is 0.00226. With a median sample size arbitrarily set at 250 (corresponding to the median of samples compared in this thesis), the sample proportion should be 0.00226 $\pm$ 0.003, which means that the probability of not finding such vessel fragments in a sample of 250 is approximately 1/6 (P=0.166). Of six such samples of 250 each, the probability of one of them not having such fragments is unity. To find at least one in a random sample, the minimum sample size is 1766 (.975 probability).

This curve is also useful for determining what sample would be necessary to insure that artifacts missing from the sample are not missing due to chance. If we expect microblades to have a frequency of ca. 14% (as they are at the Closure site), then a random sample from a population requires a minimal N of 25 to insure that an absence of microblades is not due to sampling error (at the 97.5 confidence level). Note that it is essential at this point that each artifact in the population have an equal probability of being sampled, and I doubt that we can say this for any archaeological sample.

It is possible to make the following general statement on sample size. If we have an adequately random sample of a population, we can predict that nine times out of ten (P=0.90) we will find at least one item if the minimum sample sizes (N) and the population proportion are as in Table 9.

Table 9 Minimum Sample Sizes to Insure Finding One Item From Populations With The Following Proportions at the 90% Probability Level

Minimum Sample Size	Population Proportion
1000	0.01
250	0.02
200	0.02
100	0.03
50	0.04
35	0.07
25	0.09

(After Tate and Clelland 1957)

These calculations allow us to assess the probable significance of negative evidence, but we are still hampered by the fact that the populations of artifacts from Arctic sites are seldom randomly sampled, and there is no present way to assess the systematic impact of non-random sampling. We can at least make a start towards defining sampling bias, sampling error, and the evaluation of the significance of the absence of diagnostic artifacts from an archaeological assemblage.

It may be well to emphasize at this point that the term bias refers to the non-representativeness of a sample from a population. Sources of bias are many, but the processes of gathering archaeological data perhaps contribute the most and at the same time are those over which the archaeologist has the greatest control. While some may read this portion of this thesis as overly critical of previous archaeological research in the Arctic, such is not my intent. It is imperative that we be aware of the biases which our field techniques and methods introduce, for only then can we study them, eliminate some, control for others, and otherwise insure that our analyses are of those variations caused by human behaviors of the people under study and not by archaeologists.

The Relationships Between Technological Data and Human Behavior at Different Levels of Analysis

Technological patterning may be observed and studied at a number of levels of analysis, just as in the geographical study of spatial diffusion.

> We should not be surprised, therefore, if we require many different models to help us clarify these difficult processes, which operate at a variety of levels. Shifts in scale are very common in all the sciences, and as men and women become dissatisfied with very general statements giving a very gross overview of a subject, they tend to direct their attention upon smaller and smaller pieces of the problem, shifting along a continuum from the macro- to the micro-viewpoint. The rise of many subjects with names prefixed by *micro-* attests to the shifts in scale that have taken place throughout science during the past half century. Stafford Beer, one of the great names in the field of operations research, has coined the provocative phrase "cones of resolution," implying that problems can be considered at many different levels and in varying degrees of detail. . . . In any subject, the pendulum usually swings back and forth between the extremes, and with people working at all scales of inquiry, the "cones of resolution" are eventually filled with a hierarchy of models. More general models, high up in the cones, are supported by a number of others at lower levels (Gould 1969:25).

It has been profitable to speak of continuous variation in studies of spatial distributions of populations, recognizing that the construction of discrete and isolated population clusters is an artificial process, and that such constructs are best seen as heuristic devices (see Haggett 1966:100). The study of spatial and temporal variations in human behavior may be subject to the above characterizations of the heuristic nature of units of analysis, as may be the analysis of the dimension of social complexity. We are faced with the problem that our most fundamental and useful categories of human behavior and concepts regarding human processes are analytical constructs (heuristic devices) subject to a great deal of variety in their use and utility to scientific analysis (compare Haggett et al. 1965).

Units of archaeological analysis are also heuristic devices, whether their dimensions be in the spatial, temporal, or hierarchal orders. While such attention has been devoted to the categorization of archaeological units in the Midwestern United States (McKern's Midwestern Taxonomic Method--see Griffin 1943:Appendix A), relatively little formal attention has been given to this problem in the Arctic (see Chapter 1 above). Still less attention has been given to the relationship between archaeological units of analysis and other units of anthropological analysis, in particular those dealing with the organization of social and cultural systems. The relationships between patterning in archaeological data (at various levels of analysis) and patterning in human behavior remains largely unspecified and unanalyzed in Arctic archaeology (Dekin 1973a:41).

> Most archaeologists begin with sites and artifacts and attempt to explain what they have found. One may also begin with a cover-law hypothesis and see if one can test it with excavation or analysis. The two approaches are complementary, however, for one can hardly explain what one has found without some theory, and one cannot test hypotheses without data. Thus, no matter which situation stimulated our inquiry, we must deal in the first instance with the relations between artifacts and the behaviour we are seeking to explain. In my opinion <u>this is precisely the point at which archaeology is weakest</u>. Rather than an explicit set of theory we have a set of procedures: typology, numerical taxonomy, attribute analysis and the like. And even worse, since we must work with what we have, we tend to grasp at straws, hoping that such artifacts as exist will somehow inform us on the behaviour that we question. Neither established procedures nor the artifacts at hand necessarily result in information that is meaningful in terms of the cultural categories we wish to understand (Hole 1973:25).

One of the clearest depictions of the relations between artifacts and the behaviors archaeologists seek to explain is contained in Deetz's introductory text in archaeology (Deetz 1967:Figure 17) where he presents an iconic analogue model in which the nested hierarchies of units of human behavior and of archaeological analysis are interrelated. I wish to state explicitly that this portion of this chapter is a postulation of isomorphism between specific units of archaeological analysis and specific behavioral systems at several levels of analysis and at several levels of abstraction.

It is important to note that such isomorphism does <u>not</u> extend to the contents of arbitrarily defined excavation units, with the exception that such excavation units <u>may</u>

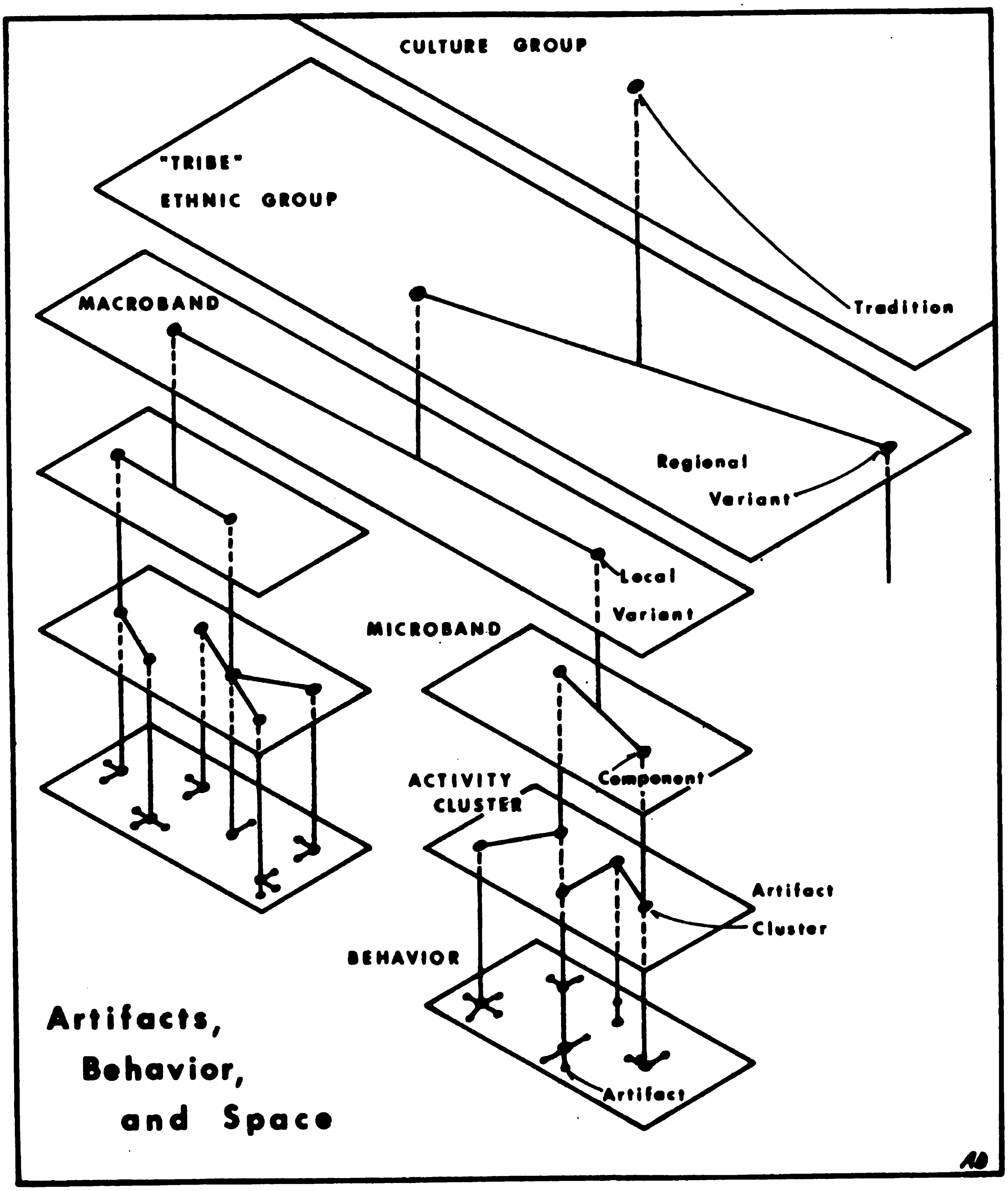


Figure 21 A Model of the Nested Hierarchies of Units of Human Behavior and of Units of Archaeological Analysis, and of Their Interrelations

contain relevant units of archaeological or behavioral analysis such as artifact clusters or structural features. As I will demonstrate below, the use of arbitrary excavation units by itself will not improve the quality of the archaeological data nor will it allow the establishment of meaningful units of archaeological analysis, leaving the selection of arbitrary excavation units open to evaluation, judgment, and criticism, as befits any heuristic device.

The structures produced by people are in somewhat of an ambiguous position in this discussion of analytical units, as they are both artifacts themselves and delimitations of the spatial dimension of certain activities. They are thus both artifacts and components of the spatial dimension of other artifacts and behaviors. Moreover, as we shall see, the remnants of the structure may not delimit the distribution of the artifacts produced by behaviors which occurred within the structure.

Figure 22 contains a more explicit depiction of the postulated relations between Artifacts Units, Spatial Units, and Behavioral Units. In general, the spatial dimension becomes larger as the complexity of the behavioral and artifactual units increases. This also coincides with increasing levels of abstraction and decreasing degrees of precision and corresponding increasing levels of uncertainty. Thus patterning in attributes and artifacts yields the most reliable inferences within a limited spatial dimension for the behaviors of an individual or relatively

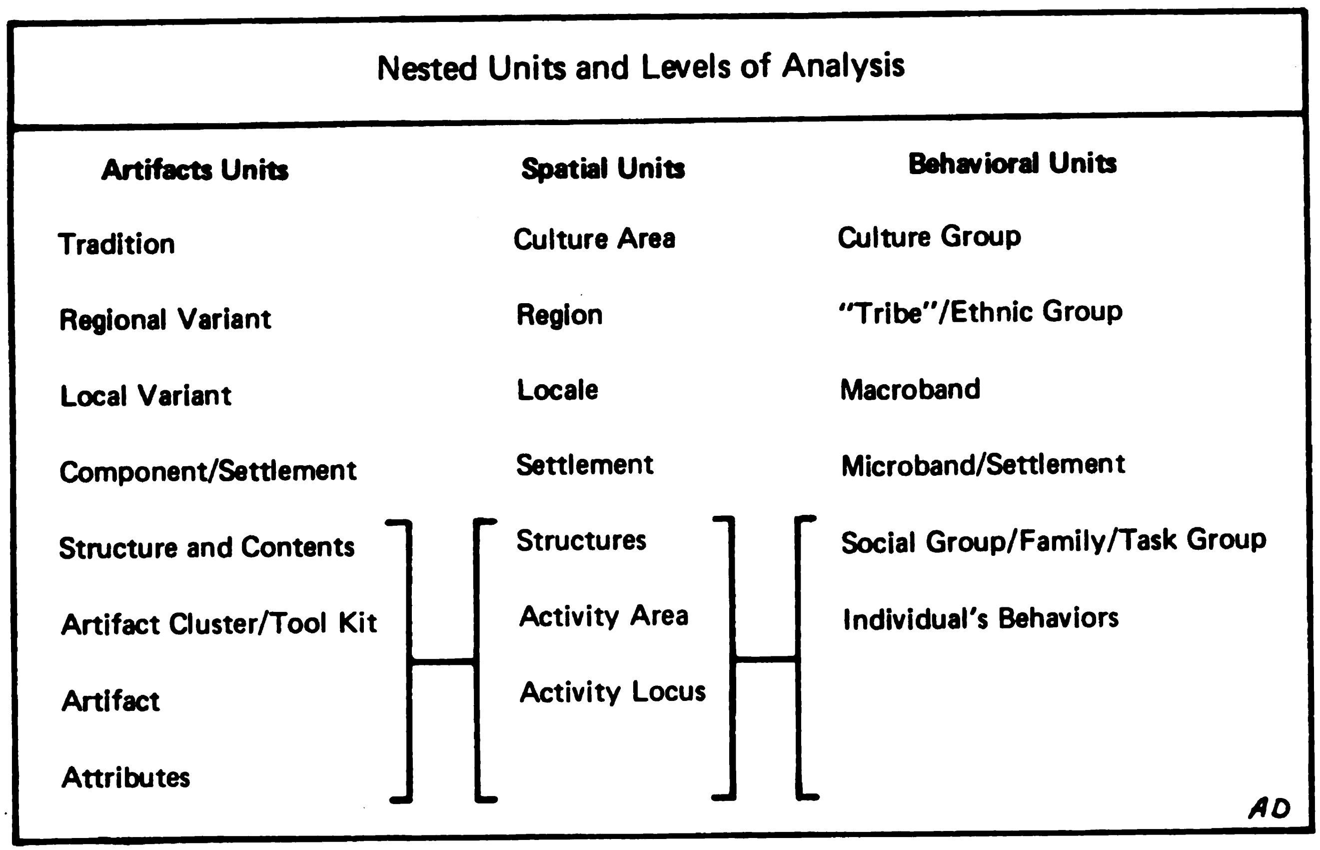


Figure 22 Nested Units and Levels of Analysis

small groups of people. Patterning at higher levels of generalization becomes subject to increasingly complex environmental constraints, and therefore greater uncertainty.

The fact that these hierarchies are nested, means that patterning at higher levels of analysis must be based on determined patterns among the units at lower levels, thus insuring the utilization of an inductive approach to specific data at lowest levels at some point in the scientific methods of analysis (Dekin 1972a; Thomas 1972:673). We cannot avoid basing our analytic procedures on the observed patterning of attributes of artifacts, moving from them up the hierarchies as depicted in Figures 21 and 22 towards more and more general levels and larger and larger areal, archaeological, and behavioral units. The analysis of the Pre-Dorset and Arctic Small Tool tradition data will proceed with such a strategy.

Retrospect and Prospect

From the above attempts to specify sources of variation in archaeological data which result from differences in field techniques and sampling, it should be apparent that little attention has been given to this problem, in spite of the realization that such problems exist. Increased attention to making our field and analytic techniques explicit should allow us to control the archaeological sources of variance in order to study those variations which resulted from prehistoric human behaviors. Instead of ignoring or postulating away sources of sampling error, we are able to make more precise statements of the possible extent of sampling bias, thereby increasing the precision with which we can deal with interpretive problems. Thus, greater explicitness will lead to greater precision in dealing with the data and in partitioning the sources of variation in archaeological interpretation.

The remainder of this part will consist of the development and testing of three models of Arctic Small Tool tradition behavior at three different levels of analysis. They are designed to demonstrate the utility of greater precision and explicitness in archaeological research at several levels of abstraction and with several combinations of data and methodology. The first is a model of behaviors within the Closure Site which is proposed for further testing on comparable data sets. The second is a model of the Arctic Small Tool horizon, which is seen as an example of the dispersal of human populations into an unoccupied ecological niche. The third is a model of culture growth, atrophy and stability in the Pre-Dorset, relating ecological variables in models of trajectories of change (Clarke 1968:75-77; Flannery 1973:47).

Model one is principally a contribution to methodology in which the proposed model is developed using a novel data structure, and suggested for further testing. The methodology may be of wider applicability than the model itself.

Model two may apply in other archaeological horizons where a newly formed or previously unoccupied ecological niche is occupied by the horizon. Potential horizons which might fit this model are the Thule horizon across the Arctic ca. 1000 A.D. and the early Paleo-Indian horizon into North America (Haynes 1974:381).

Model three may be applicable in a variety of situations where there have been significant environmental changes resulting in population and behavioral changes. As a similar model was useful in the Southwest (Plog 1974), the continued development of this model may be of wider utility.

Chapter 5

Pre-Dorset Tent Structures and Internal Activity Areas From the Closure Site

One of the research goals of the Closure Site excavations was the precise recording of excavation data to allow the consideration of hypotheses regarding the within-site structure of human behaviors. Inasmuch as such precise data preservation and presentation have not characterized much of Arctic archaeological research, we are faced with the inability to test hypotheses on the internal differentiation of behaviors within structures or find spots, since virtually none have been suggested. This is particularly true of find spots such as those from the Closure Site where obvious house outlines are not present.

I will approach these data from the Closure site with several hypotheses for testing regarding the nature of the behaviors reflected by the artifacts, rocks, and their distribution patterns. The following discussion is similar to one presented somewhat less formally at the School of American Research Advanced Seminar on Pre-Dorset -- Dorset Problems in 1973.

Arctic Small Tool structure forms vary from roughly square (Dumond 1971:8; Meyer 1970:Table 2) to square with rounded corners (Anderson 1970:9; Irving 1964; Rousselière 1964:Fig. 3; Gosselin et al. 1974:Planche 3) to round (Anderson 1970:9; Meyer 1970:Table 2) to oval or elliptical (Knuth 1967:49,Plate 7; Harp 1961:Fig. 5; Rousselière 1968:Fig. 5; Taylor 1968a:Fig. 5; Larsen and Meldgaard 1958:Fig. 18; Meldgaard 1962:Plate 5; and Meyer 1970:Table 2). The major dimensions of these structures vary from ca. 2.5 to 4 or 5 meters, although it is impossible to discern clear outlines in many cases (Anderson 1970; Knuth 1967; McGhee 1973; for example). Several archaeologists have described changes in house form as significant evidence for behavioral change, being coincidental with changes in other behaviors (Anderson 1970:9; Meldgaard 1962:93) but there does not seem to be any universal trend in changes from square to round or oval to rectangular. The Seahorse Gully site contains houses with variable outlines from square to circular (Meyer 1970:Table 2) although the house depressions are not always clearly defined (Meyer 1970:Fig. 26,b, Map 21).

Internal features, such as central passages, flagged floors, cleared and smoothed areas and fire places are also highly variable in both space and time. Some artifact scatters or central passages have no associated "house outline" (Knuth 1967; McGhee 1973) while other artifact and rock "clusters" are without apparent structure (Taylor 1968; Tuck 1973). A further complication is

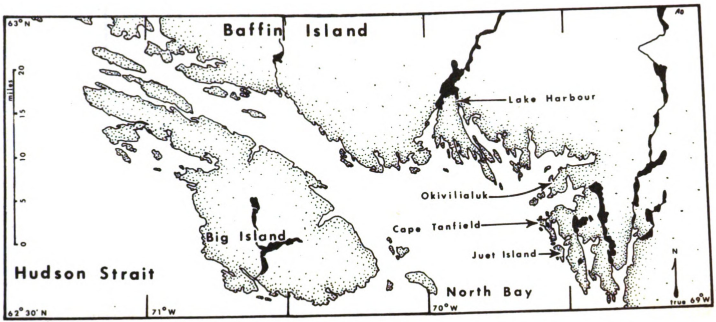


Figure 23 Lake Harbour and Vicinity

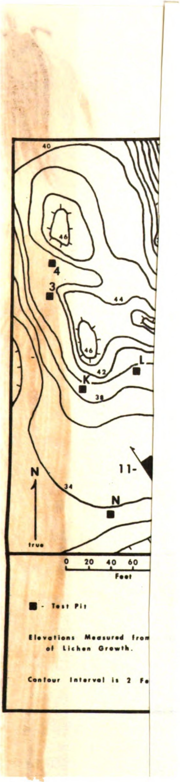


Figure 24 KdKq-11, The Closure Site

that there has not been consistent data collection and publication. Criteria for the selection of "peripheral rocks" from the general "background scatter" prevalent on many sites have not been made clear and we are forced to rely on the judgment of the archaeologist, being unable to evaluate his field interpretation (see Dekin 1972c; Meyer 1970:Map 21; Knuth 1967; Gosselin et al. 1974:Planche 3).

Applying the behavioral paradigm as set forth in this thesis, there may be several sources of variance in the reported behaviors reflected by the structural remains and internal rock and artifact distributions in Pre-Dorset sites. These may be listed as follows: the environment (including season of occupation; variegation in suitable rocks--flagstones; availability of heat sources--raw materials such as wood, animal fat, etc.; and availability of subsistence sources, which influences permanence of occupation or duration); behavioral norms (culture) or ideals and values; idiosyncratic choices (such free variation may approach randomness, at this level of analysis -- see Scheidegger and Langbein 1966 and McConnell and Horn 1972 for a discussion of macroscopic randomness, even though nothing is truly random at the microscale); and post-depositional processes (such as the removal of rocks for use by subsequent human populations) (see Figure 19, above).

It should be apparent, that by not assuming a priori that most of the variation in these behaviors is cultural, we have increased the explicitness of our consideration of possible sources of variance, and, at the same time, have allowed the formulation of hypotheses which would allow the partitioning of sources of variance. As an example, the shift in house form at the Jens Munk (Kapuivik) site at Igloolik coincides with a shift in geographical house location. All of the Pre-Dorset (called Sarqaq) houses above 22 meters in elevation are sheltered by adjacent rock outcrops and are exposed to the sea in one direction (see Meldgaard 1962:Plate 1). These are "rounded, oval houses with central fireplace" (1962:93).

The Dorset houses from 22 to 6 meters in elevation are on what Meldgaard infers to have been peninsula, with exposure to water on two sides and general location near the water on heights of land. They are located with respect to the relief and exposure of the site much as modern Eskimo winter houses are located (William Kemp, personal communication). Meldgaard describes them as "large, rectangular, dug-down houses with side benches" (1962:93).

This apparent shift in settlement location, with respect to geographic features, may reflect a shift in the season of occupation and in the activities which occurred on Jens Munk Island. Thus, the break which Meldgaard inferred from his 1954 and 1957 researches may represent a change in site utilization, rather than a change in culture or population. The changes in needle form, harpoon form, and the addition of multi-barbed fish spears, caribou bone awls and knives, and sledge-shoes and snow knives may reflect seasonal behaviors not previously practiced at this site. The subtraction of arrow points may be a similar change.

Flint flaker points are all seal penis bones in the late Pre-Dorset, but shift to walrus penis bones in Dorset (Meldgaard 1962:93). A shift in seasonal occupation may be accompanied by a shift in hunting patterns, possibly with greater reliance on walrus from this location, and a shift in the available raw materials. The sea mammal populations in the Igloolik region may also have changed in distribution and abundance, resulting from changed marine conditions. Processes of environmental change include climatic change and the continuing uplift of the land both of which may have caused significant changes in marine conditions. There may have been ample environmental reasons for a shift in the utilization of Jens Munk Island by a late Pre-Dorset occupation, and this may be reflected in changes in site location and artifacts found at Kapuivik (compare Knuth 1968:72-73 regarding seasonal variants of Independence II).

This hypothesis is also in accordance with the changes in Meldgaard's interpretations. His 1962 inference of a Dorset migration into the Igloolik area was apparently largely based on data drawn from the Jens Munk site, as he chose to discuss it at some length in support of his interpretations (1962:93). As a result of his later excavations at Igloolik, Meldgaard revised his views to see behavioral continuity as the basic theme, with rapid change and *in situ* development of Dorset from Pre-Dorset. It seems likely that, with an increased regional perspective and information, the coincidence of culture change with settlement pattern change at Jens Munk no longer dominated his data and conclusions.

Testing of such hypotheses regarding the causes of apparent changes in house form and location at the Jens Munk site would involve the analysis of faunal remains and inferred behaviors across this period of apparent change. We should be able to ascertain whether this variation results from changes in the scheduling of behaviors within an essentially unchanging system of behaviors and behavioral norms, or whether there is such a cultural change.

This example has been provided to demonstrate the utility of the behavioral paradigm which I have proposed. By focusing on the study of human behavior, and by refusing to assume changes as cultural (a priori), we facilitate the formulation of multiple working hypotheses and suggest avenues of future research regarding Pre-Dorset structural change through space and time.

The skimpy data on the seasonality of site occupation, house contents, and within-structure artifact distribution do not allow the formulation of general hypotheses regarding the spatial distribution of behaviors within Pre-Dorset structures. It is, however, useful to derive a model to describe the structural and artifactual evidence from the Closure site, with the hope that such a model could be tested against similar data from other Pre-Dorset sites, when the data permit.

From our general knowledge of Eskimo behaviors in the Eastern Arctic during the historic period and from previous ethnographic and archaeological research we can generate a number of working hypotheses for testing on the data from the Closure Site. Because our field techniques included the careful troweling and three dimensional recording of all rocks greater than fist size and all artifacts recorded and recognized in the field, we were able to prepare distributions of the in situ locations of artifacts and rocks for several loci of the Closure Site. These techniques were used in loci 11-6 + 50, 11-6 + 30, 11-7, and 11-8, resulting in the plotting of rocks and artifacts distributions (see Figures 29, 30, 31, and 32). Field interpretations were inconclusive (see Maxwell 1967) and we were not convinced that such accuracy was worth the large amounts of excavation time involved in obtaining such data. In loci 11-B and KdDq 23 and portions of locus 10 we plotted rocks only, recording artifact locations only by contents of quadrants of five-foot squares, unless an unusually significant artifact were encountered (see Figures 26, 27, and 28). Additionally, locus 11-6 + 30 has a large test pit (with unplotted artifacts) in the center of an apparent cluster of rocks and artifacts, so that the constraints of time and the needs of reconnaissance have influenced the comparability of data from within this site (minor

Burin, not poli
Burin, polished
Burin Spall
Burin-like Tool
Nosed Side-Scr
Side-Scraper
End-Scraper
End Blade, Tria
End Blade, Side
End Blade, Dou
Artifact Base
Biface Fragmen
Uniface Fragme
Microblade
Microblade Cor
Adze, Chipped
Adze, Ground
Side Blade
Slate Fragment
Pumice
Lamp Fragment
Rock
Rock, Carbon E

Figure 25 Key to Symbols Used on Distribution Maps and Profiles

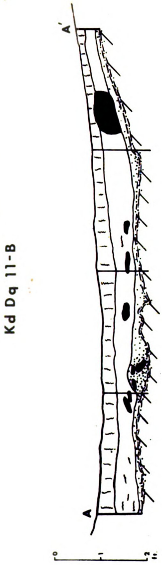


Figure 26 KdDq 11-B

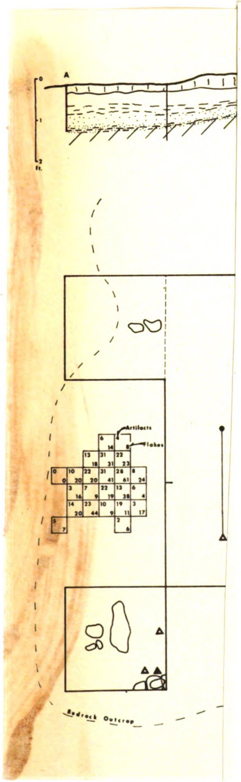


Figure 27 KdDq 11-10

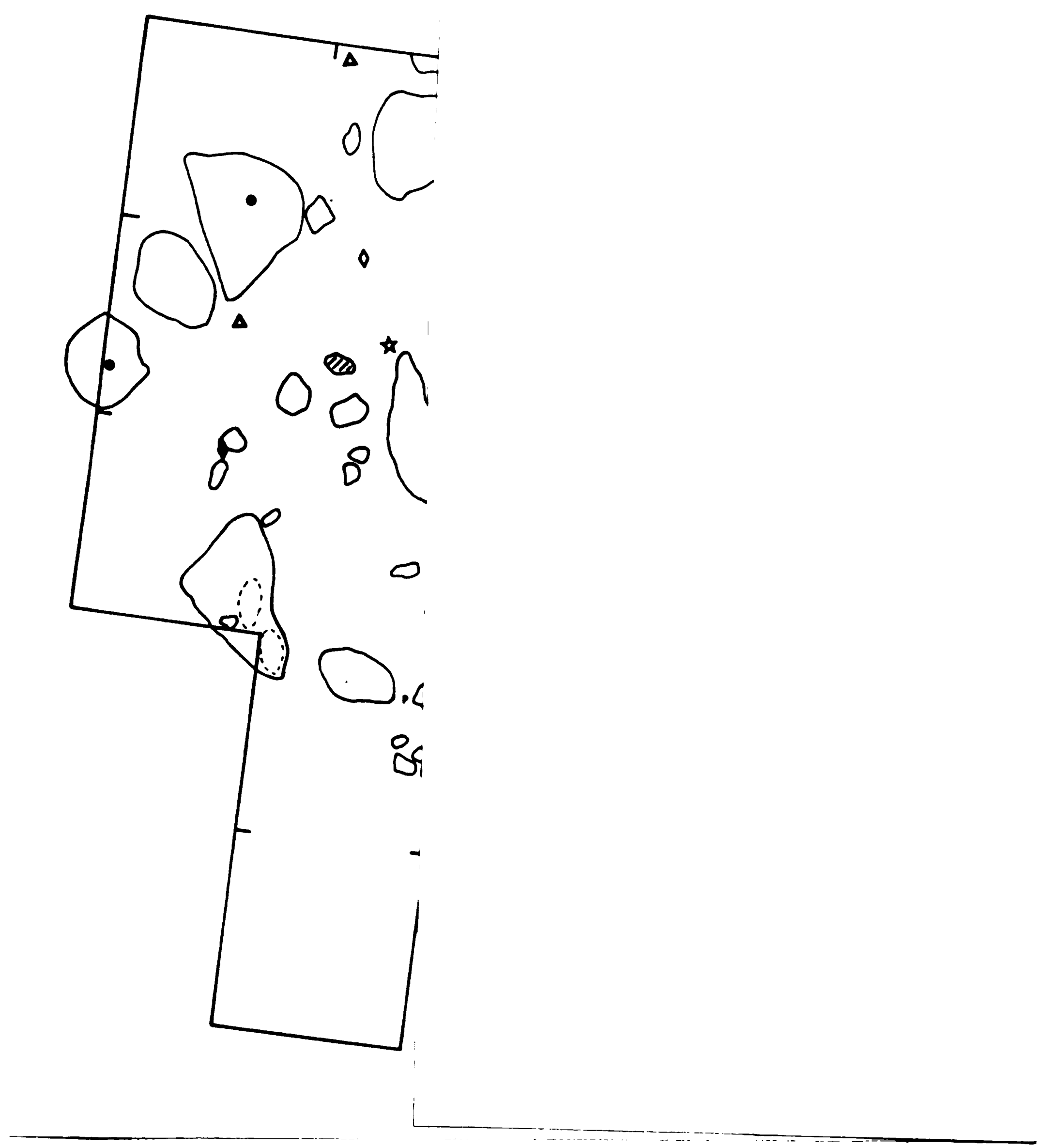


Figure 28 KdDq 23

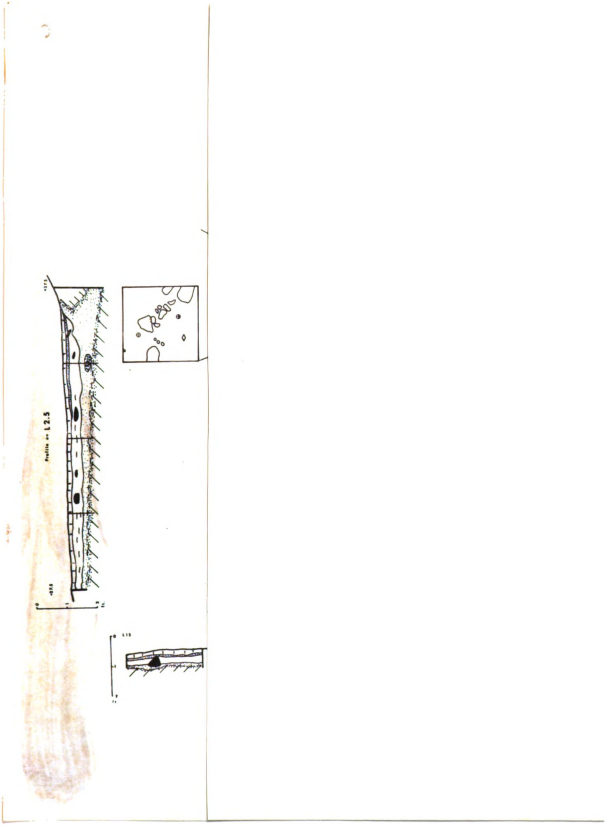


Figure 29 KdDq 11-6+30

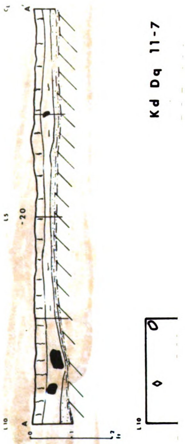


Figure 30 KdDq 11-7

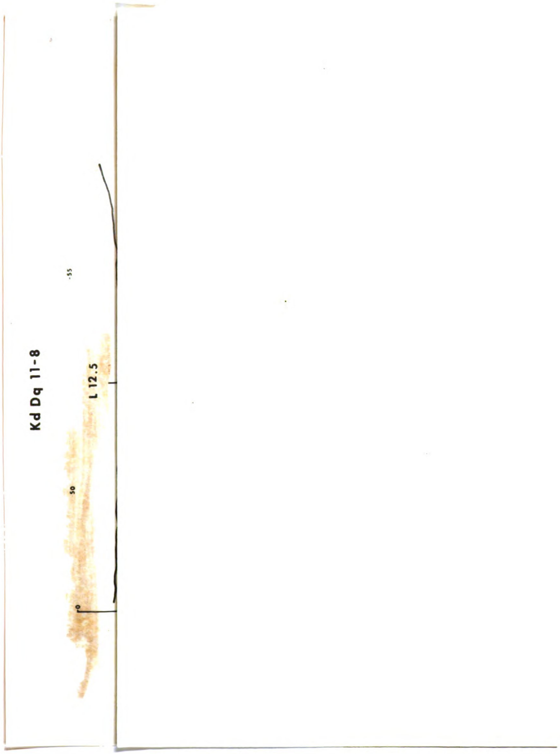


Figure 31 KdDq 11-8

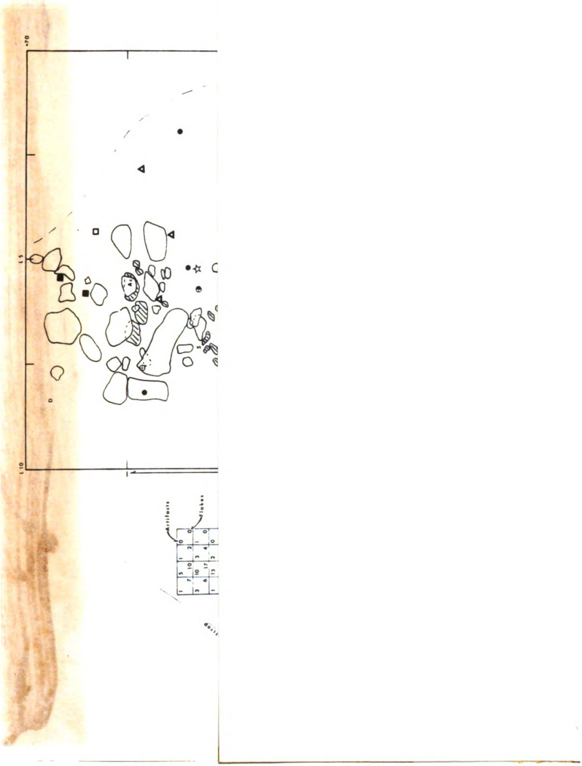


Figure 32 KdDq 11-6+50

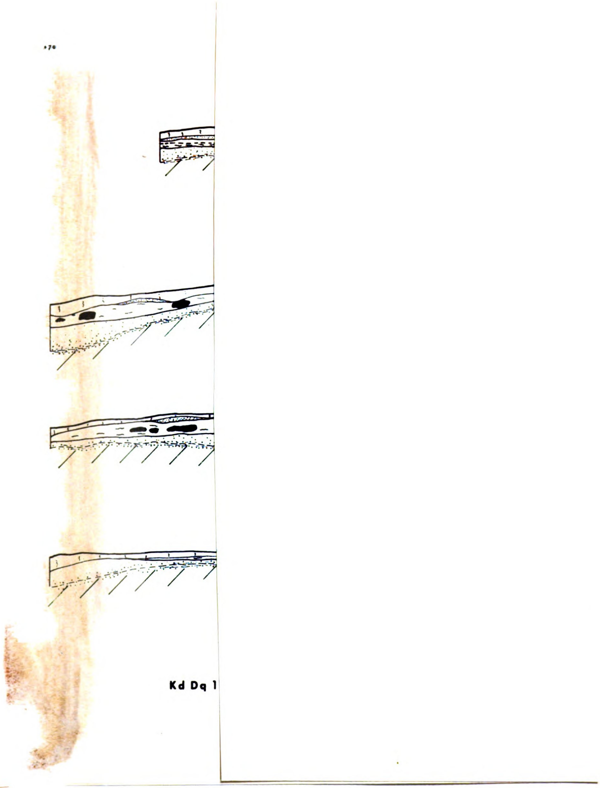


Figure 33 KdDq 11-6+50, Plus-Minus Profiles

Figure 34 KdDq 11-6+50, Left-Right Profiles

discrepancies in the number of plotted artifacts and the summary distribution chart presented on each distribution figure are the result of the recovery of artifacts in the laboratory for which precise field data were not recorded).

In the course of the analysis of these distributional data, we considered several hypotheses regarding the general nature of the activities represented by the finds from the two loci with the best evidence for artifact clustering -- 11-6 + 50 and 11-8. These were selected because I felt that the combination of excellent distributional data and observable rock and artifact clustering would yield the most reliable inferences regarding probable behaviors.

I. Hypothesized general activities reflected by the clusters 11-6 + 50 and 11-8

   a. dwelling structure, complete (preferred)

   b. portion of a larger structure

   c. exterior activity area (complete)

   d. portion of an exterior activity area

   e. secondary deposit.

There is no soils evidence that this could be a secondary deposit, and we can safely infer that we are dealing with generally <u>in situ</u> artifacts and rocks. The location of the artifact and rock clusters in the centers of slight depressions in the bedrock, bounded closely by rock outcrops suggests that these clusters are discrete entities, not directly related to adjacent depressions which are a minimum of ten feet away from the margins of these clusters. There is no evidence for continuous activity and artifacts which would link together such clusters. It is thus unlikely that these are portions of structures or that they represent a portion of an external activity area. The diversity of artifacts, their concentration in a cluster, and the ring-like arrangement of rocks suggest that these are not specialized activity areas outside of a dwelling, but rather support the hypothesis that these are complete dwelling structures in which a variety of activities occurred.

II. Hypothesized nature of these dwelling structures

a. windbreak

b. snow house

c. stone house

d. sod house

e. tent (skin) (Preferred)

There is no evidence that Arctic peoples have made extensive use of windbreaks as dwelling structures, especially given the nature of the weather and environment of Hudson Straits. A windbreak would not be sufficient protection. The present environment does not provide sufficient snow suitable for the construction of snow houses at this location (William Kemp, personal communication) and the clustering of artifacts in the general centers of depressions suggests that the micro-relief of the bedrock exposures was an important constraint in the location of these activities, which would not have been the case if there had been several feet of snow over them into which snow houses were built. Other sites in the Cape Tanfield area where sod houses were apparently constructed revealed large amounts of sod accumulation, which is not the case in these loci. The stones are not of sufficient size or frequency to support the hypothesis of a house built largely of stone. The evidence is in accordance with the hypothesis that some form of skin tent was the dwelling structure.

III. Hypothesized shape of the skin tent

a. rectangular

b. circular

c. elliptical (preferred)

By inspection, the best geometric shape that would conform to the pattern of rock distribution would be an ellipse. There is no evidence for rectangular tents in the historic period, and elliptical tents (occasionally grading into more circular tents) are frequent across the Arctic (see Spencer 1959:44; Balikci 1970:26; Thomsen 1928:294). In an attempt to model such a shape, an ellipse was constructed to enclose the majority of rocks centrally located within the clusters (Figures 35 and 36). Several trials and orientations were necessary to obtain a "best-fit". Attempts to fit other shapes to this cluster support the hypothesis that the shape of the tent was elliptical.

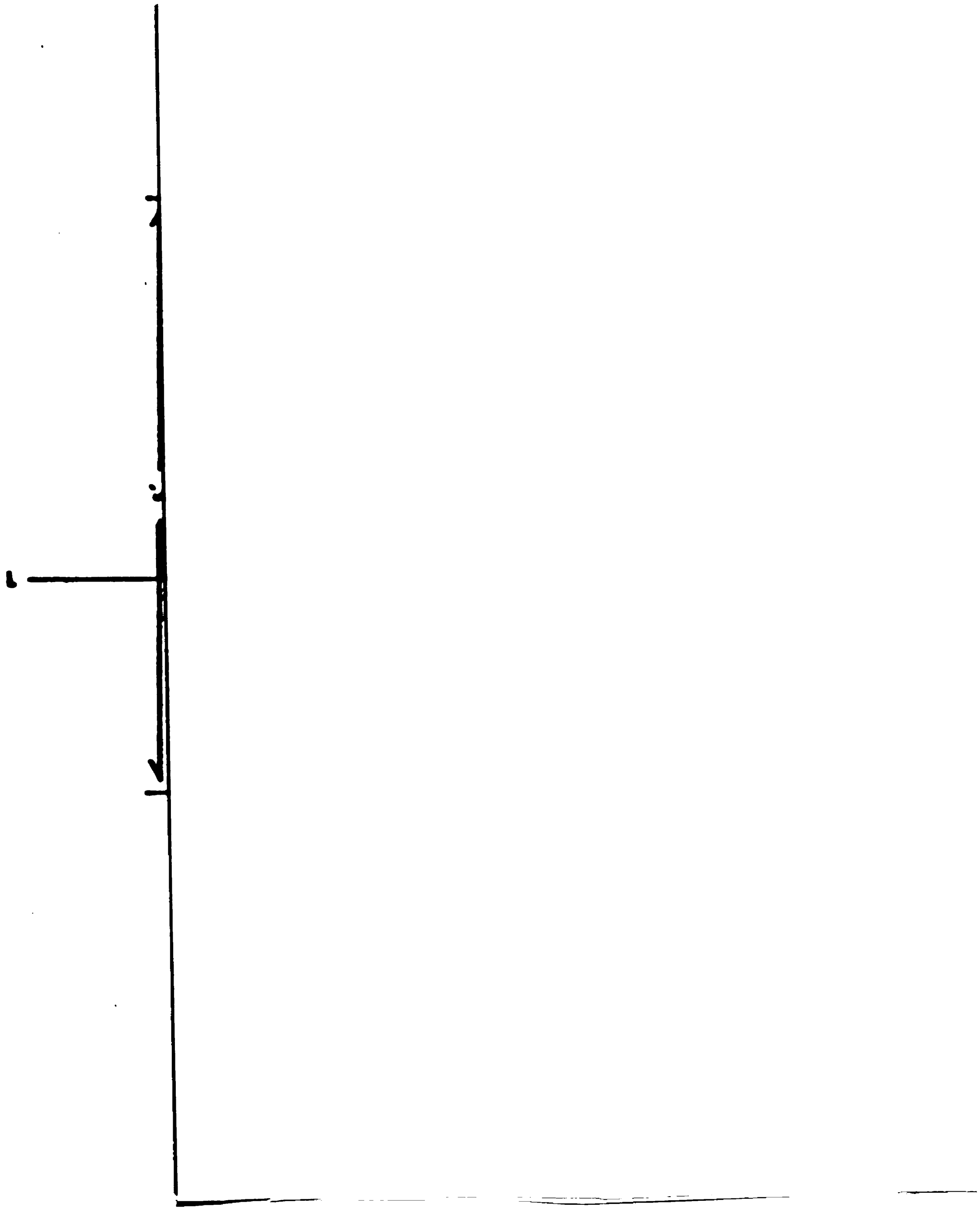


Figure 35 KdDq 11-8, Ellipses Constructed

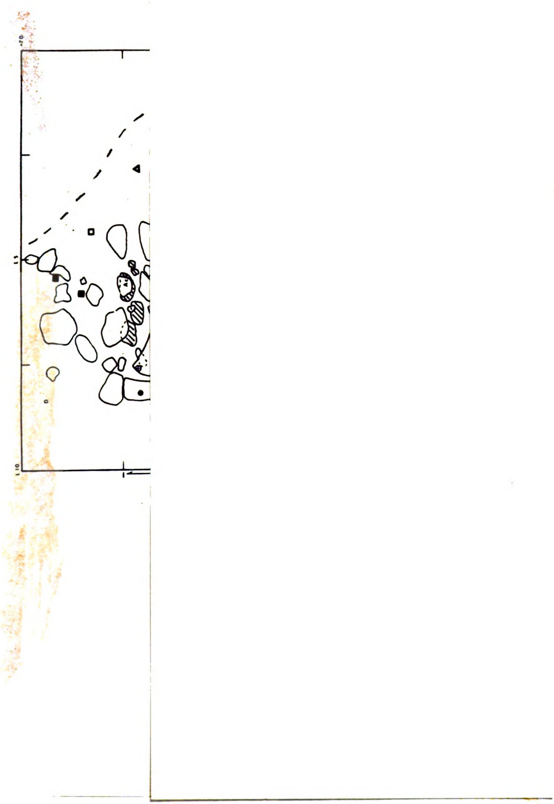


Figure 36 KdDq 11-6+50, Ellipses Constructed

IV. Hypothesized configuration of the skirt of the skin tent

a. not tucked (stops at ground level)

b. tucked inside (preferred)

c. tucked outside

There is not ethnographic evidence for tent skirts which stop at ground level, without tucking under rocks or gravel, as this would not be a desirable design where the weather is wet and windy. The Netsilik used tents with skirts of both types (Balikci 1970:26) holding down the skirts tucked inside with rocks on the inside or holding down the outside skirts with rocks on the outside periphery (see Boas 1964:145,Fig. 117). When these tents are taken down, we might expect two characteristic patterns of rock and artifact distribution, coincident with the type of skirt construction.

With the skirt tucked inside, we would expect artifacts to be distributed within the living space, with some falling between the inside rocks into the skirt itself. When the tent is to be taken down, the inner rocks are rolled toward the center of the tent, clustering on artifacts previously lost or discarded. When the skirts are lifted, artifacts that had lodged there are dropped off, some under the area where the skirt had lain, but most would be distributed towards the interior where they would concentrate among the previously deposited rocks and artifacts. Additional artifacts might be strewed or dragged in in the direction in which the tent was taken off its supporting structure. The area under the skirt would be generally devoid of rocks used in the tent construction, with the possible exception of the entry where rocks might have been rolled into the entry area and rocks used for holding entrance flaps or for seating might remain.

With the skirt tucked outside, we would expect artifacts to be distributed within the living space, with no "wall" rocks used inside the living area. When the tent is to be taken down, the outer rocks on the skirt are rolled away from the tent off the skirt (see Thomsen 1928:293-294). The area under the skirt would generally be free from artifacts and rocks, with artifacts concentrated in the center of the distribution, a peripheral area free from artifacts and rocks, and an accumulation of rocks at the margins of the cluster.

To test these alternative models, I constructed another ellipse about the first ellipse (Figures 35 and 36), so that the outer ellipse was twice the absolute area of the first ellipse, and so that both ellipses had common axes. These ellipses were constructed as heuristic devices to structure the data for the following tests of the models as proposed above. Note that the ellipses and their axes divide most of the excavated portion of the loci into eight samples of equal area, with peripheral areas containing fewer artifacts and rocks.

With the skirt tucked inside, artifacts should continue

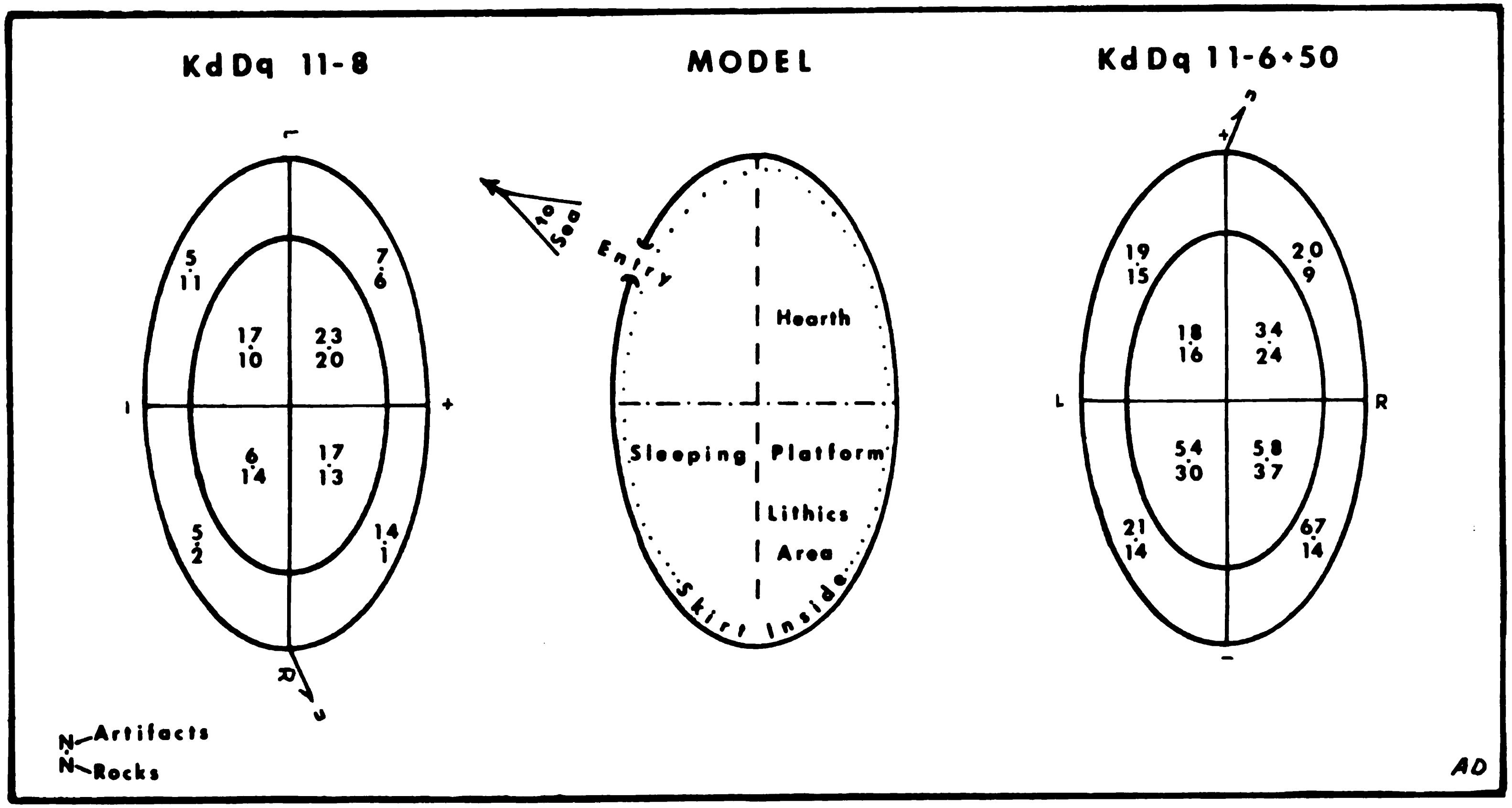


Figure 37 A Model of Pre-Dorset Tent Structures From The Closure Site

Table 10 Statistical Tests of Significance of Artifact/Rock Distributions

KdDq 11-8

I. Artifact and rock distribution, excluding Minus-Left quadrant

	Artifacts	Rocks	N
Inner Ellipse	46	47	93
Outer Ellipse	26	9	35
			128

X^2=6.37 df=1 p=less than *ca*. 0.01
Null hypothesis rejected.

II. Rocks in outer ellipse, minus quadrants

Rocks, Minus-Left	Rocks, Minus-Right	N
11	1	13

X^2=6.2 df=1 p=*ca*. 0.015
Null hypothesis rejected.

III. Rocks in inner ellipse, left quadrants

Rocks, Plus-Left	Rocks, Minus-Left	N
20	10	30

X^2=3.3 df=1 p=*ca*. 0.07
Null hypothesis rejected.

IV. Artifacts in inner ellipse, right and left halves

Artifacts, Left Half	Artifacts, Right Half	N
40	23	63

X^2=2.29 df=1 p=*ca*. 0.12
Null hypothesis probably rejected.

V. Rocks in inner ellipse, right and left halves

Rocks, Left Half	Rocks, Right Half	N
30	37	57

X^2= less than 0.16 df=1 p=*ca*. 0.70

VI. Artifacts in inner ellipse, plus and minus right quadrants

Artifacts, Plus-Right	Artifacts, Minus-Right	N
17	6	23

X^2=5.26 df=1 p=*ca*. 0.02
Null hypothesis rejected.

Table 10 (Cont'd)

VII. Artifacts in right outer ellipse, plus and minus quadrants

Artifacts, Plus-Right	Artifacts, Minus-Right	N
14	5	19

X^2=4.26 df=1 p=*ca*. 0.03
Null hypothesis rejected.

KdDq 11-6 + 50

I. Artifact and rock distribution

	Artifacts	Rocks	N
Inner Ellipse	164	107	271
Outer Ellipse	127	52	179
			450

X^2=5.13 df=1 p=*ca*. 0.03
Null hypothesis rejected.

II. Artifacts in inner ellipse, right and left halves

Artifacts, Left Half	Artifacts, Right Half	N
112	52	164

X^2=21.95 df=1 p=less than 0.001
Null hypothesis rejected.

III. Artifacts in left Hemisphere, plus and minus quadrants

Left Hemisphere, Plus Quadrant	Left Hemisphere, Minus Quadrant	N
75	125	200

X^2=12.5 df=1 p=*ca*. 0.001
Null hypothesis rejected.

into the outer ellipse from a concentration in the inner ellipse while rocks do not. A test of significance (Chisquare) demonstrated that the difference in the outward extension of rocks and artifacts was not significant in locus 11-8, when we included the entire periphery of the ellipse.

By inspection, there is a cluster of rocks in the outer ellipse at Minus fifty-six Left fifteen (-56L15). This is the only portion of the cluster that could be interpreted as an entrance structure. If this is correct, then removing the concentration of rocks in this quadrant from the sample tested would allow consideration of the hypothesis along wall areas only, stating that along wall areas, artifacts continue into theouter ellipse while rocks decline significantly in frequency. Thus, excluding the minus-left quadrant, the observed distribution is statistically significant and the hypothesis that tent skirts were tucked inside is supported. Locus 11-6 + 50 demonstrates such a significant decline in rocks in the outer ellipse along the entire periphery, and also supports this hypothesis.

Summarily, the lack of rocks outside the constructed ellipse and their concentration within the inner ellipses while artifacts continue to be found in the outer ellipse, support the hypothesis that the tent skirts were tucked inside with rocks piled on this tucked skirt to provide tension and support.

V. Hypothesized entrance location

a. Locus 11-8, in the minus left quadrant of the outer ellipse (preferred)

b. Locus 11-6 + 50, in the plus quadrant of the outer ellipse (preferred)

c. elsewhere

There is ethnographic evidence for rocks around entrances of tents, used both for support of the tent flaps and for seating (Balikci 1970:27). Additionally, at an entrance where there is a break in an otherwise continuous wall, the rocks there will not have to be rolled off into the interior for removal of the tent. They may, in fact, be rolled into the entrance passage proper. This kind of a rock concentration on the periphery of the cluster is not found at any other location on the ellipse periphery, thus there is no support for alternative locations. The concentration of rocks in and outside the outer ellipse at these locations is significant when compared with adjacent portions of the periphery, thus the data support the hypothesis that the entrance was in these quadrants.

VI. Hypothesized internal activity organization

a. no internal differentiation of activity location (activities and artifacts distributed homogeneously and randomly)

b. internal activity areas (preferred)
   1. hearth and associated work area
   2. entrance work-butchering area
   3. sleeping platform divided into two areas
      - lithics area, possibly male activities
      - non-lithics area, possibly female activities

In locus 11-8, evidence for extensive fires in the form of charcoal encrusted rocks (one of which was dated by the encrusted charcoal at ca. 4690 $\pm$ 380 radiocarbon years -- GSC-1382) is found almost exclusively in the Plus-Left quadrant. In the absence of alternatives, we can safely hypothesize that this area was a cooking-heating area, probably heated with a seal-oil lamp, as we have no evidence of wood charcoal and there were abundant rock encrustations usually interpreted as resulting from the burning of overflowed seal fat.

There is also a significant concentration of rocks in this Plus-Left quadrant of the interior ellipse, when compared with the adjacent Minus-Left quadrant interior to the entrance feature, which is in accordance with the use of rocks to support lamps or other associated objects (cooking vessels, etc.).

There is considerable ethnographic evidence for the utilization of a large portion of the interior space of a tent as a "sleeping platform" (which is kind of a misnomer, as many activities were conducted there in addition to sleeping). Throughout the historic period until the present day, Eskimo tents usually have roughly one-half of the interior space as "sleeping platform". A comparison of artifacts and then rocks in the two halves of the inner ellipse indicates that there is a significant decline in artifacts in the right half while rocks continue. Further, within this right half of the inner ellipse, there is a significant decline in artifacts in the Minus-Right quadrant.

This pattern is mirrored in the adjacent outer ellipses, as there are more artifacts in the Plus-Right area than in the Minus-Right area of the outer ellipse. These data support the hypotheses that there was a sleeping platform and that it was divided into two activity areas, one of which did not involve as many stone tools as the other.

All of the end-blades found in this locus are associated with the entrance feature or are adjacent to it. This is consistent with the activity of bringing in game and possibly processing it in this area.

The distribution of burins, burin fragments, and burin spalls in the areas adjacent to the hearth and on the Plus half of the hypothesized sleeping platform may indicate their use by men, as these grooving and engraving tools are thought to be used in the repair and construction of equipment used in the hunt (harpoons, toggles, etc.). The decline in stone artifacts frequency in the Minus half of the sleeping platform may indicate that this was an area frequently used by women whose tools were of generally perishable organic materials (antler, ivory, wood, bone, skin, etc.) and who did not use stone tools to as great a degree as the men. While the division of such behaviors may not have been based on sex, this seems a reasonable inference, in light of ample ethnographic evidence for such a division in the historic period in this area.

In locus 11-6 + 50, within the hypothesized structure charcoal encrusted rocks are found only in the Right quadrant, supporting the interpretation that this area was a cooking-heating area, similar to that from 11-8. A similar area may have occurred outside the structure between +65 and +70. One of these "cooking rock" encrustations was dated ca. 4460 ± 100 radiocarbon years (Gak-1281). There is a probability of 0.55 that this date is coeval with the date from 11-8 above (see above).

The pattern of rock and artifact distribution within the elliptical model of 11-8 is also observed here in the distributional data from 11-6 + 50, with rocks fairly evenly distributed, but artifacts concentrated in the Left quadrants (left hemisphere), supporting the hypothesized location of a "sleeping platform".

While the artifacts in the Left inner ellipse are evenly distributed, there is a much larger number of artifacts in the Minus outer ellipse quadrant, which supports the above contention that significantly more lithic utilization and working occurred in the quadrant of the sleeping platform adjacent to the hearth area. These data support the suggestion that there was differential use of the sleeping platform, and that such a differential may have been based on sex.

By using ellipses (a geometrically-specific construction) constructed about a rock cluster, the data can be structured to test hypotheses on the nature and design of the structure itself, the division of its activity areas, and on the definition of behaviors and possible divisions

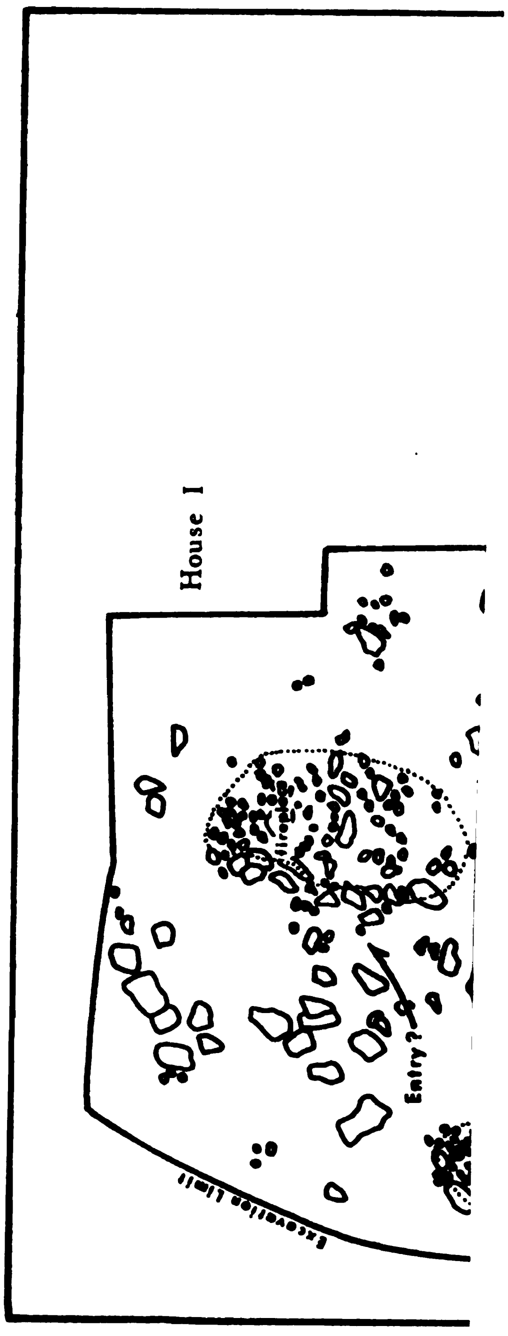


Figure 38 Igdluluarssuk

of labor and tool use.

The ellipse constructed and divided for the above analyses provide a model of Pre-Dorset tent structures at the Closure site. Fortuitously, the axes seem to coincide with internal divisions of behavioral areas within the tent. Thus, the ellipses constructed originally to test hypotheses regarding the design of the dwelling, end up providing a useful iconic model of the structure itself and of its activity areas. The degree to which this model holds for other Pre-Dorset structures is a matter for further testing, but the analytical model presented here should be considered whenever we are dealing with data of artifact and rock clusters.

The analytical techniques introduced here (use of geometric models for structuring the data) may be applied to a variety of archaeological complexes in widely scattered geographic areas wherever rock and artifact clusters comprise the available data (Fitting 1965; MacDonald 1968). This analysis suggests that precise excavation techniques may yield data useful to the study of the spatial distribution of behaviors within archaeological sites, particularly in the Pre-Dorset, and also suggests that more attention should be given to possible internal differentiation of behaviors within Pre-Dorset structures. It seems likely that this model should be tested wherever the data are relevant, such as in North and Southwestern Greenland (Knuth 1967; Larsen and Meldgaard 1958) and elsewhere (Maxwell 1973a;

Taylor 1968a).

The major contribution which explicit and precise excavation techniques combined with the use of novel data structures make is that they allow the focus on the *relations* among artifacts and the testing of hypotheses regarding the nature of dwelling structures. The modeling of these data as tent structures may solve an interpretational problem which had not been resolved by traditional techniques and methods of analysis (Maxwell 1967, 1973a:303-4, 310).

Chapter 6

The Arctic Small Tool Horizon: A Behavioral Model of the Dispersal of Human Populations Into an Unoccupied Niche

There can be little doubt that the majority of the area through which the Arctic Small Tool horizon spread was unoccupied by human populations. This is particularly true of the Eastern Arctic, with the exception of the southern fringes where they may have come into contact with Archaic peoples near the ecotone between the tundra and the taiga.

In the Western Arctic, it is apparent that there had been earlier human populations in many areas, but these later migrants appear to have occupied a previously unoccupied ecological niche. Geographically, they spread along the Arctic coasts until they had reached the maximum extent of seasonally frozen coasts, usually with adjacent tundra. While it may be that the rising sea level had flooded earlier evidence of such a coastal occupation and that this habitat had not been unoccupied as it now appears, the present data support the inference that Arctic Small Tool populations were the first to accomplish a successful adaptation to these particular conditions in the American Arctic. For the present, I wish to focus on this coastal aspect of Arctic Small Tool peoples, ignoring (or not modeling) the expanded distribution of sites in interior Alaska (Figure 1) which also occurred at this time.

This model may apply best to the eastern extension of the Arctic Small Tool tradition (the Pre-Dorset) but it may be useful to consider a wider data base during the initial formulation of the model.

The methods used in the formulation of this model rest on the utility of borrowing theoretical models from similar academic disciplines, in this case, from the study of migrations of animal species. While Service has cautioned us against the uncritical borrowing of such concepts and methods from other disciplines (and named the faddistic malady, "Mouthtalk") he suggested that the surest way to avoid problems of uncritical borrowing was to follow the sequence: "problem now: borrow later" (Service 1969:79) (see Hagerstrand 1967 for an example from geography; Pred 1967:324).

The problem is in trying to explain variations in what appears to have been a rather rapid expansion of people across approximately four thousand miles of Arctic coast. These data have been modeled as representative of two migrations spreading throughout the Eastern Arctic separated by several centuries in time (McGhee 1973, 1974), or as a relatively simultaneous migration of two related but differentiated populations, one into the high Arctic and one into the middle and low Arctic (Maxwell 1967). Thus, the variability in artifact assemblages from early occupants of the Eastern Arctic has been interpreted differently by two essentially incompatible models which have been derived inductively from considerations of the archaeological data. Since there is essential agreement that we are dealing with the migration of human populations into the Eastern Arctic which formed archaeological data which could be considered a horizon, it seemed appropriate to attempt the derivation of a model of migrations which would account for the diversity in artifact assemblages found within the horizon. In attempting to model this horizon, I initially considered concepts and analyses used in general systems theory to sensitize me to previously used techniques and methods for the analysis of changing systems. This was followed by a consideration of ecological models from which the following model is deductively derived (see Redman 1973b:16; Spaulding 1973:346). Such procedures for the derivation of hypotheses and hypothetical models are widely accepted as part of the cycle of induction and deduction which characterize the prevailing scientific paradigm (Hill 1972:94-97; Pelto 1970:17; Kemeny 1959:86).

From an ecological systems perspective, the adaptive relations between a species and its ecosystem and between a human behavioral system and its environment system may both be seen as examples of coupled systems (Clarke 1968:50-51) where the adaptive response of one system to the other

". . .is merely a particular case of jointly correlated transformations" (Clarke 1968:57). It is thus possible to hypothesize that explanatory models regarding these transformations in the coupled systems of species and environment might also be appropriate models of the coupled systems of human behavior and environment.

Wilkinson based part of his analysis of human predation and migration into North America on the assumption

> That human populations colonizing previously unoccupied areas will tend to follow the pattern observed for other colonizing species; namely, an initial spread leading to the establishment of an equilibrium with respect to resources (1972:554).

I wish to make explicit the development of a model of such colonizing.

Valerius Geist developed a dispersal theory to explain the Pleistocene evolution of one group of ruminants, and, believing that such a theory might be useful and relevant to the understanding of similar evolutionary change in other species, developed a more generally applicable dispersal theory describing pioneering populations.

> The further the species disperse, the greater the difference between individuals of the parent population and the population at the fringe. Since selection for neoteny comes to an end when the habitat is filled to carrying capacity, the gene flow is broken by a fragmentation of the species into isolated populations. . ., and individuals from the populations between parent population and the colonizing fringe will form a cline. This cline is not related to clinally varying environmental factors, but to the dispersal history of the species. . . . The. . .theory thus explains the formation of certain clines,

> and the rapid evolution of Ice Age ungulates and their giant horns, antlers, and tusks. It links rapid evolutionary change to colonization and suggests a long period of evolutionary quiescence thereafter. Thus it challenges the concept of continuous, progressive evolution (1971:288-89).

Geist went on to relate this theory to patterns of morphological diversity and zoogeography in other ungulates concluding, with particular relevance to the environments with which we are interested:

> In the periglacial zones, differentiation is related to colonization of vacant habitat, not to ecological differentiation. Hence, when the "advanced" form meets a "primitive" one, they are not compatible since they have the same ecological adaptations. They cannot live sympatrically. Therefore, one form may replace the other, they may mix, or they may remain largely separated and joined by a stable hybrid zone. . . . This implies that although evolutionary differentiation may proceed at a high rate in the northern zones, the differentiated forms cannot survive sympatrically. The rate of extinction is high, and hence, species' diversity in this rather unproductive land remains relatively low (1971:311-312).

Applying these ideas to migrations of human populations we would expect behavioral differentiation to occur as a direct result of the dispersal of the population and that clines of such differences would occur in proportion to space and time in spite of environmental homogeneity. If there were habitat differences, such adaptive variation would be in addition to clinal differentiation.

Implicit in the extension of Geist's ideas to human populations is the equivalence of the transmission of genetic information with the transmission of behavioral information, since such a migration would involve clines of

communications and behavioral similarity.

As these clines develop as a result of the dispersal process, they may be subject to other characteristics of diffusion (Gould 1969). For example, we might expect that as the habitat is filled, the expanding frontier would be subject to clinal compression, because of the inertia of the expanding population against the boundary (or barrier, see Gould 1969:12; Haggett 1966:60; Hagerstrand 1967:293). At the point of origin of expansion, there may be clinal expansion, again as a result of inertia of expansion as population growth ceases. Barriers in general may truncate frontal clines in advance of the barrier and stretch such clines after permeation of the barrier, and we might expect such changes in clinal variation at significant geographic barriers, such as Smith Sound or Northeast Hudson Bay (Gould 1969:12).

Explicitly, the model states: With human population dispersal into an unoccupied ecological niche, behavioral variation will occur in proportion to the dispersal variables of space and time.

Pre-Dorset artifacts would be expected to vary clinally in proportion to their spatial distribution, if we control time by considering early sites as a horizon and if we can examine data for non-functional variability. By studying artifact variability which approaches normal distributions and is thus not tightly constrained by functional requirements, we can examine the evidence for clinal variations which the model predicts. The following test conditions are a more explicit statement of the data necessary to evaluate the model's predictions.

Test Conditions

The following is a stipulation of the data structure which we would expect if the above model is appropriate. Note that this is not a test to establish if the Arctic Small Tool horizon is a migration of people, as no reasonable alternatives exist. Only the southern boundaries of the horizon have earlier human occupations (Noble 1971; Dumond 1971; Fitzhugh 1972; and Tuck 1973) while most of the horizon extended into unoccupied territory.

If this model is appropriate, we should expect two general types of variation in proportion to space and time. Single variables should exhibit clinal variation and there should be increased variability among functionally-equivalent artifacts. These should be seen in attributes usually categorized as "style" or as "free variation" among functional equivalents. Adaptive variation resulting from the exploitation of new and diverse ecological niches should be in addition to the above variations and should be habitat-specific.

Two fundamental problems face this analysis: 1) there is no agreement on what constitute stylistic variations in lithic artifacts; and 2) there is no agreement on which artifacts are functional equivalents. Moreover, Maxwell has suggested (on a largely intuitive basis) that the artifacts from the Pre-Dorset and Dorset occupations of Southern Baffin Island ". . .have a high degree of functional specificity" (1973a:344) in which there is a close relation between morphology and function, mitigating against significant free variation in artifact morphology.

Nevertheless, we might expect that artifact size would be subject to a degree of clinal variation (possibly as discrete variation, due to sampling) as demonstrated in such artifacts as burins and microblades (selected in part because they are ubiquitous in these assemblages and because they have most frequently received descriptive attention). We would thus expect continuously varying attributes (burin length, burin width and microblade width) to demonstrate considerable variety through space, if we are able to control for the variables of time (done by limiting the data to the horizon of 4100-3700 B.P.) and environment (raw materials, functional difference, etc.).

We might also expect that new forms of tools would appear, either replacing earlier forms or in addition to them. We will consider end blades and end scrapers, as they too have received considerable attention, focusing on variability in shape (the easiest to obtain from the published examples).

If the above model is appropriate, we will find increased variation in burin length and width and in microblade width in the Eastern Arctic and from the high Arctic

to the low Arctic. We would also expect to find new forms of end scrapers and end blades in the Eastern Arctic, with some differences between northern and southern assemblages.

Testing the Model

The data on which the model will be evaluated are summarized in Tables 11, 14, and 15 which contain data published with regard to sites in the Arctic Small Tool horizon between 4100-3700 B.P. (see Figure 6). Decisions to include sites within this horizon for purposes of testing this model were based on the adjusted radiocarbon chronology of Figure 6, with the following exceptions. The Annawak site (KeDr 1) was included on the basis of Maxwell's placement of the site within the Lake Harbour Pre-Dorset at a time earlier than its radiocarbon date (Maxwell 1973a:308; based on the overall crudeness of the artifacts and lack of sidenotching). The Loon site and site KdDq 13 were excluded on the basis of later radiocarbon dates and the apparent extent of their ground slate industry. While it is possible that the Arnapik site was occupied somewhat after the arbitrary limit of this horizon (3700 B.P.), the general similarity of the Arnapik artifacts to those from Igloolik and the southern Baffin Island sites under consideration here suggests its inclusion.

The Closure site itself was divided into three groups for purposes of this analysis: KdDq 23; KdDq 11-10; and KdDq 11-B, 6, 7, 8. Site 23 was considered separately because of its spatial separation from the rest of the Closure site (see Figure 24) and because none of its burins are polished. KdDq 11-10 was considered separately because of its prominence at one end of the site, its slightly higher elevation, and extremely low frequency of polished burins (1 out of 40). KdDq 11-B, 6, 7 and 8 were lumped together because they are all at approximately the same elevation and are contiguous, moreover, the radiocarbon dates from KdDq 11-6 and KdDq 11-8 have a probability of 0.55 that they date the same event. Their contents are similar.

There is considerable variety in sample size on which the means are computed (see Chapter 5 above) and there are several collections on which no data are available (the data from the Independence I sites in North Greenland are computed from the illustrations in Knuth 1967 and Giddings 1967, although Knuth's narrative confirms their large size -- Knuth 1967:34).

Perhaps the most striking aspect of these data is the extremely small size of the burins from southern Baffin Island (the Closure site) and the extremely large size of the burins from Independence I, both when compared with those from the Western Arctic and other sites in the East. Without complete parameters for any of these series it is impossible to assess the levels of significance of the difference among these samples, but the relative disparities are clear. Note that the clinal variability in these samples is appropriate to the model's predictions with regard to mean burin length and apparently with regard to mean burin width, although somewhat less clearly. Perhaps this is a case of even greater variety in this latter dimension, such that the stochastics approach randomness about a value constrained by limitations of hafting technology. As Table 12 demonstrates, the dispersal around the mean burin width is indistinguishable from the normal. This normalcy of dispersal is also true of the distribution about the mean burin length. This is noteworthy because to reflect "stylistic" variation, we would expect the parameters not to be tightly constrained and to vary normally. This is particularly true of those chosen from this series, especially when compared to a similar variable, haft length, which is skewed in the direction of somewhat longer hafts with a highly peaked curve. This implies that hafting requirements demand a haft length of a certain minimum size and that Pre-Dorset craftsmen held closely to this norm, allowing only little variation in the direction of increased haft length.

To reiterate, these parameters of the series of burins from the entire Closure site reinforce the suitability of burin length and burin width for the above analysis, because of the apparent lack of severe manufacturing constraints.

The interpretation is less clear with regard to microblade width, except that there is considerable variety among the samples which does not seem to reflect a dispersal vector. However, in order to assess the variability in this horizon, we can compare the variability of the means of this set of samples with the variability of the set of samples reported by Anderson (1970:14) which he interprets to represent the same technological tradition (Proto-Denbigh, Classic Denbigh Flint and Late Denbigh Flint) through time in the same area. If the above model is appropriate, we should expect greater variability in the horizon than in the tradition.

The mean of the sample means from the Denbigh Flint Complex (Anderson 1970:14, Figure 5) is 7.17 mm with a standard deviation of 0.65 mm (coefficient of variation is 9.06). This average mean is slightly smaller and has a smaller standard deviation (as a measure of dispersal about the mean) than that for the Arctic Small Tool horizon (from Table 11 samples greater than 4) which is 7.81 mm with a standard deviation of 0.93 mm (coefficient of variation is 11.90). There is somewhat greater variability in the horizon, which is suggestive that the model is supported by these data.

To accept such a difference as proof of the model without testing the statistical significance of these differences would be contrary to the stated objectives of this thesis, even though such inferences have been made in the past. If we assume that these two samples were drawn randomly from a universe of all Arctic Small Tool assemblages, we can assess the probability that they could have been drawn from the same population (see Chapter 2; Wallis and Roberts 1956:418-419).

Using the above standard deviations, their standard error is 0.316, which divided into the difference in the means (0.64 mm) yields a value for Z of 2.02 which we might expect to have occurred by chance from the same population with a probability of ca. 0.04 (Wallis and Roberts 1956: 365). A more precise test of significance used for small samples (Yeomans 1968b:104) is a t test of these means, which has a value of t=1.81 (df=23) and a probability of ca. 0.08 by chance (Hodgman 1958:216-217). In brief, these data are highly suggestive that the horizon has significantly greater variability than does the tradition (in spite of the fact that they do overlap), and that the model is appropriate.

Since these samples overlap, they are not entirely independent. If I removed the overlap in these samples, by removing four samples from the tradition and three from the horizon, the sample means would diverge somewhat more (since the overlap brackets the mean of the tradition but is below the mean of the horizon) becoming 7.95 mm for the horizon and 7.15 mm for the tradition. The standard deviations would then become 1.09 mm for the horizon and 0.81 mm for the tradition (coefficients of variation are 11.32 for the tradition and 13.71 for the horizon), but the value of z drops to 1.667 with a probability ca. 0.10 by chance.

Applying the more precise t test for small samples, $t=1.39$ ($df=15$), thus the differences between these sample means could have occurred by chance with a probability of almost 0.2 by chance (Hodgman 1958:216-217). This probability is still suggestive, even with the overlap removed. The model is supported by these data, at a statistically significant level.

The establishment of significant difference in the variation within the Arctic Small Tool horizon and the Arctic Small Tool Tradition (Denbigh Flint Complex), raises the question of assessing the significance of differences. In point of fact, the different trends seen by Anderson within the Denbigh Flint tradition may be insignificant variations within the same population.

> A comparative summary of the microblade dimensions also reveals a change from wider forms (Proto-Denbigh) to narrower ones (Early Classic Denbigh), and again an increase in width (Late Denbigh). At present this seems to be the only consistent change through time in microblade measurements at the major Denbigh sites (Fig. 5) (Anderson 1970:13).

Such testing of significance should become a normal technique to reduce the degree of spurious inference which comes from imprecise and implicit methods of analysis.

While the above data support the model at a significant level, there may be adaptive reasons why these differences occurred. The buff chert used for the majority of artifacts from the Closure site may be available in smaller dimensions than in other areas (the high Arctic in particular), thus influencing the size of the artifacts made from

Table 11 Measurements of Selected Chert Artifacts, ASTh

	Mean Burin Length	N	Mean Burin Width	Mean Microblade Width	σ	N
Brooks River Gravels	-	-	-	-		-
Iyatayet	2.64	168	1.31	.69		380
Onion Portage, DFC	-	-	-	.72		411
Onion Portage, Proto Denbigh	-	-	-	.77		102
Itivlik	2.7	5	1.7	.90		5
Dismal 2	-	-	-	-		-
OhPo 5	(2.2	2	1.8)	.90		15
OdPq 4	(3.5	1	2.1	.80		2)
St. Mary's Hill	-	-	-	-		-
Igloolik	-	-	-	-		-
Arnapik	2.93	256	1.46	.78		143
Ivugivik	2.3	49	1.26	.76		25
KeDr 1	2.08	16/20	1.28	.91	.37	6
KdDq 11-10	2.12	29/30	1.31	.88	.29	14
KdDq 23	1.98	10	1.15	(.80	.36	4)
KdDq 11-B, 6, 7, 8	1.91	50/72	1.23	.76	.24	95
KkDn2	2.15	98	1.24	.76	.17	189
KdDq 11	1.94	10/14	1.23	.74	.20	20
Cape Sparbo	2.23	13	-	.62		27
Port Refuge Ind. I	-	-	-	.66		56
Saglek, Site K	-	-	-	-		-
Independence I	3.95	7	1.88	.88		8

Measurements in Cm.

Table 12 Metric Parameters for the Sample of Burins From the Closure Site

	Mean	Standard Deviation	Skewness	Kurtosis
Length (L)	1.98	0.42	0.59	3.1
Width (W)	1.27	0.24	0.04	3.8
Thickness (Th)	0.44	0.12	0.67	3.6
Haft Length (HL)	1.07	0.36	0.80	5.0
Spall Length (SpL)	0.85	0.37	0.67	3.3
Mid Spall Thickness (MSTh)	0.30	0.10	1.20	5.7
Number of Spalls (#Sp)	3.19	1.90	0.98	3.3
			0.0	3.0
N=173	Cm.	Cm.	Normal Variables	

Table 13 Metric Parameters for Burin Samples Within the Closure Site

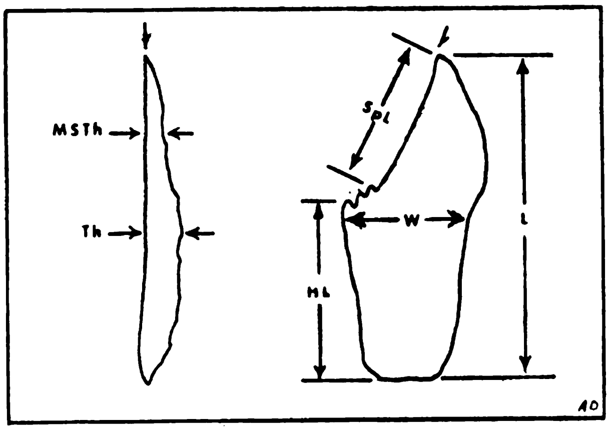
	L			W			Th		
	$\bar{m}$	σ	N	$\bar{m}$	σ	N	$\bar{m}$	σ	N
KdDq 23	1.98	0.33	6	1.15	0.21	10	0.43	0.16	10
KdDq 11-10	2.12	0.51	29	1.31	0.18	30	0.45	0.13	41
KdDq 11-6+50	1.78	0.38	21	1.21	0.23	24	0.46	0.14	25
KdDq 11-6+30	1.95	0.28	14	1.23	0.22	16	0.46	0.11	17
KdDq 11-8	2.07	0.31	10	1.39	0.34	17	0.41	0.06	18
KdDq 11-7	1.98	0.21	3	1.23	0.25	13	0.39	0.12	14
KdDq 11-B	2.15	0.18	2	1.19	0.13	2	0.49	0.07	2
KdDq 11-0	1.94	0.52	10	1.28	0.22	14	0.39	0.10	16
Total	1.98	0.42	95	1.27	0.24	126	0.44	0.12	143

Table 13 (cont'd)

	HL			SpL			MSTh		
	$\bar{m}$	σ	N	$\bar{m}$	σ	N	$\bar{m}$	σ	N
KdDq 23	1.09	0.29	9	0.98	0.48	7	0.33	0.13	8
KdDq 11-10	1.15	0.48	30	0.98	0.45	38	0.32	0.10	39
KdDq 11-6+50	0.98	0.25	24	0.71	0.32	28	0.27	0.11	37
KdDq 11-6+30	0.92	0.20	15	0.73	0.25	17	0.31	0.09	18
KdDq 11-8	1.08	0.24	16	0.89	0.17	14	0.28	0.07	14
KdDq 11-7	1.14	0.47	11	0.92	0.27	6	0.27	0.07	8
KdDq 11-B	0.92	0.09	2	1.42	0.30	2	0.43	0.06	2
KdDq 11-0	1.12	0.39	17	0.69	0.32	14	0.26	0.07	15
Total	1.07	0.36	124	0.85	0.37	126	0.30	0.10	141

	#Sp		
	$\bar{m}$	σ	N
KdDq 23	2.89	1.7	9
KdDq 11-10	3.14	1.9	36
KdDq 11-6+50	3.04	1.3	26
KdDq 11-6+30	3.13	1.7	15
KdDq 11-8	3.12	1.8	16
KdDq 11-7	3.00	2.2	13
KdDq 11-B	4.20	1.5	2
KdDq 11-0	4.17	2.7	17
Total	3.19	1.9	134

All measurements in Cm.



Length (L) -- maximum length as measured along the major axis of the burin, which is usually coincident with the mid-line of the hafted portion and with the direction of the blow which produced the original flake surface.

Width (W) -- maximum width across any portion of the burin as measured at right angles to the major axis of the burin (see above). Usually this occurs at the base of the burin spall scars or at the haft/point juncture.

Thickness (Th) -- maximum thickness of the burin, usually somewhere near the middle of the hafted portion.

Haft Length (HL) -- the maximum length of the haft as measured along the major axis of the burin from the base to the nearest burin spall hinge fracture.

Spall Length (SpL) -- maximum length of the last burin spall removed from the burin, as measured from the burin tip to the hinge fracture of the burin spall.

Mid Spall Thickness (MSTh) -- maximum thickness of the burin tip section measured at the midpoint of the last burin spall removed (i.e., as the midpoint of Spall Length). The measurement is taken with the calipers perpendicular to the major axis of the burin (see Length above).

Number of Spalls (#Sp) -- actual count of the number of distinct hinge fractures remaining on the burin (including the last one removed).

Figure 39 Description of Measurements taken on Burins it. This does not, however, explain the increased size of Independence burins (see Dekin 1974). There may also be a difference in hafting practices, constrained possibly by the difference in availability of walrus ivory as a raw materials (Dekin 1974) which is prevalent along the Hudson Straits and Foxe Basin, but apparently absent from north of Kane Basin in the high Arctic.

It should be apparent that some of the variability in these data could be explained by the operations of the model described above, but that there are probably other influences as well, and we should look further.

Table 14 depicts the variety in end blades across the Arctic Small Tool horizon, indicating the presence of end blade varieties in sites or assemblages. While the "double tapered" end blade is found across the horizon, no other blade form is so widely distributed and there are several which appear to be confined to the eastern extension. Thus, with the dispersal of the horizon, new forms appear and there is increased variety within the horizon, which is in accordance with the model's prediction. While some of the absences may be due to inadequate sampling (see Chapter 5), the overall pattern seems to be as predicted.

The increased variety within the horizon is accompanied by increased similarity among sites and regions within the horizon, as similar artifacts are found across long distances (see Table 14, for example). Such a pattern is in accordance with Maxwell's inference of functional

Table 14 End Blade Variety in the ASTh

	1	2	3	4	5	6
Brooks River Gravels	x		x			
Iyatayet	x					x
Onion Portage, DFC	x					
Onion Portage, Proto-Denbigh	x					
Ityvlik	x-or-x					
Dismal 2	x		x			
OhPo 5	x-or-x					
OdPq 4		x				
St. Mary's Hill	x					
Igloolik	x	x		x	x	x
Arnapik	x	x		x		x
Ivugivik	x	x	x		x	x
KeDr 1	x-or-x					
KdDq 11-10	x				x	
KdDq 23					x	x
KdDq 11-B, 6, 7, 8	x	x		x		x
KkDn 2	x-or-x			x		x
KdDq 11					x	x
Cape Sparbo	?					x
Port Refuge Ind. I	x	x	x	x	x	x
Saglek, Site K	x	x				x
Independence I	x	x	x			

specificity and Binford's prediction of the dispersion of curated artifacts (see Chapter 5), so we may also infer that end blades are both functionally specific and curated. The same pattern exists for end scrapers (Table 15).

However, there are some environmental variables which may also contribute to this variance. The triangular end blade found at Iyatayet and in the East is thought to be a tip for a toggling harpoon, and we might not expect to find it (or fragments) represented at interior sites (or at least in no great numbers). Each of the sites at which it is absent is either a small sample or was thought by the archaeologist to represent subsistence activities relating to fishing or land hunting. It's absence from certain assemblages may be unrelated to the prediction of the model. The converging stem end blade may have been used predominantly for land hunting, and less likely to be found in locations where fishing or sea mammal hunting was important, unless there are unusually large samples. The presence of small side-notched or straight (parallel-sided) stemmed end blades may represent knife forms added to the eastern extension of the horizon. Their addition conforms to the prediction of the model.

In general, the eastern end blades demonstrate an increased delineation of the haft-point intersection, with an increased variety in the treatment of the hafted portion. This haft portion varies from convex-converging to straight-converging to concave with straight or convex

Table 15 End Scraper Variety in the ASTh

					Sample Size (Artifacts)	Citation
Brooks River Gravels		x		x	839	Dumond 1971
Iyatayet		x		x	1078	Giddings 1964
Onion Portage, DFC		x			--	Anderson 1970
Onion Portage, Proto-Denbigh	x			x	--	Anderson 1970
Itivlik				x	--	Alexander 1969
Dismal 2	x	x		x	111	Harp 1958
				x	21	McGhee 1971

base (i.e., side-notched) to parallel-edged (i.e., stemmed) (see Table 14). There seems to be a constraint operating on haft width, such that an increased point width (or size to increase the shock of impact?) still had to fit within some constraints on hafting, but this possibility requires additional study on assemblages with large numbers of end blades.

There are thus several possible sources of variation in end blades which may need to be modeled in addition to the general increased variety predicted by the model. Once again we find that the model will not necessarily predict all of the variation which we observe and that additional models will be necessary. However, the data presently available on end blade variety in the Arctic Small Tool horizon fit the model as presented.

End scrapers are also apparently subject to variation in hafting morphology, but apparently to a lesser degree. The end scrapers depicted in Table 15 vary somewhat in size, but the major variation is in the treatment of the proximal or hafted end. Again, this varies from a concave-converging stem to a straight-converging stem to parallel-edged (stemmed) to concave-edged with straight or convex base (i.e., side-notched). While the concave-converging stem (often called flared end or even eared) is found throughout the horizon, as is the straight-converging stem, the parallel-edged (at least in its small form) is apparently found in the West, while side notched varieties appear in the East. Again, we must be careful because some assemblages have extremely few end scrapers and the vagaries of sampling error are difficult to control.

In general, the increase in side-notched artifacts in the East may reflect the operations of the proposed model, or may have an as yet unrevealed adaptive significance. There is certainly an increased variation in end scraper forms in the eastern extension of the horizon, and these data support the model as presented.

It should be apparent that, while we cannot fully accept the model as proposed, we have found no data which would cause us to reject it or to make substantial alterations. Rather, it would seem appropriate that we build other models upon it in an attempt to increase our ability to predict artifact variability in the spread of the Arctic Small Tool horizon. To accomplish this, it will be necessary for other researchers to produce their data in a manner which would facilitate this kind of analysis, with its concern for the comparability of sample variation and for the assessment of statistical significance of inferences. Obviously, such analyses will require the explicit use of rigorous and precise sampling and analytical procedures far beyond the present capability of the data available in print.

The data considered above support the prediction of the model that the dispersal of human populations into an unoccupied niche will cause behavioral variation in proportion to space and time. Specifically, by holding the time dimension relatively constant by limiting the comparison to the Arctic Small Tool horizon, the model adequately predicted that some of the variation in artifacts was a result of the dispersal vector. It is important to note that this model is not seen as explaining all of the variance in the ASTh nor is it seen as operating to the exclusion of other processes. Rather, the model demonstrates one source of variance and must be used with other models in an attempt to explain variations in human behavior. It requires further testing.

However, the formulation and testing of such models must be based on a research strategy based on explicit and precise methods. Such clarity should reduce the large amount of unspecified speculation and insignificant interence and pave the way for somewhat slower but hopefully surer steps towards a behavioral model of Eastern Arctic prehistory.

Chapter 7

An Ecological Systems Model of Culture Growth, Atrophy, and Stability in the Pre-Dorset

Previous Models of Pre-Dorset Cultural Change

Only in the last several years have students of Arctic archaeology attempted to develop explicit models, while the history of such studies is replete with the use of largely implicit models. Inherent in the concept of Pre-Dorset was the idea that such a culture developed into Dorset (Collins 1954b:304), although the exact nature of the development was unspecified.

Model No. 1 -- General Development of Pre-Dorset culture into Dorset culture.

Model No. 2 -- General Development of Pre-Dorset culture into Dorset culture in some areas, with subsequent migrations to other areas and replacement of cultures there (possibly following local extinctions, but this was not specified) (see Larsen and Meldgaard 1958; Mathiassen 1958).

Model No. 3 -- General Development of Pre-Dorset culture into Dorset culture in some areas, with subsequent cultural diffusion to other areas influencing cultures already existing there (Taylor 1968a).

Two variants of Models 2 and 3 existed, depending upon the nature of the development used in the model. Meldgaard saw significant acculturative influences from other cultural systems involved in this development, particularly from southern "boreal" cultures (Meldgaard 1962). Taylor (1968a), Maxwell (1962) and others saw these developments as evolutionary, with minimal influences from outside a Pre-Dorset cultural system. While many of these ideas were mainly concerned with the transition from Pre-Dorset to Dorset, such a transition was seen as the culmination of a series of changes within Pre-Dorset which may have been accelerated by environmental or acculturative influences (Taylor 1968a:103).

While Taylor pointed out continuities in regional sequences of development in Greenland (1968a:93) and at Igloolik (1968a:99), Maxwell demonstrated the evidence for continuous cultural development in the Lake Harbour region through the Pre-Dorset and Dorset periods (1967, 1973a). Maxwell suggested an alternative model which went beyond the demonstration of cultural continuity through time.

<u>Model No. 4</u> -- Pre-Dorset -- Dorset as a closed cultural system, with internal variations in technological style which are seen as minor stylistic drift (Maxwell 1967: 10). Maxwell emphasized the essential conservatism of the Pre-Dorset -- Dorset cultural continuum, and characterized the Eastern Arctic region as participating in a sphere of cultural interaction with varying levels of intensity

through time.

The development of these four models led to a number of attempts to reconcile the data available from across the Eastern Arctic and to a heightened interest in archaeological systematics and models. Following Maxwell's lead (Maxwell 1960) and the accumulating evidence that the Eastern Arctic environment had been subjected to a number of changes of potential significance to prehistoric hunters, I attempted to assess the potential relations between changing environments and changing cultures, hypothesizing that climatic changes caused cultural changes by influencing the rate of cultural interaction among Arctic peoples (Dekin 1968, 1969, 1970, 1972b). These studies focused on the coincidence of climatic and cultural changes in a number of different areas of the Eastern Arctic, and can be seen as initial steps in the development of a model of cultural change as a response to changes in environment. This model was made more explicit with specific reference to the prehistory of Greenland (Dekin 1972a) in which some of these hypothesized causal relationships were tested. Similar studies by McGhee (1970c), Fitzhugh (1972), and Bockstoce (1973) suggested the general utility of such an approach to the study of cultural change which might be summarized as follows.

<u>Model No. 5</u> -- Changes in the development of Pre-Dorset -- Dorset culture result from the adaptive response of a cultural system to changes in its environment. This

model makes explicit the cause-effect relationship between environmental change and cultural change, such as discussed with regard to the prehistory of Greenland (Dekin 1972a). Fitzhugh's population compression model proposed in 1973 (1973; personal communication) was related explicitly to the Hudson Bay region and to environmental changes and may be seen as an example of an attempt to apply such a model to a specific region.

However, the utility of general models for the understanding of culture change throughout the Eastern Arctic was questioned by McGhee (1973; personal communication) who proposed a model based on historical particularism, seeking no link among the apparently discrete historic events in human populations about the periphery of the Eastern Arctic. McGhee considered the extinctions of such marginal populations as a "normal" occurrence throughout much of the human occupation of this region and presented a general revision of Model No. 2 (above) to include his data on the juxtaposition of Pre-Dorset and Independence I cultures on Devon Island and Noble's Canadian Tundra tradition (Noble 1971).

Thus, the last several years have seen an increased interest in the formulation of explanatory models in Eastern Arctic prehistory, and several attempts at the derivation and testing of such models. It is in this climate and with this background that the following models are proposed.

An Ecological Systems Model of Culture Growth, Atrophy, and Stability in the Pre-Dorset

The utility of an analytical approach involving a consideration of the ecological relationships between foraging societies and their environment has been amply demonstrated in the literature of anthropology. Recent attempts by McGhee (1970c), Bockstoce (1973) and Fitzhugh (1972, 1973) have utilized an ecological framework to relate changes in environment to changes in culture, with Dekin's more formal attempt to depict the nature of these changes in linked systems using data from the prehistory of Greenland being the more explicit and complete statement of the methodological aspects of such studies (1972a). The model developed here is a direct outgrowth of this previous research, based on similar definitions and postulations using theoretical deductions from ecological and systems theory (see Rosen 1972: 47-48) (for comments on the applicability of such methods, see Hill 1972:97).

The model of environmental (ecological) change is a product of the researches and analyses of a number of scientists, including Odum (1959), Levins (1966), Dunbar (1968), Margalef (1968), Dillon (1970), Geist (1971), Slobodkin and Sanders (1969), Irving (1972), Barry and Chorley (1971), Bryson and Wendland (1967b), Barry and Perry (1973), Dansgaard et al. (1969, 1970, 1971), Johnsen et al. (1970), Lamb (1966), Langway (1970), Vibe (1967, 1970), Bryson (1974) and Dekin (1972a, 1972b), much of which has been summarized earlier in this paper.

The model of human behavioral change is a product of a great deal of anthropological research during the last century, with intellectual roots going back to Durkheim (1933), who proposed that increasing population was the general cause of increased division of labor (1933:262; see also Allen 1971:Ch. 6). The role of increased population and increased social scale (Siegel 1952:138) in causing other changes has been proposed by several anthropologists, including Siegel (1952), Redfield (1950:67), Wilson and Wilson (1945:83-88) and Swadesh (1951:3-4) (see also Kushner et al. 1962:Category Five, pp. 20-22; Dekin 1968, 1969).

However, it is only recently that archaeologists have turned their attention to the consideration of these propositions and to their testing. An explicit model of cultural growth has been used by Plog (1974) in the study of prehistoric change in southwestern prehistory, with specific reference to the Basketmaker--Pueblo transition. The incorporation of certain aspects of this model of grwoth may be seen as my attempt to expand the utility of his general model, by relating it to foragers in a changing environment. The addition of a model of system stability is derived from Maxwell's earlier suggestions (1967) and Plog's point that ". . .in order to understand change, we must be as concerned with nongrowth as with growth" (1974: 70).

The conceptual scheme which I will use is similar to that which I used in my previous paper on Greenlandic prehistory (Dekin 1972a), following trends in ecological and systems theory as exemplified by Ashby (1963), Buckley (1968), Harvey (1968), Clarke (1968, 1972b), Renfrew (1973) and Watson et al. (1971).

These, then, are the intellectual foundations on which the following model is based. A more complete statement may be found in Dekin, 1972a, and in Plog's Chapter 6 (1974), but the above outline should be sufficient to indicate the underpinnings of the following statement of a model for testing.

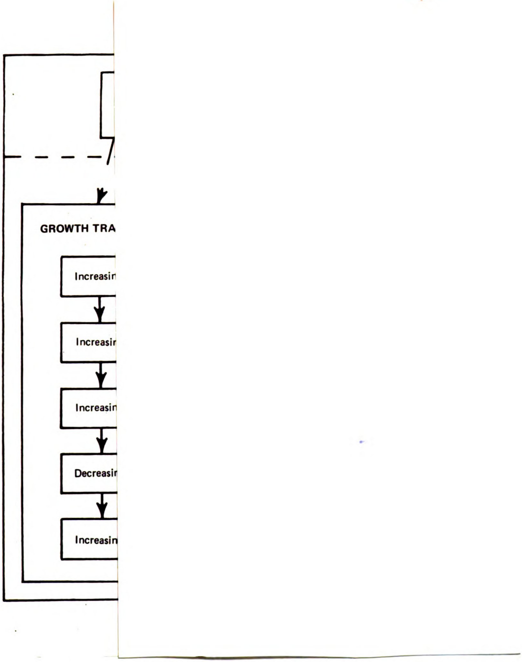


Figure 40 An Ecological Systems Model of Culture Growth, Atrophy, and Stability in the Pre-Dorset

In general, the Pre-Dorset Behavioral System is perceived as an open system subjected to numerous inputs from other systems, in particular that of the biophysical environment. Specifically, it is a complex adaptive system (Buckley 1968) coupled with an environmental system (Ashby 1963:Ch. 4/6) with linkages such that, in such a foraging behavioral system, the human behavioral system is more likely to be adapting to changes in environment than it is to be causing the environmental system to adapt to changes in human behavior (see Dekin 1972a for a more extensive discussion of such linkages) (note that such may not be the case in 1974 or with other more highly evolved behavioral systems) (Clarke 1968:50,51,57). It is important for this perspective that we not confuse this approach with those making use of other types of systems, particularly homeostatic or deviation amplifying, neither of which is implied in this particular discussion. Explicitly, the Pre-Dorset behavioral system is not herein modeled as self regulating, but as complex and adaptive (Flannery 1973:52).

By modeling the Pre-Dorset behavioral system, we are not partitioning the behaviors of "Pre-Dorset people" into those which are genetically programmed, idiosyncratic accidents, purposeful inventions, or learned as systemic norms. Implicit may be the attitude that much of the variation which we intend to study is the result of purposeful learned behavior. Much of this is the study of Pre-Dorset culture, in the many senses in which this concept has been used, but any attempt at building a priori normative systems will be avoided in the development of this model. While it is my explicit intent to model the relations between the Pre-Dorset behavioral system and its biophysical environment, neither of these systems can be considered closed systems. However, the inputs to these systems from other similar (or dissimilar) systems may be controlled in the course of our analysis, or may be accepted, although unspecified or substantiated, and postulated.

Specifically, the ultimate cause of climatic change in the Eastern Arctic is seen as external to the specific systems under consideration, being radiational or orbital changes (inputs) from our solar system and universe. While there is acceptance of this position (Willett 1961:93; Schurrman 1965; Bray 1965, 1966, 1968; Bray and Struick 1963; Dansgaard et al. 1969:378; and Dekin 1970) we will postulate that the ultimate cause of many changes in environment is external to the systems under consideration here.

Other environmental systems may have significant inputs to the Eastern Arctic during the time periods under consideration here, however these would be difficult to determine, given the data at hand. Migrations of biota into the Eastern Arctic are but one possible input of potential significance. It is also possible that other sub-systems of the earth's biosphere would have influenced the Eastern Arctic environment (such as volcanic eruptions increasing the particulate matter in the earth's atmosphere, causing weather changes, etc. Kennewell and Ellyett 1964:356), and we must be careful not to preclude such inputs into these systems.

It is also likely that other human behavioral systems could have provided inputs into the Pre-Dorset behavioral system, particularly along the southern fringes of its distribution. For our present purposes, acculturative innovations are not unlike other innovations. They may be regarded as derivations from other systems and their subsequent spread within the Pre-Dorset may be studied just as if they had been innovations within a geographic sub-set of the Pre-Dorset behaviors.

The concept of Vector of Environmental Change places emphasis on the direction of the change, or of the processes of change, through time. These are statements of tendency (Plog 1973b:656) designed for a diachronic study, rather than a study of the transition through several clearly determinable states. While it may be useful to categorize and model some changes as transitions between periods of relatively steady-state, such studies frequently focus attention on the states, rather than on the processes of change (Sopher 1973:102; Kuhn 1970:125). A recent example of this was the model of environmental change proposed by Vibe (1967) in which he defined several stages of recent climatic change in Greenland (pulsation and stagnation). While my own studies of such changes made use of a rather continually oscillating model (Dekin 1968, 1969, 1970, 1972a) which seemed to be a better fit with the data used to derive them, Vibe's model of the alternation between two stages did not prove to be as useful as he had first believed, and he altered it to present a better fit with the continuous changes which his data demonstrated (1970).

The environment of the Eastern Arctic is a dynamic system noted for changes not observable or significant in other areas. Arctic weather and climate in this region are among the least stable elements of this environment, and are believed to pass on this instability to other components of the system (see Dekin 1972a for an extensive discussion of this instability; Ashby 1963:83; Foote and Greer-Wootten 1966; Lamb 1966; Dunbar 1968; Vibe 1967, 1970; Nelson 1969:34-53). Thus, the use of the vector concept focuses our study on the processes of change themselves, rather than on the presumably steady states which border them. It should be obvious that the focus of a study on change may make some traditional concepts of less utility than others designed specifically for such studies, and I expect that the initial awkwardness of such concepts will hopefully be overcome by an appreciation of their utility.

The essential points to this model of Vectors of Environmental Change are as follows. 1. While the dynamics of the earth's atmosphere may be seen as a large mechanism for heat exchange between the poles and the equatorial region, driven by the differential solar energy

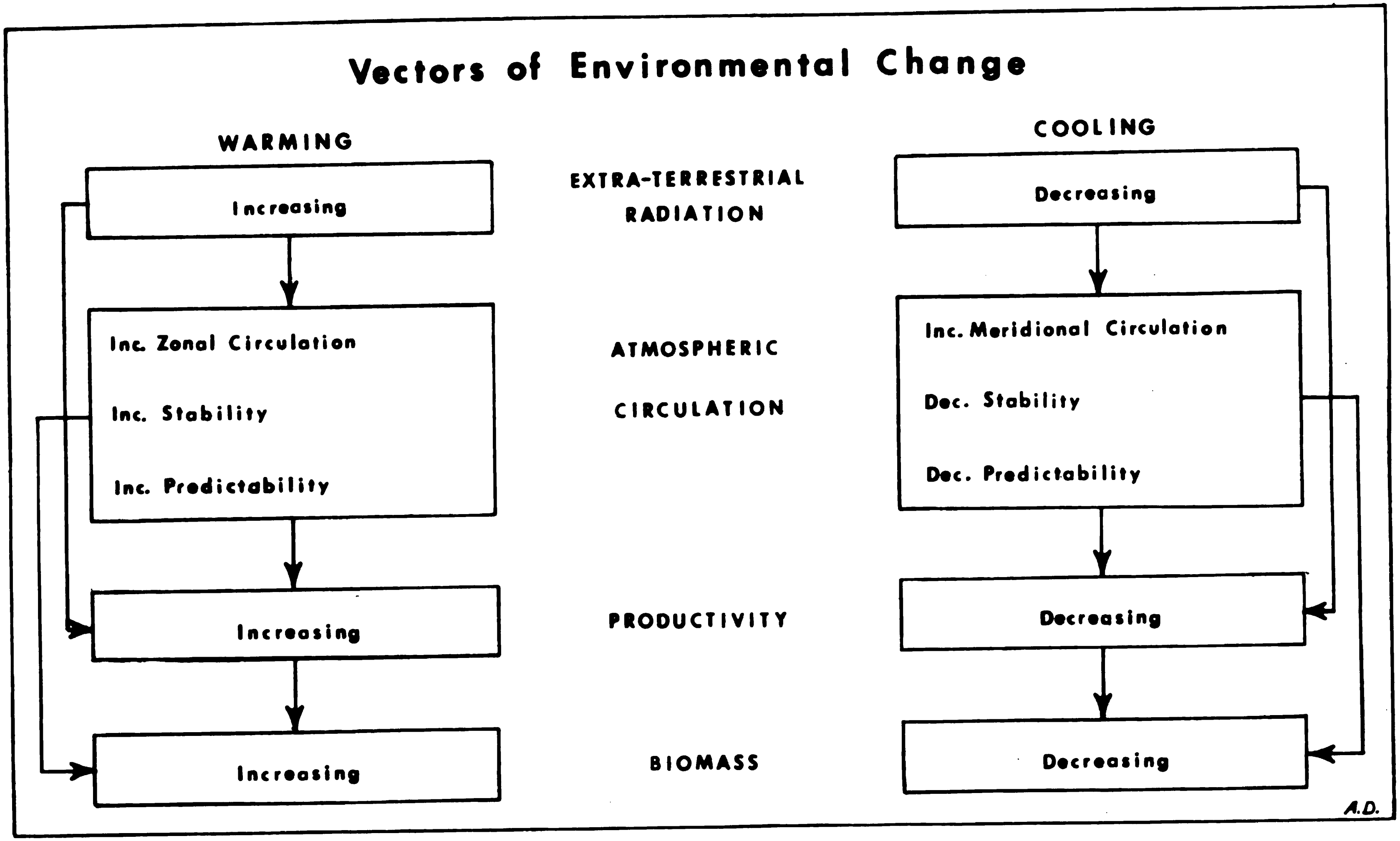


Figure 41 A Model of Vectors of Environmental Change

available in these regions, the driving forces behind this exchange mechanism are extraterrestrial in origin, and changes in the dynamics of such exchange systems may be seen as being ultimately caused by variations in the kind and amount of radiation reaching the earth, particularly in the high latitudes (Dansgaard et al. 1969; see above).

2. Such variations in radiation cause immediate and lasting changes in the earth's atmospheric circulation (King-Hele and Walker 1960; King-Hele 1964; Tucker 1964; Wexler 1956; Wilcox et al. 1973; and Willett 1951), leading to variations in air mass movements. In northern regions, these have been characterized as periods of zonal circulation or meridional circulation, depending on whether the dominant pattern was of east-west air mass movement (zonal) or north-south air mass movement (meridional). Zonal circulation implies more stable and predictable weather conditions with a decreased exchange of air between Arctic and temperate regions, while meridional circulation implies less stability and predictability with fluctuating air mass interfaces and subsequent weather conditions (Bryson and Wendland 1967b; Barry and Perry 1973:364; Barry and Chorley 1971; Barry 1967). Increased extra-terrestrial radiation causes increased zonal circulation, while a decrease in solar radiation causes increased meridional circulation, particularly with regard to northeastern Canada and the Eastern Arctic.

3. A change in solar radiation (or other forms of

radiation) results in changes in the energy available at all trophic levels and to changes in the general productivity of the ecosystem (Odum 1959:Ch. 3; Dunar 1968:1,98). Increases in solar radiation result in an increase in energy available and increased productivity, while opposite changes are similarly linked. Changes in the stability and predictability of weather and climate also influence changes in productivity as a result of the changing frequency of destruction of "marginal" communities (see MacArthur 1972; Dunbar 1968; Margalef 1968:63).

4. Changes in predictability, stability and productivity cause changes in the biomass, with obvious implications for the human exploiters in the food change. Increased predictability, stability and productivity cause increases in the biomass, and opposite changes are similarly linked (Dunbar 1968:98; MacArthur 1972:183; Odum 1959: Ch.3).

Inasmuch as the Arctic ecosystem is "immature" (Dunbar 1968; and others) with a relatively "simple" food web, fluctuations in the general stability, predictability, and productivity greatly influence the internal dynamics of the ecosystem, providing a useful example of Ashby's Power of the Veto (Ashby 1963:83). We should expect the human inhabitants of this region, especially at a foraging level of subsistence capability, to adapt to such changes. The thrust of environmental vectors such as I have described should also influence human behavioral systems, and the following is a model of such changes.

The use of explicit models of human behavioral systems has been increasing in the last decade and we may choose from any number, varying in scope and explicitness. By modeling the Pre-Dorset Behavioral System as a complex adaptive system (and an open system -- Chorley 1962:3) I am postulating that the relations among aspects of human behavior which we may wish to study are complex and that such behavioral interrelations are multivariate (Spaulding 1973: 344-5,352; Chance 1968:5). Moreover, I also postulate that the opportunity for continuing feedback among behaviors makes the search for unilineal causal relationships difficult, if not impossible, particularly at the level at which we are forced to operate because of the limitations inherent in data which are archaeological (see above). This postulate is supported by Plog's failure to substantiate a model in which simple linear relations were hypothesized, having to revise the model to include more complex relationships (Plog 1974:155-7).

By modeling the system as an open complex adaptive system and by paying particular attention to the environmental inputs into this open system, we are avoiding several limitations which have plagued previous investigators. This model eschews simplicity (Spaulding 1973:344-5,352) and seeks to model complexity. This model does not force us to consider human behaviors as a closed system, nor does it assume that such behaviors are homeostatic

(Flannery 1973:52), nor does it assume that environmental changes are "accidents" (Plog 1974:33). Rather, it is a model which attempts to make explicit the relations which occur within two linked systems, both of which may be changing. It is an adaptive system, such that a change in one (environment) may result in input to the other, with resulting response. It is apparent that there is continuous variation between daily weather and its long-range counterpart, climate. While the day-to-day changes result in day-to-day responses, it is not this level which we wish to model. By choosing a scale which encompasses those various archaeological phenomena from the Eastern Arctic called the Arctic Small Tool tradition which span the time period between 4050 and 2750 B.P. (radiocarbon years), we wish to examine changes in the entire system during this time span. This point is significant with regard to the input thresholds from the environmental system to the behavioral system. It would be difficult to imagine a change in input which occurred in the time span of one day and it would be difficult for us to determine relevant data, given the limitations with which we operate (see above). On the other hand, a change in input which took the entire time span of 1300 years to occur would also be difficult to determine and analyze.

There are probably significant differences in the amounts of changes in particular variables which are sufficient to cause different responses, and there may be time delays as well, both of which are, unfortunately, not well enough studied to be modeled at the present time (see Bryson and Wendland 1967b for a tentative model of threshold and lag among environmental variables; also Barry and Perry 1973:350).

Minor changes in environment may have an immediate effect on movements of people (halting movements in a storm, for example) and minor changes in the frequency of good traveling weather may affect the ease with which people come into contact with other social groups (Dekin 1969). Thus, I have modeled trajectories of change which have low input thresholds and which relate such minor changes directly to changes in social scale.

Major changes may also cause large-scale fluctuations in population density and distribution, which will ultimately also affect the frequency of social contacts. These are modeled as growth or atrophy trajectories. In this model, there are two levels at which environmental change may affect social contacts. These are modeled as complex, multi-varite, and subject to different thresholds and response rates.

This proposed model of the Pre-Dorset behavioral system and its changes considers changes in five dimensions of human behavior: population; behavioral differentiation; social scale; technological variegation; and technological evolution. Just as I modeled two vectors of environmental change (above), there are two significant trajectories of linked changes which we can model and two somewhat less complicated trajectories resulting from changes in social scale (Clarke 1968:75-77; Flannery 1973:47).

The results of the warming vector of environmental change are initial increased stability and predictability of weather patterns, with zonal circulation and fewer blocking highs and far northward storm tracks. These changes alone would result in improved hunting conditions and in greater ease of travel, thus influencing social scale directly (Morrill 1970). However, prolonged and increased warming vector change results in significant ecological systemic changes such as increased biomass and an increased ability to support greater numbers at higher trophic levels. Without marked human behavioral change, there is an increase in efficiency of the food quest which may result in fewer hard times and an ultimate population increase (Freeman 1971:1013; Balikci 1970:151). With this increased efficiency and population increase (increased population density or extent) there will also be an increase in behavioral differentiation, marked by increased specialization of behaviors (rise of specialists and increased divisions of labor) and specialized locations (increased spatial variegation of behaviors -- single purpose houses, sites, regions, etc.) (Struever 1968:138; Plog 1974:62-5, etc.; Durkehim 1933; White 1959:293; Allen 1971:Ch. 6).

The increased population density and differentiation lead to increased social scale (Pothier 1968:38), whether p

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purposeful from increased trade or other exchange mechanisms or simply from the increased population dynamics (Morrill 1970:147-152; Sopher 1973:112). However, as mentioned above, increased ease of transportation may also lead to increased social scale (Morrill 1970:150 reaches the same conclusion with more modern data).

If changes in the environmental vector are the opposite to those described for the warming vector, I have modeled a cooling vector which results in decreased stability and predictability of weather patterns, with meridional circulation and more frequent blocking highs and a northward movement of depression tracks. These changes alone would result in less optimal weather conditions for hunting or travel (Boas 1964:19; Foote and Greer-Wootten 1966), reducing social scale. Prolonged cooling vector change results in significant changes in the ecological system, with decreased biomass and a decreased ability of the food chain to support higher trophic levels. Without marked human behavioral changes, there is a decreased efficiency of the food quest which may result in more frequent hard times and an ultimate population decrease (possibly both gradual and catastrophic) (Freeman 1971:1013; Balikci 1970:151). These changes result in a corresponding decreased specialization of behaviors within society and decreased specialized locations (again, Plog 1974; Durkheim 1933; White 1959; and Allen 1971).

These changes result in decreased social scale as a result of decreased exchange among specialized locations or simply the result of decreased population density (Morrill 1970:147-52).

Thus, both the warming and cooling vectors are seen as causing changes in social scale, both directly and through the intervening changes in population and behavioral differentiation. Social scale ". . .refers to the number of persons consciously related in a society and the intensity of their relations (Wilson and Wilson 1945)" (Kushner et al. 1962:20). There need not be face-to-face contact and communications frequency may be one measure of intensity.

Increases in social scale result in increased communication among social groups of various sizes and at various levels of analysis. These communications tend to maintain the integrity of the social unit and to spread ideas and behaviors throughout the social system engaged in this interaction (Yellen and Harpending 1972:248).

> As long as conditions of close internal communication prevail in the society, the culture tends to remain uniform throughout; changes appearing in one part of the area either spread quickly through the whole territory or are dropped because of the cultural influence of the bulk of the society (Swadesh 1951:3-4).

Reductions in social scale result in decreased communication and in a decline in behavioral homogeneity in the social system (Sopher 1973:106; Yellen and Harpending use the concept of social nucleation much as I used social scale, 1972:249,251).

> Where the size of the territory or other circumstances prevent the fullest internal contact of the group, there is a tendency to develop local variations of the culture which may eventually amount to major differences (Swadesh 1951:4).

Changes in social scale lead to changes in technological variegation, with increased social scale causing increased exchange of ideas and technological similarity. Such similarity in technological systems results from the behavioral equivalent of the Law of Competetive Exclusion (Plog 1973a: 657; Odum 1959:231; May 1973:139; and MacArthur 1972:179) in which increased communications leads to increased competition among behaviors and the expansion of the most efficient (read adaptive or appropriate) behaviors (McGhee 1973 discusses this at an intuitive level). In general, such changes result in increased behavioral complexity (technical complexity) and diversity which we call technological evolution (Wilson and Wilson 1945:88).

Decreased social scale, then, leads to decreased communications among spatial units, decreased competition among behaviors, maintenance of diverse behaviors among spatial units (heterogeneity) (increased technological variegation) and a decreased rate of systemic technological change.

Several aspects of this model must be made explicit. It does not attempt to account for all causes of increased social scale nor for all results of increased social scale. As Plog has pointed out, we may need many models of behavioral change in order to understand such complexities

(Plog 1974:156). There is not explicit modeling of feedback or looped changes in this model, although some could be suggested (increased social scale leads to increased occupational specialization -- Kushner et al. 1962:21; Wilson and Wilson 1945:83). The linear sequence of linked changes in the behavioral system could be seen as but one of a large number of similar sequences possible in complex adaptive systems. The universality of this particular sequence remains to be assessed.

Test Conditions

The following is a stipulation of the data structure we would expect if the above model is appropriate. With reference to Chapter 4 and particularly Table 8, the inputs into this model would be as follows.

The migration of Arctic Small Tool tradition populations into the Eastern Arctic occurred during the waning phases of a warming trajectory during a time when there is no evidence for marked environmental change. The first major change in the Eastern Arctic environment occurred after most of the Eastern Arctic littoral had been occupied (with the exception of southern Greenland). This change was the advent of a less stable cooling trajectory in both the land and marine ecosystems at approximately 3500-3600 B.P.. This trajectory involved increases in marine ice, changed less-optimal marine conditions, a southern movement of the tree line, increased meridional circulation and concomitant changes in the distributions of marine and land fauna (see Chapter 4). This change is followed by a more marked change _ca_. 2750-2500 B.P. which again involved a cooling trajectory.

Were it not for the relative recency of the AST migration, we would expect the above model to apply to Pre-Dorset behavioral systems prior to the marked change _ca_. 3500-3600 B.P.. This might have been modeled as the waning portion of a trajectory that resulted from a warming vector (see Figures 23 and 24), but the dispersal trajectory and related changes (see Chapter 7) are a more appropriate model.

If the above model is appropriate, we would expect observable changes in the archaeological record to follow the marked environmental change _ca_. 3500-3600 B.P.. These could be of two general types, depending upon the strength of the cooling vector.

With a relatively minor cooling vector, we would expect the following:

1) increased technological variegation among regions populated by the ASTh (a process of regionalization resulting from a reduction in social scale); and

2) a general decreased rate of technological evolution (lack of evidence for increased technical efficiency or complexity following regionalization).

With a major cooling vector, we would expect an atrophy trajectory of changes, with the following changes in the archaeological data:

1) decreases in the number of sites, areal extent of sites, or a combination of decreased

numbers of sites but larger sites (nucleation);

2) increased homogeneity within sites (fewer areas of activity specialization within structures and sites), which is decreased behavioral differentiation;

3) increased technological variegation among regions populated by the ASTh (a process of regionalization resulting from a reduction in social scale); and

4) a general decreased rate of technological evolution (lack of evidence for increased technical efficiency or complexity following regionalization).

If the vector of environmental change is not sufficiently strong to exceed the thresholds, we would expect that none of the above changes would correlate with the environmental vector and that we would need to search for alternate models of such changes. The key implications of this model are that such changes exist and that they are correlated to the extent that vectors of environmental change are followed by vectors of behavioral change.

Explicitly, we would expect the period following ca. 3500-3600 B.P. to be characterized by the development of regional differences in artifacts and a reduced rate of artifactual change such that there is artifact stability through time within each region. If there is atrophy, we would expect sites within each region to decline in frequency, extent, or to nucleate. Sites within each region would contain artifacts representing a wide range of activities with relatively little variation among sites within each region (when site assemblages are compared, they demonstrate similar variety among artifacts). Summarily, the dimensions of archaeological data which we need to observe are as follows: 1) site frequency and size; 2) variety of artifacts within sites; 3) variety of artifacts among regions; and 4) artifactual change through time within each region.

Testing the Model

Before undertaking an assessment of the degree to which the dimensions of archaeological data correspond to the above predictions of the model, it would be advisable to reiterate some of the cautions expressed above with regard to the sample of data which we have available (see Chapter 5 above). For an accurate comparison of the dimensions of the data we should have an unbiased sample representative of the populations which we wish to study. To insure a lack of statistical bias, we generally take a random sample, in which each element of the population has an equal chance of being selected for the sample (Cochran 1953:11). To my knowledge, such a sampling technique has never been used to derive any data from any population in Eastern Arctic prehistory, thus we are not able to say that we are dealing with unbiased samples. However, we are not able to specify all of those samples which have been biased by field or analytical techniques (see Chapter 2 and Chapter 5 above), and we cannot a priori establish all sources of bias which may have influenced our samples.

This problem is most difficult to cope with when we attempt to assess variations in site frequency and site size through time. Without assurances that we have representative samples of each time period of occupation, we have difficulty in assessing this dimension of the data. Nevertheless, there is no way to convert non-random samples (Tate and Clelland 1957:47) nor to compensate for non-randomness. We must make do with what we have available, being as precise and explicit as possible so that our manipulations of the data do not serve as additional unknown sources of variance.

With the exception of absolute differences (presence/absence), it is presently impossible to determine if there are significant population changes within any of the regions in which ASTt sites existed in the Eastern Arctic just after 3600-3500 B.P., and thus it is impossible for us to evaluate this dimension of the model as presented.

On the other hand, this time period marks significant changes in the distribution of ASTt populations and in the development of regional differences. The migration of Sarqaq peoples into southern Greenland occurs just after this time. The Independence I culture of northern Greenland apparently abandoned its former range at about this time. There is apparently a hiatus in the ASTt occupations of Labrador at this time, and the origins of Noble's newly named Canadian Tundra-Taiga tradition (Noble 1974:162) consisting of derivatives of the ASTh from Victoria Island to Manitoba date from just after this time (Taylor 1967 and my Table 4). Thus, following 3600-3500 B.P. (see Figure 6), we find the development of several distinctive variants from an Arctic Small Tool horizon and several areas from which previous occupants vanished. The regional differences could be categorized into five sub-traditions: Canadian Tundra-Taiga; Pre-Dorset; Sarqaq; Refuge; and Independence II. The timing of these regional variants is as predicted by the model.

If the model is appropriate, within each of these regional variants, we would expect a limited amount of artifactual change through time, as these technological traditions are perpetuated without extensive changes from adjacent regions. Such conditions should persist (according to the model) until such time as the level of social scale is increased bringing more extensive contacts with adjacent areas, unless there are other significant inputs from the environments of these sub-systems which the model does not predict.

Let us then assess the nature of artifactual change within each regional variant to see if the prediction of the model is in accordance with the data.

During the time covered by our consideration of the model (until ca. 2750 B.P.) the northern sites of the Canadian Tundra-Taiga tradition represent a single relatively stable tradition, evidenced by the continuity in sites from Banks and Victoria Islands -- Umingmak, Wellington Bay, Buchanan, and Menez (Taylor 1967,1972).

This continuity is in "typical" chert burins, side blades, scrapers and end blades as well as the characteristic quartzite bifaces (large and thin) and occasional quartzite end blades of traditional form (Taylor 1967,1972; Gordon 1972).

The southern sites in this Canadian Tundra-Taiga tradition are three in number (Rocknest Lake Complex -- ca. 1200-900 B.C.) during the time period in which we are interested, and demonstrate their greatest similarities with those northern sites just described. These early southern sites are all at or just on the tundra side of the tundra-taiga border and all have an orientation to the adjacent waterways (Noble 1971:107-8). Neither Taylor nor Noble describes any changes in artifact form or stylistic variation through time in either of these sequences, although subsequent occupations in both the northern and southern portions of this distribution demonstrate continuity from this Canadian Tundra-Taiga tradition. It is interesting to note that subsequent developments from this general similarity demonstrate divergence, suggesting that Noble's Canadian Tundra-Taiga tradition be modeled as a diverging tradition (Wauchope 1956:43).

These data are in accordance with the model. If the model is to apply in the future in this area, we should not expect to find sites of the Canadian Tundra-Taiga tradition before 3600-3500 B.P..

McGhee's Bloody Falls site is an enigma because it is apparently bracketed in space by sites with artifact similarities, but it lacks the large and thin quartzite bifaces which characterize the Canadian Tundra-Taiga tradition. Its radiocarbon date is bracketed by those from Umingmak and Wellington Bay (see Figure 6), yet it contains no quartzite artifacts (McGhee 1970a) with a sample of ca. 250 artifacts. However the overall nature of its assemblage (with ground slate adzes and end blades, polished burins, worked copper, etc.) suggests that it belongs in the same category as the Canadian Tundra-Taiga tradition, but that it may represent atypical activities conducted at Bloody Falls (season of occupation, possible idiosyncracies, etc.) It is impossible to evaluate the adequacy of the excavated samples from the Canadian Tundra-Taiga tradition, as McGhee's Bloody Falls site is the only one for which artifact totals are published (Umingmak has more than 400 and Buchanan is in the hundreds).

The Sarqaq data from West Greenland also demonstrate continuity in artifact style and form during this period under consideration, but with moderate sample sizes (Larsen and Meldgaard 1958; Taylor 1968a:Table 23). If we accept Taylor's seriation of these data, then the continuity is more striking (again recognizing that seriation itself is biased in favor of continuity--Taylor 1968a:91). However, the data depicted by Larsen and Meldgaard and Mathiassen (and re-depicted by Taylor) when combined with the radiocarbon dates available from West Greenland (see Figure 6) demonstrate technological continuity between _ca_. 3500-3600 B.P. and _ca_. 2750 B.P., which is a data structure in accordance with the model as presented. In order for the model to continue to be in accordance with the data from West Greenland, we would expect to find no evidence for major technological change during this time and no Sarqaq sites earlier in time than the environmental changes noted above.

The recent resurgence of interest in the archaeology of the Queen Elizabeth Islands (McGhee 1973, 1974) has resulted in enigmatic data subject to differential interpretations. It is, however, clear that this region contained a rather distinctive derivative of the Arctic Small Tool horizon, most closely related to the Pre-Dorset from the core area. While the following interpretations may not be in accord with those presented by McGhee, they fit both the data and the above model.

There was an initial occupation of this region by people whose behaviors were much like those of Knuth's Independence I occupations of northern Greenland. This occupation has a radiocarbon date coeval with the earliest from Independence I sites from Greenland, covers a rather limited span of raised beach features (22-24 m above present sea level), and contains a variety of lithic raw materials, all of which suggest a rather limited duration of occupation related to the initial migration of Independence I peoples to the northeast.

This occupation was followed by what appears to have been a relatively stable homogeneous occupation of this region for a longer period of time by what McGhee sees as a Pre-Dorset variant, characterized by a general hafting of end blades, end scrapers, and burins which involved side notching to a degree not seen in the core area (McGhee 1973). Evidence for a longer and stable occupation is meagre, but consistent. Sites range in elevation from 19 to 28 meters above present sea level, are not characterized by significant differences in artifact content or stylistic variation, demonstrate consistent utilization of a blue-gray chert, and apparently last until after 1000 B.C. (I-7344 -- 2900$\pm$90 B.P.) (McGhee 1973, personal communication), all of which are in accordance with the model as presented above. It is possible that this occupation overlaps in time with the Independence I occupation, since there is overlap in the elevations of sites (Andrews et al. 1971 suggest that the 9 meter range of these sites may represent 800 to 1,000 years of postglacial emergence in this area).

While the tentative nature of these conclusions must be emphasized, these data from McGhee's reconnaissance are indicative of a stable, relatively homogeneous occupation of this region by a regional variant of the Arctic Small Tool tradition, which we might call the Refuge tradition, to distinguish it from contemporary variants in adjacent regions, one of which developed into Dorset.

The Independence occupations from northern Greenland are interrupted by a marked population decline and possible abandonment at _ca_. 3600-3500 B.P.. This population displacement probably resulted in increased interaction of Independence I, Pre-Dorset, and possibly Refuge populations (if such a tradition were in existence at this early date) contributing to the development of Sarqaq culture. However, within three or four centuries the Independence I culture had been adapted to the changed ecological conditions in northern Greenland (Independence II), and made a successful reoccupation of the region, which lasted through the remainder of the time period under discussion here. Follow-in this re-occupation, there is no evidence of significant behavioral change until the subsequent abandonment of this region several centuries after 1,000 B.C. (Dekin 1972a; Knuth 1967, 1968). McGhee's Independence II occupations may result from this abandonment, since the apparently post-date the Refuge tradition (McGhee 1973).

The above data are all in accordance with the test conditions of the model presented above. The formation of five regional sub-traditions of the Arctic Small Tool tradition (Canadian Tundra-Taiga, Pre-Dorset, Refuge, Independence II and Sarqaq) follows closely the environmental change _ca_. 3600-3500 B.P. and demonstrates subsequent stability through the remainder of the period predicted by the model.

If there were atrophy within these sub-traditions, we would expect changes within sites to reflect reduced activity specialization, yet there is no evidence for such changes. The two sub-traditions where data are available are Sarqaq (from Taylor 1968a) and the Pre-Dorset core area (Igloolik and Lake Harbour). In both of these, there is marked continuity in the contents of sites, with no evidence of regular change in the degree of variety (heterogeneity--see Whallon 1968:227-228) within each site or among sites. Table 16 depicts the variety in site contents in Sarqaq (from Taylor 1968a:Table 23) and Table 17 depicts the variety in the southern Baffin Island Pre-Dorset. An inspection of these tables demonstrates that statistical analyses to assess variability are not warranted, due to a lack of observable regular variation, except for sample size.

Thus, it appears that there is no evidence in support of an atrophy trajectory resulting from the environmental change _ca_. 3600-3500 B.P.. The regionalization (Yellen and Harpending call this macro-fragmentation--1972:251) presented above suggests a reduction in social scale, as predicted by the model, and we should assess the evidence for gradual evolutionary change within these regions. Once again we are faced with a lack of published evidence of changes in artifacts and attributes with which to test this prediction, and we are forced to turn again to the Pre-Dorset core area for data.

There are two long series of data from the core area, one at Igloolik which is not yet published and one at Lake

Table 16 Variety in Sarqaq Assemblages

	Tuperssuit	Igdlorssuit I	Igdluluassup Talerna	Sarqaq Niaqornarssuk	Igdlutsiait	Ilerfit
Burin	38	26	11	20	15	27
Burin Spalls	9	7	3	5	0	9
Microblades	2	21	16	0	13	8
Microblade Core	0	2	3	0	0	1
End and Side Blades	19	8	17	14	11	16
End Scrapers	6	3	2	3	7	4
Side Scraper	13	6	4	6	8	17
Adzes	0	0	1	1	0	0
Soapstone	1	1	0	1	11	0
Σ of the above	88	74	57	50	65	82
Total	91	74	58	50	67	85
Σ Burins, Burin Spalls, and Microblades	49 (54%)	54 (73%)	30 (52%)	25 (50%)	28 (42%)	44 (52%)

(From Taylor 1968a:Table 23)

Table 17 Variety in Pre-Dorset Assemblages

	KeDr 1	KdDq 11-10	KdDq 23	KdDq 11-B,6,7,8	KdDq 11	KkDn 2	KdDq 10	KdDq 13
Burins	35	41	10	101	31	111	89	6
Burin Spalls	49	122	24	360	20	185	84	6
Microblades	21	33	5	116	21	112	133	18
Microblade Cores	5	1	0	1	2	10	12	6
End and Side Blades	9	9	4	41	9	35	44	4
End Scrapers	9	2	1	4	8	8	23	1
Side Scrapers	7	3	0	3	3	8	21	2
Burin-Likes	1	0	0	4	1	0	5	0
Adzes	0	0	0	2	1	1	1	0
Soapstone	0	0	0	1	0	0	0	0
Ground Slate Knives	1	1	0	1	2	0	16	6
Σ of the above	137	212	44	634	98	470	422	49
Total N	177	284	71	840	104	561	503	53
Σ B,BS,MB	105	196	39	577	72	408	306	30
(%)	(59%)	(69%)	(55%)	(69%)	(69%)	(73%)	(61%)	(57%)
Σ B,BS	84	163	34	461	51	296	173	13
(%)	(47%)	(57%)	(48%)	(55%)	(49%)	(53%)	(34%)	(23%)
Polished Burins	2	1	0	11	9	16	22	3
(as % of Burins)	(6%)	(2%)	(0%)	(11%)	(29%)	(14%)	(25%)	(50%)
Slate Knives	1	1	0	1	2	0	16	6
(as % of N)	(1%)	(<1%)	(0%)	(<1%)	(2%)	(9%)	(3%)	(11%)

Harbour, to which this thesis makes a contribution. In his several preliminary reports, Meldgaard has pointed to the continuous and predictable gradual changes which characterize the Sarqaq (herein called Pre-Dorset) levels at Igloolik (for example, 1962:92). More recent communications (personal communication to Maxwell, 1972) reinforce this continuity, particularly evident in harpoon tips and in lithic artifacts (see also, notes and photographs of Elmer Harp, Jr., on these Igloolik materials). Nowhere in his papers or correspondence does Meldgaard mention significant technological change between ca. 2000 B.C. and 1000 B.C. with a period of more rapid culture change between 1000 B.C. and ca. 900 B.C.. With the possible exception of this later period of more rapid change, the sequence of Igloolik data is in accordance with the prediction of the above model.

The sequence of Pre-Dorset sites from southern Baffin Island reported by Maxwell (1967, 1973a, 1973b) and partially in this thesis also demonstrate a lack of major changes in technology (Table 17, for example), with several trends continuing throughout the sequence. These trends are in artifact attributes or gradually changing frequencies of artifact types, but in no case do they reflect more than the "refinement" of already existing techniques or relatively minor oscillations about a mean. Several of these changes through the Pre-Dorset period appear significantly different only when seen in the light of subsequent changes. These include the declining proportion of burins and burin spalls in assemblages, the increasing frequency of ground slate knives (see Table 17), the apparent increasing lateral thinning of burins (from incipient side notched to side notched) (Maxwell 1973a:308,309) and side-notching on knives and microblades. However, it is apparent that these are slow trends of change, certainly evolutionary as they develop from behaviors already present in the earliest assemblages, and are in accordance with a stable rate of technological change predicted by the model.

The following aspects of the model were _not_ substantiated by this analysis: 1) atrophy trajectory; and by implication; 2) growth trajectory. While it may be possible to test such a model on additional data from Eastern Arctic prehistory, the present data on the Pre-Dorset period do not accord with such a model and it is not appropriate.

However, the portion of the model which demonstrated the impact of a lower threshold for input from trajectories of environmental change was in accordance with the data considered above. In brief, the model predicted that the environmental change _ca_. 3600-3500 B.P. would be followed by the development of regional variants from the initial Arctic Small Tool horizon, and that each regional variant would demonstrate moderate internal change (if any) for the duration of the trajectory modeled above. The data substantiated these predictions, suggesting that such a model may be appropriately applied to other environmental changes and cultural changes in the Eastern Arctic for further testing.

Difficulties in acquiring data suitable for the testing of the above model were encountered because of the failure of the published data to be couched in terms suitable for the testing of such models. When data from different find-spots or even different sites are lumped together for the purposes of analysis and depiction, it precludes the study of variation within that data set. When variations in that data set become significant for the testing of hypotheses, we are forced to infer conclusions regarding variation from the data presented, which is often presented normatively.

Once again, the problem of sample inadequacy and bias was difficult to cope with or avoid. By making explicit possible sources of sampling error and limiting data sets to those with total artifact samples greater than 50 (in which there would be a 90% probability of finding at least one artifact in a random sample with a population proportion of 0.04--from Table 9) I attempted to minimize the possibility of inferring significant variation where none exists (a case of Type I error).

This approach to modeling aspects of Pre-Dorset behavioral systems has resolved some questions which other approaches were unable to explain. The independent deduction of the model from other relevant theoretical formulations allowed the independence of the model from the data--an essential requirement if "testing" is to occur. The model led to the recognition of several sub-traditions of the Arctic Small Tool tradition, all of which had at one time or another been lumped into the category Pre-Dorset. Moreover, the model explains these sub-traditions as resulting from the reduced social scale caused by significane environmental change. In actuality, this model only explains the maintenance of these differences and not their original source (much as the process of natural selection explains the maintenance of certain genetic combinations, but not why the original mutations occurred). For this, we may need to look anew at the ecological diversity in this region (as in Dekin 1974), focusing on the precise ecologic relations between human populations and their environment. In this way, the above model has not only contributed to our understanding of regional variations in the Arctic Small Tool tradition, but has pointed the way for future research regarding the ultimate causes of such diversity (as noted by Yellen and Harpending 1972:251).

By speaking precisely about behavioral processes and by an explicitly scientific approach to model formulation and testing, we have gone beyond the practice of "letting the data speak" and have placed the study of human behavioral processes on a firmer foundation. If there is merit to such an approach, then we will need a revitalization of concern for archaelogical strategy and techniques of analysis, in order to provide the data necessary for these kinds of analyses.

The model presented here, as substantiated by the available data on Pre-Dorset variation, goes beyond the "what" of the processes of human behavior to the "why", and is only possible through the use of precise and explicit archaeological analyses. This has been but one step in the direction which I see as necessary for Arctic archaeology.

The ultimate contribution of this ecological systems model may be to resolve the conceptual muddle regarding the nature of cultural changes in the Eastern Arctic and how to model them. The expansion of this model to subsequent prehistoric dynamics in this region should be a high priority project for future research. The integrity of the subtraditions of the Arctic Small Tool tradition (Canadian Tundra-Taiga tradition; Pre-Dorset tradition; Refuge tradition; Independence II tradition; and Sarqaq tradition) can be easily tested by future field strategies and programs of radiocarbon dating. Taken together, these five traditions may be seen to comprise Taylor's Carlsberg culture (Taylor 1968a:85).

Chapter 8

Discussion: The Conceptual Setting

Introduction -- New Archaeology in the Arctic

There are several sources of variation in archaeological interpretations. These include: 1) variations in the data under consideration (including differences in the techniques of recovery, depiction and categorization); 2) variations in the methods of analysis applied to the data; 3) variations in the paradigm which each investigator brings to the data and analysis; 4) idiosyncratic phenomena characteristic of the archaeologist (personality, intellect, and experience, for example); and 5) the history of past variations in interpretations.

Few archaeologists have studied differences resulting from the personalities of archaeologists or from historic differences in interpretations which gave rise to long-standing disputes, although recent review correspondence suggests that such an approach might be a useful contribution to the history of North American archaeology (see Dekin 1972c, 1973c; Lee 1973, 1974; Kuhn 1970:200). At present, there seem to be three sources of variation subject to study. Variations in the data available are basic to the study of archaeology, since investigators excavate sites and are, at least initially, sole possessors of significant data. Unfortunately, the trend in the last few years has been towards the publication of syntheses, summaries, and conclusions, leaving the substantive data on which these analyses and interpretations are based unpublished and subject to the continued control of the original excavator. Earlier archaeology in the Arctic saw the publication of complete data as a matter of course, with interpretations or conclusions somewhat infrequent. In short, variations in interpretations stemming from differences in the data considered are becoming increasingly significant sources of variation in archaeological interpretation.

The last several decades have seen the introduction of numerous methods and techniques of analysis of archaeological data. While it is only recently that sophisticated statistical techniques have been applied to Arctic data (for example McGhee 1972b; Dumond 1974a), trait lists and seriations of archaeological data have been commonplace, although less so since the advent of extensive reliance on radiocarbon dating for chronology building. Variations in methods of analysis are not as significant as other sources of variation, because the methods used are dependent on the data under consideration and the analytical goals of the investigator.

This brings us to a major source of variation which has received little attention from Arctic archaeologists. In spite of the winds of debate over the paradigm under which archaeological research is conducted, little of this debate has penetrated the Arctic. Few Arctic archaeologists have made explicit the assumptions which guide their research, whether these relate to the definition of problems worthy of study or to field data worthy of recording (see Kuhn 1970:47). This lack of explicitness has resulted in the necessity of searching for evidence of the implicit assumptions which underlie research -- a time-consuming and possibly unreliable process.

Much of what has passed for a prevailing paradigm in Arctic archaeology has assumed that culture, as a system of learned behavioral norms, should be the major focus of our studies, and that most of the variability in archaeological data was caused by cultural variation.

To operate under such a paradigm, the entities of archaeological analysis (i.e., attributes, artifacts, structures, components, locales, etc.) were treated comparatively and normatively (typologically). Hypotheses were presented to explain differences in the data and were evaluated as competing alternatives (Cole and Kleindienst 1974:353), assuming that one of them was "correct". Research was overtly inductive, or at least data-centered, rather than deductive. Much effort was expended in trying to develop a space-time framework based on the assumption that morphological similarity was indicative of contemporaneity (a type-fossil approach).

Site-collections were the major unit of analysis, with within-site differences ignored or disregarded. Data categories were collapsed within sites, lumping together data from contiguous structures, find spots, or even beach levels. The "homogenization" of sites precluded the effective study of the spatial dimension of human activity within a settlement.

However, evidence of the existence of such a paradigm must be gleaned from scattered statements of research intent, methodology or results, as it was invariably implicit and not exposed for examination. Thus, the paradigm under which archaeological research has been conducted in the Arctic has been rarely stated and even more rarely questioned.

Archaeologists had been lulled into a state perhaps best described as mindlessness, following Charles Silberman's criticism of American education in general. Mindlessness is ". . .the failure or refusal to think seriously about. . .purpose, the reluctance to question established practice" (1970:11); "it simply never occurs to more than a handful to ask <u>why</u> they are doing what they are doing. . ." (1970:11). Such a concern with purpose characterizes much of the rhetoric surrounding the "New Archaeology", and it is appropriate to examine here the purposes and paradigms of Arctic archaeology. By urging a shift from mindless to mindful and from implicit to explicit, we may place Arctic archaeology on a firm intellectual foundation, paving the way for non-trivial contributions to scientific method and theory.

It is appropriate to enunciate a revised strategy for the conduct of archaeological research -- one whose roots are in the distant past and which builds on recent discussions of archaeological method and theory. The study of archaeology is first and foremost the study of past human behavior, using essentially artifactual evidence. The fundamental research strategy is the partitioning of variations in human behavior, using techniques analogous to statistical techniques for the partitioning of variance (Alker 1965:73-80). Explicitly, variations in human behavior are seen as the result of multiple causes (sources of variance), and it is our major goal to understand the processes which contribute to such variations. Methodologically, we must return to the method of multiple working hypotheses, not as it has recently been utilized, but as Chamberlain originally proposed (1897, reprinted 1965). We must make explicit notice of the multivariate nature of causes of human behavior, and recognize that multiple hypotheses are not posed as alternates nor are they necessarily mutually exclusive (Cole and Kleindienst 1974:353). Rather, they are proposed as sources of variance, each of which <u>may</u> contribute to the variations in the observed behavior which is the focus of our research. Our task is to explore the nature of the variations to determine the behavioral processes which affect them.

In partitioning the sources of variation in human behavior, we do not assume the primacy of any particular process or set of processes. As anthropologists, we should not assume the awkward stance of postulating that which we wish to prove. Those who assume a priori that the variations in human behavior which they have observed (or which they have data for) result from learned normative systems (i.e., culture) are guilty of the tautology that this explicit paradigm attempts to avoid.

We should also avoid the misleading belief that one of the hypotheses we might be considering is "true", while the others are "false". As recent debates in other aspects of anthropology have suggested, previously hotly debated competing hypotheses are now seen as complementary, both contributing to variations in human behaviors (Drucker 1974: 608). Examples of such debates based on the presumed exclusiveness of hypotheses include the nature/nuture controversy in biology, psychology and anthropology. In the evaluation of methods for the analysis of behavioral data, we find again that the postulate of exclusiveness has been recently discarded. The use of the total morphological pattern as a multivariate approach to the study of human and fossil morphology, and the recent use of multivariate statistical techniques for the description and comparison of human morphological data (Lasker 1970) have replaced the 1000 cc. rubicon and other single criterion approaches

to our understanding of human evolution.

It is difficult not to fall into the same error of couching this strategy as the true and best approach to our understanding of human behavior. However, I hope that by demonstrating that this approach is holistic and open we can see how other restricted strategies are not as appropriate, nor as widely applicable as an umbrella under which archaeological research may be conducted. In particular, I wish to avoid the impression that this is a recipe to be followed mindlessly. Such a cook-book approach has unfortunately marred some of the recent advocation of statistical methods and random sampling. A random sample of an inappropriate universe will improve neither the data nor the results. The mindless application of explicit methods will not of itself result in increased understanding of human behavioral processes.

It is largely for the above reasons that this study was made as explicit as possible, so that the strategic decisions made in the course of the research may be open to question and evaluation, and so that sources of variation in techniques, methods, and results of archaeological research may be elucidated.

This study did not produce the definitive understanding of human behavior within the Arctic Small Tool tradition. Rather, it made a start and pointed the way in which further research should proceed. Much of the present and past research in this area has not been productive of an increased understanding of human behavior and has suffered from the lack of an explicit paradigm. Much of the data gained archaeologically from the Arctic was collected using imprecise field techniques, lumped into categories which reduced our ability to study variations within each site, and published in an incomplete and normative manner, which also reduced the variability available for study.

This study demonstrated the utility of an explicit and precise study by developing and testing models of human behavior analyzed at several levels of abstraction. This combination of explicit and precise methods and models should raise more questions than it answers, but such a procedure is important because it has not been done before, particularly at several different levels of abstraction.

Moreover, if this approach proves useful for future research, it has serious implications for the organization of such research. We will require different data presented in different forms than has previously been the practice. Explicit and precise raw data must be readily available for all interested researchers, and variety in archaeological data must receive as much attention as do norms. I echo Struever's call for a reorganization of the institutional framework in which we can conduct archaeological research (1968:133) to facilitate the coordination of research which an explicit and precise strategy will require. Programs of archaeological publication will require revision, to facilitate the exchange of needed data and to coordinate the research strategies.

In short, I see these pleas for explicitness and precision as leading to the re-focus of archaeological research on the study of human behavior and to the generation of non-trivial hypotheses and laws. It would be difficult and awkward to avoid the hortatory fervor which may appear to characterize this study, and I only hope that clarity has not suffered.

The Intellectual Environment

It is impossible to consider the development of a field of study without considering the intellectual environment in which such developments occurred. In this instance, the major environmental influences on the study of the early prehistory of the Eastern American Arctic came from the "parent discipline", anthropology. It is thus impossible to divorce a study of archaeological theory and method from that of anthropology, and we will benefit from a brief consideration of the interrelations between these disciplines.

Recent discussions in archaeology have been preoccupied with the questions of the history and status of scientific paradigms (Fitting 1973; Leone 1972a; Sterud 1973; Clarke 1972a; Binford 1972; etc.), following Kuhn's categorization of a paradigm as a common body of accepted theory and of appropriate methods and techniques of analysis (Kuhn 1970). From the onset of such a discussion, we are forced to assess the evidence for the very existence of such a paradigm by looking at the literature to determine if such agreement on method and theory existed or exists, both in anthropology and in archaeology.

Beginning with anthropology, the recent assessment of Manners and Kaplan (1968) is a fitting example of the state of theory in anthropological research.

> Scattered through the anthropological literature are a number of hunches, insights, hypotheses, and generalizations, some tentative and limited, some of broader scope and more generally accepted. But they tend to remain scattered, inchoate, and unrelated to one another, so that they often get lost or forgotten; and the tendency has been for each generation of anthropologists to start outfresh without any very clear sense of what is known about an area of research. (We are tempted here to paraphrase Santayana: Those who have no memory of the history of anthropology are doomed to repeat it.) Among the consequences of this failure is that theory-building in cultural anthropology comes to resemble slash-and-burn agriculture as Anthony Wallace has recently noted: "After cultivating a field for a while, the natives move on to a new one and let the bush take over; then they return, slash and burn and raise crops in the old field again" (1966, p.1254).
>
> This "slash-and-burn" character of anthropological theory-building also stems from the failure of anthropologists to be more self-conscious about the logical properties of theories and about what it means to assert that a theory "explains" a set of phenomena. A more explicit awareness of such issues would, if nothing else, greatly reduce the output of what often passes for explanation in anthropology. Here, we believe, anthropolgists may learn a great deal from philosophers of science, provided that their learning is somewhat tempered by the knowledge they have of their own discipline. After all, it is the anthropologist who knows the significant empirical problems of his discipline and not the logician or the philosopher.
>
> But there is always the danger that anthropologists can become so over-awed by the impressive technical arsenal of the mathematicians, logicians, and philosophers that, in an attempt to achieve

> greater methodological rigor and to do what philosophers and mathematical logicians say they ought to be doing, they may unnecessarily constrict the discipline rather than liberate it. We are all for greater logical and methodological sophistication. But if we permit methodology to suggest the problems we deal with rather than allowing problems to determine the methodology, we clearly run the risk of being more precise about a continually narrowing range of cultural issues or phenomena (Manners and Kaplan 1968:11).

In their view, there seems to be no generally accepted body of theory and methods that could be called the paradigm of anthropology. Theoretical statements are rarely explicit and the recent emphasis on method appears to put the horse before the cart. The recent review of the concept of culture by Singer (1968) also points to the lack of agreement on the conception and use of the concept of culture which is generally accepted as the focus of anthropological research.

It is widely accepted that the major contribution of anthropologists to western intellectualism has been this very concept of culture. Because to a great degree, the changing interests and interpretations of archaeologists reflect the changing perceptions of the concept of culture, it is useful to review briefly trends in this perception. Kroeber and Kluckhohn (1952) reviewed the variety of conceptualizations and definitions associated with culture, producing a monumental work which is frequently as confusing as it is enlightening. Trends in the use of the concept were seen as extending from Tyler's definition in behavioral terms to abstractions regarding symbol systems (see also Singer 1968:540).

While this process of change in the use of the concept of culture was generally accepted, the value of these changes was debated.

> Despite Kroeber and Kluckhohn's feeling that 'the greatest advance in contemporary anthropological theory' is the shift from 'concrete-mindedness' to traffic in abstractions, I venture to predict that anthropology will again revert to defining culture in terms of concrete, objective, observable things and events in the external world. I make this prediction with some confidence because it is the procedure in every other science--in all of the more mature sciences, at least--and we believe that cultural anthropology will mature some day. . . .
>
> We believe that they have expressed a prominent --perhaps the dominant--trend in conceptions of culture during the past twenty-five years, and they have done it effectively and well. This trend is away from the conception of culture as objective, observable things and events in the external world and toward the conception of culture as intangible abstractions. We deplore this trend because we believe that it is a veering away from a point of view, a theoretical standpoint, that has become well-established and has proved itself to be fruitful in the tradition that is science; the subject matter of any science is a class of objectively observable things and events, not abstractions. This shift in conceptions of culture, will, therefore, only make the achievement of a science of culture more difficult (White 1954:465,467).

The picture of past and present anthropological theory does not support the existence of any set of accepted theory and methods, thus it would be hard to reach any sort of agreement on the existence of an anthropological paradigm. In the sense of Kuhn, we find ourselves in a preparadigmatic stage in which there are competing theories and concepts, diverse opinions on what constitute legitimate research endeavors, disagreements on what and how to measure, and no agreement on what constitutes an explanation of anthropological phenomena. Fitting does not believe that the present state of archaeology is preparadigmatic (1973:6) but I believe his view to be misled by the fragmentation of the disciplines of anthropology and archaeology. Overall communications have been reduced and special interest groups have formed in which we are beginning to learn more and more about less and less. In certain areas of anthropological or archaeological research, there existed sets of accepted theories and possibly paradigms, but these were frequently restricted to small groups of researchers working on similar problems or in similar regions. The dissatisfaction with attempts to synthesize the results of archaeological research in North America (Willey 1966; Jennings 1968; see Fitting 1973:2) is perhaps indicative of the extent of the lack of communication among such informal research networks. I suggest that the existence of previous paradigms in anthropology and archaeology was more apparent than real, and that our view of the history of anthropology is colored by changes in the patterns of communication and of research endeavors. The recent broadening of communications among these informal research networks has increased the competition among methods and theories and may be seen as either the escalation of the competitions inherent in a preparadigmatic stage or as an attempt at paradigm revolution. Because many discussants of these trends see the explicit use of scientific methods as progress (see Watson et al 1971; Martin 1971; Johnson 1972; Thompson 1972; Clarke 1972a; Sterud 1973; Fritz and Plog 1970; Stickel and Chartkoff 1973; etc.) and because there is essential agreement that we are going through (or have gone through) a major revolution in archaeology involving the explicit use of a scientific approach, I suggest that what we are seeing is the culmination of a competition among theories and methods which may result in the final acceptance of a paradigm for archaeology in North America at least. This acceptance is made more difficult because anthropology does not itself have a paradigm (see Chaney 1972:996-997; Leone 1972a:24). The current debate in archaeology and anthropology was anticipated by White (see above) as was the difficulty in achieving a science of culture.

By and large, the "New Archaeology" is a concern for technical rigor in data gathering, for clear statement of methods and assumptions, for the explicit utilization of inductive and deductive hypothesizing, for the utilization of methods of systems and ecological analyses, and, perhaps most importantly, for the focus on the study of human behavior (Binford 1972:132; Taylor 1968:108: Deetz 1968; Leone 1972a:23; MacNeish 1974:463; etc.).

Viewing our conceptual system as a system of constraints on our scientific research, it becomes apparent that the concept of culture as some sort of abstraction from behavior constrains our use of the terms culture,

cultural system, and cultural sub-system (see for example, Taylor 1968:108; Binford 1962; White 1954). While Binford's early statements on method and theory were significant appraisals of archaeological and anthropological concepts, he has yet to articulate a well-integrated paradigm to compete with his version of traditional archaeology.

To Binford, the traditional archaeologist is concerned with the study of culture, first and foremost. Variations in artifactual data are seen as stemming from variations in culture; or more simply, artifact sets are cultures.

It is this aspect of traditional archaeology that has come into question. As anthropologists are undecided regarding the definition and use of the concept of culture and of cultures, so their constituent disciplines that seek to operationalize their research based on an uncertain paradigm create similar uncertainty (see, for example, Wax 1973:167-168 regarding the inappropriateness of the concept of cultural pluralism).

It is this uncertainty regarding the use of the concepts of culture presently under discussion and debate that is symptomatic of the present paradigmatic crisis. The present criticism of the use of the term culture as an abstraction is much the same as the earlier criticism of the use of the concept of "artifact type" and the entire normative typological approach as it has been practiced in archaeology (see Hill and Evans 1973; Kehoe and Kehoe 1973; Binford 1965).

The conjunctive force uniting the "new archaeologists" has been largely their criticism of the "old archaeology." During the middle 1960's, many archaeologists adopted a "show me" stance, in which they held judgment of the methods of the proponents of the new archaeology in abeyance until they had produced something concrete to evaluate (Taylor 1972:30). The studies of Martin et al. (1962,1964), Hill (1970), Longacre (1970) and others provided grist for the mill of criticism while the delay in formidable criticism increased the influence of their ideas on that generation of archaeologists trained in the 1960's and early seventies.

There remains one remarkable failure of the new archaeologists. This is the failure to develop an explicit statement of a paradigm to compete with that of the traditionalists. This failure is remarkable for two reasons. First, Binford and his colleagues have had at least ten years of intellectual growth and exposure in which to formulate such a paradigm. Second, the competition of theories and models necessary for a scientific revolution demands the formulation of some sort of paradigm, even as a straw man, yet it has not been forthcoming.

Instead, what we have seen is the suggestion of explicit methodologies (Watson et al. 1971), utilization of results of other models such as systems theory or ecology (Leone 1972a:23; Clarke 1972a) and most recently the focus on the study of behavior (Deetz 1967:105; Binford 1972:132) or activities (Redman 1973a:717), but no one has yet presented a consistent and explicit paradigm. The journals have presented numerous discussions of methodology, most of which appear to offer the hope of impressive theoretical contributions. Is methodology all there is to the new archaeology? I think not, but the reviewer of the literature would be hard pressed to come to a different conclusion, especially given the composition of recent monumental works in this area (Clarke 1972b; Leone 1972b; Renfrew 1973). If such a paradigm of the new archaeologists exists, we might expect that these volumes would contain it, but they do not.

I believe the message from the rhetoric surrounding the present paradigmatic crisis to be simple. We should take as our goal the study of human behavior, perhaps focusing on that portion of human behavior which is learned (i.e., culture). Our general approach should be scientific, using inductive methods where our paramount concern is with the data available, and using deductive methods from general systems theory, from ecological theory, from anthropological theory, or from other logically relevant bodies of theory. Our research techniques should be as sophisticated as possible, consistent with the nature of our data, their variety, diversity and frequency, and should be guided by the failures and successes in the techniques of other sciences. To accomplish this, we should make our assumptions, biases, premises, postulates, theories and data collection methods and techniques as explicit and precise as necessary to allow the replication of our results in so far as possible.

In a real sense, the strategy which I propose forces us to study observed variations and patterns in artifacts without a priori assumptions about the nature of such patterns resulting from "cultural" behavior, as it forces the consideration of alternatives (multiple working hypotheses) and facilitates the partitioning of numerous sources of archaeological variation. If we wish to focus our attention on cultural variation, then we must have considered and controlled for other sources of variation. To do this, we must have a strategy which facilitates this research process, and it is just such a strategy which I propose.

Chapter 9

Recapitulation

Variations in models of archaeological data have resulted from differences in the research activities of archaeologists as well as from differences in archaeological data. Differential archaeological field techniques and research strategies have contributed to sample bias (Knuth 1967; McGhee 1973a; and Taylor 1968a) which we have not always considered. Perhaps a more pervasive source of variation has been the normative approach to data sets which have reduced the study of the within-sample variety (Anderson 1970; Knuth 1967; Maxwell 1973a; and Taylor 1968a). While this approach may be appropriate with some data-sets and some problems, it should not be routinely applied to all problems, nor should it be allowed to dominate the selection of field techniques or data categorization and depiction processes.

The paradigm of archaeological interpretation which assumes that the variation in archaeological data stems largely from cultural variation has led to the interpretation of heterogeneous samples as the result of mixtures from two normative (cultural) systems (Harp 1958; Larsen and Meldgaard 1958; McGhee 1973, 1974), and a tendency to minimize variation within cultural units. By focusing on the study of normative systems, we have largely ignored the study of behavioral variability within such systems.

Our basic strategy has involved the generation of competing models (often implicit), when it is becoming increasingly apparent that there are a number of sources of variation to be modelled, probably initially separately, and ultimately collectively in a very complex system of related models. The process of model building affects the selection of methods and techniques, especially within a deductive or a problem-oriented approach to archaeological research. These selective processes will yield data amenable to certain analytical techniques and not to others. Since we cannot avoid such selection, it is essential that we specify the techniques and methods used and make them as explicit and precise as possible to allow the fullest reconstruction of the data base from its descriptive parameters. This necessitates a change in approach to emphasize the dispersion parameters as well as the modal (normative) ones, particularly in "site reports" and reconnaissances in which the data assume paramount importance.

As Arctic archaeology has developed, it has passed through a number of stages of problem orientation. These include an initial emphasis on "what" and "where" kinds of questions, with passing interest in the "who" (both biological, or racial, and ethnic). As the answers to these questions became available, increased emphasis was placed on the "when", with numerous techniques being applied (cross-dating by sequences from other disciplines, seriations, etc.). With the advent of radiocarbon dating, the answers to these questions become more easily obtained and the space-time framework of human occupations could be generally established.

Prior to this time, one major tactic was to postulate away the complexities in the data, or to operate at such a high level of abstraction that the control of time was only generally a problem. With the answers to what, where, and when within reach, our attention has turned to strategies aimed at answering questions of "why". This search for explanations had been largely speculation based on implicit assumptions, but this is changing in archaeology in general, and in the Arctic in particular.

To utilize a scientific approach to explanation, we require explicit and precise answers to the questions of what, where and when. For this reason, our radiocarbon chronology requires careful attention and we can no longer play it "fast and loose" if we wish our search for explanations to be productive. For this reason, the chronology developed explicitly in Chapter 2 is as precise as possible at this time and can serve as a framework for this research. Those areas of imprecision have been isolated and discussed and are thus open to the scrutiny of others who might wish to criticize the use to which such a framework has been put.

It is readily apparent that the prehistoric human occupation of Arctic North America occurred in an environmental setting marked by a relatively simple ecosystem of great specific variability, through both space and time. As we wish to make our study of the adaptive relations between the behavioral systems of human foragers and their environment more specific, we will need more specific and detailed reconstructions of paleoenvironmental change. However, the complex ecological relations in the Eastern Arctic are only generally known and are subject to differential selection and interpretation. In order to avoid the selection of interpretations and data which suit our predelictions, we must treat our selection process precisely and explicitly.

Such treatment has resulted in the definition of an ecological core area and variegations in significant environmental variables through space and time. At the risk of over-generalizing, the core area has been realtively unchanged by environmental fluctuations which have had greater impact in peripheral regions. Through time, its occupants have had consistent access to a variety of resources, subject to greater variability in peripheral areas.

Peripheral regions have had fewer alternate resources available and may have had subsistence patterns focused on one or more resources. Thus, the occupants of the western tundra may have exploited land mammals of the tundra to a greater extent than the eastern sea mammal hunters. The northern islands and North Greenland may have shown greater dependence on musk ox and fishing, and so forth. These regional variations on an overall subsistence capability may have been forced adaptations to environmental changes or may represent attempts to optimize subsistence strategies in areas with different resources. In order to specify the adaptive changes in subsistence, we need to pay greater attention to the specific evidence for subsistence activities (i.e., faunal remains) and changes in subsistence strategies.

The dynamics of Eastern Arctic environments through time have been the subject of considerable attention, and the sequence of changes which I have specified elsewhere continues to be substantiated by additional research. During the Pre-Dorset occupation of the Eastern Arctic, we can reconstruct general environmental changes with some reliability (see Table 8), while the specific local impacts are only occasionally assessable. However, if we wish to explain the variable adaptive relations between human behaviors and their environment, we must treat the environmental data as precisely and explicitly as possible. Their use as input into the ecological systems model is ample demonstration of their utility and for the need for care in their depiction and utilization.

Partitioning the sources of variation in the data for prehistoric human behaviors is a complex and difficult task. When we were asking relatively simple questions of these data, such variations were of less consequence than they are in our present efforts at explanation. I have attempted to list the potentially significant sources of variation as a first step in sensitizing us to their frequency and complexity. I have suggested several specific strategies aimed at controlling some of these sources of variance on the way to studying others.

The first step in controlling these variables is to recognize their potential as sources of variance. The second step is to devise research strategies which allow the study of the sources of variance in which we are interested.

It is essential that our efforts at studying cultural processes and explaining human behavior be based on explicit and precise research strategies, techniques, and methods. Because of the complexity of these sources of variance, our first steps will be fundamental, tentative, and possibly even faulty, but it is essential that we begin our research on a scientifically valid series of strategies which some may see as a paradigm of scientific archaeology.

Variations in the evidence for Arctic Small Tool tradition structures have posed a considerable interpretational problem in the Eastern Arctic. While some structures have clearly delineated outlines and patterned internal features, others were rock and artifact clusters (occasionally with central features, such as hearths and mid-passages) without defined outlines. These had been variously interpreted as some kind of dwelling (Taylor 1968a:14), possible of snow houses (Maxwell 1967; 1973a:303-304,310). The data available were not subject to testing because specific data regarding the relations among the artifacts and rocks had not been generally recorded or depicted.

By utilizing precise field techniques, we were able to study the relations among the artifacts and rocks in such clusters and to utilize a novel and innovative data structure for the testing of deductively generated hypotheses. This previously untried combination of precise data and explicit scientific methodology led to the ability to resolve the engima of these rock and artifact clusters and developed a tentative model of tent structures and behaviors at the Closure site. This model may now be tested on relevant data sets in the Eastern Arctic and the techniques developed here may be applied in the resolution of similar interpretational problems wherever they occur.

The early sites in the Eastern Arctic comprise a data mosaic which has been subject to different interpretations. The diversity in this data mosaic has several sources of variance, which include: sampling error; sampling bias; differential data depiction and lumping; different environmental settings with differential resources; and excavation and preliminary interpretation by different archaeologists.

Our perception of this mosaic is also influenced by a reliance on relatively rare artifacts (curated) as diagnostic of particular cultural systems. Such a normative approach focuses our attention on culture as a modal system and inhibits our ability to study diversity within the system. Again, we find ourselves trying to understand a complex interactive system by studying the differences between its nodes, seen normatively (through the use of diagnostic lists, guide fossils, means, etc.).

By making explicit our awareness of culture as an adaptive system of potential behaviors which may be utilized differentially in response to particular characteristics of the environment, we are better able to examine the diversity in this mosaic. By modeling the behavioral processes which contribute to the observed variance, we can study the complex dynamics of human populations, recognizing that the evidence we have for changes in behaviors may result from the adaptive competition among potential behaviors within the same cultural system (adapting to environments changing through space and time--evolving, migrating, fluctuating through seasons, etc.).

For this reason, we may need to focus our study on discontinuities in the data mosaic, partitioning sources of variance as they reflect processes of our acquisition of archaeological knowledge or processes of the behavior of the prehistoric inhabitants of this region. The use of a deductively derived model of the dynamics of human populations which focuses on human behavioral processes as they affect human technology is one step in this strategy.

The model of the Arctic Small Tool horizon accurately predicts variation in several dimensions of technology by explaining this variation as resulting from the dispersal vector of an adaptive and diversified cultural system. There is clinal variation in expedient artifacts only loosely constrained by functional requirements. There is increased variety in curated artifacts within the horizon, with similar artifacts found across large areas. Such homogeneity among sites in curated artifacts is also predicted by the model. Such an interpretation which involves the mixing of behaviors (and not "cultures") is more parsimonious than one which views these similarities as the mixture of the products of two separate cultural systems, in which the failure to find "pure" manifestations is the result of sampling error and negative evidence. The search for such cultural purity is itself reflective of a normative bias, and of a paradigm which postulates cultural difference as the major source of technological variegation.

The strategy reflected by the development of the above model will necessitate numerous complementary models if we are to be able to predict and explain the variety in the Arctic Small Tool horizon. By making explicit our focus on the behavioral processes which contribute to such variance and by modeling the relations among the nodes in this data mosaic (as well as the nodes themselves), we will move beyond the use of implicit models and imprecise concepts such as migration and diffusion to a greater understanding of complex human behaviors.

There have been a number of conceptual frameworks presented as models of the Pre-Dorset development in the Eastern Arctic, none of which provides an adequate explanation. This model depicts the macro-fragmentation of an Arctic Small Tool horizon base into regionally distinct variants as resulting from a reduction in social scale caused by a major environmental change. It is apparent that the alternate sources of variation were differential invention and differential adaptation to environmental differences (in the macro-scale), and that the model explains the maintenance of these behavioral variations through time.

Such a model is markedly similar in structure and content to those demonstrated by McKennan (1969:99) for Athapaskan linguistic variation and by Meiklejohn (1974: 135,138) for population genetics variation in band societies. Both of these have focused on the changing relations among spatially distributed nodes (or foci). All have been the result of the study of human behaviors in band societies. Inasmuch as all of these phenomena being modeled (technology, language, and genetics) are both evidence of and are influenced by human behavioral changes and changes in communications patterns among nodes, a model which relates evidence of behavioral change to changing patterns of communications and changing environments can have widespread utility. This explicit focus on relations among social entities within a widespread differentiated cultural system will require explicit and precise archaeological

data in order to facilitate the development of additional models of human behavioral processes. By proceeding with such a strategy as outlined and demonstrated in this thesis, we will place the study of human behavioral processes in a changing environmental setting on a level of sophistication which will allow the integration of models from archaeological studies with models of human behavioral processes from ethnographic studies (such as Hill 1970 and Longacre 1970) and with models of developmental change (Chance 1968 and Pothier 1968). We will then be in a position to provide the time depth and ecological dimension so lacking in present efforts at modeling behavioral and cultural change, and will demonstrate the utility of archaeological research in developing a more sophisticated and widely applicable understanding of human behavioral processes. Such models must be based on explicit and precise methodologies such as I have proposed herein.

Recommendations for Further Research

Perhaps the most pressing need for research in the Eastern Arctic is the establishment of seasonal variegation in any one archaeologically-known behavioral system at any one point in time. This will necessitate a well established chronology with established contemporaneity, as well as analysable collections of faunal and floral materials to establish the sequence of seasonal occupation. The variations in the data depicted by such a strategy could then be used as a model for studying the variations in other data less well-controlled and could be used for the further study of within-site behaviors and division of labor. While these studies could be conducted in several regions, I know of no concerted effort to study such seasonal variation within one locale or region. This would involve the archaeological survey of a variety of potential land-use patterns in a variety of accessible habitats.

From studies such as these, we could refine models of behavioral change and specify the adaptive responses to changes in environments. Changes in the scheduling of subsistence activities could be specified, and models of adaptive responses to environmental changes could be tested.

It is also time for increased theoretical attention to population dispersals (migrations), in order to develop models comparable in specificity to cultural traditions. Such horizons of archaeological data have received little attention, in spite of their frequency. Of the three most extensive and rapid archaeological horizons presently known in North America, two are found in the Arctic. These are the Arctic Small Tool horizon and the Thule horizon. The above model could be tested on the Thule horizon, as well as on the Paleo-Indian horizon in southern North America.

Ecological systems models such as I developed above could be tested on additional data in which environmental changes may be significant inputs. The most appropriate data for further testing would be the subsequent prehistory of the Eastern Arctic, in which these same processes could be expected to be significant sources of variation.

Because much of the above discussion has considered (albeit briefly and with a certain lack of specificity) the partitioning of sources of variance in human behaviors known archaeologically to be the major research strategy of archaeology, it would be useful if an explicit attempt were made to study both the process of archaeological research and the interpretation of archaeological data with the express intent of partitioning the variance. This would require a region in which the archaeology was well known and in which the archaeological data were readily available. It would also require the use of statistical and mathematical models, as well as specific knowledge of paleoenvironments and ecological relations. This seems a large study, utilizing various specialists comparable in scope to MacNeish's research efforts in the Tehuacan Valley.

It would also be appropriate to study the developments in the Western Arctic following the Arctic Small Tool horizon, in particular to see if the models developed above apply there. The development of the late Arctic Small Tool tradition at Punyik Point and Walakpa could be tested to see if growth (as defined above) occurred. Preliminary interpretations by Irving regarding lithic specialization and the Arctic Small Tool technique of lithic flaking suggest that such research might be very fruitful in this area.

There are undoubtedly other adaptive, ecological, cultural or behavioral processes which contribute to the variations in the Arctic Small Tool horizon and in the subsequent Pre-Dorset period. With increased attention to precise field techniques and ecological analyses, we may be able to specify and model such additional sources of variance. Alternatives to be considered include: the importance of raw material size for lithics manufacture (particularly, whether variation in size of finished products are related to raw material size); the variations in flint tools necessitated by differences in hafting materials or raw materials on which they are used (particularly, the differences between the use of ivory and the use of antler, both as hafting material and as raw material); and differences in projectile points and end blades (including harpoons) resulting from hunting practices and other subsistence activities.

I hope that the methodological contributions of this thesis and the models presented herein may result in the resolution of previously unresolved problems and may place Arctic archaeology in a firm position to contribute to our developing understanding of human behavior.

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