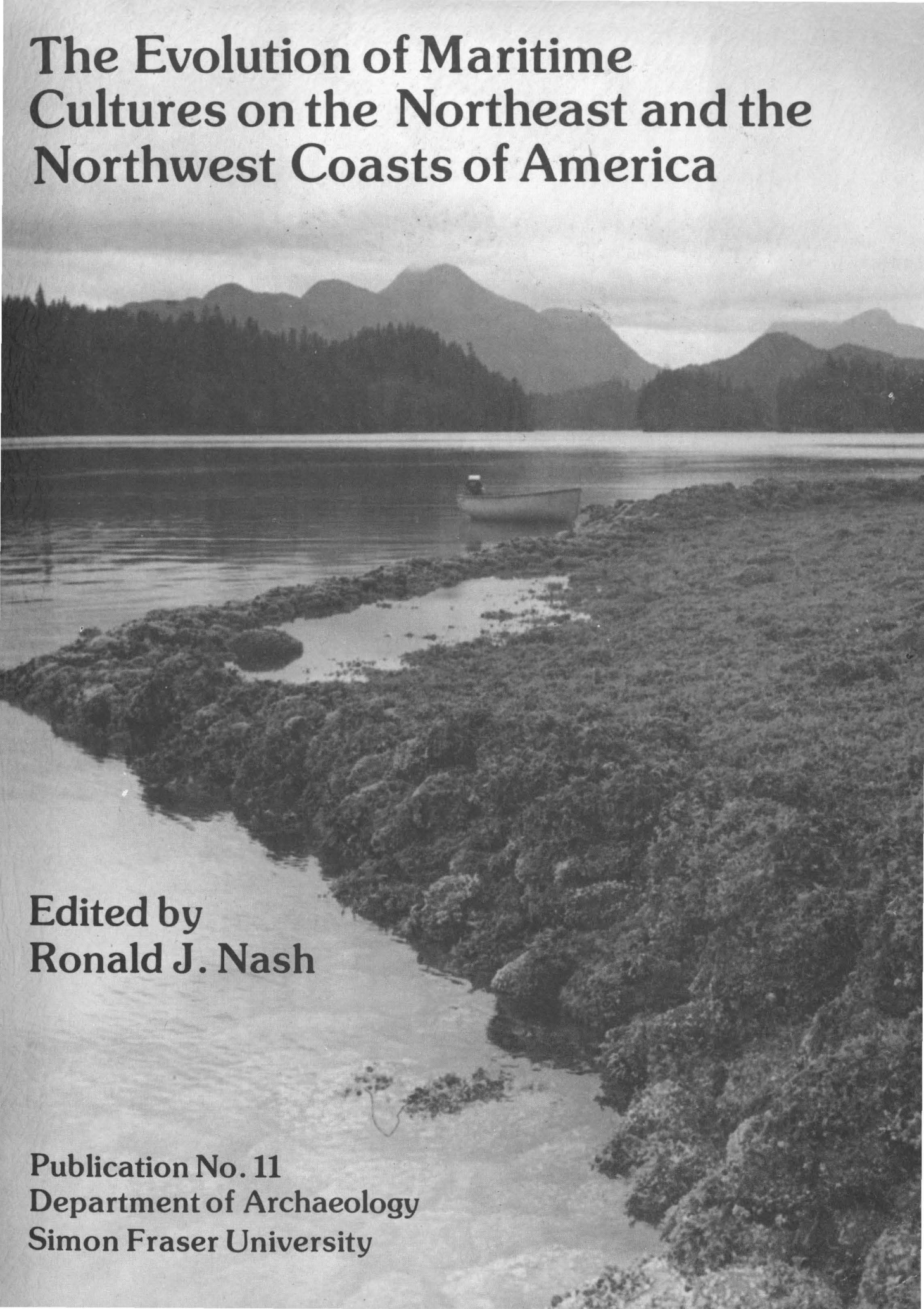
The Evolution of Maritime Cultures on the Northeast and the Northwest Coasts of America



Edited by
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PREFACE

Ronald J. Nash

Those of us living and working along the seacoasts are increasingly recognizing that maritime cultures are most often quite specialized and qualitatively different from interior cultures. Some authors have identified distinctive subsistence-related features, others, demographic or socio-political attributes or even dialectical features. We are only beginning to appreciate the nature and complexity of maritime cultures and their dynamics of change, but it seems clear that societies living at the land/sea interface warrant special examination. The papers in this volume do not deal directly with questions of definition for most authors would accept the uniqueness of "life at the edge"; rather, these papers consider the evolution of cultural complexity in two maritime settings -- the Northeast and the Northwest Coasts.

The areal coverage focuses on the Northeast Coast -- Labrador, Newfoundland, the Maritimes and the state of Maine -- and the Northwest Coast--southeast Alaska, British Columbia and Washington. These coastal zones have enough general similarities to warrant comparative examination of the cultures that developed there. In particular, both coastal zones were glacially formed and are adjacent to temperate (or subpolar) oceans with high primary productivity of the surface waters leading to large fisheries (Gross 1977). There are of course, major differences, particularly the presence of the Coast Range in the west which produces a fjord-type coastline as well as increased precipitation and a corresponding rain forest vegetation. Again, the marine life of the North Pacific is considered to be more varied and abundant than that of the North Atlantic, a consequence of the Pacific's longer history as a temperate ocean (Bodsworth 1970:101).

Given such obvious macro-environmental differences and the historical independence of the two areas, it is reasonable to seek only the most general similarities in form and process, those things that reoccur as a consequence of a maritime way of life. In some of these papers there are efforts to specify recurrent processes or necessary boundary conditions for the development of cultural complexity, but there is no consensus as to the potential of nomothetic generalizations for those two areas. These papers reflect disagreements about the classification of cultures and, not surprisingly, conflicting explanatory models, but the conflicts are not fundamental theoretical cleavages. We can appreciate the progress represented by these papers if we examine their historical context.

Economic, political and ecological developments in recent years have brought about a renewed interest in the seacoasts and the continental shelf. The adoption of a 200 mile territorial marine limit, the search for offshore oil, gas and minerals and a concern about pollution and declining fish stocks have forced affected countries to accelerate their studies of the coastal zones and instigate resource management programs. Unfortunately the baseline research has usually been conducted through government institutes and with little reference to archaeology. In this regard, it is significant that it was not until 1981 that a formal symposium involving oceanographers and archaeologists appeared on the program for the Society of American Archaeology (Oceanography and Prehistoric Archaeology, S.A.A. meetings, April 1981) -- a long overdue event. However, as cultural resource management work increases, archaeologists are becoming increasingly aware of maritime conservation issues (Whitlam 1981) and some basic inventory studies have been accomplished on parts of the continental shelf (Dincauze 1979). In addition, traditional fieldwork done for a variety of purposes has greatly increased the data base on both coasts (e.g., Shimabuku 1980; Fladmark 1981).

Following consolidation of the theoretical advances made under the "new" archaeology of the 1960s and 1970s, university and museum based researchers in proximity to the coasts began belated theoretical studies on the origin and nature of maritime cultures. Interests here was not centered on the historical origins of particular patterns or cultures, but with the recurrent features and processes characteristic of maritime societies. A pioneer effort in this regard was *Prehistoric Maritime Adaptations of the Circumpolar Zone* (1975) edited by William Fitzhugh. The papers, however, concentrated on synchronic events (adaptations) in northern or subpolar environments with little or no coverage of the Northeast and Northwest coasts. In 1979, David Yesner chaired a comparative symposium at the SAA/CAA meetings (*Coast to Coast: Comparative*

Maritime Adaptations) which again was synchronic in perspective and focused on New England and the American Northwest. More recently, Yesner (1980a) had proposed a definitional model which attempts to identify what it is that maritime hunters and gatherers have in common. These initial comparative and interdisciplinary studies of coastal societies suggest that maritime archaeology is destined to be a growth area within the discipline and that at present, we are in much the same pioneering position as an earlier group of archaeologists who set out to study the domestication of plants and animals and the origins of settled village life.

In 1980, the state of the art was such that it seemed appropriate to organize another bicoastal symposium to deal with the temperate/boreal climatic zones of Canada and the adjacent regions of the United States, hitherto untreated in a comparative way; and also to take a diachronic perspective and in so doing, move beyond the study of maritime adaptations. Participants were invited to contribute papers of a comparative and/or evolutionary nature dealing with theoretical and methodological issues common to the two coasts. Particular issues were to include comparison of cultural complexities between the two coasts and discussion of evolutionary developments (trajectories, rates of change, etc.) and the reasons behind these developments. The topic of cultural complexity on the Northeast and Northwest Coasts is of special interest at two time periods: 1) the ethnographic "present" where the Northwest Coast cultures are usually considered to be more complex than those of the Northeast Coast; and 2) the Late Archaic/pre-Marpole period where the Northeast Coast societies are, in some aspects of culture, more complex than their west coast counterparts.

Accordingly, at the 1981 meetings of the Canadian Archaeological Association, I chaired a symposium entitled *Cultural Complexity and Cultural Development on the Northeast and Northwest Coasts* with the following participants: R. Nash, R. Whitlam, K. Fladmark, R. Matson, D. Burley, D. Sutton, W. Fitzhugh, D. Keenlyside, K. Ames, D. Mitchell, V. Miller, P. Hobler, D. Sanger, A. Spiess/B. Bourque/ S. Cox, D. Yesner and S. Campbell. Discussants were R. Carlson and D. Sanger.

The papers in this volume are revised versions of ones presented at the meetings and are organized according to related topics. The accompanying maps (Figs. 1, 2) illustrate the areas under consideration plus prominent sites and localities on each coast. The lead-off paper by Ronald J. Nash is a review of the frameworks which have structured archaeological investigations on the two coasts and an effort to provide a context for the papers which follow. These concerns are extended by Roy L. Carlson who

reviews the goals, theory and methods which have guided archaeological work on the Northwest Coast. Two ethnographic papers follow which depart from traditional reconstructions of the historic Micmac and Tsimshian peoples. These two papers by Virginia P. Miller and Donald H. Mitchell should stimulate some re-evaluation of late prehistoric cultural complexity and they effectively narrow the cultural-evolutionary gap between peoples of the two coasts. In this regard it is interesting to note that William Fitzhugh's recent discoveries in Labrador have revealed new evidence for hitherto unsuspected complexity in social organization among late Maritime Archaic cultures of the Northeast.

One of the most parsimonious models is that proposed by Knut R. Fladmark who examines east and west coast developments with regard to coastal stabilization. Similarly, David R. Yesner stresses the need for controlling coastal geomorphology in seeking to explain economic changes, but his emphasis is on local variability and change rathern than broad scale changes. Arthur Spiess, Bruce Bourque and Steven Cox summarize the economic and oceanographic changes at the Turner Farm site, before proceeding to a comparison of cultural complexity in Maine and the Northwest Coast. These three authors stress the large number of specific differences (especially social differences) between the culture patterns on the two coasts, whereas in the following paper, Robert Whitlam attempts to model the economic adaptations on both coasts using a single ecological model of considerable generality.

In the final group of papers, there is increased consideration of social complexity, i.e., the specialized or hierarchial arrangements of stratification and ranking. For R.G. Matson, such complexity can emerge where intensive exploitation of a resource is possible as is the case with the Pacific salmon. But, as Philip M. Hobler cautions, even settlement patterns are not dictated by the distribution of the Pacific salmon. Like R.G. Matson, David V. Burley compares the Micmac and the Coast Salish, but he places less emphasis on salmon specialization and argues for a regional ecological approach to understanding intensification and evolving complexity. However, in the concluding paper, Kenneth M. Ames continues development of a general model of ranked societies as stable systems, a model which could be applied to coastal or non-coastal forager societies.

This collection of papers should provide some significant theoretical advances in our understanding of the evolution of maritime cultures, not only for temperate North America, but for the study of coastal fishing societies everywhere. Some progress is apparent in specifying the nature of complexity in maritime

cultures, the preconditions associated with such development, the methodologies needed for study and the processes which can lead to complex maritime cultures; and these advances result from a mixture of ethnographic, ecological and evolutionary approaches. A pessimist might complain that cultures on the two coasts do not have similar evolutionary trajectories and that these papers do not converge towards a unified middle range theory or even neat nomothetic propositions. However, the papers by Whitlam and Ames in particular are headed in this direction, and while there are some key variables (coastal stabilization, salmon), it is apparent that single variable models or single models will be insufficient for handling complex problems concerning the origin and functioning of the Northeast and Northwest Coast cultures. This is not a controversial observation and it is to be hoped that maritime archaeologists with their specialized data will play a leading role in the development of propositions of restricted scope which can be incorporated into middle range anthropological theory.

Acknowledgements:

I am indebted to Roy Carlson for his assistance in the final preparations for this volume and to the Department of Archaeology, Simon Fraser University, for publication funding.

THE PROGRESS AND PROCESS OF THEORY-BUILDING: THE NORTHEAST AND NORTHWEST COASTS

Ronald J. Nash

This paper is a review of the progress and process of model-building as it applies to populations on the Northeast Coast (Maine and Atlantic Canada) and the Northwest Coast (mainly Washington and British Columbia). On both coasts, we are dealing with non-agricultural populations of hunters-gatherers-fishers and one reason for doing a review is to measure the progress being made in developing middle-range and general theory for such maritime cultures. During the 1970s, there was considerable interest in constructing models of the subsistence-settlement systems among hunters-gatherers and these models tended to be predictive with respect to adaptive responses. Bettinger (1980) has reviewed the recent trends in modeling hunter-gatherer cultures, but the review is not concerned with maritime hunters-gatherers. The review offered here covers not only present, but past efforts in model-building and thus affords a historical perspective on the process of paradigmatic change. In addition, such a review serves to focus attention on and delineate issues and problems common to archaeologists working on both the Northeast and Northwest coasts. Finally, in an attempt to avoid simply talking about theory, a dialectical model is advanced to describe some structural and evolutionary developments in these coastal cultures.

In keeping with these objectives, the models are reviewed in terms of: 1) paradigmatic affiliation; 2) applicability to coastal cultures; and 3) degree of generality. The paradigm concept has

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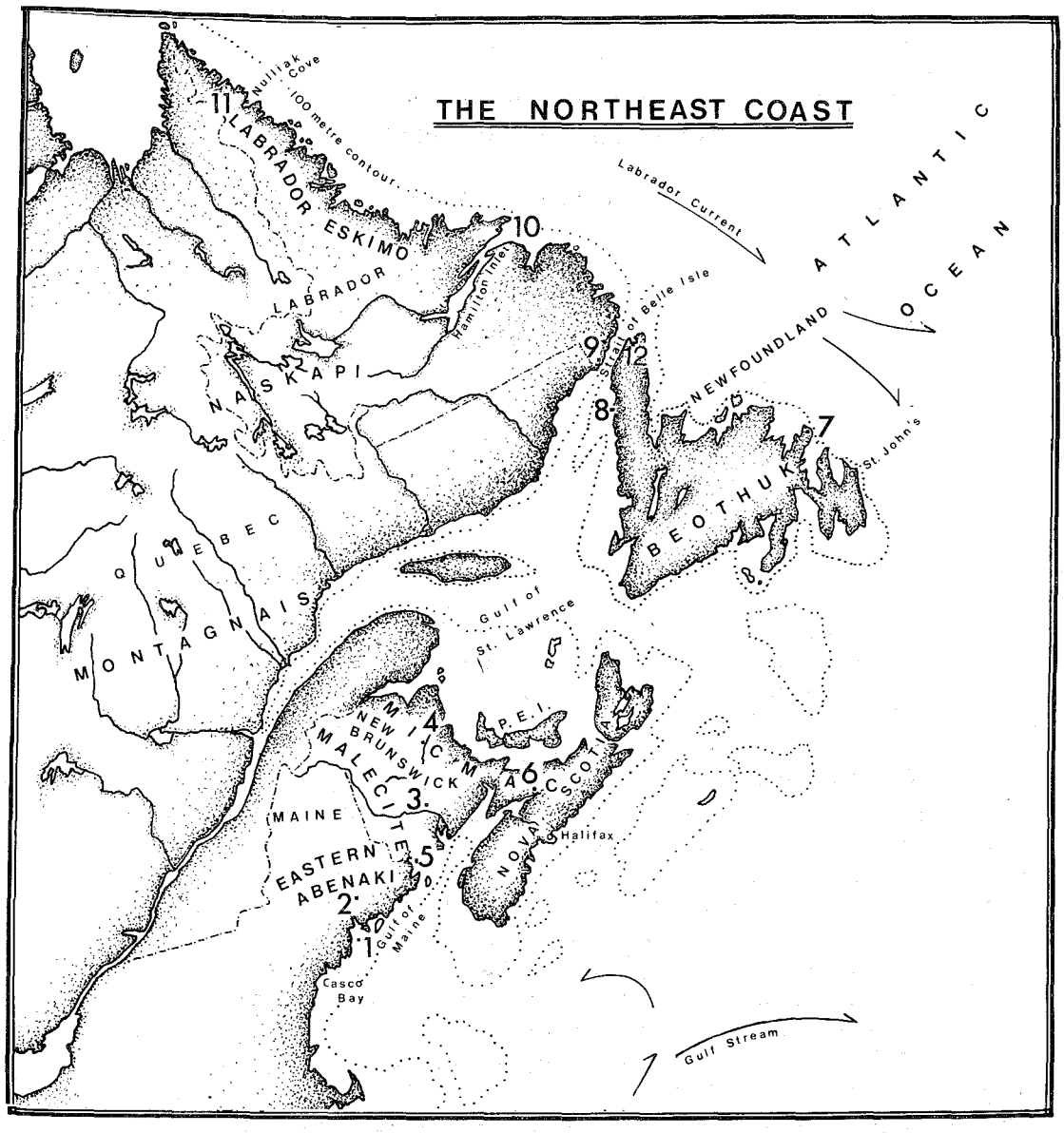
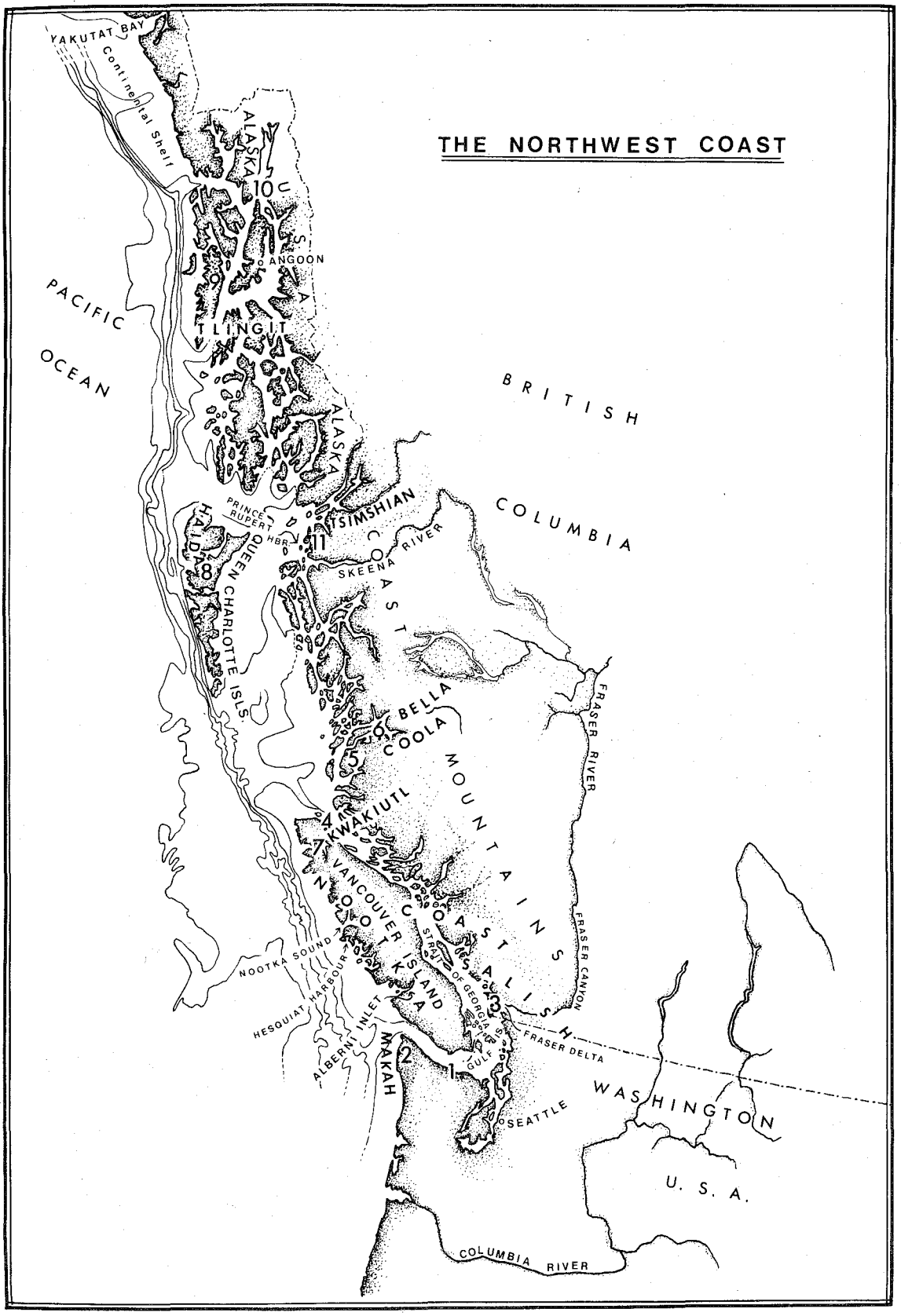


Figure 1. The Northeast Coast. Archaeological Sites and Localities: *1*, Turner Farm Site, Maine. *2*, Hirundo Site, Maine. *3*, Cow Point, New Brunswick. *4*, Augustine and Oxbow Sites, New Brunswick. *5*, Teacher's Cove Site, New Brunswick. *6*, Debert Site, Nova Scotia. *7*, Cape Freels Site, Newfoundland. *8*, Port au Choix Site, Newfoundland. *9*, L'Anse-Amour Site, Labrador. *10*, Rattler's Bight Site, Labrador. *11*, Saglek Bay Locality, Labrador. *12*, L'Anse aux Meadows Site, Newfoundland.

Figure 2. The Northwest Coast. Archaeological Sites and Localities: *1*, Manis Site, Washington. *2*, Ozette Site, Washington. *3*, Glenrose Cannery Site, British Columbia. *4*, Bear Cove Site, British Columbia. *5*, Namu Site, British Columbia. *6*, Axeti Site, British Columbia. *7*, Quatsino Site, British Columbia. *8*, Lawn Point Site, British Columbia. *9*, Hidden Falls Site, Alaska. *10*, Groundhog Bay Site, Alaska. *11*, Boardwalk Site, British Columbia.


THE NORTHWEST COAST
YAKUTAT BAY
Continental Shelf
ALASKA
10 C
ANGOON
TLINGIT
PACIFIC
OCEAN
BRITISH
COLUMBIA
ALASKA
PRINCE RUPERT
HBR
TSIMSHIAN
11
HAIDA
8
QUEEN CHARLOTTE ISLS.
SKEENA RIVER
COAST
BELLA COOLA
5
MOUNTAINS
FRASER RIVER
4 KWAKIUTL
7
VANCOUVER ISLAND
NOOTKA
COAST
STRAIT OF GEORGIA
SALISH
3
FRASER CANYON
NOOTKA SOUND
HESQUIAT HARBOUR
ALBERNI INLET
MAKAH
2
1
GULF
FRASER DELTA
SEATTLE
WASHINGTON
U. S. A.
COLUMBIA RIVER

been critically evaluated (e.g. Meltzer 1979), but it is convenient as a classificatory device for the purposes here and is used in its narrow definition following Kuhn (1970:175) as "the concrete puzzle solutions which when employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science." The paradigmatic distinctions used in this paper are methodological rather than metaphysical (*as per* Meltzer) and the particular paradigms discussed below are among those identified by Clarke (1972:7) and Knudsen (1978:342). Many models crosscut paradigms, but tend to be weighted within only one. The dimension of applicability to coastal cultures addresses the problem of whether coastal cultures are simply a subset of hunter-gatherer cultures, or so distinct as to warrant a separate line of model development up to the level of middle-range theory. Turner states the issue as follows: "However, until it is demonstrated that maritime biomes possess a class of unique traits (attributes) or that maritime hunter-gatherers operate within a unique class of norms, a theory of adaptation cannot be devised solely for maritime biomes" (1980:743). Finally, the degree of generality refers to low, middle-range and general theory. Goodyear *et al.* (1978:161) have recently defined middle-range theory as "... those constructs with assumptions and propositions whose implications can be examined *empirically*, but which are sufficiently *general* to be incorporated into ever broader generalization."

THE CULTURAL-HISTORICAL PARADIGM

The traditional goal of archaeology has been writing culture history. This exercise involves excavations at stratified sites and the qualitative use of stylistic modes to construct a classification of normatively defined cultures in space and time. Migrations and diffusion were the usual agents of culture change. It is important to note that studies of historical processes and a search for laws have long been of interest to historians, but in practice, the emphasis has been on the chronology and description of particular, non-recurrent situations. These concerns were paramount until 1948, when the publication of W.W. Taylor's "A Study of Archaeology" laid the basis for subsequent widespread acceptance of cultural reconstruction as an additional goal of archaeology. It has been observed that many aspects of Taylor's "conjunctive approach" were being quietly carried out well before 1948 and in this respect, the work by Smith and Wintemberg (1929) on the shellheaps of Nova Scotia is precocious in its reconstruction of the lifeways of the late prehistoric Micmac. An interest in cultural reconstruction continues today, particularly in the ecological paradigm but cultural-historical work, although not complete in every sense, is

currently more of a spin-off product from investigations centered in other paradigms. This situation is a consequence of the conclusion reached by some archaeologists in the 1960s that cultural-historical studies had reached a point of diminishing returns, that they were an endless jigsaw puzzle which generated no theory. The result of this malaise was the well-documented paradigmatic shift towards a multivariate, systemic view of Culture and a new goal -- the study of culture process (Binford 1968a).

Models of Northwest Coast Prehistory

Although subject to frequent revision, there are presently a number of regional sequences covering the prehistory for the Late Period (the last 5000 years) of the south, central and north portions of the Northwest Coast (e.g. Carlson 1970a). On the South Coast, the exemplary work in Kuhn's sense was done by Borden for the Fraser Canyon (1968) and the Fraser Delta (e.g. Borden 1970) regions. In his earlier speculations (e.g. 1954a), Borden relied upon external influences, diffusion and migration, as the agents of change. Present models have most or all of the major linguistic groups on the North and Central Coasts in place during the last 4000-5000 years. The debate as to continuity in the Late Period centers on the origin of the Marpole Culture on the South Coast. One interpretation stresses continuity and *in situ* evolution from the Locarno Beach culture (Mitchell 1971) and perhaps St. Mungo, through Marpole to the historic Coast Salish people; while the discontinuity model (Borden 1970, Burley 1979b) postulates a break in the prehistory owing to the movement of Marpole peoples from the middle Fraser River locality to the coast about 400 B.C. The seesaw battle of discontinuity vs. continuity may be resolved in the process of conducting work in other paradigms, such as the ecological paradigm where the "discontinuity" might be understood as more of an adaptive change (c.f. Thompson 1978a).

On the Central Coast, archaeological work at Namu during the late 1960s and early 1970s involved historical and ecological research goals (Hester and Nelson 1978:6). These investigations produced a key sequence of historical periods spanning 9000 years, and an inductively derived model of accretion and synthesis to explain the cultural development. The predicative capacity of the model is in question however, since it is unclear why the many traits with origins outside the Northwest Coast should have been accepted and synthesized as they were.

The project of largest scale on the North Coast is the North Coast Prehistory Project initiated by George MacDonald in 1966 "...

in the belief that there must be sites with sufficient time depth to detail the development of the elaborate and highly integrated cultural pattern known historically" (MacDonald and Inglis 1981:37). Ecological studies were soon added to these historical and ethnographic goals. The historical data spanning 5000 years are interpreted in terms of a three-period model of cultural continuity. The archaeological sequence "... is seen as a series of developing technological traditions which have an accumulative effect through time. New elements are appended to a basic pattern, but do not significantly alter it" (MacDonald and Inglis 1981:42). Like the accretion and synthesis model inspired by the Namu excavations, this model is inductively derived and a continuity model, but is smaller in scope (regional vs. areal) and lacks the emphasis which Hester and Nelson place on diffusion. Diffusion in Prince Rupert Harbour is modeled by means of the area co-tradition concept (MacDonald 1969:244).

The Early Period, prior to 3000 B.C., has been the subject of recent studies (*Canadian Journal of Archaeology, Vol.* 3, 1979), but there are still only a few pertinent sites. The three most explicit models are those proposed by Fladmark, Carlson and Borden and which address somewhat different historical problems. Fladmark (1979) tackles the large problem of the entry of people to North America during the late Wisconsin glacial period and puts forward a coastal migration model with maritime adapted populations moving south *via* a chain of sea-level refugia. Deductive testing of the model will be difficult because much of the archaeological evidence has been submerged by rising post-glacial sea-levels. It is a promising model nonetheless, and notable in its direct applicability to *maritime* prehistory. Carlson's model (1979a:224) has a much wider field, covering both the coast and the interior, and is a descriptive model of the cultural interaction and the basal traditions during the Early Period. It is a continuity model, but Hester (1979:231) has questioned the extension of these traditions through to the historic linguistic groups. Charles Borden's concerns are with the historical origins of the Northwest Coast Culture Pattern and in his later formulations (1975, 1979) he presents an economic merging model whereby the economic foundations for this pattern arose as a historical merger between two contrasting subsistence strategies (and technologies) having origins on different parts of the coast. Without denying the real differences in the early postglacial economics and technologies on the North and South Coasts, it is not certain that all of the early North Coast sites reflect the same kind of adaptation, and as Catherine Carlson notes (1979:192), technology and subsistance patterns need not coincide at least on the Central Coast.

Models of Northeast Coast Prehistory

There has been less archaeology done on the Northeast Coast and the culture history is correspondingly more piecemeal, especially in the Maritimes. There remains considerable potential for basic cultural-historical work, not only as an end in itself, but as a prerequisite for some aspects of model-building in the evolutionary paradigm. More remarkable limitations on the data arise from continuing coastal submergence in the Maine-Maritimes region which has left most of the former coastal Paleo-Indian and Archaic occupations now under water and on the continental shelf (Simonsen 1979). Models of the early prehistory will thus be deductive and difficult to test. Computer simulation models seem most suitable or models extrapolated from the rising coast of Labrador. Summaries of the culture history of the Northeast can be found in the Handbook (Trigger 1978b), and Snow (1980) has synthesized the prehistory of New England. More general discussions include monographs by Sanger (1979g) for the Maine-Maritimes region and Tuck (1976a) for Labrador/Newfoundland.

There are two principal and related historical problems facing archaeologists working on the Northeast Coast, particularly in the Maine-Maritimes region. The first of these is the paucity of sites in the 10,000-5000 B.P. time range and the second is the issue of cultural continuity. Both Sanger (1979g:23-24) and Snow (1980:168) have evaluated the models presently competing to explain the scarcity of Early and Middle Archaic sites in Maine (a situation also covering the Maritimes). Of the four hypotheses considered, Sanger argues that his River Gradient Hypothesis offers the best fit while conceding that all four hypotheses may be valid to some degree. Snow (1980:158) is of the opinion that mathematical catastrophe models might explain the apparent depopulation in Early Archaic times.

The second issue is a continuity vs. replacement debate analogous to that existing in the culture history of southern British Columbia. In the Northeastern case, disagreement centers on several time intervals: 1) the late Paleo-Indian -- Early Archaic (ca. 7500 B.C.); 2) the Middle/Late Archaic (ca. 3500 B.C.); and 3) the Late/Terminal Archaic (ca. 1500 B.C.) and is focussed on the Maine-Maritimes regions. The continuity model has been presented by Tuck (1975, a,b,c) and postulates *in situ* cultural development from Paleo-Indian to historic times. The model derives from geochronological, subsistence and typological data in southern Labrador, but depends elsewhere upon accepting the notion of a Maritime Archaic Tradition. The model stresses the maritime nature of even Paleo-Indian adaptations. The model may not cover the

origins of the Maritime's peoples (Tuck 1978:34) or the Naskapi.

Cultural discontinuity or population replacement models have been advocated by Sanger (1975, 1979g) in an effort to explain certain episodes of accelerated culture change south of the Gulf of St. Lawrence. Such models are said to be less parsimonious, but better able to accommodate environmental change. With regard to the few Early and Middle Archaic components in Maine, Sanger argues that there is no case for continuity with Paleo-Indian cultures. Similarly, the Late Archaic cultures with their elaborate mortuary cults are not considered expressions of the Maritime Archaic Tradition but rather as related to the Vergennes phase of the Laurentian Tradition which has spread into Maine by 5000 B.P. (Sanger 1973:128-130; 1979g:42, 71). The decline of these Late Archaic cultures is attributed by Sanger (1975) and Dincauze (1975) to a population replacement in concert with significant changes in the ecology, the new arrivals being identified as the Susquehanna Tradition. Cook (1976) argues that there was no such migration.

Before leaving the continuity issue, it should be noted that on the Labrador-Newfoundland coasts (but not further south), there have been several population replacements involving the appearance of Independence I, Dorset, and Thule Eskimo populations. The reality of such discontinuities is accepted owing to the absence of classification difficulties in dealing with these Eskimo cultures, and models are concerned with the nature of the contact between Indian and Eskimo groups (Tuck 1976a; Fitzhugh 1972).

These culture changes of northern origin did not directly impact south of the Gulf of St. Lawrence. On the other hand, several of the major trait complexes in Maine and the Maritimes -- agriculture (which reached the Abenaki of southern Maine), shellfish collecting and ceramics are considered to have southern origins (Snow 1978), but there are few diffusion models to deal with these important changes.

Problems and Anomalies

Cultural-historical studies are alive and well, but in spite of much progress, there remain problems in classification which delay resolution of continuity questions for example. In the Northeast, descriptive units tend to be traditions using both technological and/or adaptive criteria, but there are persistent difficulties in sorting out the overlapping definitions of the Archaic traditions. Further progress may depend on continued assessment of existing maritime units (such as the Maritime Archaic) as well as the use of

cross-cultural units such as adaptive type (Fitzhugh 1975b) and defined so as to deal with historical problems in coastal area. Archaeologists on the Northwest Coast are encountering difficulty with the phase concept (Abbott 1972), but one path through such difficulties lies in the use the techniques of numerical taxonomy (Matson 1974). It may be, however, that the continuity problems constitute persistent anomalies in Kuhn's sense and that their solution will come about in the course of work within other paradigms. As suggested above, discontinuities might represent adaptive changes explicable with ecological models, or the discontinuities might represent episodes of evolutionary change explicable with punctuated-equilibria models. Ecological, evolutionary or demographic models might also eventually explain the apparent lack of sites dating prior to 5000 years ago. The persistence of such problems may help to explain why cultural-historical work has been superseded on these coasts as elsewhere, by work centered in other paradigms, but a more fundamental factor has probably been the continued absence of generalizations about coastal prehistory.

THE ETHNOGRAPHIC PARADIGM

Research conducted in this venerable paradigm typically involves drawing analogies between ethnographic and archaeological cultures, occasionally in the context of ethno-archaeology, with the aim of producing fuller cultural reconstructions. Recent work has also employed ethnographic data in a search for general adaptive principles by means of which hunters-gatherers cope with environmental uncertainty. Emphasis has been on distilling out "pure" economic systems or supplementing archaeological models and it is significant that perhaps because of the materialist bias, archaeologists have made little use of ethnography to determine the influence of goals, satisfaction levels, value systems and preferences.

Ethnographic models are effective for local and regional cultural reconstructions, but they may often be describing only refugee populations (Whitlam 1980a:13) and in any case, their predictive capacity drops off as the model is extended back beyond the protohistoric period. Moreover, as Bettinger points out (1980:205), while it is possible to make some descriptive statements about hunters-gatherers, it is difficult to make inductive or deductive generalizations owing to the particularistic nature of ethnographic studies.

The ethnographic paradigm has been and continues to be of

greater importance in Northwest Coast archaeology owing to: 1) the fact that sustained Euorpean contact was some two centuries later than on the Northeast Coast; and 2) the relative complexity of the historic cultures on the Northwest Coast produced a natural interest in their origin and development. While there is no doubt that depopulation and acculturation radically transformed traditional cultures on the west coast (Burley 1979a), there was nothing equivalent to the genocide of the Beothuks, nor did the western fur trade lead to a total collapse of the subsistence-settlement system. This was not the case in the east where some historic subsistence-settlement systems may well be a mirror image of the late prehistoric systems (Sanger 1979g:12) and only such band-level peoples as the Naskapi retain any potential for ethnographic work (e.g. Fitzhugh 1972:180).

Thus, the availability of ethnographic data on the Northwest Coast could be coupled with historic research on the time depth of the ethnographic pattern. The ethnohistocial work of de Laguna *et al.* (1964) represents the tightest integration of these two paradigms, but ethnographic work has been linked to most historical work, either at the problem formulation stage (MacDonald and Inglis 1981) or in syntheses involving cultural reconstructions (Mitchell 1971). Ethnographic research has also been effectively linked with ecological research, although usually in an adjunct role, and it has been especially important in the Hesquiat Project (Haggarty and Boehm n.d.) and the Ozette Archaeological Project.

On the east coast, ethnography has offered limited analogies (MacDonald 1968:129, Tuck 1976b) or, as in Hoffman's work (1955), ethnographic records were used to develop a comprehensive model of precontact Micmac society. Continuing archaeological research has questioned the accuracy of this latter model (Nash 1980b) and by implication, the utility of ethnographic-based research.

Archaeologists on the east coast have never operated within an ethnographic paradigm, but have used ethnographic data in a selective manner for cultural reconstructions. We are unlikely to formulate accurate models of societies on the east coast during the 16th century by relying on historical records. Baseline reconstructions will likely emerge as a result of work in other paradigms. This seems also to be the case on the Northwest Coast where continuing archaeological work together with re-evaluation of the classic ethnographic pattern is leading to a revised conception of late prehistoric society (Burley 1979a). Accurate reconstructions are obviously necessary for any discussion as to the comparative complexities of these cultures at the time of contact. The Micmac and their neighbors to the south were ranked societies

like those of the west coast, but at least some of the ethnic groups on the West Coast may have been chiefdoms (Service 1978:221-240; Price 1979:187) and therefore socially and politically more complex. Miller and Mitchell (this volume) assess this issue for the Micmac and Coast Tsimshian respectively. *If* there is a significant difference in cultural complexity, we are faced with the problem of explaining why this should have occurred, especially since these cultures are situated on oceans of approximately equal biological productivity (Gross 1977:335). The ethnographic paradigm appears increasingly unsuitable for reconstructing proto-historic cultural systems, dealing with questions of comparative cultural complexity or generating any theory and will probably shift from its position as a minor paradigm on the west coast to a cluster of models having very restricted application.

RECONSTRUCTING SOCIO-POLITICAL ORGANIZATION

One of the objectives of the "new" archaeology was the reconstruction of prehistoric socio-political organization, not by means of ethnographic analogies, but by recognizing the variability in the archaeological record and translating this variability into models of social behavior and social organization. The exemplary studies were done in the Southwest (Longacre 1968, Hill 1968) and while such efforts have had mixed success (Dumond 1977), identifying social units, residence patterns and community relations remains an area of contemporary research (Redman *et al.* 1978).

There has been relatively little such research on the Northeast and Northwest Coasts. Architectural studies and house floor analyses at Ozette (Mauger and Daugherty 1980) will provide unusually accurate descriptions of Makah community organization and will permit evaluation of ethnographic models which will have some validity in this late context. In most other cases, such as shell middens, it has been difficult to isolate discrete occupations and interest on the West Coast has centered on the identification of social ranking and stratification. Donald and Mitchell (1975) have discussed the relationship of ranking and salmon resources, and Ames (1981, this volume) has provided models for the evolution of ranked societies. Ranking appears 2500-3000 years ago on the Northwest Coasts (Ames 1981:797), while on the Northeast Coast, there are at least status differences between men and women by the Late Archaic Period (Tuck 1976b:89).

The principal issue confronting comparative archaeologists is the possibility that the Late Archaic cultures of the Northeast Coast are more complex than their counterparts (St. Mungo, Mayne and

Locarno Beach phases) on the Northwest Coast. This is the reverse situation to that discussed for the time of European contact. There are adaptive and technological parallels with the west coast cultures (Fitzhugh 1975b), but the east coast cultures may be socially and ideologically more complex. The Late Archaic is regarded as a Period of cultural floresence with its widespread exchange systems and energy-expensive mortuary cult. Snow (1980:211) suggests that in New England, the bands may have been larger and more sedentary than previously. Tuck (1976b:84) has offered a reconstruction of the social organization and other aspects of culture among the Maritime Archaic peoples at Port au Choix. If we grant considerable isomorphism between social and ideological complexity, then the elaborate and widespread Maritime Archaic burial cult probably indicates more than simply status differences between men and women. A ranked society is implied and at a comparatively early date. It should also be noted that the burial mounds of Labrador are the world's earliest (McGhee 1976).

At present, there is little progress in developing models of prehistoric social organization, on the coasts or elsewhere. The paradigm itself is promising, but it lacks middle range theory and operational models which tell us what social correlates can be expected given certain kinds of variation in the material culture. This is the domain of behavioral archaeology (Schiffer 1976) and it will be difficult to specify community organization, ranking or degree of cultural complexity until the paradigm is developed beyond an embryonic stage. When this has been accomplished, we can attempt to define the relationships between social organization and coastal environments. In the meantime, I offer the proposition that on average, maritime hunters-gatherers have a more complex social, political and ideological structure than neighboring interior hunters-gatherers.

THE ECOLOGICAL PARADIGM

Ecologically structured research is concerned with the ways in which cultures are adapted to their natural environments with the result that the archaeological work is focussed on faunal and floral remains more so than artifacts. Ecological studies tend to be synchronic and oriented towards reconstructing subsistence systems and the paleoenvironments in which they functioned. Such reconstructions of the resource base, procurement strategies, schedules etc. require consideration of settlement systems -- an area of research sometimes identified within a "geographical" paradigm (Clarke 1972:7) and characterized by locational models borrowed from geography and geomorphology. Because these studies

are so frequently linked as subsistence-settlement models and causal priority given to subsistence pursuits (e.g. Jochim 1976:13), subsistence-settlement models will be reviewed under the ecological umbrella.

The ecological paradigm is commensurate with several other paradigms and disciplines. As noted above, there is a logical link to settlement studies and geography; connections with culture history and ethnography were discussed earlier and the concept of adaptation forms a bridge to the evolutionary paradigm. Population ecology is shared with the demographic paradigm and ecology underlies the conservationist ethic of cultural resource management studies.

The ecological paradigm is the dominant paradigm in contemporary American archaeology, a situation which resulted from the mushrooming ecology movement which began in the 1960s and the recognition among archaeologists that within the incomplete nature of the archaeological record, floral and faunal remains and site locations were hard data readily convertible to ecological models. Such epistemology underlies the statement by Hester with regard to west coast shell middens that "... the primary data preserved in the midden is ecological in nature and therefore techniques of collection and analysis of these data should be stressed" (1978:6). Some of the inherent limitations of the ecological paradigm have been pointed out (Vayda and Rapport 1968, Trigger 1978a and Nash 1980), but its use is pervasive.

Faunal and floral analysis are standard parts of the research design whenever excavations are contemplated either at a single site (e.g. Matson *et al.* 1976, Bourque 1975) or in the course of regional projects (Bonnichsen and Sanger 1977; MacDonald and Inglis 1981). The ecological information is commonly directed towards constructing subsistence-settlement models for a region (e.g. Fitzhugh 1972, Thompson 1978a) or a more precariously, for a time period (Snow 1980) or a tradition (Tuck 1975a). General cultural-ecological descriptions often employ the focal-diffuse distinctions for subsistence patterns (Cleland 1976) and the classification by Beardsley *et al.* (1956) to describe mobility. At present, such models are in the initial stage of development and most explicit for Hamilton Inlet, Labrador (Fitzhugh 1972), Hesquiat Harbour on the Central part of the North west Coast (Haggarty and Boehm n.d.) and the southern Gulf of Georgia (Thompson 1978a). Such work needs to continue, but given the tremendous cost, considerable thought needs to be given as to where the point of diminishing returns lies with respect to building theory.

Modeling subsistence-settlement systems seems only routinely troublesome until the resource list comes to shellfish, for what explanations of the sacred cow complex are to anthropology, the exploitation of shellfish is to coastal archaeology. The shellfish issue exposes the deep divisions between the Cultural (ideational, emic, mentalistic) Paradigm and the Ecological Paradigm and to some extent, the effects of the separation between archaeology and cultural anthropology. The humble horseshoe clam has become one focus of an entertaining struggle between cultural materialism represented by Marvin Harris and the Cultural (structuralist) perspective argued by Claude Levi-Strauss (see Harris 1979:202), while archaeologists continue to battle from their trenches.

On the Northwest Coast, Borden would seem to argue from a Cultural perspective in claiming that many early populations ignored or neglected inter-tidal food resources such as clams and mussels (1975:113). The ecological position is represented by Fladmark (1975) who argues that intensive shellfish collecting is the logical outgrowth of the decreased mobility which accompanied stabilization of the eco-system. In the Northeast, Nash (1980b:21-22) has argued that the distribution of shellheaps in eastern Nova Scotia cannot be modeled satisfactorily solely within the ecological paradigm, and Snow (1972) has emphasized the technological variable in shellfish collecting and the desirability of avoiding deterministic ecology (1980:179). Braun (1974), Brennan (1976) and others have argued for interpretations in Ecological rather than Cultural terms.

As Bettinger notes (1980:211), hunters-gatherers are usually selective whether under conditions of scarcity or plenty. *Culturally*-ordered selectivity of resource use is an issue that requires increased recognition and investigation. Shellfish exploitation may be the proximate issue, but the larger issues are of people-nature relationships and human rationality. The only recent model to include Cultural goals within subsistence strategies is Jochim's (1976) model of hunters-gatherers which focuses on the need for choices and decisions in resource use scheduling. His two major subsistence goals (secure income and population aggregation) are not exceptional, but he includes four secondary desires or preferences which are much more independent in expression. Jochim's model may constitute middle-range theory; certainly it is comprehensive, but it has yet to be tested on coastal cultures. Nonetheless, such decision models, which derive from the economics and management fields, offer a welcome bridge between the Cultural and Ecological Paradigms.

Progress in developing theory is most evident in the ecological paradigm and there are at present, three related models which

provide some propositions about maritime hunters-gatherers. These models are: 1) a Systemic Integration model (Fladmark 1975); 2) a definitional model (Yesner 1980); and 3) an optimum diet model (Perlman 1980). Sanger (1979e) is also developing an adaptation model for the Gulf of Maine. The middle-range generality of these three models and the incipient nature of such theory suggests they warrant some examination.

Fladmark's Systemic Integration model was developed to explain the accelerated culture change and intensification that began about 3000 B.C. on the Northwest Coast. It is more parsimonious than the other models in that the key variable is coastal stabilization, for until stabilization occurs, biotic populations such as anandromous fish cannot reach climax productivity, nor can the higher consumer societies which depend on the fish. The model has stimulated considerable thought and debate (e.g. Sanger 1979f:851; Borden 1975:113) and in this volume, Fladmark extends its application to include the Northeast Coast. Sea-level change and fish populations are also central to Sanger's (1979g:30) River Gradient Model for the Gulf of Maine.

Yesner's model is a more elaborate attempt to define the common features of maritime adapted populations. it is an inductively derived model consisting of statements about resource availability, settlement patterns, technology and demography. As Yesner concedes, (1980:733,745), the subsistence related features are less controversial than the demographic ones. It is a normative descriptive model and suffers from the fact that the relationships among the features are not specified. A next step could involve checking the accuracy of this model on the Northeast and Northwest Coasts. The model will be subject to rapid revision, but will remain of historic interest in initiating attempts to recognize maritime hunters-gatherers as a special class of society requiring special theoretical treatment.

Perlman also considers coastal populations "atypical" with respect to other hunters-gatherers and like Yesner, he characterizes coastal environments as biologically productive -- in contrast to Osborn's (1977) earlier assessment. Perlman's principal model is a lest effort -- least risk model, one of a series of optimal foraging models developed in biology. He applies it in an informal, deductive fashion to the archeological record of the United States east coast. One such "expectation" derived from the model concerns the early initial use of sea resources and the necessity of coastal stabilization for intensified use of these resources. This is compatible with Fladmark's model and Yesner's summary of maritime prehistory (1980a:733-734). Other general conclusions concern the

role of shellfish, the probability of storage techniques, the sedantism of coastal cultures (also one of Yesner's ten features) and the probaility of non-band organization. The difficulties in this application of the model are that : 1) the "expectations" said to derive from the model appear reasonable, but are not rigorously derived; and 2) while the data do not invalidate these expectations, this is due to mininal or negative evidence in three cases (shellfish, storage, social organization) rather than positive correlations. The complexities of shellfish exploitation and social organization have been discussed earlier.

It is satisfying to see the emergance of some theory in ecological archaeology, but there are some problems besides the limitations inherent in the paradigm. References for critiques of the paradigm have been noted earlier; the comments here are concerned with the application of ecological models in a coastal context and the direction and destiny of continued ecological reconstructions.

Ecologically-minded archaeologists have often drawn their models from general biology and applied them directly to archaeological contexts. There are problems with this procedure. First, there is little consideration given to the commensurability problem. Vayda and Rapport (1968) noted the general lack of correspondence with general plant and animal ecology that occurs when cultures rather than human populations become the units of adaptive study. If human populations are used, can Culture be factored out? It would seem preferable for archaeologists to develop their own theory of cultural ecology as Steward (1955) originally proposed. Concepts rather than whole models can be borrowed from biology and, in the case of coastal archaeologists, from marine biology and oceanography.

A second point on borrowed models is that there seems to be insufficient borrowing from oceanography. Oceanographic data has been most prominent in debates over transoceanic contacts. The problems are historical, centering on whether similarities in traits found on opposite sides of the ocean are the result of independent invention or diffusion. The mechanics of contact have hinged upon the circulation patterns of the ocean's currents. One such well-argued debate concerns the possibility of early culture contact between Japan and Ecuador *via* a North Pacific route (Meggers and Evans 1966, McEwan and Dickson 1978). Oceanographic data have also been of concern in modeling the migrations and initial settlement of the islands in Micronesia and Polynesia.

Productivity estimates for marine ecosystems (e.g. Yesner

1980a), tidal action -- resources studies (Ham 1976:74) and study of circulation patterns (Mitchell 1971) are important to archaeologists, but we can probably also modify hydraulic simulation models of the type used by oceanographers in order to understand the effects of physical processes -- shoreline changes, the mixing of estuarine waters, the effects of tides and tidal currents -- on the lives of coast dwellers. Estuaries, which Robert Ingle called "the crossroads of evolution" (1954:65) could become foci for interdisciplinary work on sea/land transitions, rather than studied simply as microenvironments with a higher concentration of food resources and sites.

A cautionary note seems in order with regard to ecological studies. A principal pitfall for culture historians has been the temptation to be content with "mopping-up" operations involving the filling in of gaps in history's endless jigsaw puzzle. A similar dead-end is possible for ecological investigations which are, like ethnography and the modeling of social organization, largely directed towards synchronic cultural reconstructions. In ecological archaeology, the pitfall lies in producing reconstruction after reconstruction, each one ever more detailed as analysis progresses from macro to micro to a grain of sand in concert with a swollen army of interdisciplinary specialists. In some cases, the structure collapses under its own weight after the principal investigator dies or becomes bored, leaving only a residue of preliminary reports. Finally, it should be recognized that the current emphasis on ecological studies has diverted attention from evolutionary and processual studies which are of greater scientific significance.

THE DEMOGRAPHIC PARADIGM

In his recent review, Hassan terms demographic archaeology "... an analytical and interpretive approach within archaeology" (1978:49). It can also be identified as a paradigm concerned with size, density, growth and other population parameters in archaeological contexts. It is closely linked with ecology (population ecology) and geography through settlement patterns studies and when population pressure creates change, there are evolutionary implications. In recent years there has been a proliferation of demographic models of culture change, although such models are curiously under-represented in the archaeology of the Northeast and Northwest Coasts.

Demographic data has traditionally been used as an adjunct to cultural-historical work or cultural reconstructions. Early investigations sometimes used skeletal data to bolster arguements

about migrations and origins (e.g. Hill-Tout 1930), but more often, osteological information from burials and cemetaries has been analyzed to provide information on physical characteristics, age and sex profiles, mortality, paleopathology and population distances as well as for knowledge concerning mortuary customs and social organization (e.g. Anderson 1976, Cybulski 1978).

There have been relatively few estimates of population size or density for individual sites, regions or areas. California archaeologists have long used quantitative studies of shell middens to make population estimates (Cook 1946), but this remains to be done on the Northwest and Northeast Coasts. Where estimates have been made, they are likely to be based on ethnography and are conservative given the depopulation which followed European contact. As things stand, we cannot for example, rule out the presence of Micmac "towns" during the Woodland Period in the Maritimes. On a regional scale, Miller (1976, 1980a) has used ethnohistorical records and depopulation ratios to arrive at an estimate of 26,000 Micmacs at the time of contact -- a figure far higher than traditional estimates and a figure which has dramatically different implications as to cultural complexity. Present estimates for the various groups on the British Columbia Coast (Duff 1964) extend back only to 1835. These numbers are probably also conservative and need to be extrapolated back towards the mid 1700s. Relatively high population densities is one of the features which Yesner (1980a) identifies as characteristic of coastal populations -- a generally acceptable proposition.

Beyond the difficulties in obtaining reliable estimates of population size and density, there is the topic of population growth which is presently the centre of a debate as to its operation with respect to cultural change. The issue is whether population growth is inherent and thus an independent variable (e.g. Cohen 1975) useful for explaining adaptive change, or whether population growth is a dependent variable as seems to be the case in Yesner's model (1980a) and Perlman's model (1980). The issue is an important one, although Bettinger (1980:228) suggests that no *a priori* assumption need be made and that the position taken depends on the nature of the problem.

There have been few attempts to use population growth as an independent variable or any sort agent of change. On the West coast, Ames (1979) has used the concept of optimum population size to help explain some aspects of the enculturation process among groups on the Skeena River. In a later paper (1981), population growth is viewed by Ames as one of the processes (but not a prime mover) responsible for the formation of ranked societies. Snow has

outlined an inductively derived comprehensive growth model for New England prehistory. It is not intended to cover other regions, nor is it specifically a coastal model. Very simply, this model depicts long-term population not as one or several lines on a graph, each line with a gradual slope, but rather as a wavy line reflecting "... episodes of rapid growth and sharp decline" (1980:256). In such a wave model, population growth can be either an independent or dependent variable at different times.

There is little demographic work to review and the most obvious conclusion to be drawn is that there is great potential for the use of demographic models in coastal archaeology. In both academic and cultural resource management studies, it is common practice to employ regional sampling methodologies. Such methodologies are very conducive to producing population data, so that demographic problems can easily be accommodated in many research designs. Determination of growth rates and trajectories is an obvious need, and in particular, it would be interesting to determine whether the presence of the Coast Range compressed living space so as to produce logistic growth curves for populations on the West Coast.

THE EVOLUTIONARY PARADIGM

Evolution has recently been defined as systemic organizational change (Gall and Saxe 1977:256) and in a somewhat different vein, as "... a particular framework for explaining change as differential persistence of variability" (Dunnell 1980:38). The evolutionary paradigm shares the idea of adaptation with ecology, but evolutionary studies are diachronic, not synchronic. Dunnell (1980) has reviewed evolutionary studies in archaeology finding them to be largely transformational and progressive in philosophy and yielding merely historical generalizations about the results of change. Stages are a typical classificatory product. This type of cultural evolution (which also typifies schemes in cultural anthropology) lacks any selective mechanism and is unrelated to Darwinian evolution. Although more recent processual archaeology is compatible with scientific evolutionary biology, there have been few evolutionary models of any kind and little advance in developing archaeological evolutionary theory (Meltzer 1979:654, Dunnell 1980:82). This situation holds true on the Northeast and Northwest Coasts as well and is ironic in view of the diachronic strength of archaeology. In the two areas of interest here, a basic question remains to be answered prior to model-building. Has there been any macro-evolutionary change? By this I mean major systemic organizational change (*sensu* Gall and Saxe) or revolutionary changes (Service 1971:13) as distinct from growth, social change or

development which are incremental in nature. The issue is pursued below.

The nineteenth century unilinear model of cultural evolution appears briefly in Hill-Tout's 1895 paper "Later Prehistoric Man in British Columbia" (Maud 1978:37), but I am unaware of other evolutionary advances until the use of developmental stages (e.g. Paleo-Indian, Archaic) became common in Northeastern archaeology (e.g. Willey and Phillips 1958). In the 1970s there were a few excellent contributions concerned with the evolution of coastal societies. Fitzhugh (1972:191-194) advanced five propositions said to characterize coastal cultural dynamics. These propositions, relating culture change to the resource base and to climatic changes, were inductively derived from Labrador data, but can be tested on the Northwest Coast. To some extent this has been considered, for in 1975 Fitzhugh suggested (1975b:374-375) that the intensified maritime adaptations and technological changes that began about 6000 to 5000 years B.P. among circumpolar cultures might be explicable as a response to the ecological changes accompanying the onset of the Atlantic (hypsithermal) climatic episode. Somewhat earlier, Mitchell (1971:71) proposed a similar climatic model for these changes in southern British Columbia. A final evolutionary model, which has the virtue of distinguishing constraints and processes, is Ames' model for the evolution of social ranking on the Northwest Coast. A basic hypothesis is that "... ranking evolved on the coast through the constraining of a resiliant system" (1981:798). This model would probably not apply to the east coast societies since one of the constraints, environmental circumscription, was likely to have been operative at a much lower level.

The tortise-like progress of evolutionary studies seems partially attributable to the issue of commensurability between cultural and biological evolution. Dunnell (1980) has outlined the historical cleavage between these two and the confusion that often exists when biological concepts have been borrowed by archaeologists. Similar problems were noted earlier with respect to ecological models in archaeology. Dunnell stresses the need for a new kind of evolutionary theory, but one similar to that of evolutionary biology. Yoffee (1979) on the other hand, attributes the lack of progress to over-reliance on the mechanisms of biological evolution, which he argues, do not involve the internal sources of change characteristic of culture change. With the exception of the Ames model, the models mentioned above rely upon external (environmental) causes for culture change.

Evolutionary biology is currently in upheaveal over the issue of gradual *vs.* punctuated equilibria and perhaps even the

possibility that acquired characteristics can occasionally be inherited. The first issue has already surfaced in cultural anthropology (Diener 1980), the second waits to be explored by sociobiologists. The punctuated-equilibria model is designed to explain rapid change on the premise that "... gradual environmental changes generally give use to *quantum* adjustments in system behavior when the response under consideration is complex ..." (Diener 1980:425). Such models might assist in explaining such rapid, macro-evolutionary changes as the floresence of the Late Archaic cultures in the east, the widespread, roughly synchronous appearance of shell midden sites on the west coast ca. 5000 B.P. and, on a smaller scale, the appearance of the Marpole Culture.

Evolutionary models, punctuated-equilibria types and others, will undoubtedly be borrowed from biology and applied with mixed success to problems such as those mentioned above. But, since cultural evolution *is* fundamentally different from biological evolution, it seems apparent that tracking the progress of evolutionary biology is likely to yield only half-truths.

THE CULTURAL PARADIGM

Cultural theories and models define socio-cultural phenomena from an emic perspective and ascribe causal priority to the mental conditions and processes operative at the level of superstructure (ideology etc.) and structure (domestic and political economy) rather than the infrastructure (modes of production and reproduction). This is in contrast to the etic perspective and the causal priority attributed to ecological, economic and demographic variables (the infrastructure) under a cultural materialist strategy (Harris 1979).

Cultural theories, often termed mentalistic or idealistic, have a long history in anthropology, beginning with the social philosophers of the 18th century and continuing through 19th century evolutionary anthropology where proximate causation might involve material conditions, but ultimate causation was attributed to the pre-eminence of the mind (see Harris 1968:212). The ideological bias continued even during the eclecticism of the Boasian period of historical particularism and it was basic to most culture and personality studies. British social anthropology gave social structure priority of analysis and despite the general eclecticism prevalent in anthropology today, Cultural theories are favoured within French structuralism and they are critical to dialectical materialism.

Mentalistic theories were never overtly popular in North American archaeology. Cultural-historical studies have been largely atheoretical, although presumably diffusion and perhaps migrations operated within mental rather than material constraints. Schwartz (1978) has proposed that psychic archaeology represents an emergent paradigm, but at the moment, its only coastal application has been in British Columbia and it represents only an emergent curiosity. Harris (1968, 1979) has written extended polemics against ideational anthropology and goes further in arguing that there is an "insuperable difficulty" preventing application of ethnosematic models to archaeology.

> The archaeologically recoverable portion of most of human history consist of the environmental modifications which different varieties and expressions of energy quanta have brought into being. Binary oppositions, contrastive features, skewing rules etc. have this in common: they have no measurable energy cost (1968:604).

Most archaeologists accept this position and increasingly pursue explanations within the ecological paradigm and follow cultural materialist theory in attributing causal priority to the infrastructure. Cultural variables, ethnosematic models and in fact much of cultural anthropology are all systematically excluded. Cultural models are models of last resort and regarded as untestable since we do not know how to measure such variables archaeologically (at the present time). Nonetheless, decision models (e.g. Jochim 1976) offer some potential for including Cultural variables. The dialectic model outlined below is also an attempt to develop a comprehensive model which is not wholly etic in definition and causation.

A Dialectical Model of Maritime Cultures

In dialectical materialism, the infrastructure also assumes causal priority, but there is a distinct epistemology in that the content of the infrastructure, as well as its mode of development, is dialectical in nature. Dialectics, which originated with Hegel, is an idealistic concept whereby interacting things and ideas are believed to be in a state of tension and opposition and that resolution of contradictions and historical development proceeds through a series of negations -- thesis, antithesis, synthesis. similar ideas are to be found in the Yin Yang school of early Chinese philosophy. The trouble with dialectical epistemology "... is the lack of operational instructions for identifying causally decisive 'negations'" (Harris 1979:145). I suggest, however, that a modified, less dogmatic version of the dialectical materialist model

will prove useful in describing and explaining some structural and evolutionary aspects of coastal cultures.

Infrastructure. On the face of it, maritime hunters-gatherers would seem to be qualitatively distinct from interior hunters-gatherers by virtue of being located at the interface of two major biomes, terrestrial and marine. Coastal peoples will exploit the rich and diverse resources of both ecozones to varying degrees with the subsistence pursuits being weighted towards one or other of the pair of ecozones. It seems reasonable, therefore, to say that the economic system is binary in nature. Ethnosemantic information relating to this point is generally unavailable except for the Nootka who "... broadly categorize their world on the West Coast as "kla'a" or "outside" in English, and "hilsitis" or "inside" (Dewhirst 1977:1). "The 'outside' is the "long" unprotected low-lying outer coastline," while the "... 'inside' is basically the setting of the inlets and river mouths that empty into them" (Dewhirst 1977:1,3). The "inside" is interior related.

Structure and Superstructure. Is the binary characterization of maritime infrastructure superfluous to our understanding of them? I think not, if for no other reason than this -- it would follow from the principle of infrastructural determinism (i.e. casual priority) that we might expect a greater dualism to characterize the social and political organization and the ideologies of maritime cultures. At the moment, this must remain a hypothesis, for there has been little investigation along these lines other than Duff's analysis of the symbolic logic underlying prehistoric stone sculpture on the Northwest Coast. This art "... uses inherent structural and conceptual dualisms in the artifacts and images: outside-inside, head-body, front-back, part-whole, and so on" (Duff 1975:14). It seems there *are* paths into the heads of prehistoric peoples and some potential for Cultural variables. Dialectical societies are likely world-wide, the best known cases being the Gê and Bororo peoples of central Brazil. They state quite explicitly that their societies are imbued with oppositions, because "opposition is immanent in the nature of things" (Maybury-Lewis 1979:13). It remains to be seen whether dialectical societies are more frequent along coastal zones.

Dialectical Evolution. Marxist anthropologists are most closely identified with dialectical evolution, but it has also been applied to general human evolution by Belasco who comments on the non-deterministic nature of dialectics and the uneveness of the process which results in an asymmetrical or mosaic pattern (1975:87,91). The model proposed here has a more restricted scope

and carries with it the hypothesis that the evolutionary trajectories of coastal cultures will have a greater than expected tendency to follow a dialectical path. That is, dialectical evolution is associated with the evolution of coastal cultures.

Theoretically, the dialectic will involve a long-run tendency towards shifts in the relative weights of subsistence pursuits -- from maritime emphasis to relative equality to interior emphasis and back again -- an oscillating economic mix. The shifts are likely to be precipitated by infrastructural changes -- in the ecology of one or both biomes for example -- or more remotely, as a consequence of inherent tension or lack of congruence between lifeways on land and sea (the dialectical opposition). Changes in systematic organization and perhaps complexity would accompany such shifts, but it follows that there would be no unilinear trend towards intensification of maritime adaptations. It must be admitted that there is little evidence for such swings in the cultural pendulum, although the Fraser Delta (Borden 1970) and Hamilton Inlet (Fitzhugh 1972) sequences could be re-examined from this perspective. It is also difficult to recognize and evaluate historical links between coastal/inland societies classified into different cultures. Dialectical shifts will result in a synchronic mosaic of subsistence-settlement systems -- a situation which seems to characterize the Woodland cultures of eastern Nova Scotia (Nash 1980b). The uneveness of such evolution would also produce significant differences in complexity among contemporaneous cultures in a single area.

Nonini (1980:433) suggests a revived "dialectics of nature" may be forthcoming. In spite of its traditional metaphysical difficulties, such concepts open new possibilities for articulating material and mental variables and integrating paradigms. Harris (1968:71) disputes any connection between dialectical thinking and historical processes, but we can hardly ignore the emerging (and sometimes binary) models of the brain being developed by neurobiologists.

CONCLUSIONS

There are several conclusions to be drawn from this review.

a) While this investigation is not an explicit test of Kuhn's model, it appears that the process of paradigmatic replacement that Kuhn describes is not characteristic of the history of archaeology on the Northeast and Northwest Coasts. New directions in model-building occur not as responses to persistent anomalies or

from the falsefication of existing models (in fact models are rarely tested), but rather, as in the case of ecology, from changes in the infrastructure of the society in which scientists operate.

b) Progress in the construction of theory is thus incremental, although not linear. Archaeology is presently in considerable flux and is a multi-paradigmatic discipline with the greatest progress occurring in the ecological paradigm and the greatest effort being expended in derivative CRM studies. In contrast to the Kuhn model, it appears that paradigms (in a methodological sense) are quite commensurate. Accordingly, greater effort is required towards integrating the paradigms and clarifying the bridges, feedback loops and critical paths among paradigms. Sociologists are also contending with the integration issue (Ritzer 1975).

c) There is some progress, notably in the ecological paradigm, towards generating theory pertinent to maritime hunters-gatherers. Unfortunately, there is a tendency to lean on purely biological models and a temptation to simply produce increasingly fine-grained synchronic reconstructions which are costly, labor-intensive and destined to reach a point of diminishing returns with respect to theory. There remains considerable potential for theoretical progress in the demographic paradigm and particularly in the evolutionary paradigm, which ought to be one of archaeology's strengths.

d) In Northeast and Northwest Coast archaeology, as elsewhere, Cultural variables have been ignored owing to present difficulties in measuring them. But, until ideational models and variables are integrated into archaeological theory and the relations between base and superstructure are made operationally (and archaeologically) explicit, we can place only modest levels of confidence in explanations of cultural variability, or greatly restrict the field in which we offer explanations.

METHOD AND THEORY IN NORTHWEST COAST ARCHAEOLOGY

Roy L. Carlson

> We are still searching for the laws that govern the growth of human culture, of human thought; but we recognize the fact that before we seek for what is common to all culture, we must analyze each culture by careful and exact methods ... before we can build up the theory of the growth of all human culture, we must know the growth of cultures that we find here and there ... and the progress of the civilizations of antiquity and of our own times. We must, so far as we can, reconstruct the actual history of mankind, before we can hope to discover the laws underlying that history.
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> Franz Boas
> Introduction to the
> Memoirs of the Jesup
> North Pacific Expedition
> Vol. 1, part 1. 1898

Since its inception Northwest Coast archaeology has been characterized by both an interest in the data of prehistory, and in their meaning. Research goals have been relatively constant, although methods and models have ridden the winds of change and followed the fads of scientific inquiry as with other anthropological disciplines. Boas' thoughts quoted above underlay the conception of the Jesup North Pacific Expedition in which the

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first major archaeological undertaking on the Northwest Coast took place. Such thoughts have continued to be basic to most Northwest Coast archaeological research up to the present day. Prehistory is the paradigm in which Northwest Coast archaeology has been undertaken; it is the checkerboard on which the movements of peoples, the diffusion of ideas, and the interplay of culture and environment have been plotted and replotted as new data have been unearthed, new methods and techniques employed, and new theoretical principles expounded. Although Boas wrote of discovery of "laws that govern the growth of human culture," it is clear that he viewed these laws as fundamentally psychological in nature, and that archaeology contributed to their eventual discovery indirectly through reconstruction of the past. In recent years another paradigm called cultural resource management or conservation archaeology has come into its own. The primary goal of this field is *management* of past cultural remains (e.g., Lipe 1974, Clark 1980). It is not unrelated to prehistory since without proper management there would be nothing left, no unexcavated data base for prehistory.

The basic *modus operendi* of Northwest Coast archaeology has been the comparative method which is based on the determination and assessment of similarities and differences in the archaeological record and leads to explanation of these same phenomena. Theoretical principles are employed in reaching this explanatory level. Ethnographic analogy has been and continues to be an important part of the comparative method in which comparisons are made between ethnographic facts and archaeological ones. The inferential process works the way Thompson (1958:1-8) says it does in proceeding from the indicative quality of the data through analogy to a concluding inference. The comparative method also involves continuous reassessment of conclusions as new information is brought to light.

Northwest Coast archaeology has been remarkably free of the recent trend frequently called "new archaeology" in which the search for general laws, "nomological generalizations" of cultural behavior is purported to be the major goal. Johnson (1972) has provided a succinct and devastating critique of this "avant garde" archaeology. Northwest Coast archaeologists seem to be content to let other disciplines such as psychology serve as the guide to behavioral laws on the assumption that actual observation of human behavior is a much more reliable base for such generalizations than are the data of archaeology, the distributions and spatial inter-relationships of artifacts and non-artifacts in the ground, which result from many factors. In most Northwest Coast archaeology undertaken to date the unstated goal has been the accumulation of

sufficient archaeological data to provide a substantive base for cultural-historical and cultural-environmental hypotheses. This quest has sought data relevant to the following questions: 1) When was the coast first occupied? 2) Where did these initial inhabitants come from? 3) What type of culture did they bring with them? 4) Has the coast ever served as a migration route? 5) What are the origins of the important Northwest Coast culture patterns, i.e., fishing, whaling, and the arts? 6) What changes in culture have taken place in Northwest culture through time? 7) What are the cultural and ecological relationships throughout prehistory? Guiding the archaeological investigations seeking answers to these questions have been three bodies of theory: cultural theory; associational theory; and sampling theory. Archaeology has not been conceived of as a generator of theory, but as a user of theory aimed at reconstruction and explanation of the past (Carlson 1970).

Theory refers to a coherent group of general propositions used as principles of explanation for classes of general phenomena, whereas methodology refers to how problems are solved. Archaeological data which consist of artifacts, non-artifacts (i.e. faunal or carbon samples, depositional strata, etc.), and their frequencies and observed spatial interrelationships, are general phenomena which when explained in terms of these bodies of theory permit the archaeologist to actually predict the past. The general propositions about the theory of culture are that culture is learned, shared, patterned, cumulative, integrated, and manifested in artifact styles, types and distributions. Cultural theory is to archaeology as evolutionary theory is to the biological sciences. The theory of spatial associations involves all those propositions concerning the principles of superposition, intrusion, burial associations, and all those statements about the meaning of spatial distance between artifact and artifact, artifact and non-artifact, site and site, or other observed phenomena. (The purpose of most archaeological field techniques is actually to permit observation and recording of such spatial distances.) Associational theory is also basic to paleontology and historical geology. Sampling theory consists of a third group of propositions, and is based on the assumption that certain populations of objects are randomly distributed within certain universes. Sampling theory is the youngest of these three bodies of theory to receive much attention in archaeology. With an emphasis on sampling, the question changes from simply what has been unearthed archaeologically, to how many have been found, and what is their frequency relative to the entire population from which they came. The use of specific sampling procedures permits somewhat greater significance to be attached to both negative evidence and frequency data, although data on presence are still by far the most important in archaeological inference.

Principles drawn from all three bodies of theory are related to techniques employed in excavation and data analysis.

If precedence is given to associational theory, excavation techniques aimed at gathering stratigraphically associated samples will be given the highest priority. The deposits at the surface of many Northwest Coast sites are of different ages, and deeper deposits from different excavation units are not necessarily of the same age even though they occupy the same position relative to surface strata. Strata of different ages are frequently not directly superimposed. For this reason a series of connected trenches will be employed in order to observe the continuity of strata from pit to pit, if the goal is to obtain samples of associated material. Much of the skepticism in regard to the proposed sequence at the Cattle Point Site (King 1950) was engendered by the many disconnected excavation units. Recent C-14 dates have corroborated this viewpoint, that the intrasite correlations of strata from non-contiguous excavation units is in error, and consequently so are the cultural phases based on these correlations.

If an archaeologist in interpreting his finds gives precedence to the theory of culture then one occurrence of a specific artifact type in context is sufficient to warrant positive placement of the customs indicated by this one artifact within the inventory of customs of the culture under investigation. Such an archaeologist would probably conclude with a simple trait list of artifacts or customs following the model of the ethnological culture element distributions. Site reports by C.E. Borden (1950a, 1951a, 1968a) tended to follow this method.

Sampling theory enters into many facets of archaeology. It may be used to select sites to excavate as Mitchell (1974) has done in the eastern Queen Charlotte Strait region, or it may be employed to determine where to dig at a single site, and in comparison of assemblages and components from the same or different sites after excavation has taken place. The problems lie both in defining the appropriate universe to sample, and in obtaining samples of associated materials of sufficient size to validate statistical results. Spurling (1976) has looked at results of judgmental and random sampling of the same site and found little difference. Matson (1974) introduced advanced statistical techniques in his *Clustering and Scaling of Gulf of Georgia Sites* to the problem of definition and comparison of coastal archaeological assemblages. He obtained a high degree of correspondence between the established chronology and the patterns obtained by clustering and scaling. Burley (1980b) has since employed similar techniques in elucidating

the Marpole phase, and many students are employing them in thesis research. Cultural resource management is fostering further development of sampling strategies for survey work aimed at predicting numbers and kinds of sites in given areas. A statistical revolution has actually taken place, and quantitative techniques based on sampling theory are now widely employed in surveys and excavations, and in making comparisons among assemblages.

Several explications of method and theory have served as guides in Northwest Coast archaeological research. Before the statistical revolution the most important were the following:

1916	Sapir	*Time Perspective in Aboriginal American Culture: A Study in Method.*
1948	Kroeber	*Anthropology.*
1948	Taylor	*A Study of Archaeology.*
1958	Willey and Phillips	*Method and Theory in American Archaeology.*

Edward Sapir's contribution, written while he was at the National Museum of Man in Ottawa, was the only codification of method and theory relating specifically to New World prehistory until Taylor's work in 1948. Sapir's monograph was used by an entire generation of culture historians as a body of theoretical principles related to the reconstruction of past cultures; there was no other. The past was reconstructed by distributional analyses of culture traits and complexes, typological comparisons and assessment of similarities and differences, isolation of discrete parts of cultural complexes and consideration of their distribution in time and space; these methods were applied to ethnographic, archaeologic, and linguistic data. They survive today in archaeology because of the nature of archaeological data. Kroeber's *Anthropology* was a far more general work, but the principles of culture (chapters 7, 8, 9, 14 particularly, and 19) expressed there and in his other publications provided the theoretical guide necessary to archaeology.

Taylor's (1948) "conjunctive approach" was a guide to data collection, and Willey and Phillips (1958) volume was in turn an important guide to the classification of time, space, and culture. Both were more methodological than theoretical, and broached issues of typology and cultural taxonomy which are essential to the comparative method. Archaeology like other sciences requires the reduction of masses of data to meaningful units of manageable size, and this is accomplished through classification. Various

classificatory systems have been employed at different times on the Northwest Coast, and the more common ones are reviewed below.

Drucker (1943) introduced the first specifically archaeological classifications to the Northwest Coast. His chief archaeological predecessor, H.I. Smith, operated in a taxonomic vacuum and barely got beyond grouping all hunting and fishing implements into one taxon, and root digging tools into another. Culture areas, complexes, traits, and strata which were in widespread use in studies of coastal prehistory (cf. Birket-Smith and DeLaguna 1938; Kroeber 1939), were classifactory units derived for the most part from ethnology rather than archaeology. In 1943 Drucker introduced both artifact typology and the McKern taxonomic system to the Northwest Coast. Those ethnological units mentioned above came to be partly complemented and partly paralleled by artifact types, and by defined cultural "aspects" within the Northwest Coast culture "pattern." The aspects corresponded closely to the known ethnographic divisons: A) Northern Aspect, comprising Tlingit, Haida, Tsimshian territories; B) Central or Milbanke-Queen Charlotte Sound Aspect, coextensive with Kwakiutl territory; and C) a Strait of Georgia/Puget Sound Aspect, the territory of the present Coast Salish (Drucker:1943, 123-127). Drucker's artifact types were never widely adopted, although the names he employed for some of the artifact classes have seen considerable use. There is still no standardized typology used by all Northwest Coast archaeologists. The most significant innovation since the statistical revolution is Flenniken's (1981) replicative systems analysis. This method looks at artifactual data from start to finish, from raw material through fabrication and its byproducts, to the finished tool and its use. Each artifact and piece of debitage can be plugged back into the system. This type of analysis has rendered most stone tool classifications used on the coast obsolete.

No system of cultural taxonomy has yet been universally accepted and employed. King (1950) used "phase" to designate the divisions of the sequence at Cattle Point, and at Five Mile Rapids Cressman (1960) used "stage" in much the same way. Borden (1950a, 1951a) began by using both "period" and "horizon" for units of time and culture. Cultural taxonomy remained at a standstill until Willey and Phillips' publication on method and theory in 1958. The classification system offered by Willey and Phillips divided time, space, and culture into manageable units, and provided specific definitions of various concepts such as "tradition" and "horizon" for integrating these dimensions. "Component," also used in the McKern system, has been the most widely adopted concept, and "phase" has enjoyed considerable although not universal usage. While I was revising *Chronology and Culture Change in the San Juan Islands* in

1959 for publication, Borden and I reached mutual agreement that we would use "phase" in the Willey and Phillips sense rather than "focus" for Marpole and similarly constituted units of culture content. We never accepted the social realities, tribe etc., suggested for these taxons which are largely the basis for Abbott's (1972) criticism of the concept of phase. Even on the ethnographic time level, tribes are abstractions based on proximity, language affiliation, and culture content and not on socio-political unity. The use of phase does not imply that any such unity existed in the past.

It is still not widely understood that a phase is a working tool and that it is defined on the basis of culture content, not on time and space. The fact that such a unit of culture content has a limited distribution in time and space is simply a function of its validity as a useful tool in reconstructing the past. Mitchell (1971) has employed the term "culture type" in a manner identical to that of phase, and has thus rendered its more usual usage as a generalizing term for types of cultures not limited by time or space considerations somewhat ambiguous. (See disscussion in Willey and Phillips 1958:12-13.) Burley (1980) has further complicated the picture by using phase in local sequences, and culture type for the regional equivalent. Anyone attempting to disentangle the taxomonic picture might well conclude that above the level of component Northwest Coast archaeologists don't know what they're talking about. The taxonomic system that best helps to both explain and understand the past is the one that is obviously the most useful, and I am sure that new systems will continue to be developed. Higher level integrative concepts such as horizons, traditions, co-traditions, such as MacDonald (1969) has profitably used on the Northern Coast to show the interaction of Tlingit, Haida, and Tsimshian cultures through time, and just plain archaeological cultures are actually one step removed from pure taxonomy and approach the realm of explanatory models.

Cultural-historical models are composed of sets of hypotheses which simplify complex observations and eliminate unnecessary information. Simple analogue models have been the rule. All such models have emphasized one or more of the trilogy of diffusion, migration, or adaptation as the mechanisms responsible for growth and change of prehistoric Northwest Coast cultures. Kroeber (1923:7-8) presented a methodological approach to the problem of formulating cultural historical models which has not been superceded. He separated aboriginal American culture into four groups: 1) original common traits brought by the first immigrants, simple and widely distributed; 2) cultural elements developed on American soil which spread widely; 3) elements locally developed

which remained local; and 4) elements later introduced from the Old World. He concluded:

> ... Northwest Coast culture shares with American culture only basic universal elements presumably derived from Asia; that it lacks regularly the generic American elements that were developed on American soil and became diffused; and that what is specific in it is either a direct outgrowth on the spot from the relatively undifferentiated primitive American culture or the result of later Old World influences.
>
> (Kroeber 1923:7-8)

Archaeology has modified this model only slighty by demonstrating that some generic American elements such as pipe smoking did indeed diffuse to parts of the Northwest Coast.

Kroeber later (1939:28) suggested a developmental model for Northwest coast cultures based on change from an interior riverine subsistence to a fully maritime one:

> ... the Northwest Coast culture was originally a river or rivermouth culture, later a beach culture, and only finally and in part a sea going one.

Archaeological research has recently reversed this model (Carlson 1981) so that it now reads that Northwest Coast culture

> ... likely originated as coastal, later became river mouth, and even later, but only in part, riverine, as it accompanied the spread of lake spawning species of salmon further and further up the rivers into the interior as part of the postglacial environmental adjustment.

Whereas Kroeber's model stressed adaptation, all the other early models emphasized migration and diffusion with either an initial southward or westward movement of people or culture, followed by a similar movement from the direction other than that from which the first inhabitants came. Some of these models preceded Kroeber; others followed. All were based largely on ethnology, although the specifically archaeological data of the Jesup Expedition were recognized. Boas (1905) looked to an Asiatic-Northwest Coast cultural continuum broken by the migration of the Eskimo. Hill-Tout (1932) also used a displacement model, but in this case Eskimo culture as far south as the mouth of the Fraser was displaced by peoples from interior British Columbia. Birket-Smith and de Laguna

(1938) and de Laguna (1947) employed a diffusion model involving a sequence of cultural strata marked by elements of an "ice hunting stage," followed by those of a "snowshoe stage" and then by "circum-Pacific drift." H.I. Smith had difficulty finding any model to which to relate his finds, although he eventually did subscribe to population movements from the Interior. Drucker (1943:117) likens Smith to an Archaeological Ancient Mariner:

> culture stratigraphy all about, but not a sequence could he find.

Drucker however fared no better; he found no sequence either and had to rely on a seriation of Smith's and Hill-Tout's collection of skulls in order to arrive at a migration model of a broad-headed population replacing a longheaded one, at least in the Coast Salish region. This seriation plus both the absence of stratigraphy in the middens of the Central Coast where he had excavated, and the presence of numerous traits in the Puget Sound/Gulf of Georgia aspect which had interior distributions, led him to consider the latter to be a modified aspect of Plateau culture overlying an older coastal component (Drucker 1943:126-127). He later (1955) identified the latter with the Ice Hunting stratum. Beattie (1980:10-18) has now reviewed all data relating to this seriation of skulls, and has shown that these two types do not exist. He concludes,

> an overconfidence in Hill-Tout's hypothesis, combined with a misreading of the work of Kidd, has fostered the durable concepts of two prehistoric physical types and a related population migration focused at the mouth of the Fraser River.

Arden King (1950) excavated and fully published the first stratified site on the Northwest Coast, and Borden (1950a, 1951a) developed the first chronological sequence based on excavated materials which has withstood the test of time. King's model of cultural development based on the Cattle Point material did not follow that of those researchers who saw the earliest cultures immigrating from the north, but instead tended to follow Kroeber's line of thought. King (1950) related the earliest phase at Cattle Point to life on land, and compared it to the inland Archaic cultures of North America, although recognizing the later diffusion of ice hunting traits, and circum-Pacific drift. Borden working at the same time as King developed the opposing model: his earliest horizon, the Early Maritime, was specifically Eskimoid in type even to the point of speculation about skin boats in use along the Strait of Georgia; this culture was later replaced by cultures from the

Interior. What is interesting is that both King's and Borden's earliest phases are very similar in culture content, and we know today that both date to much the same period! We also know now that these components do not represent the earliest cultures of the Strait of Georgia.

In the late fifties the models of migration and diffusion from the north and east began to take on a new direction and this direction was migration and diffusion from the south:

> Small groups wandering gradually northward as the areas became free of ice, as the forests grew and as the salmon began their annual migrations up the rivers probably moved out into the (San Juan) islands.
>
> Carlson 1960:583

This shift in emphasis was the direct result of new archaeological knowledge concerning the antiquity of man in the New World. Evidence of periglacial occupations to the south had been presented by Daugherty (1956) and Cressman (1960). The culmination of this direction of thought was Borden's (1962) article which postulated the northward diffusion of ground slate, an industry traditionally associated with Arctic and Eskimo cultures. Today, the Arctic holds but a slight edge in regard to the antiquity of this industry.

Butler (1961:70) offered a genuinely constructive model of Pacific Northwest prehistory with his publication of the Old Cordilleran Culture, an early cultural tradition defined as:

> ... a tradition characterized by a leaf-shaped point and blade complex, along with a generalized assortment of cutting, chopping, and scraping implements. That its carriers pursued a generalized hunting-fishing-gathering economy. That ... the tradition developed along independent lines becoming a maritime tradition in the Northwest Coast area.

While this concept was not initially greeted with unmitigated acclaim (Carlson 1962), it was soon modified (Butler 1965) and became widely known. The concept involved both a way of life, and a particular artifact assemblage. It survives today in both these forms: as the earliest unit of culture content in the Glenrose site on the lower Fraser (Matson 1976) typified by an assemblage very much like that originally ascribed to the culture, and in the second sense as a way of life exemplified by a barely prehistoric component from southern Puget Sound with quite a different artifactual content (Hedlund 1973). The Protowestern (Borden 1975) was a similar

construct, but like the Old Cordilleran tends to obscure the differences between the earliest coastal and interior plateau cultures.

In the last ten years the emphasis has shifted to those models in which adaptation plays the key role, although diffusion is by no means dead and even migration is mentioned occasionally. The prehistory of the Northwest Coast is after all both a history of new techniques and tool kits used to exploit the same ecological niches, and a history of known tools and techniques extended to different niches. Adaptation to resources was never denied by early researchers, but the gathering and interpretation of substantive data was slow to come about. By the early forties during the attempt to identify the faunal remains from Drucker's survey, it was discovered (Fisher 1943) that there were no comparative fish bone collections, an obvious need for any empirical archaeological investigation of the aboriginal "salmon area." Many archaeologists worked on identifying bones but it was Cressman (1960) who made the first major contribution in this area. Today we have not only comparative collections, but specialists in this study (Huelsbeck 1981, Boucher 1976, Boehm 1973, Stewart 1975) and the emphasis has gone beyond simple identification to problems of terminology and sampling. Mitchell's (1971) sub-areas of the Strait of Georgia are fundamentally adaptive zones. Fladmark (1975:292) looks at the entire development of the Northwest Coast village pattern in terms of the late stabilization of the post-glacial environment. Donald and Mitchell (1975) have demonstrated a positive correlation between salmon abundance and local group rank, and Pomeroy (1980:222-223) has postulated a relationship between salmon abundance and local group stability. The ultimate in environmental mindedness has been Hester's (Hester and Nelson 1978) research design at the Namu site predicated on the reconstruction of prehistory on faunal evidence alone!

The current emphasis on adaptation is fully evident in the papers in this symposium. None of the authors attempt to trace historical threads through the multiple adaptations necessary for survival and the development of cultural complexity during the post-glacial. Instead, their focus is on the adaptations themselves. Such an emphasis seems justified if not inevitable in attempting to explain any culture area which retained a food gathering subsistence base long after cultures of other areas achieved food production, and for which there is archaeological evidence of long standing cultural continuity and abundant middens containing remains of food gathering enterprises. It has long been realized that the artifact complex of a given site should express to some degree the peculiar ecological manifestation of the particular

site which should not be construed as a total cultural complex, and that the latter would probably be expressed only at a winter village site, if there (Carlson 1954:10). In this respect Northwest Coast archaeology is only now in full flower with attempts to actually identify seasonal sites and tool kits and correlate them with ecological niches. The papers in this symposium contribute to this end, although some are more replete with data and ideas economically expressed than are others lost in the jargon jungle of the peers and mentors of their authors. Once the above goals are achieved, however, there still remains the problem of integrating the historical with the ecological as neither provide adequate explanatory models by themselves. The emergence of cultural complexity on the Northwest Coast must be conceived of as a result of both progressive adaptation to the environment and the diffusion of techniques and artifacts (Carlson 1960:584).

The best models are those which provide the most information in the most economical manner. The one I prefer for the early period is an acculturation model (Carlson 1979, 1981) involving the mutual influencing of three early basal cultural traditions: the Pebble Tool Tradition, the Lind Coulee or northern variant of the Stemmed Point Tradition, and the Microblade Tradition. Different primary subsistence strategies -- fishing for the first, land hunting for the second, and fishing and marine hunting for the last are assumed and partially documented for these early cultural traditions. Borden (1968a, 1969), Ackerman (1968, 1974), Dumond (1973) and Hester (1978) present some of the hypotheses necessary to this model, which also predicts that these basal cultural traditions are ancestral to the historic Indian peoples still present on the Northwest Coast. Burley's (1980:72) systemic model of processes involved in the development of the Marpole phase provides a framework for explaining archaeological data of later periods.

Several trends are apparent in current research that in all probability will be of continued concern. The "research projects" of the sixties which gave way to the "salvage archaeology" of the seventies have now given birth to archaeological "resource management," a conception of archaeological sites as non-renewable resources which must be conserved and protected for the future. Regional inventories and impact assessment have replaced archaeological serveys in contemporary jargon. The accumulation of field data will continue, and some of this data gathering will still be basic research into the unknown just to see what is there, but more will likely be involved with surveys and excavations undertaken as part of systematic inventories and problem oriented conservation archaeology. Archaeology will likely become more esoteric on the one hand as statistical fine tuning and computer simulation are

employed more and more, and more general on the other as it presents its conclusions to a public ever fascinated by the past. The continued development of method and the application of various bodies of theory are a necessary part of the future of archaeology.

SOCIAL AND POLITICAL COMPLEXITY ON THE EAST COAST: THE MICMAC CASE

Virginia P. Miller

The Micmac people of eastern Maritime Canada have been classified by anthropologists in the Sub-arctic culture area of North America (Driver 1969:Map 2). And indeed, to early anthropologists, the Micmac did display a number of Sub-arctic characteristics: traditionally they were hunters and gatherers, their population seemed to be sparse and dispersed, their sociopolitical organization was apparently characterized by a loose band structure, and, following contact with Eruopeans, they became intensively involved in the fur trade.

But what the early anthropologists studying the Micmac didn't take into account was that they were collecting information on a culture more than 400 years after that culture had come into contact with Europeans and had undergone great culture change. The result, needless to say, was that the anthropologists got quite a distorted impression of aboriginal Micmac culture. Recently some of these distortions concerning aboriginal population have been corrected through study of primary source materials dating from the seventeenth century -- material recorded by intelligent and reasonable, albeit untrained, observers only 100 years following Micmac contact with Europeans (Miller 1976, 1980a). But another distortion which remains to be corrected concerns the traditional view of Micmac social and political organization. When reconstructed from seventeenth century accounts, this turns out to be considerably more elaborate than the loose aggregation of egalitarian bands described

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by Wallis and Wallis in their classic ethnography (1955:171), or, more recently, by Bock (1978:116). This paper re-examines Micmac social and political organization according to the scale presented in Appendix A; this scale is taken from the work of Sahlins (1968) and Service (1962, 1963), and incorporates the six significant attributes of basic structure, integration, specialization, leadership, polity, and stratification for the cultural levels of band, tribe, and chiefdom.

BASIC STRUCTURE

Nuclear families among the Micmac were grouped into living units of bilaterally extended families, with a tendency for these family units to be patrilocal. As an early seventeenth century Jesuit priest in Nova Scotia described them:

> There is the Sagamore, who is the eldest son of some powerful family, and consequently its chief and leader. All the young people of the family are at his table and in his retinue; ... The young people flatter him, hunt, and serve their apprenticeship under him, not being allowed to have anything before they are married, for then only can they have a dog and a bag; that is, have something of their own, and do for themselves. Nevertheless they continue to live under the authority of the Sagamore, and very often in his company; as also do several others who have no relations, or those who of their own free will place themselves under his protection and guidance, being themselves weak and without a following.
>
> (Biard in Thwaites 1896 III:87)

Sometimes the sagamores who headed these groups practiced polygyny, giving two reasons for this:

> One is, in order to retain their authority and power by having a number of children; for in that lies the strength of the house, in the great number of allies and connections; the second reason is their entertainment and service, which is great and laborious, since they have large families and a great number of followers, and therefore require a number of servants and housewives...
>
> (Biard in Thwaites 1896 III:100-101)

From these accounts, it is apparent that Micmac social organization was based on groups of kin extended bilaterally with other unrelated individuals who might choose to ally themselves with

a particular sagamore. Hoffman has termed this type of grouping one of bilocally extended families (1955:590).

No specific citations of the size of these bilocally extended families exist in the seventeenth century sources, but from statements such as "they have large families and a great number of followers (Biard in Thwaites 1896 III:101), and from one partial list of a family group, we may make some deductions. When the great sagamore Membertou and his family were baptized in the Catholic faith in 1610, the list of those baptized totaled 21 individuals (Lescarbot in Thwaites 1896 I:77) and was incomplete at that. Other unrelated individuals in Membertou's following may have been away or may have chosen not to be baptized. Support for fairly sizable groups also comes from the fact that each family unit formed a village for summer occupancy (Speck 1915a:303). One recent source (Bock 1978:109) baldly states that these summer aggregations totaled "200 or more." Conservatively then, one of these family living units probably consisted of 30 or 40 members as a minimum.

LEADERSHIP AND POLITY

The bilocally extended family units which formed the base of Micmac society are not inconsistent with the basic social structure of Sub-artic groups generally. But let us go further to consider the structure of Micmac leadership and polity.

The account cited earlier of a sagamore and his following describes what may be called a "local chief," or that of the lowest level. The territory governed by a local chief was the area surrounding his summer village site; this area was occupied and used by his followers (Hoffman 1955:516). Duties of a local chief included settling disputes among his followers, planning seasonal movements of his group, delegating tasks among members of his group, ensuring that there were dogs and equipment for hunting, planning and organizaing feasts, seeing that there were adequate food reserves, looking after widows and orphans, and presiding over the council of elders (various; Thwaites 1896 III:87ff). This council of elders, which included the adult male heads of families within the village, assisted and advised the chief in decision making. Unanimity was required from the council before a local chief was empowered to act in a given situation; others in the village followed "without question" decisions of the chief and council (Hoffman 1955:516).

The next level above the local level was that of the district. Seventeenth century Jesuit priests' accounts reveal that the priests

were aware of the Indians' political districts and how they were governed (Thwaites 1896 III:89), but it remained for a nineteenth century observer to record the specifics: "They [the Micmac] divided it (their territory) into seven districts, each district having its own chief, but the chief of Cape Breton, which comprised one district, was looked upon as head of the whole (Rand 1875:81). This latter Cape Breton residence requirement for the grand chief apparently was not an invariant rule, however. The seven districts were named, and each of them contained a number of summer villages and local chiefs. Overseeing the smooth functioning of each district was the district chief, who could also have been one of the local chiefs in the district. Duties of the district chief generally included directing the local chiefs in planning their seasonal movements and ensuring that district affairs functioned smoothly. Like the local chief, the district chief had a council to assist him. The council consisted of all the local chiefs in the district plus respected shamans. All were free to voice their opinions and all received equal weight in the decision-making processes (Thwaites 1896 III:91). District councils of chiefs met several times a year and, aside from providing chiefs the opportunity to renew acquaintances and discuss matters of common concern, they seem to have had two principal political and economic functions. First, in conjunction with the council of chiefs, the district chief each year re-assigned hunting territories to heads of families, and the Indians are said to have obeyed strictly the boundaries of their assigned territories (LeClercq 1910:237). The second important function of the district chief and council of chiefs was to make decisions regarding war and peace (LeClercq 1910:234). When an important matter such as war was to be considered, messengers were dispatched to other districts to summon additional chiefs for consultation (Thwaites 1896 III:91; Lescarbot 1914:264). Decisions made by the council of chiefs had to be unanimous, and, once made, could not be rescinded (Lallement in Thwaites 1896 XLV:239). Chiefs and their councils were so highly respected that their decisions were said to be observed "with much submission and fidelity" (LeClercq 1910:234).

At the head of the entire Micmac nation was a single "grand chief." In the early seventeenth century, when the French first settled at Port Royal in western Nova Scotia, the grand chief Membertou resided in that district. Membertou became a close and loyal friend of the French, and their descriptions of him portray a truly outstanding individual. Physically, Membertou

> ... was the greatest, most renowned and most formidable savage within the memory of man; of splendid physique, taller and larger-limbed than is usual among them; bearded

> like a Frenchman, although scarcely any of the others have hair upon the chin; grave and reserved; feeling a proper sense of dignity for his position as commander.
>
> (Biard in Thwaites 1896 II:23)

Continuing,

> He is at least a hundred years old, and may in the course of nature live more than fifty years longer. He has under him a number of families whom he rules [Membertou was a local chief and probably a district chief as well], not with so much authority as does our (French) King over his subjects, but with sufficient power to harangue, advise, and lead them to war, to render justice to one who has a grievance, and like matters. He does not impose taxes upon the people, but if there are any profits from the chase he has a share of them, without being obliged to take part in it ... his reputation is far above that of all the other Sagamores of the country, he having been since his youth a great Captain, and also having exercised the offices of Soothsayer and Medicine-man, which are the three things most efficacious to the well-being of man, and necessary to this human life.
>
> (Lescarbot in Thwaites 1896 I:75-77)

Membertou had a fourth quality respected by the Micmac: "He has been a very great and cruel warrior in his youth and during his life" (Lescarbot 1911:354-355). For all these reasons, Membertou was "greatly dreaded" by his enemies (Biard in Thwaites 1896 III:91).

Like the local and district chiefs, the grand chief was assisted by a "grand council" of all lower chiefs and respected men. No accounts of grand council meetings remain from the seventeenth century, except for a priest's reference late in the century to "those large assemblies in the form of councils" (LeClercq 1910:234). But a report exists of a grand council meeting held in the late eighteenth century and provides an example of the type of situation which would convene the grand council.

> A great alarm was excited here [in Pictou, Nova Scotia] in 1779 by a large gathering of Indians from Miramichi (New Brunswick) to Cape Breton, probably a grand council of the whole Micmac tribe. In that year some Indians of the former place having plundered the inhabitants, in the American interest, a British man-of-war seized sixteen of them, of whom twelve were carried to Quebec as hostages and afterwards brought to Halifax. This is what led to

> this grand gathering. For several days they were assembled to the number of several hundred, and the design of the meeting was believed to be, to consult on the question of joining in the war against the English. The settlers were much alarmed, but the Indians dispersed quietly....
>
> (quoted in Hoffman 1955:548-49)

Some of the perquisites of chieftainship have already been mentioned. Aside from respect and deference from other Micmac, chiefs were presented with a share of the game and pelts taken, were given places of honor at feasts, were served first and given the choicest foods. As stated previously, they were frequently accompanied by retinues of young men and, after the Micmac got guns in trade, chiefs expected to be saluted with gunshots or even cannonshots when approaching trade posts or other villages (LeClercq 1910:246).

How did chiefs succeed to their positions? The answer comes from a seventeenth century source. "The captains among them take their rank by inheritance ... provided always that the son of a Sagamos imitates the virtues of his father, and is of suitable age ..." (Lescarbot 1914:265). Two factors were thus important in determining who would be a chief. The first was kinship affiliation. Chieftainships were customarily passed down from father to son in families; in fact, after examining known historical chiefs and their known relatives, Hoffman has suggested that there might have been just two or three "chiefly families" (1955:573). As recently as 1915, Speck found corroboration for this among the Cape Breton Micmac (1915b:506).

But more than birth into a chiefly family was required for an individual to become a chief. The second requirement cited by Lescarbot (above) hinged on an individual's personal qualities. Hoffman (1955:515) has suggested qualities which were especially emphasized here:

1. leadership ability;

2. superior intelligence;

3. a dignified manner;

4. generosity toward others (some chiefs deliberately made it a point to be the worst dressed among all their people, freely giving away their food and clothing in order to command love, respect, and loyalty from their followers);

5. courage and aggressiveness in war (Membertou had been "a very great and cruel warrior" who was still leading successful war parties at the age of 100!); and

6. superior ability in hunting (even though chiefs may not have been required to hunt in their chiefly positions).

The eldest sons of chiefs were trained from their childhood to develop these qualities. If a boy in training did not show promise, a second son or other close male relative of the incumbent chief would be chosen and trained in his place. As Hoffman points out, it is worth noting that even though it was thus theoretically possible for a boy from a non-chiefly family to become a chief, it was highly improbable that this would happen because of the training involved. And Hoffman adds that this is confirmed in historic times by the presence of the few "chiefly families" (1955:574).

STRATIFICATION

Aboriginal Micmac society was ranked. At the top of the ranks were the chiefs. We have already mentioned the existence of "chiefly families." Such families took pride in their position by reciting their genealogies on public occasions and at feasts (Denys 1908:410). An eighteenth century source corroborates the existence of high-ranking families as opposed to other families, by relating an instance when the Micmac went to war against the Malecite over the treatment the Malecite accorded some young Micmac women who had been given them as wives in a demonstration of peace and friendship. The Micmac asserted that the Malecite had abused "these girls of the most distinguished rank" (Maillard 1758:23), adding that "our resentment would not have been so extreme with respect to girls of more common birth, and the rank of whose fathers had not a right to make such an impression on us" (Ibid.:24). Doubtless in this case, "high ranking" is synonymous with chiefly families. Finally, and interestingly, the archaeological record has provided some evidence suggestive of Micmac ranking. One of the very few Micmac burial sites excavated in Nova Scotia, at Pictou in eastern Nova Scotia, contained the remains of at least half a dozen individuals buried at different times, but probably all during the first half of the seventeenth century. Buried with them was an extensive assortment of grave goods, including native goods (birchbark containers, mats, bits of wampum and leather) and trade goods (copper kettles, swords, knives, axes, beads). The quantity and variety of the goods suggest that the individuals interred there were of some prominence in the area (Harper 1957).

Sometimes equal to, and certainly close behind, the chiefs in rank were the shamans. Curing the sick, controlling natural forces, directing hunters in the game quest, and divining the future were some of their functions. Shamans acquired their power in several ways, but the principal way was by inheritance. Membertou's oldest son, for example, expected to follow his father as a shaman as well as chief (Lescarbot 1914:111). Good shamans were powerful people who held great influence among the other Micmac. As we have seen, they participated in the district councils of chiefs with equal voice to that of the chiefs. And successful shamans could become so wealthy through the gifts they received for their services that they no longer hunted or fished for themselves. One disapproving seventeenth century source stated that:

> ... medicine men were lazy old fellows who would no longer go hunting, and who received from others everything they needed. If there were any fine robes, or other rarity in a wigwam, that was for Monsieur the Medicine-man. When animals were killed, all the best parts were sent to him. When they had cured three or four persons, they never lacked anything more.
>
> (Denys 1908:418)

Most of the Micmac people were of a ranking which can best be termed "commoners." This group included family members and relatives within local districts, who followed the directives of their local chief and who organized their daily lives along the lines of a sexual division of labor. Commoner males spent their time fishing, hunting, or participating in war raids, while females prepared and preserved the fish and game, dressed the skins, collected the odd plant foods, took care of the children, and did other tasks around the wigwam. If a commoner male was a good hunter and warrior, with age he would accrue respect and prestige, and receive the satisfaction of having some influence in the village council of elders. Women commoners, too, might gain respect as they got older, either as the wife of a good hunter or warrior, or perhaps in their own right as an herbal curer or for some other quality which made them outstanding and valuable persons in their villages.

At the bottom of the Micmac social order were the slaves. The source of slaves was war, although not all war captives became slaves. Adult male captives were frequently killed on the field of battle, but some may have been taken home and given to the women to torture in revenge for past injuries and insults (LeClercq 1910:271). Some adult male captives were kept as slaves and made to do menial tasks, usually helping women with their domestic chores

(Lescarbot 1914:200). Women and children war captives, however, were often adopted into the tribe and treated humanely despite their status as slaves (Lescarbot 1914:269, 271). One notable case has been recorded of an Inuit woman slave in Cape Breton (Nova Scotia), who was "ransomed" by Jesuit priests and converted to Catholicism (Thwaites 1896 XLV:69). Death was the penalty for slaves who attempted to escape and were apprehended.

SPECIALIZATION

Most likely there were no recognized positions for full-time specialists, either in religion or production. Some of the shamans apparently were amply supported by gifts of food and furs so that they did not have to hunt (Denys 1908:418). Less successful and prominent shamans may well have done some of their own fishing and hunting.

There were no specialists in production. Each family provided its own fish and game, giving selected portions of the game and some pelts to their chief and their shaman, and sharing with other families in time of hardship. Young unmarried men living in the family group turned over all their products of the chase to the chief, who in turn fed and provisioned them (Thwaites 1896 III:87-89).

The closest to a full-time political specialist would have been the grand chief. We have seen that grand chiefs were greatly honored and accorded much respect from others; in their capacity as grand chief, they enjoyed numerous perquisites. While they were expected to have a reputation as outstanding hunters, in all likelihood they were not expected to go hunting often, but had their needs supplied through the gifts of their followers, thus freeing their time for chiefly activities. District chiefs probably did not do much hunting, either. Local chiefs were probably most involved in the quest for food as they directed the movements of their group.

INTEGRATION

There were no pan-tribal sodalities such as age-grade or secret societies among the Micmac. As stated previously, the basic structure of Micmac society was the bilocal extended family unit consisting of a chief and his related and unrelated followers. According to an early priest, these units did not have "ties and bonds of union, since they are scattered and wandering" (Thwaites 1896 III:87), but were largely self-sufficient economically, coming

together several times a year for councils and feasts. Bonds between bilocal extended family groups were forged by marriage, creating a "great number of allies and connections" with other bands for each chief (Thwaites 1896 III:101). The periodic meetings of chiefs within each district and across the districts, with their requirements for unanimous decisions on topics, served to integrate the Micmac people at a higher level.

DISCUSSION

Where does all this put the Micmac on our scale (Appendix A) of social and political organization among bands, tribes, and chiefdoms? Let us examine how Micmac organization fits into the scheme.

Basic Structure

The bilocal extended family groups which characterized Micmac culture would seem to be closest to Service's tribe, "corporate residential units ... largely self-sufficient economically, generally equivalent in size and organization, autonomous in large measure." Certainly the Micmac groupings of related and unrelated persons under the guidance of a chief are more cohesive than "an association of family residential units ... losely allied by marriage ties," which characterizes bands. In any case, the differences between the basic structure of bands vs. tribes is one of degree rather than kind, and the Micmac seem to fall closer to tribes on the continuum.

Specialization

Again, in specialization (or the lack of it), there is little difference between bands and tribes. Simply put, neither has economic or religious specialists creating mutual dependence among groups. Specialization is a characteristic only of chiefdoms. The Micmac, then, fall somewhere on the band-tribe continuum, probably closer to tribe than band since Micmac society was wealthy enough to support some full-time shamans and chiefs.

Integration

On this characteristic, we see that "kinship ties ... are the integrating mechanism" for bands, while pan-tribal sodalities serve to integrate tribes. The Micmac did not have pan-tribal sodalities

in aboriginal times; instead, their three-tiered political organization with its local chiefs coming together periodically for district meetings, and district chiefs coming together periodically for grand councils was the effective integrating mechanism. Since they had such an integrative mechanism while no such mechanisms are found in band societies, we would have to classify the Micmac more with tribes than with bands.

Leadership and Polity

On the criteria for leadership and polity, we see a shift away from band and tribe. According to our scheme, informal leadership provided by family heads and ephemeral leaders typifies bands and tribes. As we have seen in this paper, leadership among the Micmac was neither informal nor ephemeral; instead, the Micmac had three different levels or "offices" of chief, each with its own respective duties. Moreover, these three "offices" of chiefs were organized into a definite hierarchy governing minor and major subdivisions of the tribe. Micmac political organization was indeed established above and beyond the community level. These qualities conform more to the characteristics of a chiefdom level of leadership and polity, as specified by Service and Sahlins in our scheme.

Stratification

According to our scheme, egalitarianism characterizes both bands and tribes. We have seen that Micmac society was a ranked one, with ranks ranging from chiefs down through commoners and slaves. Ranked society is one of the characteristics of chiefdoms.

An overall review of Micmac social and political organization, then, places the Micmac about midway between the levels of tribe and chiefdom: on the attributes of basic structure, integration, and specialization, the Micmac are closer to a tribal level, while on the attributes of leadership, polity, and stratification, the Micmac are closer to the chiefdom level. But beyond quibbling over terminology or levels of organization, there is a larger point to be made here: such a complex political organization as that found among the Micmac is quite unusual for a non-horticultural people.

The complex Micmac political structure was not the result of contact with Europeans, as one might suggest. There was certainly much cultural change among the Micmac following contact, but the very early cultural change affected tangible aspects of the culture: material culture and diet changed very quickly following

contact and the development of the fur trade (Bailey 1969:8ff; Burley 1981a). Even before the first written accounts were made, the material culture had changed significantly. This did not happen with the non-material culture, e.g., the political organization. The main reason for this was that for most of the early period, circa A.D. 1500 - 1600, contact and trade with fishermen took place offshore in Micmac territory. Fishing ships practiced what is called the "wet fishery" in the area (salting the fish and packing it away immediately to be cured later in Europe), rather than the "dry fishery" (landing and drying the catch onshore and then packing it for transport back to Europe). The wet fishery was practiced for two reasons: the fishing banks were relatively far offshore, and, the frequent summer fogs around Nova Scotia made impractical a satisfactory drying of fish (Hoffman 1961:198). Moreover, it was the practice for fishing ships to take on supplies and fresh water in St. John's, Newfoundland, before proceeding to the fishing grounds, thus further reducing the need to land in Nova Scotia. The result of all this was that Europeans rarely came ashore in Nova Scotia, while the Indians paddled out to the ships in their canoes to trade. Once settlement and more intensive contact commenced after A.D. 1600, not much time elapsed before Micmac non-material culture, including political structure, started to change rapidly. By the late seventeenth century, a priest who had spent many years among the New Brunswick Micmac lamented the demise of government:

> One sees no more among these people those large assemblies in the form of councils, nor that supreme authority of the heads of families, elders, and chiefs, who regulated civil and criminal affairs, and in the last resort decided upon war and upon peace, giving such orders as they thought absolutely essential, and enforcing the observance thereof with much submission and fidelity.
>
> (LeClercq 1910:234)

If Micmac political structure was not post-contact, then how may it be explained? It shows obvious affinities with Eastern Woodlands groups. The famous League of the Iroquois comes to mind when we consider the neat Micmac hierarchy with its district and grand council meetings called for matters of importance. Micmac political structure could have been the natural pre-contact result of a dense Micmac population (Miller 1976, 1980a) organizing itself to cope with the pressures of war from the Iroquois and New England groups. The whole idea of a Micmac political hierarchy might indeed have diffused from these other groups. Admittedly, such is speculation at this point, but interesting speculation which may well justify further pursuit.

What we have done in this paper is demonstrate the surprising complexity of political organization among a people previously thought to have a simple band type of structure, and who have been classified with Sub-arctic groups because of this. The paper has show that the Micmac share some important affinities with the Eastern Woodlands groups. Now that we are starting to gain an appreciation of the complexity of Micmac culture and political organization, some time might next be spent profitably comparing Micmac political structure and other aspects of Micmac culture to that of groups of the western maritime area of North America. Other aspects of culture to compare might include warfare and slavery practices and economic and redistribution systems of the two areas. It could well be that a number of unsuspected similarities between the Northeast Coast and the Northwest Coast remain to be uncovered. When this is done, we may be able to formulate some generalizations concerning maritime environments and the cultures which develop in them.

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Finally, it should be mentioned that Bernard Hoffman in 1955 completed an exhaustive ethnography of Micmac, in which he also reconstructed the aboriginal social and political organization. Unfortunately, this ethnography has never been published and consequently is of benefit to only a few scholars. My own scheme of Micmac social and political organization presented in this paper has been reconstructed independently by researching primary source material in the process of writing an ethnohistory of the Nova Scotia Micmac. The sources are so explicit that it would be virtually impossible not to develop the scheme that I have presented here and Hoffman developed in 1955. I have continued the use of Hoffman's terminology and have incorporated some of his interpretations into this paper, as cited.

Appendix A

ATTRIBUTE	BAND	TRIBE/SEGMENTARY TRIBE	CHIEFDOM
INTEGRATION	"lacks special integrative mechanisms excepting those common to all human societies. ...kinship ties...are the integrating mechanism" (Service 1963)	Pan-tribal sodalities are not supplemented by other integrative means as in chiefdoms. They do the whole job in tribal society. E.g., clans, age-grade assns., kindreds, secret societies (Service 1963)	"Clans, secret societies, warrior and curing societies, and the like become less significant in well-developed and stable chiefdoms than in tribes simply because their integrative functions, and even their special purposes, often have been rendered less necessary by the solidary organic nature of chiefdoms." (Service 1962)
STRATIFICATION	egalitarian in that no one of the families or residential groups is politically superior or more powerful in rank than any other (Service 1963)	egalitarian in that no one of the families or residential groups is politically superior or more powerful in hereditary rank than any other (Service 1963)	Non-egalitarian. Differences in hereditary rank. Social, but not economic, classes may exist. Have marked social stratification but no true socio-economic classes (Service 1963) Ranked society (but not class society). (Sahlins 1968).
SPECIALIZATION	no special economic groups or special production units (Service 1962) corporate residential units are like one another, largely self-sufficient economically, generally equivalent in size and organization, and autonomous in large measure (Service 1963)	No full economic specialization. No full-time religious specialists. "Advanced over bands in multiplication of parts and in integration of parts, but they are not so strikingly advanced in specialization of parts" (Service 1963) Not integrated by localized division of labour and the exchange of complementary goods (Sahlins 1968)	Labour specialization. Contribution not directly related to reward. Whole families, even districts, specialized (Service 1963) Greater specialization of labour evolved. Economy comparatively organic. "Division of labour <u>within</u> a chiefdom may be as great as that <u>between</u> adjacent segmentary tribes." (Sahlins 1968)
LEADERSHIP	"modest informal authority of family heads and ephemeral leaders" (Service 1962)	Charismatic, based solely upon the qualities of the particular person who rises to lead some specific enterprise (Service 1963) Confined in scope to primary community. May be big men or petty chieftains. Big men must use artful means to accumulate wealth (Sahlins 1968)	An "office" of chief. "A position in a socio-political structure that has ascribed functions and conventionalized attributes no matter who occupies it" (Service 1962). Chief directs activity of a chiefdom's interdependent parts. Centralized leadership (Service 1963) Regulations separate the chief from all others; sanctify or otherwise legitimatize him; codify his rights, privileges, duties; prescribe the form of succession. Sumptuary rules or taboos set aside the chiefly persons into a special category (Service 1962) Official authority is considerable. "Not just greater chiefs but a <u>system</u> of chieftainship" A hierarchy of major and minor authorities holding forth over major and minor subdivisions of the tribe (Sahlins 1968)

ATTRIBUTE	BAND	TRIBE/SEGMENTARY TRIBE	CHIEFDOM
POLITY	"no separate political life and no government or legal system above the modest informal authority of family heads and ephemeral leaders" (Service 1962)	No group dominant over another (Service 1963) Politically equal communities - no structural subordination (Sahlins 1968) "Each group, exploiting like environmental opportunities, underwrites, by its ecological completeness, its political autonomy" (Sahlins 1968) Sharply divided into independent local communities (Sahlins 1968) No separate bodies of political control (Service 1963)	Local community a political subdivision. Political organization is established above and beyond the community level (Sahlins 1968)
BASIC STRUCTURE	"a few associated bands made up of related nuclear families" (Service 1962) "every person is one or the other kind of consanguineal or affinal relative (Service 1963) "an association of family residential units...which ordinarily include only from 20 - 60 people loosely allied by marriage ties" (Service 1963)	Segmental. Corporate residential units are like one another, largely self-sufficient economically, generally equivalent in size and organization, autonomous in large measure (Service 1963) Structural and functional equivalency of the primary segments (Sahlins 1968)	Organismic. Differentiated and specialized parts (Service 1963)

TRIBES AND CHIEFDOMS OF THE NORTHWEST COAST: THE TSIMSHIAN CASE

Donald H. Mitchell

Of the several schemes devised to classify cultures and societies, the one of greatest interest to recent students of cultural evolution is that developed in the late 1950s by Marshall Sahlins and Elman Service. It primarily scales society on the basis of some fairly subjective measures of increasing societal complexity -- and particularly with respect to the degree of political integration. The four classes -- band, tribe, chiefdom, and state -- are reasonably well entrenched in the anthropological literature.

That the framework is imperfect is obvious to anyone attempting to classify specific societies. Most notably, it suffers from a lack of precision in delineation of the levels of complexity as no scoring guidelines have ever been articulated for the assessment of what are obviously polythetic sets. Yet the scheme does provide a useful shorthand for the description of societies at different levels of complexity and one can feel comfortable with even the intuitive scaling of a great many societies. Perhaps its greatest importance is that it continues to draw attention to the fact that societies do differ in structural complexity and to keep before us the problem of how these differences relate to the general evolution of culture.

Northwest Coast societies have proven particularly fractious subjects for this classification process, but after Service (1963) the practice has often been to treat them as chiefdoms and to

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characterize them as therefore anomalous among hunters and gatherers. That the nineteenth century Northwest Coast society examined in this paper was unusually complex will not be denied, nor will the view that it and neighbouring Northwest Coast societies were unusual among hunters and gatherers in this respect. But I will argue that this particular society was not at the chiefdom but the tribal level of social complexity and that to characterize it as a chiefdom is to misinterpret its significance for an understanding of cultural evolution.

Tribes and chiefdoms may be distinguished from each other on a number of bases. Although no one has thought these through with the kind of rigour that would be necessary to permit their use in any unequivocal classification of societies, a compendium (see Miller, Appendix A, this volume) is offered, drawn primarily from three sources: Service's 1962 *Primitive Social Organization* and his 1963 Introduction to *Profiles in Ethnology* and Sahlins' 1968, *Tribesmen*.

The data discussed in this paper really only bear on leadership and polity but these would seem of primary importance in evaluating the position of a society on this particular progression of social elaboration. With respect to these characteristics, the tribal and chiefdom levels may be distinguished as follows:

1. For the chiefdom there is an identifiable and continuing office of chief while for tribes there is not;

2. Chiefs have authority over other lesser leaders including some in other communities and, in effect, this means two or more communities come under the control of the one leader who is chief. In contrast, leaders of tribal communities have authority over only their own group -- and this group itself may be only part of a village community.

THE TSIMSHIAN CASE

In the nineteenth century, the Tsimshian occupied a portion of the northern mainland coast of British Columbia centering on the Skeena River, but extending from the Nass River south almost to Milbank Sound. They were comprised of several named village groups or "peoples" who were designated by such terms as Kitkatla, Kitlans, or Kitsumgalum, each term usually meaning the people of some place or area. Each group had several seasonal villages. There were slight variations, but in general many went to the mouth of the Nass River in the early spring for eulachon, to their winter or "principal"

villages for a while after that, then to their salmon streams, and finally back to their winter villages. The groups that are here referred to as the Metlakatla Tsimshian followed this pattern. In summer they were ranged along the lower part of the Skeena River, in winter and late spring they were at Metlakatla Pass and on the coast north of there at a small bay known to the traders as Pearl Harbour, and in early spring at the Nass collecting eulachon. For much of the year the 10 groups that comprised the Metlakatla Tsimshian formed a kind of loose aggregation of settlements.

The Tsimshian have all the trappings of perfectly good tribes. They have matrilineages, including a sort of maximal lineage referred to as a "House," clans, and phratries -- and cross-cutting these are the dancing or performing societies, or "Secret Societies" as some of the literature would have it. There are certainly enough sodalities to indicate we are not dealing with band level society.

Prominent men within a village community are ranked with respect to one another and for inter-village affairs there appears also to have been a ranking of the individuals who participated in potlatches -- in this case, perhaps just lineage heads.

The contention that there was a Coast Tsimshian chiefdom is based mainly on the suggestion that one of these high ranking lineage heads achieved a measure of dominance over the members of lineages other than his own and even over the occupants of villages other than his own. And it is generally considered that he managed to do this through gaining control of the trade between the Hudson's Bay Company at Fort Simpson and groups living up the Skeena River.

During a recent study of the Hudson's Bay Company post journals for Fort Simpson it became obvious to me that they contained information on this "chief," Legaic, and his trading activities -- and while the journal entries do not seem to throw much light on the process by which Legaic gained his monopoly, they do tell us something about when it happened and something about the extent of political integration that ensued.

The period covered by these historical sources runs from 1832 to 1866. Fort Simpson was founded in 1831 on the Nass estuary then moved in 1833-34 to its present location on Tsimpsean peninsula near the entrance to Portland Canal. This was a few miles from Pearl Harbour, where some Metlakatla Tsimshian groups wintered.

By 1840, many, but apparently not all, of the Metlakatla Tsimshian had shifted their winter quarters to the post and by 1852 (perhaps earlier) all seem to be residing at Fort Simpson. That

place continued as their principal settlement for at least a decade until, in 1862-63, the missionary William Duncan took a considerable number of them back to a portion of Metlakatla Pass.

So the post journals provide a reasonably good view of Metlakatla Tsimshian activity for the period 1834-1863 and more remote observations for a few years before and after this. However, we should remember two additional characteristics of that view:

1. It is incomplete in the sense that some journals are missing. We have entries for the periods 1834-42, 1852-53, 1855-59, and 1863-66.

2. The observations are of quite uneven quality. Over this period there are at least seven writers and they differ in their interests and powers of observation.

Despite these problems, the journals do permit examination of several questions concerning Legaic's trading activities and the extent of his control.

1. *What do we learn about the native trade with the interior?*

There are no entries referring to the trade at all until 1836 but from that date on they continue until the journal series ends in 1866. Over that 30-year period are references to trips during 13 of the 18 years for which there are journals. These 13 years record at least 32 different trading excursions up the Skeena River. Seven refer only to Tsimshian trading; ten identify the traders as Gispaxlaots -- the local group to which Legaic belonged and of which he was head; and 15 make specific reference to Legaic as the trader.

In the late 1850s the names of two other individuals turn up. One whose name appears twice, is a Gispaxlaots; the other, whose group affiliation has not yet been discovered, appears once and that is as someone who accompanied Legaic on a trip.

It seems clear that Legaic and his group, the Gispaxlaots, did monopolize the Skeena River trade and that they did so for at least 30 years. Indeed, on October 28, 1840, the post journal makes specific reference to this exclusive privilege: "A canoe with 4 Indians of Illegaich Gang (no other gang of the Chym. tribe being allowed to trade there) arrived from Skeena River." Within that 30-year period the number of voyages recorded reaches a broad peak in the late 1850s although Legaic's own trading trips were consistent at one or two a year from the start.

2. *What do we learn about the question of "chief-ship"?*

From 1832-1839 the sources do not single out Legaic for special attention although he is obviously an important leader. A few examples from the post journals and related documents should make this clear.

1832 -- In Donald Manson's account of his exploratory trip from the Nass to the Skeena he refers to "the two Pearl Harbour chiefs Neeshoot & Cacus" and on his return journey mentions his visit to "all the chiefs" at Pearl Harbour.

March 15, 1835 -- "The three Chiefs started and all their followers left today."

March 2, 1836 -- "Gave a suit of clothes to each of the Chiefs Cockas, Noshoot & Illegayauch."

April 1, 1837 -- "One of the Chymsyan chiefs ... "

June 20, 1838 -- "... one of the chief's Neeselkameek..."

The 1840's record is fragmentary and although Legaic's name comes up, it is never in a context that tells us anything about his status among the chiefs.

From 1852-63 are many references of the sort just quoted.

January 21, 1852 -- "... every chief has left the village ..."

January 30, 1852 -- "All the chiefs still away ..."

May 30, 1852 -- "One of our chiefs arrived back from a war excursion."

June 2, 1852 -- "Nistowack one of our chiefs ..."

February 13, 1853 -- "All the Chimshian chiefs were present."

March 18, 1853 -- "All the chiefs but *one* are still here."

November 19, 1855 -- "One of the 'Kit-lan' chiefs gave a feast of rice to all the 'upper ten.'"

January 10, 1857 -- "Most of our Big Chiefs started in Eight canoes for Sebassa."

March 24, 1857 -- "Camp quite deserted of Chimshians except the Chiefs who are *all* here and will remain."

During this period there are also references that indicate Legaic had a different status than just "one of the chiefs."

August 11, 1852 -- "'Ligyeek' or 'Ilgeth' the principal chief here headed the party."

March 26, 1857 -- "Ilgeth the chief is the most persevering beggar in camp."

During 1859 and 1860 P.N. Compton was at the Fort as a clerk. Much later, in 1878, he provided H.H. Bancroft with a description of the Tsimshian which refers to the Tsimshian at Fort Simpson being divided into 10 tribes each under its own chief, but all owing a species of allegiance to the head chief of the Kishpocholots (Compton 1878:98).

After 1863, Legaic had moved to Metlakatla with Duncan and the two references from this period are from 1866 when there is mention of "The Methlakathla Chief Legaic" (October 15) and "The Chief Legaic" (November 17).

When these and other entries are examined, they disclose a pattern to use of the term "chief." Between 1852 and 1859 there are 30 times that the words "chief" or "chiefs" are used. Twenty-five are of the "a chief" or "one of the chiefs" kind and five of the "the Chief" of "principal chief" kind. All five of the latter refer to Legaic and he is never referred to as just "one of the chiefs." I would conclude that in the period 1840-1851 either Legaic's status changed or else his high rank became apparent to the Hudson's Bay Company traders and I would incline towards the former view.

3. *How much authority did Legaic, and the other Tsimshian "chiefs," have?*

A few incidents and observations from the journals are offered in chronological order. Afterwards we can see what impression has been gained.

July 1, 1837 -- Some Kygarnie (Haida) arrive. "The Chimsyans to whose camp they very foolishly went, felt disposed to take their goods from them and give them just what they chose in return. Legegh done all he could to prevent any disturbance but like all the rest of their chiefs he has no influence among them when interfering with their own interest."

May 30, 1838 -- Five canoes of Tongass arrive. Some Tsimshian go out to meet them to invite them "to their huts." Some other Tsimshian fired on the two parties. Shots were returned by Tongass and by their Tsimshian hosts.

June 20, 1838 -- Shakes visits Legaic; and Quatke, a Stikene chief, visits Neeselkameek or the Crippleman, "chief" of the Kitlans. "When Quatke was here last spring he had a quarrel with Elgegh's people and threw away a large copper a valuable article amongst them, which was reckoned a great insult to Elgegh whose people were now threatening to take vengeance but the above chief interfered and no molestation was offered."

May 26, 1839 -- A number of Skidegates who have been visiting Neestoyogh's people are preparing to leave. "Numbers of the Chimsyans who were friendly to them mixed with them and assisted them to get their canoes in the water and loaded but before they had all embarked they were treacherously fired upon by Neeselcameek's people." In this incident, too, Tsimshian end up firing at Tsimshian.

October 22, 1855 -- Edensaw of the Massets arrives. "He had just landed at 'Ilgeths' house when Cush-what took an axe out of the canoe and split it. Ilgeth could do nothing to stop the fellow... The Chiefs here nowadays have little or no influence and the bad characters do as they like."

July 5, 1856 and October 30, 1856 -- Two incidents of Gispaxlaots and other Fort Simpson Tsimshian firing at one another. In each case Legaic and the other "chief" settle their difficulty.

July 3, 1858 -- "Two canoes of Skidagate people arrive at 'Nistoacks' camp, 'Cascas' people fired into them." Nistoack's people returned the fire, "both parties now went into the 'fun' with a will."

November 28, 1863 -- "Neshwakes (Nistoacks) chief of the Keenahtoicks (Ginadaoxs) came round to the various camps to invite the chiefs to a feast, his canoe was fired at by the Kishpocolats." A two-hour battle ensues.

January 19, 1865 -- "The cannibal chief died at 10 a.m. upon which a great shooting match took place between the Kishpocolots, Kittandaws and Killowtsaas."

These data provide no support at all for the notion that the Metlakatla Tsimshian had achieved a higher than tribal level of political integration. The so-called "chiefs" -- even Legaic, the "principal" one -- appear in these incidents to have very limited authority. And when you find sub-units of the putative chiefdom allied with outside groups and both in armed conflict with other members of the "chiefdom," the case for its existence is exceedingly thin.

CONCLUSION

It seems undeniable that Legaic and his people -- the Gispaxlaots -- had some kind of exclusive right to carry the fur trade up the Skeena River and into the interior. It also seems obvious that Legaic was or became the individual of highest rank among the Metlakatla Tsimshian lineage heads. In this sense he was the "principal chief" of the Tsimshian although he may not have attained this status until the 1840's.

But the contemporary observations of Fort Simpson traders make it seem most unlikely that Legaic headed a political unit that could in any useful sense be termed a chiefdom. He ruled over no group but his own, and even there his hold seems fragile. In short, there was no chief and I would argue that the Tsimshian case provides us with no evidence for a Northwest Coast chiefdom.

A COMPARISON OF SEA-LEVELS AND PREHISTORIC CULTURAL DEVELOPMENTS ON THE EAST AND WEST COASTS OF CANADA

Knut R. Fladmark

The history of past relative sea-level positions must be considered critically important in any effort to understand the evolution of coastal ecosystems and cultures, because a changing sea-level is the only significant environmental variable able to simultaneously, immediately and often drastically affect the nature of both marine and terrestrial ecozones. This paper is a first attempt to compare generalized sea-level sequences and culture histories of the eastern and western seaboards of Canada, to test the role of sea-level induced environmental change in the development of coastal cultures.

Sea-level positions and the velocity and sign of relative elevation changes will have differing effects on coastal biota, both marine and terrestrial, depending on specific details of local physiography, environment and adaptive tolerances of the various species. To precisely determine the productivity and diversity of a coastal ecosystem for any particular sea-level condition one would require at least the following data: Exact values for coastal relief and gradient; exposure; tidal amplitude; water exchange rates; salinity; temperature; nature of the substrate; precise rate of sea-level change; prior sea-level history; pre-existing biota and their ecological relationships, environmental tolerances, reproductive rates and rates of colonization; and many other individual factors. While some of this information exists for contemporary coastlines, virtually none is available for pre-modern

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sea-level positions anywhere. In short, present data simply do not permit detailed or quantified statements of paleo-shoreline ecological conditions for either the east or west coasts of Canada. Indeed, it may be argued that local variability was sufficient to overshadow any possible generalized trend in space or time. Nevertheless, we do know that there have been substantial fluctuations in sea-levels on both coasts since man has been present, which almost certainly had some effects on existing ecosystems and cultures. How can we begin to understand these effects in the absence of data required for detailed paleo-environmental reconstructions? One possible tactic is to model the theoretical gross environmental implications of major changes in sea-level "condition," on coastlines of broadly different type.

At least two major environmental factors are directly influenced by sea-level conditions. These are (1) amount of low-gradient slope or terrain on each side of the tidal line and, (2) degree of equilibrium of littoral and coastal-riverine geomorphic systems. That is, the elevation of the tidal limit at any moment in time absolutely controls the amount and quality of low-lying coastal plain suitable as terrestrial habitat, and the amount and quality of low gradient intertidal zone and shelf suitable as littoral and near-shore habitat, with obvious implications for the nature and productivity of associated biota. Sea-level fluctuation around a newly attained base will affect beach erosion and deposition, as well as aggradation or down-cutting of coastal drainages. The magnitude of these effects will depend on local coastal physiography, and rate of sea-level oscillation.

It is possible to characterize certain coastlines as physiographically "complacent" and others "sensitive" to changing sea-levels. Complacent coastlines would include those with relatively steep, regular slopes, uninterrupted by major changes of gradient within the maximum elevation range of sea-level rise and fall (Figure 1A). On complacent coasts a changing sea-level would cause only regular and directly proportionate shifts in horizontal shore position, and a physiographic transect through the tidal zone at the top of the range of sea-levels would be basically the same as a transect observed at the bottom of the range. Sensitive coastlines on the other hand are those with major changes of topographic slope within the range of possible sea-level elevations (Figure 1B,C). Here, even minor oscillations in marine limits could cause disproportionately large alteration in amount and quality of near-shore and fore-shore habitats as critical threshold levels are passed (Figure 1D) and coastal environments at maximum and minimum sea-level positions might be profoundly different.

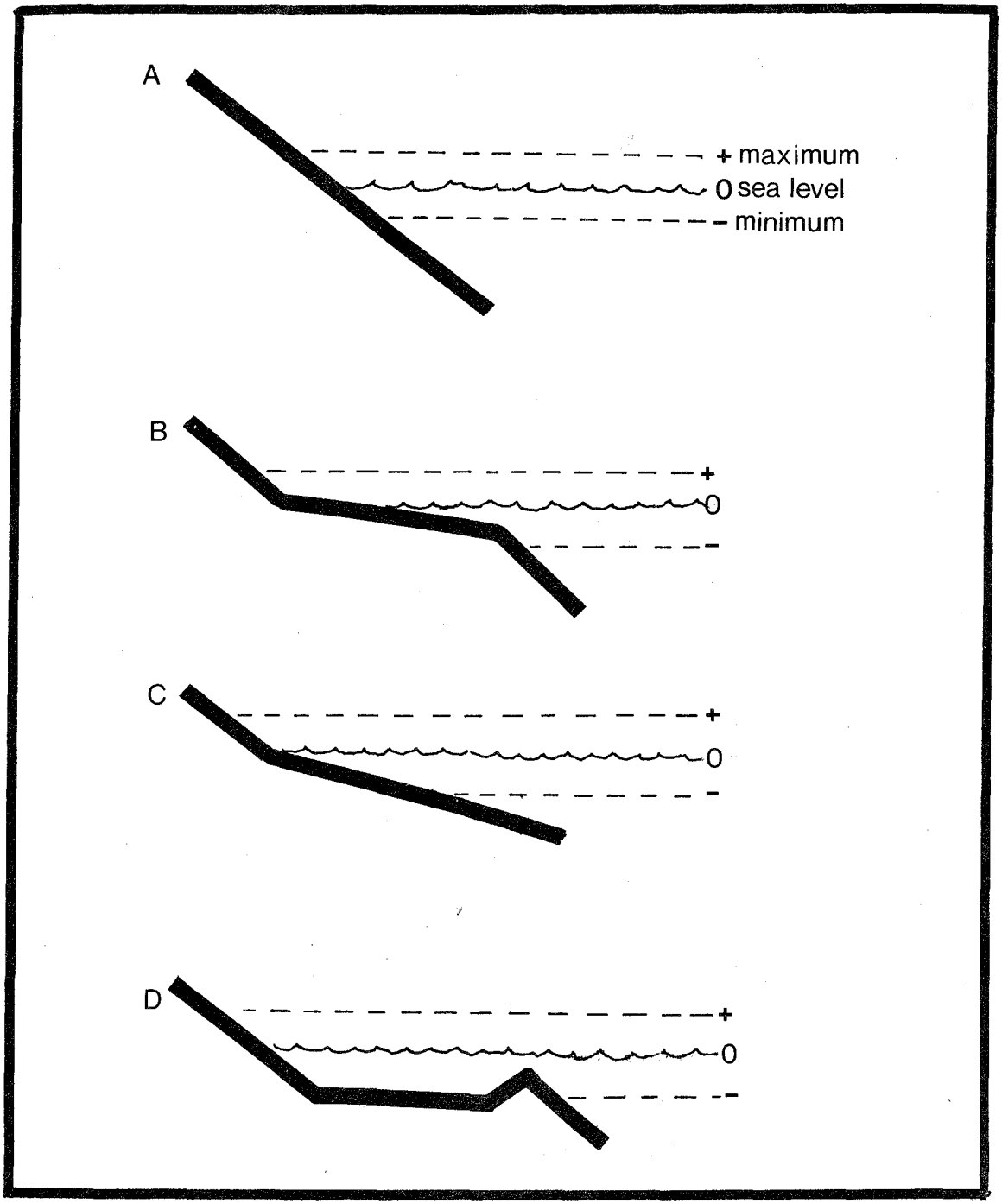

Figure 1. Generalized coastline types: A. Complacent, regular coastal gradient; B. Sensitive, variable coastal gradient; C. Sensitive, low coastal plain, steepening inland; D. Sensitive coastal basin and threshold.

In addition to physiographic considerations, rate of sea-level variation is also important in controlling equilibrium of coastal geomorphic and ecological systems. After attainment of a new sea-level position some length of time must pass before coastal sediment erosion and redeposition reaches equilibrium, particularly on shorelines formed of unconsolidated materials. Coastal drainage systems must also incise or aggrade their channels to attain gradient equilibria with new marine base-levels. When sea-level change continues at a rate beyond the capacity of coastal geomorphic processes to maintain equilibrium, the littoral system will be reduced to an unstable condition. It is easy to envision a situation in which a geomorphically immature shoreline, with scoured or aggrading intertidal zones and uplifting or turbid estuaries would have significantly reduced biotic productivity. At the other extreme it is logical that a coastline which has maintained a quasi-stable state sufficiently long to develop mature, equilibrium shoreline and riverine systems, would be, generally speaking, the most likely context in which to expect a rich climax coastal ecosystem. Of course there has never been a completely stable relative sea-level anywhere in the world, only degrees of instability. For the purposes of this paper I propose to define a quasi-stable sea-level as "prolonged oscillation within $\pm$ 2 m of a mean value." Variation of about 2 m may represent the range of small-scale eustatic modulation at any time, and certainly a spread of *at least* 2 m is encompassed within sources of error in relative sea-level curves. Employing this definition, it is possible to divide any given sea-level sequence into "quasi-stable" and "unstable" portions, with the cut-off formed by any short-term variation exceeding $\pm$ 2 m.

Classification of coastline physiography as "complacent" or "sensitive," and rates of sea-level change as either "quasi-stable" or "unstable," creates a taxonomy of four gross sea-level related environmental modes or "conditions." These in turn may be used to infer generalized ecological attributes, particularly relative productivity and ecosystem stability.

1. *Complacent/Quasi-stable:* A complacent coastline with quasi-stable sea-levels should have geomorphic equilibrium and optimum possibilities for stable ecosystems. Biotic productivity ought to be relatively high compared to other environmental modes, and all other factors being equal, this sea-level condition possesses most potential for attainment of stable, specialized coastal cultural orientations, as a response to stable optimized coastal ecosystems.

2. *Complacent/Unstable:* A regularly sloping coastline experiencing rapid but unvarying rates of relative sea-level change may also be geomorphically and biotically stable in the sense that it will not experience any marked discontinuity in topography, sedimentation patterns, and biotic habitat through time. On the other hand it is unlikely that biological productivity would be as high as during a state of quasi-stable sea-levels.

3. *Sensitive/Quasi-stable:* Geomorphic stability and biotic productivity in this sea-level condition would be highly dependent on mean shoreline elevation and type of terrain intersected. Thus a quasi-stable coast fronting extensive gently shelving foreshore would be much more productive of intertidal resources for instance, than a quasi-stable shoreline high along a steep coastal escarpment. Likewise, even $\pm$ 2 m of sea-level fluctuation around a mean located at a break in slope might produce short-term geomorphic and ecological instability far in excess of the normal quasi-stable condition.

4. *Sensitive/Unstable:* This coastline condition is least likely to attain geomorphic equilibrium and associated ecosystems would probably be sustained well below theoretical climax levels for the area. All other factors being equal, this shoreline type possesses least potential for stable, optimized coastal adaptations.

These four sea-level conditions, crude and generalized as they are, may represent a potentially useful set of predictive models applicable to the prehistory of any coast. Given data on topography and sea-level history, it should be possible to characterize any coastal segment by one of the four shoreline types, and thereby develop tentative inferences about its generalized ecological stability and productivity through time, even though data are not adequate to precisely reconstruct the history of individual species. These inferences could, in turn, be extrapolated to cultural elements of coastal ecosystems, allowing formulation of predictions concerning their stability, specialization and complexity. Where sufficient archaeological data exist, it may also be possible to test such predictions against the actual paleocultural record. In the following section this procedure will be attempted for the east and west coasts of Canada.

Ignoring localized variability, it is possible to describe much of the northern portion of the Atlantic region as a generally "complacent" coastline. This includes the north coasts of

Newfoundland and Labrador where, within the limits of late Quaternary sea-levels, coastal relief is relatively steep and gradients generally relatively regular and continuous. In contrast, the southern Maritime region, including the south-east coasts of Newfoundland and Nova Scotia, must be characterized as physiographically "sensitive." Here, uplands often reach to the present shoreline forming abrupt breaks-in-slope with extensive gently sloping continental shelves and off-shore banks. Even minor changes in relative sea-level elevation in this region could have exaggerated effects on availability of low-lying coastal terrain and its quality as a human habitat. During the early post-glacial transgression, lateral displacement of shorelines and degradation of aquatic and terrestrial habitats must have occurred very rapidly on the outer shelf (e.g., Fairbanks 1977). Later in the Holocene, drastic changes in marine environment could have resulted from the episodic attainment of "threshold" sea-levels, such as that needed to produce vigorous tidal exchange between the open ocean and previous sheltered near-shore basins (e.g., Sanger 1975).

In contrast, a generalized physiographic description of the Pacific coast must acknowledge greater homogeneity, with most of the region falling into the "complacent" shoreline type. Exceptions include scattered outer coastal zones, such as the west coast of Vancouver island and the east cost of the Queen Charlotte Islands, where broad areas of low-lying continental shelf may have been exposed by late glacial minimum sea-levels. However, even here there is no quantitative parallel to the huge emergent shelf and bank areas of the southern Maritimes. Certainly, within the known dated time frame for human presence on the Northwest Coast (i.e., 9-10,000 years) the vast proportion of the shoreline can be described as tolerant or complacent.

Late Quaternary sea-level sequences of the Atlantic seaboard have been described by Grant (1977 a,b, and 1980) and are illustrated in Figure 2. Northwest Pacific sea-level sequences have been summarized by Mathews, Fyles and Nasmith (1970); Fladmark (1975); and Clague (1975) (Figure 3).

Northeastern sea-level curves include those located close to centers of former glacial loading, such as northern Newfoundland, in which sea-level trends are dominated throughout by rapid isostatic rebound. Average rates of uplift fall clearly in the "unstable" category of sea-level fluctuation. According to Grant (1977b, 1980) northern Newfoundland sea-levels attained quasi-stability ($\pm$ 2 m of the present position) about 2800-3000 B.P., although the relative sea-level continued to fall to nearly -2 m by about 1000-2000 B.P. In contrast, curves located far from glacial centers, such as on the

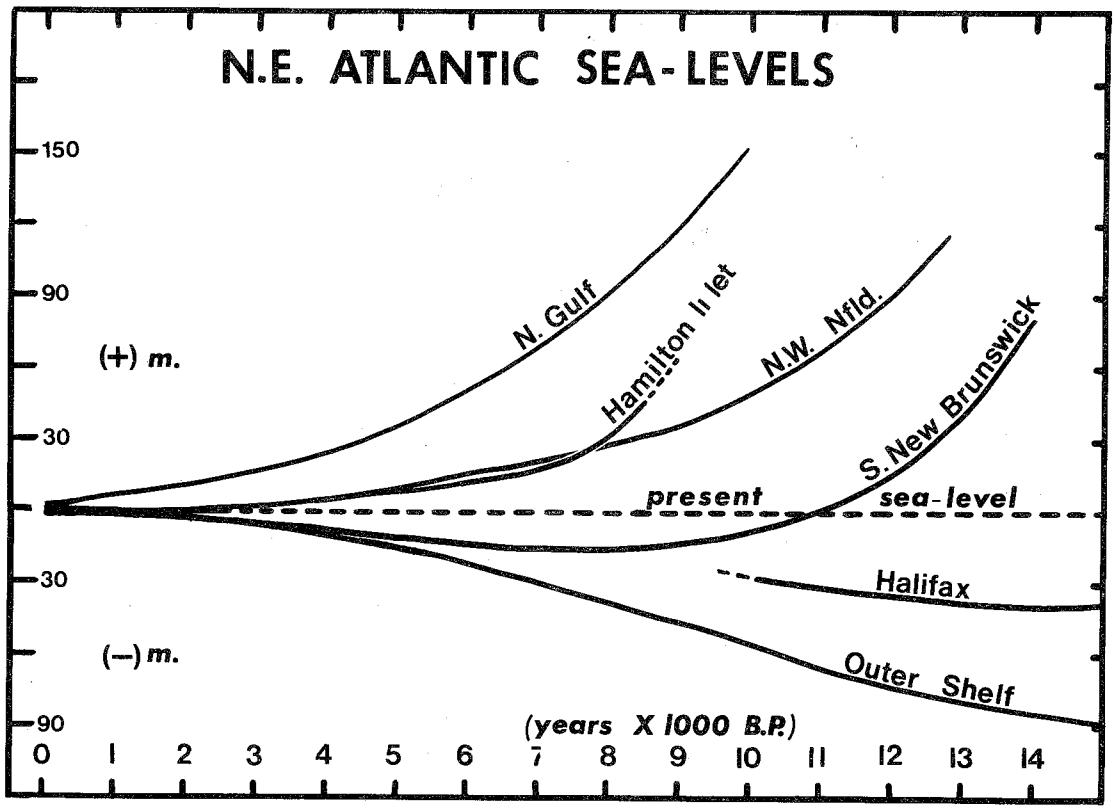

Figure 2. Northeast Atlantic Sea-Levels. (After Grant 1980 and Fitzhugh 1975a).

southern outer continental shelf (Figure 2, Grant 1977b, 1980) are dominated by continuous eustatic transgression following late glacial maximum emergence. Again, rate of sea-level change is clearly within the "unstable" category until the -2 m mark is passed about 1300 B.P.

According to the coastal taxonomy discussed previously, the pre 2-3000 B.P. period of northern Newfoundland must be classed as a Complacent/Unstable shoreline condition, followed after ca. 2000 B.P. by a Complacent/Quasi-stable situation; on the same basis southern outer shelf coastal conditions have always been Sensitive/Unstable, possibly developing quasi-stability after about 1000 B.P. Since the rate of sea-level change in the pre 2-3000 B.P. northern curve does not vary greatly it can be argued that coastal ecosystems remained relatively stable, but probably with sub-climax communities and lower productivity throughout the entire interval. After ca. 2-3000 B.P. increasingly stable sea-levels would have led ultimately to coastal geomorphic equilibrium and climax ecosystems. If coastal cultures paralleled this pattern we would expect: (a) a relatively stable and long-lasting but generalized coastal adaptation pre-dating 2-3000 B.P.: (b) some evidence of culture change or adjustment beginning ca 2-3000 B.P., with (c) after ca.

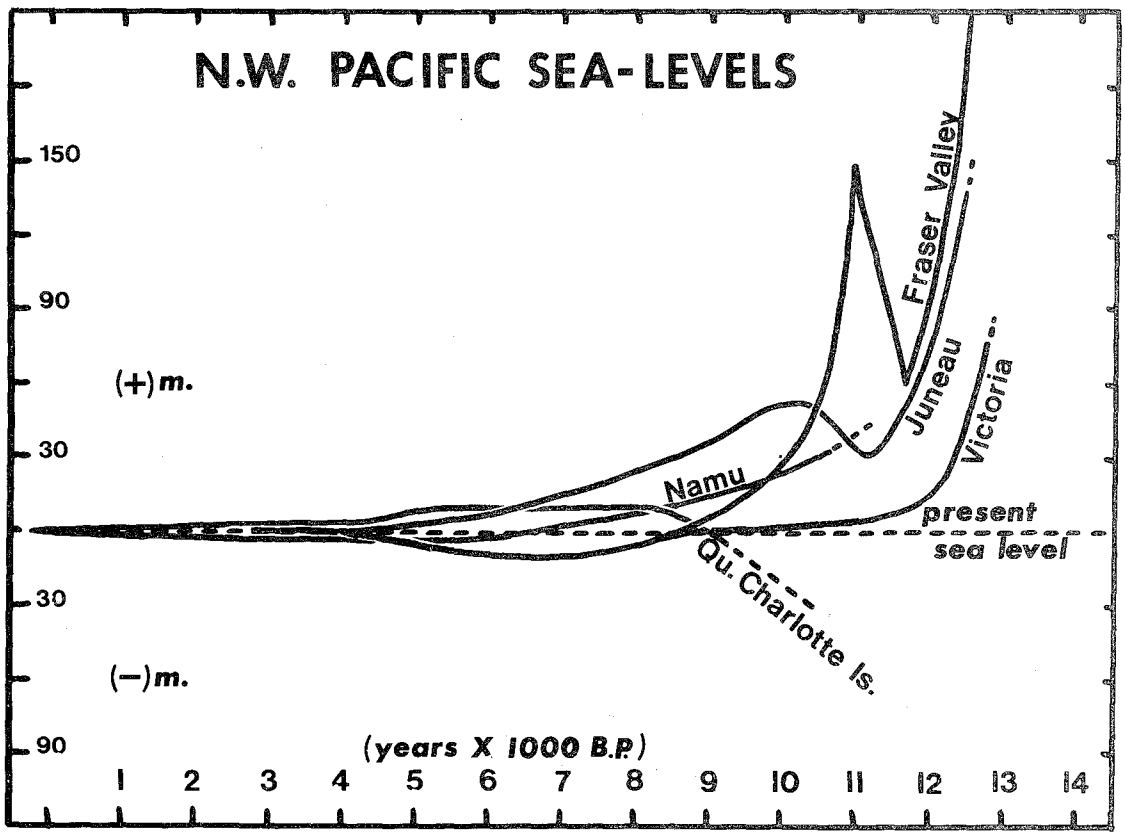

Figure 3. Northwest Pacific Sea-Levels. (After Clague 1975, Fladmark 1975, Andrews and Retherford 1978).

2000 B.P. a developing trend towards increased cultural specialization and complexity as man participated in the optimizing relationships of a maturing ecosystem. These predictions, based on the general model discussed earlier, bear a reasonable similarity at least in part, to the archaeological record of northern Newfoundland and Labrador including: (a) a "Maritime Archaic" tradition dated approximately 7500-3000 B.P. (Tuck 1976a), which exploited various marine and terrestrial resources; (b) its replacement after ca. 3000 B.P. (earlier on the central Labrador coast) by "paleo-Eskimo" or later Indian groups and; (c) the probability that the Dorset, at least, possessed a more specialized (maritime) orientation than the Maritime Archaic.

In the outer shelf region of the southern Maritimes Sensitive/ Unstable sea-level conditions are predicted to have maintained coastal ecosystems at a low level of stability, productivity and carrying capacity. It seems improbable that this would have been a good area for coastal cultural adaptations due to the high rate of lateral shoreline displacement and degradation of terrestrial habitats in a low-lying landscape, but firm archaeological data to verify this prediction are unavailable. For the southern coast of

New Brunswick, Grant (1980) illustrates a quasi-stable sea-level between about 5000 and 9000 B.P., with a mean position of approximately -14 m (Figure 2). A Sensitive/Quasi-stable shoreline condition could be at least as productive an environment as the Complacent/Unstable situation of the northern Gulf of St. Lawrence, but its actual carrying capacity as a human habitat would depend on specific details of shoreline location (etc.) which are not currently available. Between about 4000 and 1300 B.P. the New Brunswick curve exhibits a less stable rate of sea-level rise, paralleling the pattern of the outer shelf. While after ca. 1300 B.P. all southern Maritimes curves stay within $\pm$ 2 m of the present position, their slope is little changed from that of the preceeding ca. 3000 years. Gross cultural inferences based on the southern New Brunswick sea-level sequence would suggest: (a) a potentially stable and productive coastal adaptation ca. 9-5000 B.P., situated well seaward of the present shore; (b) a period of relatively increased culture change, adjustment and instability between about 5000 and 1000 B.P., accompanied by a landward displacement of people as a result of lateral shoreline translocations, and: (c) possibly followed by a trend towards renewed cultural stablity after ca. 1000 B.P. Although there are many gaps in the archaeological record, at least some of the existing information tends to generally agree with these predictive inferences. Thus while there are very few data pertaining to the 5-10,000 B.P. period, between about 5000 and 3500 B.P. there are strong manifestations of a coastally orientated Archaic culture similar to the Maritime Archaic of northern Newfoundland (Tuck 1975c; but see Sanger 1975). The brief appearance of this way of life in inland areas distant from the contemporary shoreline, ending about 3500 B.P., may reflect the final state in the landward dislocation of originally coastal peoples hypothesized above. Cultural changes after ca. 3500 B.P., rather than reflecting solely population replacement (e.g., Sanger 1975), may also be cultural responses to rapidly shifting coastal ecosystems between ca. 5000 and at least 1000 B.P.

Pacific coast sea-level sequences (Figure 3) also reflect the varied interplay of isostatic, eustatic and tectonic factors. Most areas close to the main Coast Range and centers of regional glaciation exhibit extremely high relative sea-levels before about 10,000 B.P., accompanied by rates of up-life in some cases so rapid that significant vertical changes in sea-level would have been readily apparent to any human observers in just a few years. By about 9000 B.P. the curves begin to level out, in some areas staying above the present relative level, and in others dipping slightly (ca. -10 m) below the present "0" elevation. By 4000-5000 B.P. virtually all Northwest Coast sea-levels attained quasi-stability, close to the present elevation. On outer islands distant from major

glacial accumulations, isostatic effects are apparently overshadowed by general eustatic influences, although really only the Queen Charlotte Islands possess sufficient data points to verify this. The Charlottes sequence includes rapidly rising relative sea-levels before 8-9,000 B.P., followed by a period of relatively stable higher conditions until about 4000 B.P., when it declined to a quasi-stable position around the modern level. As noted earlier, excluding some coastal lowlands, off-shore shelves and other localized exceptions, the majority of the Northwest Coast can be generally described as relatively "complacent" in terms of predicted environmental tolerance to sea-level shifts. Thus, for the type of coarse-grained generalized modelling of coastal environmental trends being attempted here, it is sufficient to simply determine episodes of relative stability vs. instability in rates of sea-level change. However, in future studies it would be of interest to take into account the considerable localized variability of Northwest Coast shoreline physiography, and apply the model proposed in this paper to specific inter-locality comparisons.

As indicated in a previous study (Fladmark 1975) most Northwest Coast sea-level sequences reach quasi-stability at or near the present base about 4-5000 years ago, without any later significant fluctuation. Thus, the generalized overall pattern of sea-level change on the west coast of Canada is a pre-4/5000 B.P. period of relative instability, followed by a post-4/5000 B.P. period of quasi-stability. It should be noted that rates of sea-level change are much more rapid early in the Holocene than by 5-6000 B.P., although at least one sequence (Charlottes) suggests an early period of sea-level stability ca. 10-15 m higher than present, between about 8/9000 and 5000 years ago. The local significance of this latter event is not clear, but it may help explain some aspects of the prehistory of the Queen Charlotte Islands which seem out of phase with developments elsewhere on the Northwest Coast. However, in general a shift from unstable to quasi-stable sea-level conditions about 4-5000 B.P. characterizes most of the Pacific region of Canada.

According to the model developed in this paper, 5-10,000 year old ecosystems of the Northwest Coast, existing in complacent/unstable sea-level conditions, were, themselves, possibly relatively stable, but maintained below climax levels of productivity. This was probably particularly true for salmon, which have a spawning success rate easily degraded by fluctuations in stream run-off and sedimentation characteristics. After 4-5000 B.P., attainment of quasi-stable base-levels would have permitted development of equilibrium in coastal shoreline and riverine systems and provided the physical environmental basis for highly productive biotic communities. Therefore pre-5000 B.P. inhabitants of the Northwest

Coast possibly possessed relatively generalized cultural adaptations. These shifted around 4-5000, in response to increased ecological productivity, towards specialized exploitation of certain coastal-riverine resources (salmon), which in turn permitted the development of semi-sedentary settlement patterns, complex social organization, ceremony and other well-known energetically expensive aspects of the ethnographic Northwest Coast. The archaeological validity of this model is still indicated by the profound change in the nature of archaeological sites and their contents about 4-5000 B.P. everywhere on the Pacific coast. All major shell midden accumulations, themselves direct and undeniable evidence of at least seasonally sedentary settlements, post-date the transition between unstable and quasi-stable shorelines, while all older sites are apprently only small lithic dominated encampments, in all aspects (except for some esoteric factors of lithic artifact typology) indistinguishable from non-coastal hunter-and-gatherer sites.

Thus a brief review of sea-level and culture sequences of the east and west coasts of Canada tends to grossly confirm the generalized predictive model generated earlier, relating coastline "tolerance" and rate of sea-level fluctuation to stability of coastal ecosystems and the stability and general nature of associated cultures. In the absence of much needed direct information about the Quaternary history of coastal biotic communities, a brief perusal of a relative sea-level curve may provide a simple and useful initial indicator of the direction and chronology of major cultural-ecological events.

Additionally, direct comparison of east and west sea-level sequences may indicate some general clues to causes of parallel or non-parallel cultural developments in the two regions. While a virtually infinite range of specific environmental parameters might be involved at any given time and place in shaping synchronous cultures of the Atlantic and Pacific shores, sea-level sequences may provide a summary statement of the general state and comparability of whole complex coastal systems in the absence of any more detailed information. From this perspective, at least one marked difference is evident between east and west. Eastern sea-levels exhibit overall a much greater vertical range and rate of relative change over the last ca. 10,000 years than do Pacific examples. This is particularly true in the last 5000 years, when virtually all western ocean relative levels stay close to the modern position, while most Atlantic curves still display wide variation and rapid rates of change. It seems possible, therefore, that unstable shoreline positions may have preressed attainment of climax coastal ecosystems and complex maritime adapted cultures over the last 4-5000 years in the Atlantic area, while on the Northwest Coast contemporary stable sea-levels encouraged development of more complex societies.

ON EXPLAINING CHANGES IN PREHISTORIC COASTAL ECONOMIES: THE VIEW FROM CASCO BAY

David R. Yesner

The importance of coastal economies in prehistory has been recognized for a much longer period in the Old World than the New. Long-term research projects dealing with the origin and development of maritime economies are currently being undertaken in areas such as northwest Europe, South Africa, and the circum-Mediterranean region. In part, this interest may be due to the somewhat greater antiquity of maritime adapatations in the Old World, although our knowledge of this antiquity is relatively recent, and intensive use of marine resources apparently did not occur until late Pleistocene or early Holocene times. More likely, the difference in interest derives from the Old World perception of coastal life-ways as forming a distinct stage or period in prehistory. While this is not universally true -- e.g., in Australasia, Oceania, or the North Pacific Rim, where maritime adaptations were an important part of *late* Holocene prehistory -- in western Eurasia the use of marine resources has been considered a "definiens" of the Mesolithic period, a generalized epipalaeolithic hunting-and-gathering phase preceding the development of agriculture and animal husbandry. In North America, however -- perhaps because of the longer period between the end of big game hunting and the development of agriculture and ceramics -- coastal life-ways tend to be viewed less as a "stage" phenomenon than as local variants of regional hunting-and-gathering sequences. For this reason, it has become somewhat of a struggle to identify common elements in the increasing use of marine resources in North America during mid-Holocene times, e.g., on both the Northeast and Northwest Coasts.

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This is not to say that North American archaeologists have been totally uninterested in the problem of maritime adaptations; coastal sites have been a focus of interest since the late nineteenth century (Trigger *in press*). However, in its earliest phases, American coastal archaeology was primarily involved in refining local culture-historical sequences, because the preservation of certain classes of artifacts (bone tools and even ceramics) was greater in midden sites, and simply because such sites were more visible and often contained potentially larger archaeological samples (perhaps at least partially a result of greater population sizes and annual length of occupation at coastal sites). In certain areas, such as in California, an interest grew in the remains of the marine resources themselves, and what they could tell us about prehistoric coastal economies; however, this quickly degenerated into a largely methodological exercise, as researchers (e.g., the "Berkeley school") literally played in their backyards with various new approaches to midden excavation and analysis. Much of the theoretical concerns involved local questions of prehistoric time depth, population size, and so on; although a few investigators asked more basic questions concerning the nature of subsistence and settlement patterns in coastal environments, most of the concern with ecological and demographic variables involved either methodological experimentation or solving local questions of environmental adjustment. The degree to which these variables played a part in the *origins* of maritime economies was not even considered.

Similarly, in spite of ethnographic accounts suggesting considerable social complexity among some coastal hunter-gatherers -- particularly on the Northwest Coast -- archaeologists did not develop a vision that such social complexity was widespread among prehistoric coastal societies. This may be at least partly attributable to: (1) a perceived homogeneity of middens dating within the last few thousand years, which obscures understanding of population growth and evolutionary change; and (2) the low ratio of artifacts to food remains, resulting in an impression of low cultural complexity. Perhaps for this reason, reevaluation of cultural complexity among coastal hunter-gatherers in California and (more recently) the Northeast has predominantly come from mortuary rather than from habitation contexts. (Although the same site characteristics are also found on the Northwest Coast, it has always been assumed that those sites were part of a sequence leading to the development of the complex coastal societies of historic times.)

There is much reason to suspect that this picture is changing; there has recently been a significant increase in attention given to maritime adaptations in North America. From a theoretical

viewpoint, this may be the result of increasing recognition that intensified use of marine resources in North America may be part of a broader-scale evolutionary picture and not simply the result of the playing out of local ecological adjustments. In addition, from the viewpoint of cultural resource management, coastal sites are receiving significant attention as a set of resources threatened with extinction by natural erosion and coastal zone development.

As a result of this increased attention, traditional assumptions about the population levels, stability, and complexity of maritime hunter-gatherers are being increasingly scrutinized with the use of archaeological data. In addition, environmental and demographic factors are being examined to increase our understanding of both the origins and development of coastal economies. As far as the former is concerned, recent debate among scholars has focused on the relative roles of environment change, population growth, and simply site loss resulting from coastal erosion in explaining the timing of initial use of marine resources in various parts of North America (cf. Yesner 1980a; Perlman *in press*).

Nowhere has the historical disparity between researchers been more apparent, however, than in attempts to explain *changes* in prehistoric coastal economies. The number of factors suggested for the development, as opposed to simply the origin, of maritime adaptions has been over-whelming, even *within* individual geographical regions. For example, in coastal California, changes in the use of shellfish species have been variously attributed to natural changes in species abundance (Nelson 1909; Greengo 1951), species "preferences" (Gould 1956; Warren and Pavesic 1963), or pressure on available resources resulting in over-exploitation (Gifford 1916; Botkin 1980), whether or not induced by human population growth. Rarely have there been systematic attempts to integrate these various types of explanations into a holistic, multivariate theory of maritime adaptation based on human responses to recognized geological and biotic features of maritime zones (Yesner 1980a). Such an approach would involve simultaneous analysis of changes in environment, technology, and settlement pattern within individual geographical regions.

In addition, when examining coastal adaptations, it is insufficient to analyze environmental change simply at the level of broad-scale changes in temperature as reflected in regional pollen diagrams, or broad-scale changes in sea-level reflected in regional sea-level curves. The abundance and distribution of the coastal biota on which humans depend for food -- sea-mammals, birds, fish, and invertebrates (particularly shellfish) -- respond to a wide variety of factors, including water temperature and salinity,

location and extent of nutrient upwelling, and (in the case of invertebrates) availability of appropriate substrate for growth and reproduction. These are factors which are likely to vary substantially even within any given geographical region, since they are affected by local geomorphological configurations. For example, appropriate substrate for shellfish growth is dependent upon intertidal sedimentation which can only be understood in terms of local sediment sources and wave energy, the latter dependent on such factors as submarine topography and shore wave refraction. Similarly, salinity changes that occur under conditions of rising sea-level as a part of the process of estuarine formation and drowning, can only be understood in terms of local subsurface stratigraphy and geomorphology of river basins emptying on the coast. Before the response of humans -- as reflected in archaeological data -- to *broad-scale* changes in environment can be understood, the impact of such localized geomorphological factors must be assessed.

An illustration of this point is a debate that took place between scholars in the pages of *American Antiquity* in the early 1970s, relative to prehistoric cultural ecology of northern New England. Snow (1972), citing in particular data previously obtained by Loomis and Young (1912) from archaeological sites in Casco Bay, southwestern Maine, described changes in shellfish types in these sites over time, attributing them primarily to technological development and changes in species preferences on the part of the prehistoric inhabitants during late Holocene times. Braun (1974), on the basis of archaeological data from the Boston Harbor islands, concluded instead that shifts from species such as oysters (*Crassostrea virginica*), quahogs (*Mercenaria mercenaria*), and bay scallops (*Pecten irridians*) to soft-shell clams (*Mya arenaria*) ca. 3000-2000 yr. B.P. -- roughly corresponding to the transition between the "Archaic" and "Woodland" or "Ceramic" periods -- was the result of a downturn of ocean temperatures in the Gulf of Maine. Sanger (1975) has similarly attributed prehistoric shifts in fish exploitation from swordfish to various modern species to cooling ocean temperatures after the late Archaic period. Both Braun (1974) and Sanger (1975) have interpreted the cooling of ocean temperatures as the result of broad-scale climatic deterioration following the xerothermic maximum of ca. 5000 yr. B.P. as recorded in several regional pollen cores. Compounding this was the southward deflection of the cold labrador Current into the Gulf of Maine after ca. 2000 yr. B.P. (Fillon 1976; Andrews 1972; Yesner 1979, 1980b).

Again, more than simply broad-scale temperature change is involved in understanding changes in utilization of shellfish or

other marine species in northern New England. Species such as oysters not only require relatively warm but also brackish-water conditions, and prefer clayey as opposed to sandy substrates. Therefore, understanding changes in the presence or absence of such species in coastal sites must also involve the analysis of changes in salinity and sedimentation which occur particularly under conditions of estuary formation associated with rising sea-level. This in turn requires reconstruction of changes in paleotopography and sedimentation in the coastal zone itself, as well as the geometry, gradient, and sediment load of streams emptying into particular sections of the coastal plain (cf. Matson 1976 for a similar analysis of the effects of local geomorphological change at the Northwest Coast Glenrose Cannery site).

Beginning in 1980, the University of Southern Maine has been undertaking a major effort to assess the magnitude of local geomorphological impacts on prehistoric subsistence and settlement change in coastal ecosystems, using the Casco Bay region of southwestern Maine as a model. The goal of this effort is to develop a model of *variability* in human adaptation to marine ecosystems, which can then be applied to understanding the effects of broader-scale processes of population growth and environmental change. In order to develop such a model, archaeological sites are examined for controlled comparison, primarily on the basis of whether or not they show changes in shellfish types in the manner described by Snow (1972). Appropriate analysis involves: (1) paleogeomorphological reconstruction of the site environs; and (2) archaeological analyses of changes in human adaptive patterns, focused primarily on faunal remains.

Casco Bay (Figure 1) is the first major invagination of the coastline of the Gulf of Maine, north of Boston Harbor (ca. 43°N. Lat., 70°W. Long.). It contains the so-called "Calendar Islands," supposedly 365 in number, but actually closer to 220 if one discounts unvegetated rocks and shoals. The bay is characterized by very high primary productivity, primarily as a result of strong upwelling patterns (Hurlburt 1970; Hurlburt and Corwin 1970). The upwelling is particularly pronounced in the passes between the islands, where water temperatures of ca. 13°C continue to occur during mid-summer, as cooler offshore waters are transported upward through the water column (Hurlburt 1968). Sediment influx from the Harraseeket, Royal/Cousins, Presumpscot, and Fore Rivers undoubtedly also contributes to the nutrient load (cf. Sutcliffe 1972). One consequence of this high primary production is a high secondary production of various species of fish and shellfish, which in turn support large numbers of seals in the bay (Little 1976; Hurlburt and Corwin 1970).

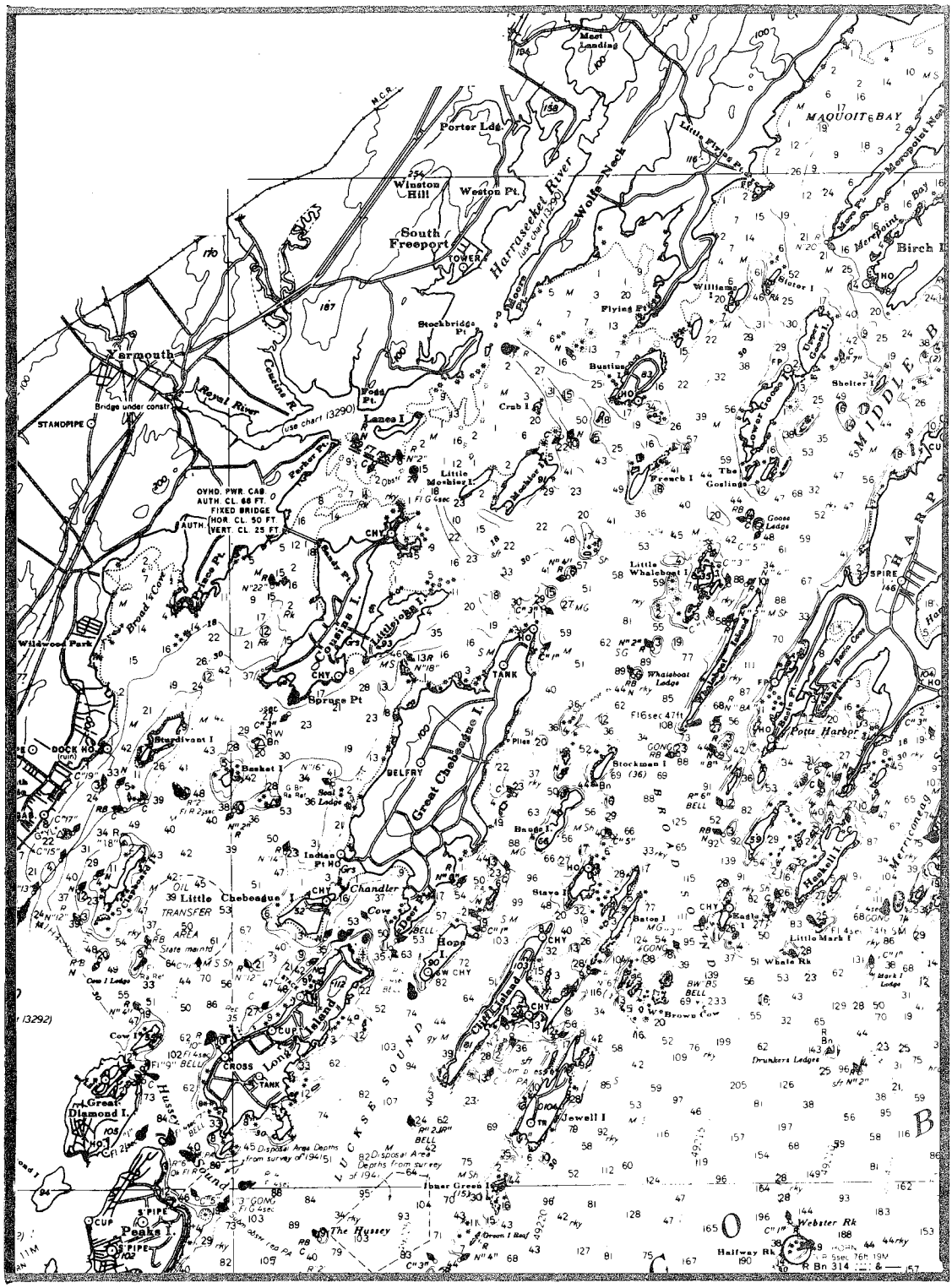

Figure 1. Casco Bay, Maine, and the submarine topography of the area adjacent to Moshier Island.

Sediment sources within Casco Bay, then, derive primarily from two sources: from riverborne sediment load, as described above; and from the reworking of glacial or glaciomarine drift in the coastal zone by (1) wave erosion of unconsolidated aeolian deposits in the marine fringe and (2) wave and tidal landward transport of submerged sediments. With Holocene sea-level rise, substrates available for shellfish growth would have changed as changes occurred in coastal topography (affecting wave refraction) and local river gradionts (affecting riverborne sediment load).

Although early Holocene sea-level rise was very rapid in northeastern North America, and has continued to be rapid in much of the Northeast, several data sources suggest that late Holocene sea-level rise has not exceeded the eustatic rate in the region of southwestern Maine and coastal New Hampshire: modern tidal data (Hicks 1972); comparisons of changing positions of benchmarks relative to sea-level (Tyler *et al.* 1979); basal peat dates from salt marshes (Keene 1971; Nelson and Fink 1978); buried shells in recent sediments (Fink 1977); and drowned intertidal tree stumps (Hussey 1959).

Taken together, these data suggest that a relative still-stand of sea-level may have occurred in Casco Bay after ca. 3000 B.P. One result of this may have been the cutting of rock platforms through wave-base planation of the dipping bedrock primarily on the eastern shores of the islands. With an increase in sedimentation (as the rate of sea-level rise slowed), and possibly an increase in productivity of the Gulf of Maine after ca. 5000 B.P. (Sanger 1975), both the substrate and food base became available for clam flats to form. This may help to explain why the Gulf was unexploited by human populations prior to ca. 5000 B.P., and why shellfish did not become a major item of interest for at least an additional thousand years. In a sense, this argument is analogous to Fladmark's (1975) model relating geological stabilization of the Northwest Coast to the florescence of late Holocene shellmound sites.[1]

In Casco Bay, dating of basal oyster layers in two sites in Casco Bay (White and Moshier Islands) suggests that these layers were deposited between ca. 4000-3000 B.P. (Yesner 1980a). Most of the Casco Bay middens, which are composed nearly entirely of soft-shell clams (*Mya arenaria*), date later than 2000 B.P. Similarly, Braun (1974) found that basal oyster shell layers in Boston Harbor islands middens dated to ca. 3000 B.P. These data suggest the following scenario: with slowing sea-level rise, after 4000 B.P., rock platforms were cut, sediment was deposited, and shellfish became a potential element in the human diet. At this time, water temperatures were sufficiently warm to encourage the

growth of oysters, quahogs (*Mercenaria mercenaria*) and bay scallops where salinity conditions and substrate permitted, i.e., in the estuaries at the margins of the bay. After 2000 B.P., when the Labrador Current was deflected into the Gulf of Maine, cooler ocean temperatures prevailed, and these species were replaced by the now ubiquitous soft-shell clams.

However, this process was by no means uniform within Casco Bay. In particular, sites showing large concentrations of oysters in basal levels overlain by soft-shell clams tend to be found on islands ringing the shoreline, particularly near areas where streams open on to the coast. During the summer of 1980, one such site -- on southwestern Moshier Island -- was selected for detailed examination.

Maps of submarine topography in the area of the bay adjacent to Moshier Island (Figure 1) show the presence of a submarine channel very close to, and terminating near, the site area. It appears that this channel may represent the previous seaward extension of the Royal/Cousins River system. Today, there is only a metre of water covering the area to the west of the island at low tide (Figure 1), whereas deep, open ocean is exposed to the east. If sea-level were ca. 1 m lower at 3000 B.P., consistent with the slow rate of sea-level rise in Casco Bay described above, there may well have been a river mouth close to the position of the site. This would have provided a source of brackish water to support the oyster populations exploited by the local inhabitants.

In order to substantiate this hypothesis, of course, it is necessary to determine the nature and depth of the local sedimentary environment during late Holocene time, i.e., to determine whether riverine sediments are overlain by marine sediments and at what time period. To this end, during the 1981 field season a series of sediment cores were taken in coves directly off Moshier Island, as well as in the large, shallow embayment that separates the island from the modern coast. Hopefully, analysis of these cores will allow us to delimit the courses and depths of ancestral river valley systems as well as to delineate the patterns of Holocene sediment erosion and accretion. In addition, core samples will eventually be examined for marine, brackish, and fresh water diatoms, as the most sensitive index for salinity changes accompanying local coastal evolution.

These shifts in shellfish exploitation accompanied changes in the technological inventory of the prehistoric occupants of Casco Bay. Data from survey and test excavation in Casco Bay suggest that the first intensive use of this coastal and insular zone was made by

Late Archaic peoples bearing a "small stemmed point" culture (cf. Tuck 1978), which Snow (1980) has recently termed the "Mast Forest Archaic." As on the island of Martha's Vineyard (Richie 1969) -- and elsewhere in southern New England (Dincauze 1975) -- this occupation includes a mixed assemblage which is characterized by small stemmed projectile points, eared points, plummets, whetstone fragments, and red ochre deposits. Although the Moorehead Phase (Maritime Archaic) culture is well represented immediately to the north and east of Casco Bay, the area evidently represents a boundary zone for this cultural tradition. While both Middle and late Archaic manifestations are highly visible in the Sebago Lake region to the north, the suddenness of the apparently widespread appearance of this culture -- dated at ca. 3850 B.P. at the Great Diamond Island site -- suggests the possibility of an immigration from the south, and lessens the likelihood that earlier coastal occupations existed but have simply been erased by rising sea-level. Evidence of a 3 x 3 m ovoid semi-subterranean house with a central hearth resting on bedrock was found in association with this occupation at Moshier Island during 1981. Also clearly in evidence is the Terminal Archaic "Susquehanna" tradition, reflected at Moshier Island by an apparent cremation burial associated with a basal fill of black soil and a cluster of broken broadpoints; this was located adjacent to a large pit with a floor containing beach gravel and red ochre, as well as several postmolds. This feature yielded a date of ca. 3400 B.P.; however, as at Martha's Vineyard (Ritchie 1969), this feature cannot be stratigraphically separated from the small stemmed point features, and is clearly associated with the same basal oyster-quahog shell stratum. Thus, whether or not the Susquehanna culture represents an additional intrusive migration from the south, little difference in coastal ecological adaptation is suggested.

Paradoxically, while later Woodland (Ceramic) cultures of coastal Maine are universally thought to be derivative from the Susquehanna tradition -- a notion which the data from Casco Bay would tend to support -- this transition appears to have been marked by substantial ecological change. Unlike elsewhere on the Maine coast, the Early Woodland period is well represented in Casco Bay by substantial amount of "Vinette I" (thick, grit-tempered, exterior/interior cordmarked) pottery, dated at Great Diamond Island to ca. 2300 B.P. At this time, an initial shift was made to the exploitation of soft-shelled clams -- at first very small in size -- as well as substantial numbers of mussels deposited in thin but extensive bands, and some sea-urchin remains. Following this, an intensive concentration on soft-shell clams is marked by extensive deposits of very large specimens, which gradually decrease in size through the Woodland period. The most parsimonious interpretation

of the evidence would suggest the evolution of an estuary, with sea-level rise initially removing the warm, brackish-water conditions that formerly supported an oyster/quahog/bay scallop assemblage; this was followed by a more rocky intertidal zone, supporting mussel, sea-urchins, and small soft-shell clams; and finally a soft-shell clam economy became prevalent in Middle to Late Woodland times. These events occurred within a backdrop of cooling oceanic temperatures, that may be reflected in the shift from Terminal Archaic to Early Woodland coastal exploitation patterns.

In order to test this hypothesis archaeologically, it becomes necessary to analyze categories of faunal remains other than shellfish alone. In this regard, interesting corroborative information comes from analyses of fish remains at the Moshier Island site (Hedden n.d.). This information suggests that originally the populations concentrated on species such as sturgeon, which were trapped at river mouths; later there appears to have been a shift toward exploitation of cod and other deep water fish. In sum, both the fish and shellfish remains primarily suggest human adaptation to the evolution of a local estuary, followed by the drowning of that estuary by continued sea-levels rise, within the context of broader-scale paleotemperature change. Seal hunting continued to be important throughout this period, as indicated both by faunal remains and by bone tool inventories (i.e., multi-barbed harpoons).

What of the remains of the terrestrial species also found in these sites? Several researchers have recently argued that, in fact, most coastal hunter-gatherers are highly dependent upon terrestrial food resources. Even on offshore islands, those closer to the coast (which also tend to show the greatest changes in marine food utilization) would be likely to show greater use of terrestrial resources. It must be realized that the degree of sensitivity of these species to environmental change differs radically from the marine species, and reflects primarily broad-scale temperature patterns -- of the type likely to be reflected in regional pollen curves -- rather than local geomorphological change. In a sense, contrasting changes in the frequencies of marine and terrestrial species helps to calibrate the relative strength of the two different underlying processes of species change.

Casco Bay is particularly fortunately situated for such studies. It is "ecotonal" in character, lying at the western boundary for coastal spruce forest (Westveld *et al.* 1956; Davis 1966). Spruce dominates the vegetative assemblages on islands of the eastern part of the bay, while deciduous forest dominates islands of the western part of the bay. The region forms a "tension

zone" for boreal and deciduous species, with implications for shifts in prehistoric faunal ranges. In this regard, the increased percentages of moose (relative to white-tailed deer) in late Woodland layers of Casco Bay sites seem to reflect a trend toward "borealization": the southward movement of spruce forest indicated by regional pollen diagrams (e.g., Bostwick 1978). However, terrestrial snail remains studied from the Great Diamond Island site (Barber n.d.) suggest that deciduous forest dominated the islands from the time of their earliest occupation until relatively recently.

To this point we have considered exogenous environmental change as the primary factor underlying observed changes in species frequencies in these sites. To what degree might changes in the utilization of both marine and terrestrial foods also be attributed to local growth of human populations and consequent pressures on resources? Decreasing size of soft-shell clams throught the Woodland period in Casco Bay sites -- a pattern similar to that in California middens described by Tartaglia (1976) and Botkin (1980) -- seems to indicate continued pressure on these resources. Whether this constitutes overexploitation of the resources, however, is difficult to judge, since shifts in a group's settlement pattern may well have allowed for periodic exploitation of alternate coastal and insular locales, temporarily abandoning sites for periods of time and thereby allowing the resources to regenerate. This phenomenon would be nearly impossible to detect archaeologically (Bailey 1981; Perlman 1982). For this reason, I would suggest that we turn to other sources of information to determine whether the observed economic changes may have been the result of local population growth and pressure on resources. Following Cohen (1975), one test for the latter might be based solely on whether increased utilization of more marginal environments was occurring over time. Island environments are particularly suited for such a study, since it is possible to determine through accurate dating of sites whether or not more intensive use is made over time of smaller islands, those further from the coast, those that contain fewer microhabitats for fishing or shellfish collecting, and those that are further from locations of bird colonies or sea-mammal rookeries. To date, our data suggest that, like the Boston Harbor islands to the south (Luedtke 1980), more intensive use was made over time of marginal habitats on the Casco Bay islands, possibly reflecting regional population growth, at least through Middle Woodland times.

CONCLUSION

In sum, in order properly to understand the meaning of species changes in coastal archaeological sites, one must be able to exercise control over the following:

1. The impact of local changes in coastal geomorphology on marine biota. In terms of shellfish, this involves changes in salinities and available substrate for implantation and growth. In terms of fish, this involves changes in local productivities as well as the position and geometry of river systems emptying on the coast.

2. The impact of broader-scale temperature changes as reflected in regional pollen diagrams. The degree of sensitivity of local human populations to this factor relative to smaller-scale changes in coastal evolution can be ascertained partly through studying remains of terrestrial species in coastal sites.

3. The impact of human population growth and pressure on available resources. This requires not only intensive study of faunal remains from coastal sites, but also independent sources of information from site locations and distributions.

This methodology would apply equally to the Northeast and Northwest Coasts of North America (although to the south of Casco Bay, the advent of horticulture in late prehistoric times becomes a complicating factor). Clearly, both Northeast and Northwest Coasts demonstrate a general phenomenon of coastal stabilization leading to increased importance of anadramous fish and shellfish resources, although in both cases the nature of the shift was complicated by local geomorphological factors and, in the case of northern New England, by oceanic temperature change as well. Nevertheless, the Northeast coast does appear to show the same basic long-term trend of population growth that characterizes the Northwest Coast (cf. Barber 1980). Unfortunately, we still know little about the nature of aboriginal socio-political systems of the Northeast Coast that were supported by what was apparently a rich coastal hunting and gathering lifestyle. However, once we begin to perceive the fact that many of the same broad-scale patterns apply to the prehistory of both the Northeast and Northwest Coasts, it should become easier to isolate the extent to which specific parallels -- either archaeological *or* ethnographic -- might be applied to either region.

FOOTNOTE

1. Fladmark's data also suggest that geological stabilization of the Northwest Coast led to establishment of the modern anadromous fish runs, contributing to population growth and the rise of the ranked societies of the ethnographic present. Similarly, on the Northeast Coast, stabilization of the coastline in mid-Holocene times may have resulted in the development of effective anadromous fish runs, exploited inland primarily during the late summer and fall, as well as coastal shellfish beds, exploited primarily during the winter, spring, and early summer months (Bourque 1973; Sanger 1979g; Yesner 1980b).

CULTURAL COMPLEXITY IN MARITIME CULTURES: EVIDENCE FROM PENOBSCOT BAY, MAINE

Arthur E. Spiess
Bruce J. Bourque
Steven L. Cox

INTRODUCTION

Modern professional archaeology in Maine is in its second decade, and only recently has begun to produce syntheses beyond the culture-historical sequence. This synthesis is based largely upon data from sites in Penobscot Bay, including the Turner Farm site on North haven Island, excavated by Bourque between 1971 and 1981 and still undergoing analysis by Bourque, Spiess and others.

The "rockbound" coast of Maine is characterized by a series of large and small drowned river valleys trending generally north-south (Figure 1). Penobscot Bay is the largest such feature, over 20 miles wide at the mouth and 30 miles long before narrowing to the modern estuary. Deer Isle forms the eastern margin of the Bay. North Haven, Vinalhaven, and associated islands (the Fox Island group) form an approximately 100-square-mile area of land and protected waters at the mouth of the Bay. At present the Penobscot Bay coast is covered with mixed coniferous and deciduous tree growth dominated by spruce.

The Turner Farm site, a shell midden, is located on Fish Point on the south side of North Haven Island, midway along the Thoroughfare, an east-west trending deep-water passage that separates North Haven and Vinalhaven. An unknown portion of the

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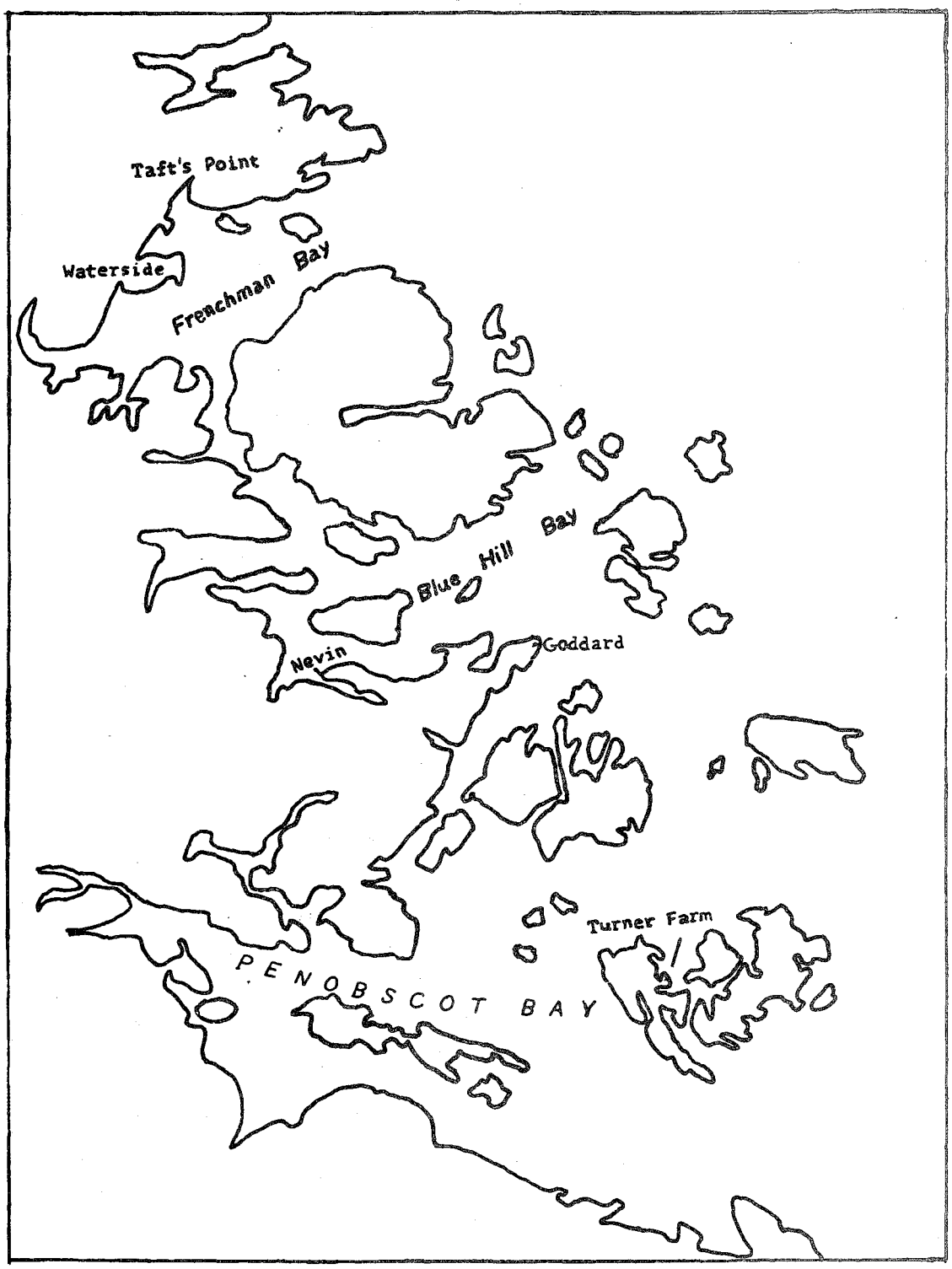

Figure 1. Penobscot Bay and the Turner Farm Site, Maine.

site has been eroded, and other portions are submerged. Bourque has excavated 4,500 square feet (about 15%) of the remaining site area.

EARLY OCCUPATIONS

Our only indications of Paleoindian (12,000 to 10,000 B.P.), Early Archaic (10,000 to 8000 B.P.) and Middle Archaic (8000 to 6000 B.P.) use of what is now the Maine coast are a few isolated finds of diagnostic artifacts. However, subsidence of the land surface along the entire Maine coast has submerged earlier coastlines, and presumably prehistoric coastal sites as well. Paleoindian remains have been found with some frequency near the present coast of northeastern North America, at such locations as Bull Brook, Massachusetts; Quaco, New Brunswick; and Debert, Nova Scotia. However, none of these finds have been in contexts suggesting coastal exploitation *per se*. Evidence from the Early Archaic period is only slightly more abundant along the coast, including a few near-coastal sites in Massachusetts (Dincauze and Mulholland 1977) and two or three point finds in Maine. Middle Archaic presence on the New England coast is indicated by occasional artifact finds in or near surviving multicomponent sites from Martha's Vineyard, Massachusetts (Ritchie 1969) to at least as far east as Penobscot Bay (Bourque 1971; Bourque and Cox 1981).

An indication of the effects of coastal subsidence has recently come to our attention. In an area off Lazygut Islands near Deer Isle commercial scallop draggers working in about 25 feet of water at low tide have recovered large (to approximately 14" in length) oyster shells (*Crassostrea virginicana*) and several artifacts: a ground slate semilunar knife (Rice 1979), two pecked stone mauls or celts, a felsite biface, and rumored other artifacts. No direct association between the oysters and artifacts has yet been proven. The scallop fishermen state that oyster shells are often brought up along a several-mile stretch of water extending north from the Lazygut Island area to Deer Isle. Examination of the area's bathymetry indicates a drowned channel in the area, possibly the former estuarine habitat for the oysters. A radiocarbon date of 6100 ± 65 (SI-4650) has been obtained on one of the oyster shells, which seems consistent with our current estimates for the age span of the one chronologically diagnostic tool form recovered, the semilunar knife. Thus, it is possible that Middle Archaic coastal occupation was associated with oyster-producing areas, perhaps resulting in shell midden deposits. A similar association is reported for Early and Middle Archaic on the lower Husdon River (Brennan 1974). Oysters are not present today in Penobscot Bay, but their availability during the Hypsithermal would be in accordance

with the reconstruction of Gulf of Maine paleo-environment detailed below.

The oldest known surviving component on the central Maine coast is Occupation I at the Turner Farm, which dates ca. 5000 B.P. (Bourque 1976). The component lies at the base of a 4500 year stratigraphic sequence which will be discussed in subsequent paragraphs. Occupation I is characterized by a series of small stemmed points, many of quartz. Typologically similar artifacts are found in the small stemmed point assemblages at the Davis-Tobie site, Sheepscot, Maine, the Neville stie, Manchester, New Hampshire (Dincauze 1976), and numerous sites of southern New England (e.g. Ritchie 1969). A very small faunal sample pertaining to Occupation I includes soft-shell clam (*Mya arenaria*), two deer bones (*Odocoileus*), a seamink jaw (*Mustela macrodon*), and a post-cranial fragment of swordfish (*Xiphias gladius*). The occurrence of swordfish in Occupation I antecedes the abundant remains of this species in the next Turner Farm component, Occupation II, described below. However, it should be noted here that swordfish remains of possibly equivalent antiquity have been recovered from the Seabrook Marsh site in coastal New Hampshire (Robinson 1977).

THE MOOREHEAD PHASE

The next identifable archaeological manifestation in the central Maine coastal area is the Moorehead phase (Bourque 1971, 1976), which spans the period c. 4,500-3,700 B.P. in Maine. Occupation II at the Turner Farm falls near the beginning of the Moorehead phase, with radiocarbon dates ranging between 4,555 and 4,390 B.P. Artifacts recovered from Occupation II strata include a large variety of piercing weapons made of mammal bone and swordfish sword, numerous stone plummets, pecked and ground adzes and gouges, abundant pecking (or hammer) stones, and a small series of long, narrow-stemmed chipped stone bifaces. Typological parallels and in particular decoration of several bone artifacts link this assemblage quite closely to some of the other habitation and cemetery sites of the phase.

Cemeteries of the Moorehead phase, known as "Red Paint" cemeteries, have long been the most visible and famous archaeological remains in the state. Most were carelessly excavated during the early years of this century, but sufficient data from them remain to outline their main characteristics. Though variable in size, they often included more than fifty interments. Burial styles included flexed, extended and probably bundle burials. Burial ceremonialism apparently did not include cremation, though fires were ignited over some graves. Red ochre was included in virtually all graves and

many were well furnished with both utilitarian and non-utilitarian grave goods. In addition to tools similar to those described for Occupation II at the Turner Farm, grave furnishings included ground slate bayonets, marine mammal and fish figurines, and other objects of indefinite function, often beautifully executed in stone and bone. Some cemeteries contain flaked stone bifaces of exotic origin, notably tapered stem points made of Ramah chert from sources in northern Labrador and a series of eared stem points made of exotic lithics of unknown source.

The known distribution of Moorehead phase sites suggests that a variety of environmental zones were exploited by the population. On the coast, substantial habitation components have been identified at the Taft's Point and Waterside sites in Frenchman's Bay, at the Goddard and Nevin sites in Blue Hill Bay, and at the Stanley site on offshore Monhegan Island (Bourque 1971, 1975; Sanger 1975). Cemeteries are also known from the coast, but are more numerous along the estuaries and lower reaches of rivers and their major tributaries from the Kennebec drainage eastward. It now appears that many cemetries occur near relatively small Moorehead phase camps, and taken together these interior sites suggest a seasonal focus upon anadromous fish resources on navigable river stretches.

Earlier excavations in the coastal components have left us little clear data regarding artifact associations and faunal exploitation patterns. Therefore, a major objective of the Turner Farm project has been to address issues of resource exploitation patterns and seasonality. Spiess has analyzed all mammal and bird bone and has supervised the analysis of fish bone from the Turner Farm. Shell analysis is not yet complete. Seasonality investigation techniques included observation of tooth eruption in young Cervidae (*vide* Spiess 1979, for methodology); tooth sectioning (Bourque, Morris and Spiess 1978); shell sectioning (Hancock, n.d., Kennish, Lutz and Rhoads 1980); observations of growth states in fish vertebrae and otoliths (Williams and Bedford 1975); observation of medullary bone deposits in bird longbones (Rick 1975; Taylor 1970); and species presence/absence.

During Occupation II times, heavy emphasis was placed on offshore fishing for cod during late summer and fall and swordfish during the summer. By late fall the economic focus had shifted to deer hunting, which continued to be the economic mainstay into April or May. There is an apparent hiatus in deer hunting from May or June into September or October. Moose, bear, beaver and seal hunting, bird hunting (for great auk, loons, ducks, and geese), and fishing for species other than cod and swordfish are all definitely of secondary importance.

Although swordfish were apparently taken sporadically during later occupations, there are an order-of-magnitude more swordfish rostrum fragments, and at least three to five times more swordfish postcrania, in Occupation II strata than in any later ones. Likewise, the relative importance of codfish is three to five times greater in Occupation II than in any later occupations. By contrast, bird bone in Occupation II strata is only 1/2 to 1/5 as common (compared with all mammal hunting) as in later occupations, and the relative frequency of seal bones is a whole order-of magnitude lower in Occupation II. Moderate use was made of shellfish (*Mya arenaria, Mylitus edulus*) during Occupation II, although quantification has not been completed.

Definite evidence for the use of the Turner Farm site in April or May exists in the form of a foetal/newborn deer jaw, and a great auk longbone with medullary bone desposits. Fishing in May-June can be certainly demonstrated for tomcod (*Microgadus*); however, general intensity of site use appears to have been low during late spring and early summer. Seasonal interpretation of cod fish vertebral growth rings suggests that the codfishing intensified by September, peaked in October-November, and probably continued into early winter. The codfishing pattern gives us an impression that storage of cod for winter use may have been important.

The April/May/June period coincides with the heaviest anadromous fish runs in Maine rivers (principally shad, then alewife, then Atlantic salmon). The low frequency of Occupation II bone attributable to the late spring at Turner Farm, combined with the presence of Moorehead phase riverine sites, suggests that at least part of the population moved inland to fish during the spring.

A number of other Moorehead phase coastal sites, including Taft's Point, Waterside, Nevin, and Goddard, have produced swordfish remains. However, faunal collections from most of these sites have not been adequately quantified or analyzed. The Goddard site, located on an exposed point of land in Blue Hill Bay, contained a large Moorehead phase component but produced little bone from the period due to a lack of shell in the site and consequent poor bone preservation (Bourque and Cox 1981). A sample of about a dozen identifiable calcined bones from Moorehead phase features produced roughly equal counts of deer and swordfish postcrania, suggesting vertebrate faunal exploitation patterns similar to those at Turner Farm.

The Stanley site on Monhegan Island (an isolated island 15 miles off the coast of Maine and southwest of the Turner Farm) has also yielded an important Moorehead phase component in a non-shell-

midden context (Sanger 1975:62). Swordfish remains, both rostrum and postcrania, are extremely abundant at the site, and a date of 3750±80 B.P. (SI-1532) has been obtained on swordfish vertebrae. The abundance of postcranial swordfish remains at the Stanley site, and their relative scarcity in Occupation II at Turner Farm, indicates that much swordfishing may have been undertaken from relatively offshore marine exploitation camps like Stanley involving only the more able bodied members of the community, with meat and sword (an industrial raw material) being returned to the base camp. Early season codfishing may have begun at these offshore sites, with a move to more protected inshore waters during the fall.

At present, the available data suggest that between ca. 4500 and 3700 B.P., Moorehead phase populations were rather strongly oriented toward coastal resources. Sites such as Turner Farm, and possibly Waterside and Taft's Point, were multi-seasonal villages, perhaps best regarded as base camps. The numerous estuarine and interior camps seem to be oriented primarily to anadromous fishing, presumably in the spring, though some use as fall or winter camps cannot be ruled out. Thus, Moorehead phase settlements were probably divided between relatively permanent coastal multiseasonal villages and relatively transient interior and offshore sites situated for advantageous access to special resources.

The Moorehead phase has been included within the Newfoundland/Labrador Maritime Archaic tradition by several researchers (Tuck 1971; Snow 1980), in part on the basis of implied similarities in subarctic maritime hunting strategy (seals, other sea mammals and caribou supposedly) between Maritime Archaic and the Moorehead phase. Data from Turner Farm and other sites directly contradict that hypothetical subsistence parallel. Sea mammals were relatively unimportant in the Moorehead phase economy, and caribou have played no demonstrable part in prehistoric subsistence patterns of the central Maine coast since at least 5000 B.P. This lack of congruence has caused most Maine workers to reject the Maritime Archaic appelation for the Turner Farm Occupation II and related sites, although not all have agreed upon the appropriateness of the more provincial term "Moorehead phase" used herein.

SUSQUEHANNA TRADITION

Occupation III at the Turner Farm is a component of the Susquehanna tradition (Witthoft 1953; Dincauze 1968, 1972; Bourque 1975, 1976). In coastal Maine, this manifestation appears to have replaced the Moorehead phase as far east as Penobscot Bay around 3700 B.P. Dincauze (1975), Bourque (1975), and Sanger (1975) have

hypothesized that this change represents a population replacement. The Turner Farm is currently the easternmost well-defined component of this tradition, though it is known to have extended, perhaps in attenuated form, as far east as the mouth of the St. John River in New Brunswick.

The technological changes between Occupations II and III are marked. Unlike Occupation II, the Occupation III midden yielded virtually no bone artifacts. The most abundant tools recovered are broad, straight to expanding stem bifaces which fall within the morphological range defined for the tradition south of Maine by Dincauze (1968, 1972).

Faunal remains indicate that deer (*Odocoileus*), moose (*Alces*) and bear (*Ursus americanus*) were staples during Occupation III. Seal were taken twice as frequently (relative to all other mammal bone) at Occupation II, with seasonal data indicating that both grey and harbor seals were generally taken at rookeries during pupping/mating seasons. Birds were also twice as frequently taken relative to mammals as in Occupation II, and shellfish were collected in abundance. Some swordfish, sturgeon and other fish species were also taken. In sum, we reconstruct Occupation III at the Turner Farm as the least marine-oriented occupation of the site. Seasonality data strongly suggest year-round use of the site, though fluctuating group size and even brief periods of total abandonment cannot be ruled out. The presence of Susquehanna tradition components at a number of interior riverine and lacustrine sites indicates the possibility of a spring focus on anadromous fish resources.

The Susquehanna tradition, like the Moorehead phase, includes well defined cemeteries with elaborately furnished graves. However, these burials and their contents are strikingly different from those of the Moorehead phase. Such a cemetery was encountered and partially excavated in a portion of the Turner Farm site. Burial forms included primary interments, secondary bundle burials and, most numerous, secondary cremation deposits. Burial furnishings included flaked bifaces like those from the midden, a variety of distinctive flaked and ground adzes and gouges, and, in surprising contrast to the midden, a wide variety of unique bone ornaments, tools and weapons, better preserved here by the high shell content of the midden than at other known Susquehanna tradition cemeteries.

CERAMIC PERIOD

The Susquehanna tradition occupation of central Maine appears

to have been short-lived compared to its tenure further south, ending ca. 3400 B.P. or slightly earlier. The millenium which follows is perhaps the least understood in the region's prehistoric sequence. After 2500 B.P. the number of known sites show a steady increase, perhaps reflecting a real population increase. Maine researchers include the subsequent prehistoric sequence in the Ceramic period. The term "Woodland," widely used for this period elsewhere in the northeast, is eschewed by Maine researchers because of its implications of agriculture, sedentism and Adena-Hopewell ceremonialism. In Maine, agriculture apparently never penetrated further east than the Kennebec River during prehistoric times, and a continuing primary reliance on hunting and gathering activities is indicated for the entire region by both archaeological and ethnohistoric data.

The Ceramic period in Maine is commonly subdivided on the basis of ceramic styles: early (ca. 2500-2000 B.P., Vinette I-like pottery); middle (ca. 2000-1000 B.P., grit tempered dentate rocker-stamped pottery); late (ca. 1000-400 B.P., cord-wrapped stick decorated pottery with grit or shell temper). Thin, collared, incised vessels similar to those of southern New England also appear occasionally in late prehistoric sites, and it appears that pottery use was discontinued either just prior to European contact or very early in the contact period.

At the Turner Farm, the earliest post-Occupation III strata have a faunal character very similar to that of Occupation III. However, by 2000 B.P. we see a dramatic increase in relative reliance upon moose, flounder and birds, a noticeable increase in relative reliance on seals, beaver and sturgeon, and a drop in relative reliance upon deer and cod. A gradual increase in the use of bear and sea mink (*Mustela macrodon* -- presumably for fur) occurs between 2000 B.P. and ca. 900 B.P. After ca. 900 B.P. another increase in moose, seal and flounder relative frequencies occurs. Finally, the plow zone contains evidence of an even greater proportionate reliance on seals and another relative increase in moose hunting at the expense of deer.

We can detect no seasonal hiatus in occupation within the Turner Farm Ceramic period strata, and we suspect that the site was used during all months of the year. If a shift toward more seasonal use of the coast occurred at the end of the period of occupancy its evidence has been smothered by mixture within the plow zone.

However, this perceived pattern of year-round residence at a single site may not be the norm for the Ceramic period. Earlier research by Bourque (1973) on shell middens in the Deer Isle region

and by Sanger (1979d) on Ceramic period sites in the Passamaquoddy and Frenchman Bay areas produced evidence for late fall or winter to spring occupations for most or all of the investigated shell middens. This evidence, together with the presence of numerous Ceramic period sites on interior waterways, led both researchers to suggest that the Ceramic period seasonal round may have seen occupation of the coast during the late fall to spring months and a shift to interior riverine resources during the warmer months.

More recently, investigations at the Goddard site have revealed a major late Ceramic coastal village occupied during the warm weather months (Bourque and Cox 1981). Associated faunal data show a heavy focus upon grey and harbor seal, and upon sturgeon supplemented by other fish species, moose, deer, and furbearers. Seasonal data indicate that occupation of the site occured between April/May and September/October.

Thus, the available evidence suggests that late prehistoric populations adopted a mix of settlement options ranging from fairly continuous occupation of a single site to seasonal transhumance between coastal sites or between coastal and interior sites. There is clearly much that we still do not understand about Ceramic period settlement patterns, but continuing analysis of fieldwork data from the past decade of research on Ceramic period sites promises to redress the earlier imbalance toward the spectacular Moorehead phase cemeteries and to clarify our understanding of what was probably a complex system of seasonal settlement choices.

By around 1600 A.D., when we begin to get significant information from European accounts, Maine Indian populations were gathering in large villages at coastal or estuarine locations, at least during the summer. The influence of European trade upon this practice is unclear (Bourque 1973; Snow 1980). However, it may be that coastal settlement during the summer to facilitate trade is a precontact phenomenon. The late component at the Goddard site, for example, has produced substantial evidence of participation in an extensive precontact exchange network primarily oriented to the northeast. Exotic lithics recovered there include Ramah chert worked into a variety of late Ceramic tool forms and clearly not part of the site's late Archaic assemblage, hundreds of endscrapers and bifaces made of Nova Scotian chalcedonies, and a number of varieties of New York cherts, including Onandaga and Normanskill. Other exotics include nuggets and artifacts of native copper, probably from Bay of Fundy sources, an eleventh century Norse coin, and a Dorset Eskimo burin-like tool (Bourque and Cox 1981).

The prevalence of furbearer remains in the Turner Farm and

Goddard late components, and butchering practices similar to those employed in the historic fur trade, hint that furs may have played a role in late prehistoric exchange networks. If future research bears out the hypothesis of intensifying trade during the late prehistoric period, this may explain the ease with which Europeans induced Maine's population into extensive economic contact.

We have little evidence for elaborate mortuary ceremonialism during the Ceramic period. Burials range from individual interments to mass graves. The latter, however, may pertain to the post-contact period when Indian mortality reached extremely high levels. No instances of cremation have been reported, though bundle burials may have occurred and red ochre is occasionally present. Grave furnishings, if any, are usually meagre and seem to represent personal adornment and equipment.

At some point just before or shortly after European contact, the Ceramic period pattern of mortuary practices appears to undergo intensification, if not elaboration. Individual graves were occasionally furnished with large volumes or complex arrangements of copper and shell jewelry as well as metal tools and weapons. Both males and females have been identified in such graves, including an infant accompanying its mother in one instance. Some burials are located near habitation sites, but some appear not to be. Beyond these general observations, little is known about these late burials.

It may be that these relatively well furnished burials are those of local headmen and their relatives. Certainly, the existence of such individuals during the post-contact period is attested in the historic record, and it is not unreasonable to suggest that they emerged, or became increasingly differentiated from their peers, as a result of the development of exchange systems among native populations or with Europeans.

GULF OF MAINE PALEOENVIRONMENT

We shall now present a summary of some oceanographic changes between ca. 6000 B.P. and the present together with some suggestions regarding the influence these changes may have had on regional culture history.

Grant (1970) has proposed that after ca. 4000 B.P. sea level rise led to increasing tidal amplitudes in the Gulf of Maine. Sanger (1975) has argued that these changes led to "... upwelling and water mixing, followed by a cooling of surface water, and finally by affecting marine organisms, especially warm water fauna

such as swordfish and cold water adapted soft shell clams" (*ibid*:72).

More recent data on tidal amplitudes, however, place the onset of increase apparently earlier than Grant believed (Amos 1978). Between 8600 and 6300 B.P., the Minas Basin (part of the Bay of Fundy) was non-tidal. Since 6300 B.P., the tidal amplitude has been increasing linearly. Thus doubt is cast upon the oceanographic basis for Sanger's model. Nevertheless, there remains convincing evidence at the Turner Farm for decreasing water temperature in Penobscot Bay by ca. 3700 B.P.; the disappearance of quahog (*Merceneria merceneria*) from the sequence, the drastic decline of swordfish and the increase in average cod size after Occupation II. Tidal increase now seems the most likely explanation for these changes.

Fillon's sedimentological study of the shallow Hamilton Bank off southern Labrador suggests broad scale oceanographic changes during the Holocene (1976). He concludes that shallower ocean depths and weaker Labrador current before ca. 3500 B.P. allowed the Hamilton Bank to deflect a significant portion of the cold Labrador current east of the bank, pushing it to the seaward edge of the Grand Bank. This situation "... permitted northward penetration of warmer water along the coast as far north as Newfoundland ... and might have locally amplified the effects of the climatic optimum." The stronger modern Labrador current, he claims, began ca. 3500 years ago as a result of climatic cooling in the Canadian arctic.

Today, the Labrador current influences the Gulf of Maine by contributing to the cooling of subsurface water entering near the bottom of the Northeast channel (Apollonio 1979). This water is upwelled by a vertical eddy which parallels the Maine coast from Matinicus Island to Jonesport, producing a surface cooling effect (*ibid*:38-9). The absence of this cold Labrador current water prior to ca. 3500 B.P. in the Gulf of Maine, and incidentally in the Gulf of St. Lawrence as well, would presumably have meant warmer waters in both.

Thus, the decline of swordfish at the Turner Farm site after Occupation II may well reflect a real decline in their abundance in the area, and we agree with Sanger (1975:72) that such a decline might help explain the cultural changes which had occurred in the region by Occupation III times. Following Occupation III, however, the importance of continued oceanographic change for subsequent shifts in subsistence patterns, which we have outlined above, is unclear. Fillon suggests that cooler water near the coast would have reduced the coastal-interior climatic contrast after ca. 3500 B.P., and Amos' model implies a continuing rise in shoreline,

particularly intertidal zone, productivity and a decrease in surface water temperature. Added to these possiblities is the probability that sea level rise has continually affected the Gulf's environment significantly. The full implications of these kinds of variables and their interaction remain unclear at present.

DISCUSSION: MAINE AND THE NORTHWEST COAST

In comparing cultural development in Maine and the Northwest Coast some general similarities and a rather larger number of specific differences appear to be present. General similarities may include economies focused on marine resources and anadromous fish, burial elaboration, and some technological complexes such as the ground slate industry in the late Archaic. Yet even within these general patterns of similarity we see a number of specific differences not only between the northeast and northwest, but also between different prehistoric cultures within Maine.

In the following discussion we will focus briefly on three aspects of culture: subsistence-settlement systems; economic exchange systems; and mortuary complexity as a possible reflection of social complexity. We make the following assumptions about Northwest Coast cultures:

1. at least during the later stages of Northwest Coast prehistory, social ranking is present and is expressed in part through burial elaboration;

2. economy focused on marine resources and anadromous fish, with the fall salmon harvest providing the major source of stored food during the winter. Food storage technology was well developed;

3. exchange systems were well developed and included acquisition of exotics from outside the region as well as internal redistribution networks; and

4. the largest population aggregates and the greatest social/ceremonial intensification occurred in winter villages.

Subsistence-Settlement Systems

We have presented evidence that during the late Archaic period, the Turner Farm was occupied on a year-round basis, though fluctuations in group size and brief periods of abandonment are

probable, especially during Occupation II. Other sites such as Goddard may have experienced similar patterns of use during the Moorehead phase. However, despite the apparent use of the coastal base camps, local populations paid a significant amount of attention to other resource zones, particulary those associated with anadromous fish runs. Pelagic fishing stations are also indicated for the Moorehead phase. A generally similar pattern of use of the Turner Farm site apparently persists into the Ceramic period, though data from other coastal sites indicate a variety of seasonal options which we are only beginning to explore. Interior resources, particularly anadromous fish, continue to be exploited during the Ceramic period.

Within the Maine sequence there are variations through time in relative dependence on marine resources, even in coastal sites. The Moorehead phase, with a primary dependence on cod and perhaps swordfish, and the late Ceramic period with an emphasis on seal hunting and fishing for a variety of species represent economies strongly focused on marine species. At the other end of the spectrum, Susquehanna retained an emphasis on terrestrial species (mainly deer) in its coastal sites, apparently using insular situations to more effectively drive deer populations.

Both the Archaic and Ceramic period patterns of settlement and subsistence in Maine contrast with what we understand to be the basic Northwest Coast pattern in two ways. First, though there may have been some storage of cod for winter use during the Moorehead phase, we see nothing in the Gulf of Maine sequence which approaches the magnitude of food storage and consequent winter village population aggregation attributed to the Northwest Coast. We must temper this statement, however, with the observation that certain food storage techniques will not leave visible or readily interpretable archaeological remains; and we may therefore be missing significant instances of food storage in the Maine prehistoric record.

Second, though anadromous fishing was important to both areas, it occurred under very different circumstances. Although early depletion of salmon stocks in Maine makes reconstruction of pre-contact conditions difficult, Maine anadromous fish runs (salmon, alewives, shad, sturgeon) are primarily spring and early summer phenomena. The salmon run peaks in June, although Atlantic Salmon runs are not tightly restricted seasonally, and numbers of fish move upstream all summer (Bigelow and Schroeder 1953). There are no major anadromous fish runs in the fall, although the catadromous eel runs downstream in September/October, when it is easiest to harvest on small streams.

The Northwest Coast salmon runs peak in late summer/early fall, and we feel that this difference in timing may be significant culturally. Since summer in Maine is a time of relative plenty, motivation to preserve and store spring surplus catches was probably low. During the late Ceramic period at least, the largest population aggregates occurred during the summer, at a time when game availability was at a peak and there was a heavy focus on subsistence activities. This is in contrast to the Northwest Coast winter villages where resource availability and subsistence activities were at a low point, perhaps leaving more time/energy for social and ceremonial intensification (Fladmark 1975:92-93).

Two additional factors relating to anadromous fishing may have had cultural significance. We lack data to compare the relative numbers of available salmon in Maine and the Northwest Coast rivers during the pre-contact period, but it may be significant in terms of resource stability, if not absolute numbers, that there is one species of Atlantic salmon (*Salmo salar*) versus five species of Pacific salmon. Secondly, to a greater extent than on the Northwest Coast, Maine's anadromous fishing stations usually occurred relatively far in the interior, or at the heads of long estuaries, making transportation a significant added cost of any preserved surplus intended for winter consumption on the coast.

Exchange Systems

There is evidence for the presence of significant long-distance exchange networks only twice within the Maine prehistoric sequence: in the Moorehead phase and during the late Ceramic period. In the Moorehead phase exotics were imported in the form of finished tools, primarily bifaces, and appear only in burial assemblages. In contrast, during the late Ceramic period, tools and ornaments made of exotic materials are commonly found in habitation sites and appear to have been a part of everyday life. For example, several hundred Ramah chert resharpening flakes have been recovered from the Goddard site late Ceramic component. Both early historic accounts and the archaeological evidence indicate that ornaments made of native copper and other materials were commonly worn in addition to appearing as grave goods during the late prehistoric period.

Exchange systems involving exotic goods are certainly also present on the Northwest Coast; but the evidence from that region also indicates that the development of structured internal and regional redistribution systems involving both exotic and local products and of the concept of individual and lineage wealth were of much greater social importance than the acquisition of exotic items

alone. Unfortunately, we have very little data pertaining to redistribution systems in Maine. Evidence from burials, discussed below, indicates that concepts of status differential and individual wealth were not well developed during the late Archaic, or at least were not expressed in burials. During the late Ceramic and early contact period there is limited evidence from burials and early accounts of status differential, possibly based in part on differential access to exotic goods, but none of the early historic accounts give evidence of the existence of complex, structured redistribution mechanisms comparable to those of the Northwest Coast.

Mortuary Complexity

Mortuary ceremonialism reaches peaks of relative complexity in Maine during the Moorehead phase, the Susquehanna tradition, and at the end of the aboriginal cultural sequence, either just before or after contact with Europeans. Each seems historically unconnected to the others, and each manifests itself somewhat differently. Although burial complexity in Maine as measured by grave size, numbers of grave goods, presence of exotics and, in some periods, artistic expression, rivals that of the Northwest Coast, there appear to be some significant differences between the two regions in terms of the social correlates of mortuary ceremonialism.

At Nevin, the one Moorehead phase cemetery which contained preserved human remains, there was no demonstrable age/sex specificity in burial goods, nor other indications of status differential (Byers 1979). The same is true of the similar and possibly culturally related Maritime Archaic cemetery at Port au Choix, Newfoundland (Tuck 1976b). Unfortunately, data are not available for age/sex correlations within Susquehanna burials. Although no systematic comparative studies have been done for Ceramic period and early contact burials, it is our impression that there is no evidence to contradict the hypothesis that the relatively small numbers of grave goods found in late Ceramic burials are the personal property of the deceased, and there are no dramatic differences in grave wealth which would suggest significant status differential. It is only with the beginning of European contact that we can definitely see a significant shift in burial patterns, with a few graves containing large numbers of artifacts, presumably reflecting the development of increasing status differential at least in part as a result of differential access to European trade goods.

CONCLUSIONS

There is no evidence within the Maine prehistoric cultural sequence for social complexity (specifically status differential) approaching that of the Northwest Coast. Mortuary complexity does not seem necessarily to correlate with individual status differential. It is only in the early contact period that we see clear evidence for increasing status differential, but even then there is no evidence for the presence of the highly structured redistribution mechanisms or formal social ranking present on the Northwest Coast.

Data from the Turner Farm indicate a surprising degree of sedentism through the Maine sequence, but again this sedentism does not seem to result in increased social complexity. Might it be that a large gathering of people in an intense social situation, albeit temporary, has greater correlation with social/ceremonial intensification than do small groups maintaining residence at one place through the year?

We have suggested several environmental and cultural factors which may have limited the development of social complexity in Maine relative to that of the Northwest Coast. These factors include the timing of anadromous fish peaks, development of food storage mechanism, and location of anadromous fishing stations.

Although we have emphasized the cultural differences within the Maine prehistoric sequence, we do find it interesting, and probably significant, that two of the three cultures which exhibit mortuary complexity in Maine, Moorehead and late Ceramic, share with Northwest Coast cultures a strong economic focus toward marine resources and are the only ones which exhibit long-distance exchange systems. The exception is the Susquehanna tradition, which developed outside of the northern New England region and enjoyed a relatively brief life span in Maine. While recognizing the probable relationships between a maritime economy, long-distance exchange, mortuary complexity, and possibly social complexity, the differences in manifestation and detail that we see in the Maine sequence suggest that any single model for interaction between these variables may be inadequate to explain the full range of cultural variability we see in the northeast and northwest.

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MODELS OF COASTAL ADAPTATION: THE NORTHWEST COAST AND MARITIMES

Robert Whitlam

INTRODUCTION

Modelling the character of regional hunter/gatherer patterns of resource exploitation and settlement distributions represents an important area of research in contemporary archaeology (cf. Butzer 1971; Binford 1964; Struever 1971; Parsons 1972; Jochim 1976, 1979a; Rice 1975; Roper 1979; Winters 1969; Yellen 1977). Identifying variability in patterns of scheduling behaviors (Binford 1978; Lee 1968; Thompson 1939), changes in group organization (Thomas 1972; Steward 1938), and the character of resource exploitation (Cleland 1966, 1976; Dunnell 1972) are seen as critical in understanding the nature of specific prehistoric adaptations. The resulting knowledge, besides facilitating comparative studies in prehistoric human ecology, may also substantially contribute to larger issues within archaeology, anthropology, and sociobiology. Archaeologists can contribute data, representing long time spans from a variety of extant and extinct environments, to research questions concerning the correlations between cultural forms and environmental characteristics, the nature of cultural change, and the adaptive significance of culture.

It is within this general perspective that I have developed a regional settlement-subsistence model to facilitate comparisons between aboriginal coastal adaptations along the Northwest Coast and in the Maritimes. It will also hopefully contribute both to our

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understanding of Canada's prehistory and aid in developing a more general understanding of and appreciation for the complexity of hunter/gatherer coastal ecology.

In constructing regional settlement-subsistence models two contrastive approaches may be employed. First, the archaeological record for specific coasts can be reviewed and attempts to identify specific adaptations can be made for each area for particular time periods. This approach has several disadvantages besides the major time/research expenditures that are involved. It necessitates the creation of a series of detailed and systemic models that would attend the interaction of specific environments and settlement-subsistence adaptations. Typically, this kind of approach requires both detailed knowledge about the precise relationships between a given set of environmental and cultural variables along with specific knowledge about the structure of the relevant paleoenvironments. The success of this kind of approach is heavily dependent upon making inferences about the specific composition, structure, and significance of both the cultural and environmental variables, besides placing a critical reliance upon the ability to make detailed palaeoenvironmental and subsistence resource reconstructions (cf. Butzer 1971; Davis 1963; Grayson 1973, 1978, 1979). Further, because of the detailed information requirements, this kind of strategy almost always requires concentration upon one or a few locations to be examined in great detail, making it difficult to distinguish purely local events and conditions from more general and important relationships until a large number of such studies have been completed.

An alternative strategy, and the one that is employed in this paper, is to construct a more general model by drawing upon principles in the ecological and anthropological literature and relying upon repetitive patterns in the archaeological record for larger areas to identify critical differences in the character of settlement-subsistence adaptations across space and through time. If such a model is conceived in terms of documenting a number of relatively easily measured parameters of the coastal archaeological record that are sufficient to differentiate between different forms of adaptation and allow correlation with broad environmental parameters, the likelihood of obtaining definitive conclusions about the general character of prehistoric settlement-subsistence adaptations on the Northwest Coast and in the Maritimes is enhanced.

While these two strategies are competitive, they are also complementary. Ultimately, detailed, systemic, functional studies will have to be conducted to allow the specifics of particular coastal adaptations to be identified and their relationships to

particular resource and palaeoecological configurations assessed; however, if a more general approach is taken initially, far more informed decisions can be made about where future detailed studies should be undertaken and specific objectives for testing can be clearly defined once the general structure has been identified. In the final analysis, the choice of a more general model is simply a matter of taking the most cost and information effective approach first and then using the information resulting from that research to guide subsequent detailed field investigations (Binford 1968b; Redman 1973). This situation is particularly relevant to the Northwest Coast and the Maritimes where there is still much basic research to be done.

THE VARIABLES

In creating a general model of aboriginal adaptation for the Northwest Coast and the Maritimes there are several variables that must be investigated and their inter-relationships discussed. The rationale for the selection of these variables is grounded in both ecological and anthropological theory. These variables also have direct entailments in the archaeological record that make clear determinations possible. Additionally, these entailments are not so subtle as to preclude identification with the types of data available for use in this paper: published archaeological site and regional reports.

There are two variables of the settlement system that are important in assessing how populations occupy an environment. The first critical variable of the settlement system is the scheduling behavior of the population. The scheduling behavior represents how a population is distributed across a landscape during the course of the annual cycle. An assessment of the state of this variable is important because it indicates the duration of utilization for particular locales and environments. The scheduling behavior reflects whether the population resides in the same place during the entire yearly cycle without regard to seasonal changes in the environment (termed Sedentary) or whether there is cyclical relocation of populations to differing locales during the course of the annual cycle (termed Mobile) (cf. Winters 1969; Parsons 1972; Lee 1968; Yellen 1977). To be sure, there is substantial variability subsumed by this definition of Mobility, but the distinction recognizes a major structural difference between all Mobile forms versus a Sedentary adaptation. It is assumed that a Sedentary strategy, others things being equal, maximizes cost/benefits to a population by allowing the full-time exploitation of a territory besides reducing relocation costs and enhancing

potential reproductive success (Binford 1968b; Brown and Orians 1970; Fretwell 1972; Emlen 1966; MacArthur and Pianka 1966; Moseley 1975). On the other hand, the archaeological record quite clearly indicates that sedentary settlements are the exception rather than the rule until relatively late when they are associated with a particular kind of subsistence practice: agriculture. All others things are not always equal. A basic assumption behind approaches like site-catchment analysis (Jarman, Vita-Finzi and Higgs 1972; Higgs and Vita-Finzi 1972; Roper 1979), and an assumption that is well founded in human anatomy, is that only a limited amount of space can be exploited from a single location. Mobility during the course of the year can increase the exploitable space although it does so at the expense of continual occupation of a given territory (Jochim 1976; Yellen 1977). Mobility as a strategy is also important as it interacts with changing seasonal resource availability. Seasonal changes affecting the occurrence, distribution, and productivity of subsistence resources can radically alter resource potentials available to populations in given areas. Changes in the productivity of particular resources and their locations during the course of the year may require population relocation for effective exploitation (Binford 1978, 1980; Schalk 1977; Winters 1969; Yellen 1977).

The second variable of the settlement system is the nature of the group structure. This variable reflects the degree to which the effective economic unit is also the cohesive residential unit (cf. Coe and Flannery 1964; Flannery 1976; Parsons 1972; Rice 1975). An assessment of the state of this variable is important because it indicates the nature of population distributions across a landscape and the density of occupations for differing environments. For the character of the group structure, two forms can be discriminated: nucleated and dispersed. For the nucleated form, the community is identical with the discrete settlement. For the dispersed form, the economic unit encompasses several distinct settlements distributed across a landscape at a given point in time.

The occurrence of particular forms of the group structure relates to environmental conditions in an important manner. The spatial distribution of exploitable resources can be important in understanding the nature of group structure. Where resources are of a low density and are randomly or uniformly distributed across an environment, dispersion of the population may be necessary for their effective procurement (cf. MacArthur and Pianka 1966; Harpending and Davis 1977; Pianka 1978). Dispersing the consumer population increases the search area covered per time expended thus increasing potential prey/resource contact (Schoener 1971; Harpending and Davis 1977; Pianka 1978). Conversely, when resources are of a high

density, tightly clustered and/or seasonally limited, nucleation of the consumer population may be required for effective acquisition and processing (Binford 1978; Schalk 1977; Thomas 1972).

The form of the group structure that is manifest in the settlement pattern data is also critical because it bears directly upon the relationship between the cultural system and the representation it leaves in the archaeological record. Different densities of populations and their variable distributions across an environment will produce different amounts and distributions of debris and hence produce different thresholds of archaeological visibility. Nucleation can produce relatively large, dense sites (Moseley 1975) while dispersion of the population into smaller units can produce diffuse distributions of archaeological materials across a landscape (Thomas 1972).

The differing states of these two settlement variables can interact in complex ways within a functioning adaptation. While individually, either dispersion or mobility can increase the total amount of exploitable space; taken in conjunction, a mobile strategy can further vary the space exploited at any given point in the seasonal cycle by population dispersion. Both dispersion and nucleation of the population at different times during the yearly cycle may be an important mechanism for regulating local population densities in relation to seasonal resource abundance and distributional changes (Lee 1968; Steward 1938; Thomas 1972). In a similar fashion, the mobility of special segments of a population may be incorporated into an adaptation that is essentially sedentary. For all cultures, the subsistence and raw material resources required for system survival and continuity are not found within the boundaries of the settlement. The temporary detachment of special labor forces appropriately partitioned by function is more efficient in the specialized short-term procurement of specific resources than moving the entire population between resource locales (Binford 1980; Jochim 1976). As can be envisioned, different combinations of the states of the group structure and the scheduling structure can entail very different requirements for space, resource distributions, and social interactions.

A consideration of the interaction of these two settlement variables introduces the matter of subsistence as critical in determining whether or not a particular kind of settlement adaptation is competitive or not at a particular time and place.

Subsistence activities are often simply detailed in terms of the presence/absence or relative abundance of recovered fauna (c.f. Willey 1949). As an alternative to this approach, subsistence systems can be conceived in terms of the organizational structure

and diversity of resources exploited apart from a listing of the specific resources. Cleland (1966, 1976) in developing his "focal -diffuse" model emphasized an evaluation of the diversity of resources exploited by a population during the course of the entire annual cycle. He noted his model may be conceived as a continuum in which two types of adaptations are polar opposites.

At one end, "focal" adaptations, alternatively termed "specialized" (Pianka 1978; MacArthur 1965) or "intensive" (Dunnell 1972; Whitlam 1980b) are based upon the exploitation of a single resource or a set of closely related resources that can be exploited and processed in a similar manner (Cleland 1966, 1976; Rice 1975). While intensive strategies may be based upon either naturally occurring resources (cf. Frison 1978) or domesticates (cf. Flannery 1973), all intensive adaptations, because of their reliance upon a restricted set of resources, require a high degree of reliability in the availability, abundance, and distribution of the exploited resource.

Conversely, "diffuse" adaptations, often termed "generalized" (Pianka 1978; MacArthur 1965) or "extensive" (Dunnell 1972; Whitlam 1980b) exploit a wide array of different kinds of resources. An extensive strategy is organized around the careful scheduling of exploitation timed to resource availability. The key to an extensive adaptation is the shifting resource base in time and space during the yearly cycle. Supporting a population with an extensive adaptation not only requires the careful scheduling of resource procurement keyed to natural availability, but also the ability to substitute alternative resources should a given resource be temporarily unavailable (Cleland 1976). The ability to substitute alternative resources and subsistence tactics also distinguishes extensive from intensive adaptations in an important respect. Because intensive adaptations rely upon a single or a few highly productive resources, alternatives, both in terms of subsistence resources and procurement tactics, may not be readily available (Cleland 1966, 1976; Pianka 1978).

There are both ecological and cultural factors to consider in assessing the potential occurrence and adaptive significance of these two kinds of subsistence strategies. Ecological theory on optimal foraging tactics relates extensive adaptations with decreasing resource abundance (Emlen 1966, 1968; MacArthur and Pianka 1966; Schoener 1971; Pianka 1978). For environments with a low abundance of resources, consumers cannot affort to bypass food items because search time is long and expectations of potential prey encounter is low; therefore, an extensive strategy emphasizing the exploitation of a variety of different resources is more competitive

since it will maximize resource return per unit time expended (Schoener 1971). Conversely, for environments with an abundance of resources, search time is low since consumers encounter numerous potential prey. Under these conditions, inferior resources can be bypassed since the expectations of encountering a superior prey item in the near future is high; thus food rich environments favor selective foraging and specialization in resource procurement over time (MacArthur and Pianka 1966; Pianka 1978; Schoener 1971; Brown 1964; Davies 1976).

There are, however, complex relationships between resource diversity, availability, spatial distributions, productivity, and a culture's technology to consider in assessing the potential occurrence of each of these subsistence adaptations. In environments where resource diversity is low, individual resource populations can be immense and highly productive, effectively creating a food dense environment of a single exploitable species. This factor, coupled with a subsistence technology that can efficiently procure and store the resource, would favor the occurrence of an intensive strategy (cf. Pianka 1978; Frison 1978; Dunnell 1972; Rice 1975). In contrast, for environments where resource diversity is high, but individual resource populations are small, unpredictable in occurrence, and mixed in distribution, and when a consumer population lacks efficient procurement and storage technologies, an extensive strategy that can exploit all available resources would be favored (MacArthur and Levins 1964, 1967; Pianka 1978; Caldwell 1958; Cleland 1966, 1976; Dunnell 1972).

The nature of diachronic change for these two kinds of adaptations differs substantially. Because extensive strategies can readily substitute alternative resources, they can, through time, diversify or "adaptively radiate" and incorporate more and more different kinds of resources into their diverse resource base (Pianka 1978; MacArthur and Pianka 1966; Dunnell 1972; Caldwell 1958; Rice 1975). Additionally, extensive strategies also have the potential for changing into intensive strategies under certain conditions. Under conditions of diverse resource exploitation, where consumer contact with prey resources is high, selective utilization on the part of the consumer in bypassing inferior for superior resources favors the development of specialization in resource acquistion and the development of intensive strategies through time (MacArthur and Levins 1964; MacArthur, MacArthur and Preer 1962; Dunnell 1972; Cleland 1966, 1976; Rice 1975). Intensive strategies, because of their restricted resource focus, lack the variability to readily substitute alternative resources and procurement strategies and tend to remain specialized. Through time, increased resource return is accomplished by increasing

efficiency in resource procurement, increasing resource productivity, or increasing storage and redistribution facilities (Frison 1978; Cleland 1966, 1976; Schalk 1977). In short, extensive adaptations may remain extensive or they can change into intensive adaptations through time; intensive adaptations remain intensive.

THE MODEL

The interaction of these differing variables states produces a number of different kinds of functioning systemic adaptations. However, in developing a general model applicable to both the Northwest Coast and the Maritimes let me discuss one kind of adaptation I will call the MNE type of adaptation. MNE stands for *M*obile/*N*ucleated/*E*xtensive, characteristics that distinguish this form of adaptation. Figure 1 illustrates the structure of this kind of adaptation and its potential relationship to resource seasonality and abundance changes in a schematic fashion. There are several points that deserve mention regarding this model. First, the mobile structure of the adaptation produces a situation where settlements and populations shift to different environments or microenvironments as resources change in seasonal availability or abundance. Secondly, the group structure can also change during the course of the annual cycle. The dispersion of the population into smaller units, the short-term detachment of specialized labor groups, and the movement of individuals across the landscape may all be effective mechanisms for increasing consumer/prey contact, for obtaining specialized raw materials, and for regulating local population densities in relation to resource spatial distributional and productivity shifts. Thirdly, the extensive nature of the subsistence orientation is reflected in the diversity of environments, microenvironments, and resources that are exploited.

The complexity of this kind of adaptation has a number of critical implications for the structure of the archaeological record and our perception of it. Most importantly, the mobile character of this kind of adaptation produces a situation where the complete adaptation is only represented on a regional level. No single site or environment will portray the entire systemic adaptation.

Changes in group structure will produce a situation where sites can vary greatly in size, density, duration of occupation, and ultimateley archaeological visibility. The extensive character of the subsistence orientation assures us that sites will vary in the kinds and quantities of the flora and fauna exploited, along with the functional tools and facilities that are required for their procurement, processing, and potential storage. All these factors

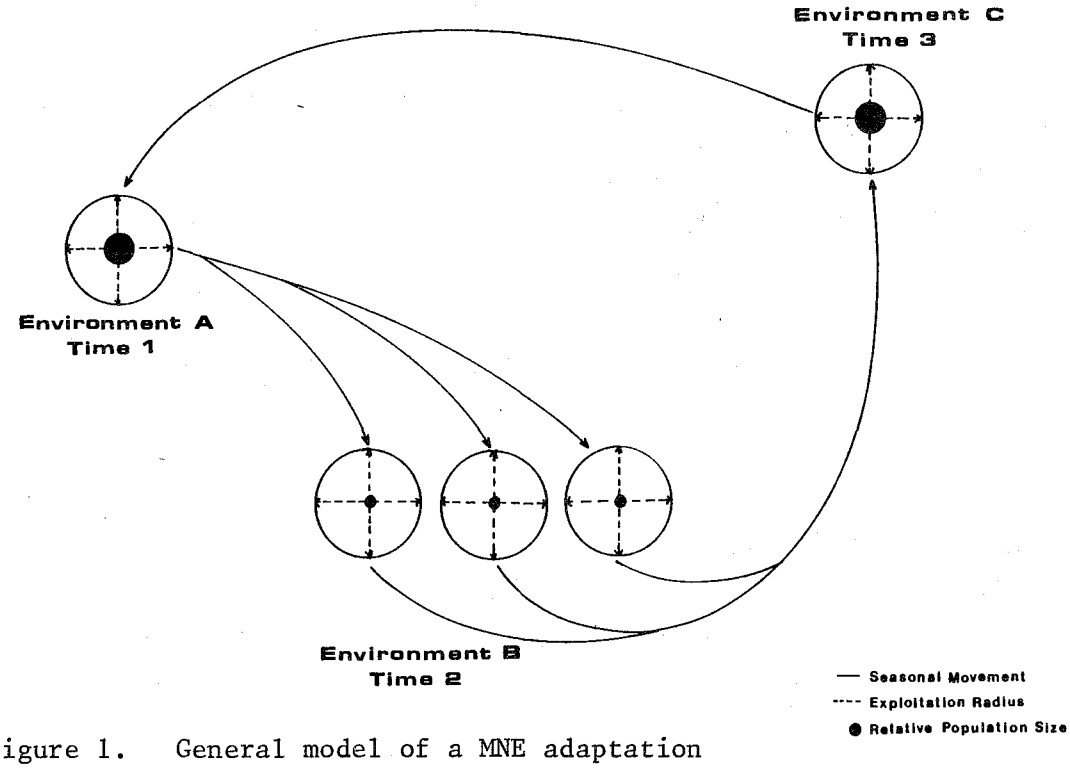

Figure 1. General model of a MNE adaptation

interact to produce a complex and heterogenous pattern in the archaeological record on a regional level.

The character of diachronic change can be modelled in one of two general fashions. First, given the nature of a MNE adaptation with its extensive subsistence orientation, it can adaptively radiate or diversify over time. More and more different kinds of resources and environments may be incorporated into an increasingly complex, seasonally defined, and territorially differentiated exploitative pattern (Figure 2). Alternatively, it is also possible that, over time, specialization may occur. Given a resource and environment that is highly productive, reliable, and one that can support the subsistence requirements of the population through the entire cycle, an intensive adaptation can develop (Figure 2). Selective pressures may favor the concentration of a population's time and energy upon the exploitation of that resource/environment at the expense of other resources/environments.

Differences in resource productivity, and reliability along with potential scheduling conflicts, differences in transportation costs, risk minimization, capital investment costs in subsistence technologies, and cultural values may all interact to produce a situation where one or a set of closely related resources will become the focus of an intensive adaptation (Pianka 1978; Cleland 1966, 1976; Earle and Christenson 1980; Binford 1978, 1980; Jones 1976, 1977, 1978).

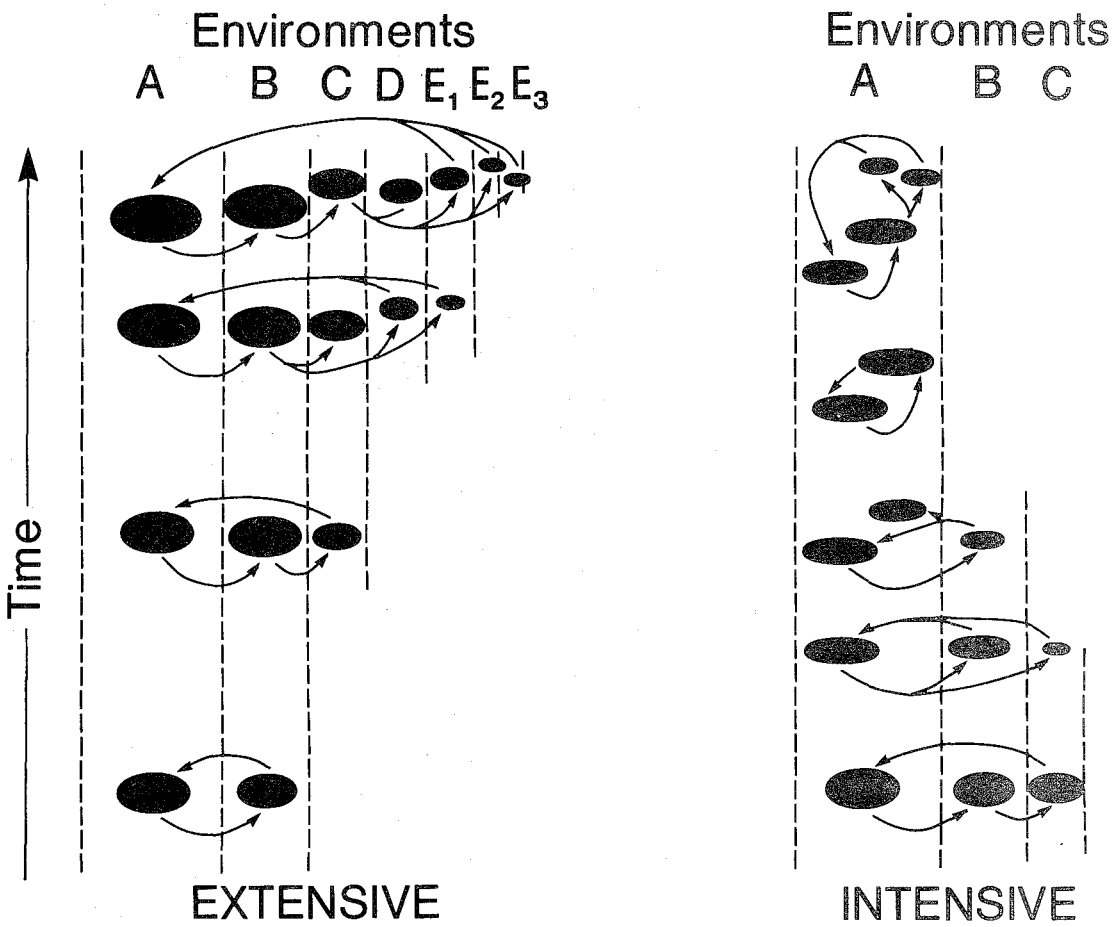

Figure 2. Trajectories of extensive and intensive adaptations through time.

Given these theoretical considerations and the constructed models, the substantive research problem becomes one of loading the models with empirical referents and assessing which model or models is most appropriate for the Northwest Coast and the Maritimes. Accomplishing this goal in a detailed and systematic fashion is obviously beyond the present scope of this paper, and is perhaps even beyond the capabilities of a single researcher. Nevertheless, let me present to you, in a summary fashion, what available conclusions and data I have been able to glean from the published literature. Hopefully, these data and conclusions will serve as a foundation for further research, stimulate discussion, and be amenable to future testing. In the final analysis, what follows represents a first approximation rather than a fully tested and accepted end-product.

THE NORTHWEST COAST: GULF OF GEORGIA

For the Gulf of Georgia and Puget Sound region, archaeological

research has been conducted since the turn of the century (cf. Deans 1891, 1892, 1900; Eells 1878; Thacker 1898a, 1898b; Hill-Tout 1902; Smith 1899, 1900, 1904a, 1904b, 1906, 1907, 1909; Smith and Fowke 1901). However, it was not until the 1950s that the first attempts to rigorously establish a cultural historical framework were undertaken through the pioneering research of Borden (1950a, 1950b, 1951b, 1954a, 1954b, 1962, 1968b, 1969, 1970, 1975, 1979) in the Fraser River/Vancouver area and King (1950) and Carlson (1954, 1960, 1970a) in the San Juan Islands. Their research and subsequent investigations during the 1960s to the present (cf. Mitchell 1968a, 1968b, 1969, 1971, 1973, 1979; Matson 1976; Haggarty and Sendey 1976; Thompson 1978a, 1978b) has produced a tentative culture history. Although there are still substantial questions and points of contention about the chronological sequence (Abbott 1971, 1972; Burley 1980b; Mitchell 1971; Matson 1974; Thompson 1978b) and the cultural affiliations of particular assemblages, it is apparent that humans have been in the general region since the Late Pleistocene (Gustafson, Daugherty and Gilbow 1979) and have exploited coastal resources from Early Holocene times (Borden 1975, 1979; Carlson 1979).

Thompson's research (1978a, 1978b) provides a regional overview and synthesis of many previous site specific investigations. Her examination of the region's sites and their microenvironmental location, functional tool types, and chronological placement provides a set of data that can be readily interfaced with the model of a MNE adaptation and its adaptive radiation through time (Figure 3). As is schematically illustrated in this figure, the nature and number of exploited environments changes through time. More and more environments are incorporated into a seasonally and environmentally differentiated territorial round. Despite the fact that particular suites of resources have yet to be systematically identified for many of the sites, and though there may be potential problems in sampling and quantification (cf. Casteel 1971, 1974, 1976a; Grayson 1973, 1978, 1979), it would appear that a variety of different kinds of resources, including numerous fish, bird, land and sea mammal and molluscan species were incorporated into an extensive pattern of exploitation.

While the general character of the site's differing environmental locations, variability in functional types, and reported fauna accord well with the general MNE model, there are several cautionary points that should be made. First is the question of how widely applicable this model is to other areas within the Northwest Coast. A general appreciation of the complexity and differential distribution of resources and environments within this region makes it quite likely that

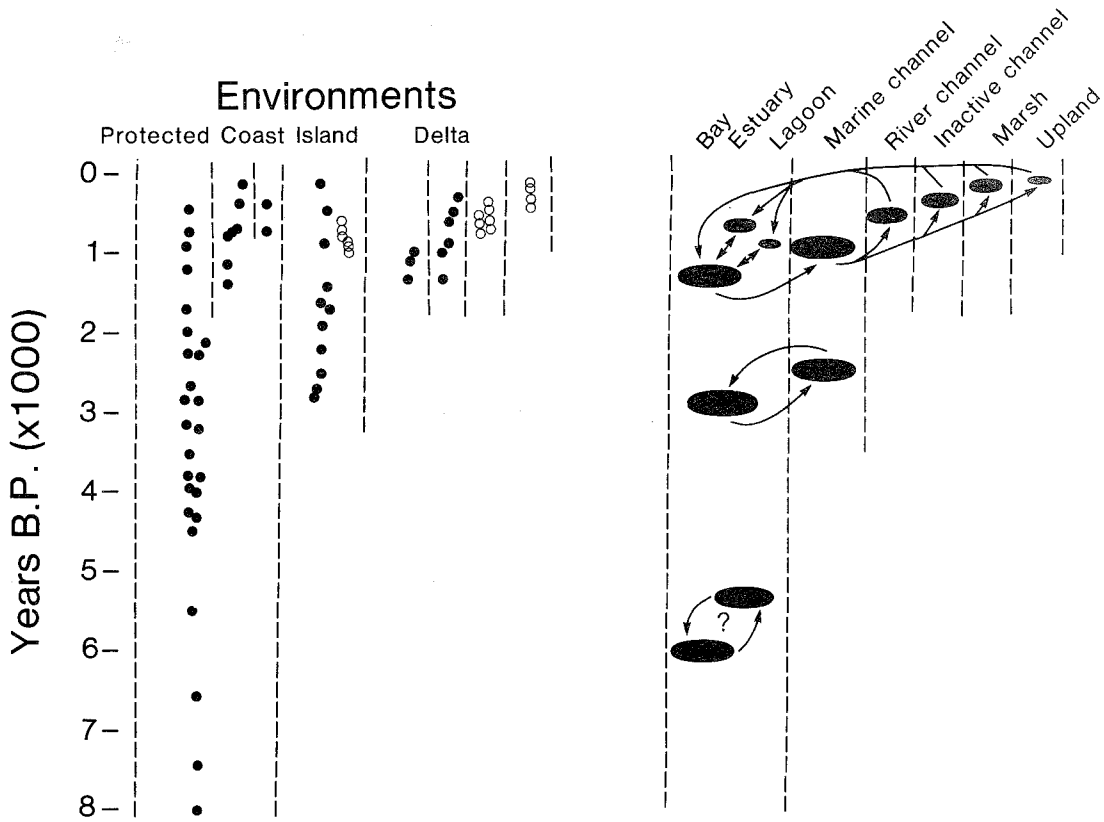

Figure 3. Model of Northwest Coast adaptations through time. Solid circles represent ^{14}C dates, open circles represent estimated dates. Data from various sources.

substantial variability existed at given points in time between local populations in terms of their particular settlement and subsistence adaptations. There is some evidence to suggest sedentary adaptations in locales characterized by microenvironmental compaction and complexity (Kenady 1971). Also intensive adaptations focusing upon anadromous fish exploitation may characterize some populations (Schalk 1977). What will probably become clear in the future is the fact that Northwest Coast populations exhibited a mosaic of differing adaptations through time and across space. The challenge is to empirically document that variability, establish the boundaries for particular forms, assess their adaptive significance, and determine their relative competitive success.

Secondly, this model and the supporting data suggests the observed ethnographic pattern may not characterize all of the kinds of adaptations represented in the archaeological record. This evidence, coupled with our knowledge of the region's palaeoenvironments, especially the documented changes in the character of the vegetation (Hansen 1938, 1940, 1941, 1947, 1950; Hebda and Rouse 1979; Heusser 1955, 1960, 1973, 1974, 1977; Mathews 1979), sea levels (Andrews and Retherford 1978; Clague 1975; Dawson

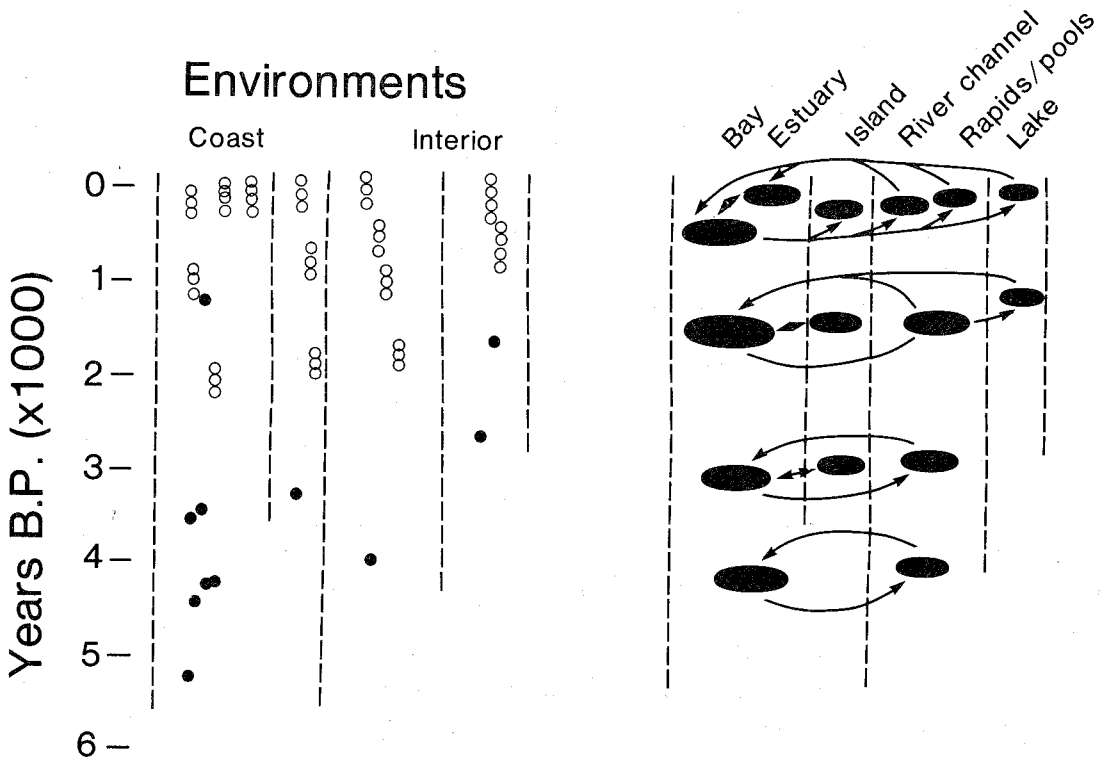

Figure 4. Model of maritime adaptations through time. Data from various sources.

1877; Easterbrook 1963; Grabert and Larsen 1973), geomorphology (Alley 1979; Alley and Chatwin 1979; Clague, Armstrong, and Mathews 1980; Kraft, pers. com.) along with changing species biogeography (cf. Fladmark 1974, 1975) make it reasonable to suppose there existed environments and adaptations to those environments for which no specific ethnographic analogues exist.

THE MARITIMES

For the Maritimes, archaeological investigations have been conducted since the mid-1800s (cf. Ambrose 1863; Bailey 1883, 1887; Baird 1881; DesBrisay 1879; Dixon 1914; Duns 1880; Fewkes 1896; Ganong 1898, 1899, 1905; Gilpin 1873; Goodwin 1892; Gossip 1864; Honeyman 1879; Jack 1883; Jones 1863; Kain 1901, 1902, 1904, 1905; McIntosh 1909, 1914; Matthew 1884, 1900; Matthew and Kain 1905; Miller 1887; Patterson 1881, 1888; Piers 1889, 1894, 1912; Smith 1914; Smith and Wintemberg 1929; Wintemberg 1929, 1937). More recent research has been able to identify Paleo-Indian occurrences (cf. Byers 1965; MacDonald 1966, 1968; Stuckenrath 1964; Turnbul 1974; Turnbull and Allen 1978) and obtain a broad outline of subsequent aboriginal occupancy (Burley 1974, 1976; Davis 1976; Erickson 1978; Harper 1957; Nash 1978; Sanger 1971a, 1971b, 1973).

However, modelling the variability of aboriginal Maritime coastal adaptations and their change through time is exacerbated by the fact that the Maritimes are experiencing rapid, though variable, rates of coastal submergence (cf. Bloom 1960; Clarke *et al.* 1967; Dawson 1866, 1856; Deevey 1948; Forward 1960; Frankel and Crowl 1961; Gesner 1861; Grant 1970, 1977b; Harrison and Lyon 1963; Johnson 1913 1925; Lyon and Goldthwait 1934; Lyon and Harrison 1960). The changing configurations of Holocene shorelines and the erosion of coastal sites poses serious problems in obtaining empirical evidence on the character of aboriginal coastal exploitation (Simonsen 1978, 1979).

From the available information contained in the published literature it appears that the general structure of aboriginal Maritime coastal utilization conforms well with the MNE model. The composite model (Figure 4) based upon data from the Maritimes (cf. Burley 1976; Nash 1978, n.d.; Sanger 1971a, 1971b, 1973; Smith and Wintemberg 1929; Turnbull 1975) and adjacent areas of Maine (cf. Bourque 1973, 1975, 1976; Sanger 1975, 1979e; Sanger and MacKay 1973; Yesner 1979) suggest a mobile scheduling organization with coastal resources and environments playing an important, though potentially seasonally differentiated, role in an extensive subsistence adaptation exploiting a wide array of different fish, bird, molluscan, land and sea mammal forms.

What is not evident from this figure, and a challenge facing Maritime archaeologists in the future, is establishing the duration, season, and relative importance of particular resources and environments exploited by local populations. A substantial research issue also involves systematically monitoring diachronic changes in coastal-use patterns. Did shifts occur in the nature of settlement and subsistence strategies for local areas as the Maritime environments changed during the Holocene? What impact did the documented changes in the nature of the vegetation (Bradstreet and Davis 1975; Livingstone and Estes 1967; Livingstone and Livingstone 1958; Mott 1975), coastal geomorphology (Grant 1970, 1977b), current patterns (Clark *et al.* 1967) and species biogeography (Clark *et al.* 1967; Loomis 1911; Morse 1882; Sanger 1975) have upon aboriginal adaptations? There is clearly much research to be done.

CONCLUSIONS

In concluding, there are several points that must be stressed. First, this model of a MNE adaptation and its potential change through time was developed so it could be applied to hunting and gathering systems regardless of particular historic and ecological

circumstances. The rationale for the model, and the selection of relevant dimensions to monitor, were derived from a body of ecological and subsistence theory that should be applicable to aboriginal systems regardless of their particular resource base or technological level (Cleland 1966, 1976; Dunnell 1972). Additionally, these dimensions are not so subtle so as to preclude identification with the kinds of data normally available to archaeologists; artifacts, fauna, and their variable distributions across environments. The challenge is to empirically load the model with archaeological entailments so as to identify the particular ecological adaptations and evolutionary history of specific populations.

Secondly, the variables of the model also interact with environmental characteristics in an important and direct manner. Differences in resource seasonality, spatial distributions and diversity will produce selective pressures for varying forms of scheduling behaviors, group structure, and subsistence orientation. What is significant about both the Northwest Coast and the Maritimes is the relatively well documented evidence for Holocene environmental and resource shifts. Most frequently, empirical studies on the relationships between environmental change and cultural change have been conducted in regions where changes in the climatic or resource regime would have pronounced and frequently detrimental effects upon prehistoric cultural systems. Field studies in the arid Southwest U.S. (Plog 1974), the U.S. Great Basin (Bettinger 1977), and the Canadian High Arctic (Barry *et al.* 1977) have documented both the adaptive success and failure of specific kinds of settlement-subsistence strategies in environments that today would be characterized as extremely rigorous to man. However, archaeological attention has yet to systematically assess the nature and impact of environmental and resource changes in more temperate and resource-diverse regions such as the Northwest Coast and the Maritimes. The models developed in this paper may effectively interface with such studies.

Finally, the models and theoretical foundations developed in this paper can be applied to the archaeological record independent of ethnographic analogues. While not denying the contribution that ethnographic data can potentially make to archaeological research (cf. Gould 1978, 1980b), the position taken in this paper is that archaeological data must be ultimately analyzed independent of ethnographic descriptions. There are both theoretical and pragmatic reasons for such a stance. Since the explicit focus of archaeological research is upon long time spans and the nature of diachronic change, the ethnographic record frequently cannot adequately address nor serve to model the nature of diachronic change.

Additionally, the archaeological record may contain the record of systems or modes of adaptation to particular ecological circumstances and environmental conditions that are no longer extant. Thus ethnographic data may have little relevance to modes of adaptation to extinct environments for which no analogs exist. This point may be especially relevant for the Northwest Coast and the Maritimes where dramatic and pronounced changes in the character of palaeoenvironments are clearly documented.

More importantly, and a point that may have great significance to the disciplines of archaeology and anthropology as a whole are the implications of the impact that European contact had upon aboriginal systems in these areas. The documented serious effects and epidemic diseases that followed European intrusion into these areas resulted in the rapid depopulation and collapse of aboriginal systems as independent cultural entities (Duff 1964; Miller 1976).

Thus, ethnographic generalizations may reflect statements about systems in collapse, successful and unsuccessful attempts at readaptation following European contact, and the incorporation of European technologies and behavior to varying degrees. The ethnographic data may more accurately represent Refugee systems rather than functioning independent cultural strategies that characterize successful adaptations to these regions' diverse environments.

In short, the ethnographic data from this perspective becomes a body of data that requires examination and analysis in terms of the characteristics that represent successful prehistoric strategies rather than being viewed as the structure to portray prehistoric aboriginal adaptations. The transition from prehistoric to historic times -- from the archaeological record to the historic record -- involved massive death, system collapse, settlement abandonment and relocation, the loss of environmental utilization, and in some cases, cultural extinction. Archaeology may be able to contribute information that will be useful in understanding the problems facing contemporary refugee populations.

In the final analysis, the archaeological resources of the Northwest Coast and the Maritimes can contribute critical data to a variety of contemporary research topics in archaeology, anthropology, and other disciplines concerned with man, his environments, and the past. A major challenge will be the construction of models that can be employed in comparative research. In this paper I hope I have raised a number of issues and provided a general model of settlement-subsistence adaptations that will facilitate future studies.

INTENSIFICATION AND THE DEVELOPMENT OF CULTURAL COMPLEXITY: THE NORTHWEST VERSUS THE NORTHEAST COAST

R.G. Matson

INTRODUCTION

The relationship between the development of social complexity and intensification of resource use is the subject of this paper. I examine aspects of both the general case and two specific examples -- one in which intensification and cultural complexity developed, and one in which they did not, although the resources were similar in both areas. First I will describe what I mean by "cultural complexity" and "intensification." The process of intensification will be discussed in the abstract and two brief examples illustrating some of the key points will be given, one of intensification and the other of non-intensification. The main substantive portion of the paper, which follows, gives my views on why salmon fishing was intensified on the Northwest Coast and how this was related to the development of cultural complexity. Finally, the question of why intensification and complexity did not develop in the Northeast, at least not to the extent they occurred in the Northwest will be examined.

CULTURAL COMPLEXITY

Given that the two areas that are under examination both have hunting and gathering economies, cultural complexity is used here as simply indicating social organizations that are more complex than the

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basal hunting and gathering egalitarian pattern. Fried has called this basal level of complexity the egalitarian society (1967:27-107) and has defined it as "... one in which there are as many positions of prestige in any given age-sex grade as there are persons capable of filling them" (1967:33). Egalitarian societies include most "typical" hunting and gathering band societies. Complex societies, on the other hand, are those which have limitations on positions of prestige or, as Athens states (1977:361) "A cultural system having a social hierarchy as a permanent institutional feature." Such societies are labelled by Fried (1967) as rank or stratified societies.

While in both the Northeast and Northwest coasts ethnographically we find foraging modes of subsistence, we find substantial differences in cultural complexity. If we look at typical hunting and gathering societies, whether the !Kung of Southern Africa, the Shoshone of the Great Basin or the Athapaskans of the Subarctic, we find a number of traits in common. The largest effective social unit is the band and the band size is usually small, 30-40 individuals (Martin 1973). Typically the band resides as a unit during only a small part of the year with families or small numbers of families being the face-to-face group during other times. The primary, and often only, economic unit is the nuclear family, the length of time spent at any single location is usually short, seasonal movements are the rule, habitations are usually flimsy and division of labor limited to age and sex. Ownership of property and ascribed status are weakly developed and many societies are agressively egalitarian.

The formal definition of a foraging egalitarian society, then, just hints at a variety of shared traits and insitutions. The general rule is, if social ranking is absent, the rest of the features listed above are present. But the Northwest Coast groups do have ascribed social statuses, and differ in most other features as well, from the picture presented above.

The Northwest Coast maximum social unit was the village, which often, if not usually, consisted of several hundred individuals. Further, this unit stayed together much of the year. While other seasonal settlements were present, much of the time these were short term work camps and, at other times, the groups involved consisted of a number of families. For most of the year, the usual social group was several times larger than that of egalitarian hunters and gatherers.

The primary economic unit on the Northwest Coast was usually the household which was probably about the size of an egalitarian band. These household units are most easily recognized on the north

coast where they are basically equivalent with the inhabitants of a gable roofed house, and hardest to see in the south where they may inhabit either separate shed roofed houses or compartments within them. While the organization within a household varied widely, the basic theme remained the same along the coast. Much of the year was spent at the winter village house site; in some cases these were occupied all the year around, while in others, the entire village moved as a unit to summer locations. Other situations of greater mobility and smaller units occurred, but the amount of movement and the size of the average social group differed greatly from that found in egalitarian foragers. Habitations, particularly during the winter, included truly large, well built structures. Division of labor included a fair degree of specialization, and ownership of property went to some lengths, including many resource locations as discussed below. While the amount and nature of social ranking varied widely, even among the Coast Salish, competition for prestige was important, and the amount of status positions and of the ascribed component differed greatly from that seen in the egalitarian foragers.

Many of these differences are more quantitative than qualitative; they can be seen as more complex variations on a theme. Having a larger social group at least allowed for more complex social interactions (Blau 1977); the complicated ownership patterns seen on the Northwest Coast can be derived from simpler patterns elsewhere; large substantial dwellings developed from small simple ones, rigid status hierarchies grew from less rigid ones, and so forth. The combined effects of these differences, however, result in a qualitative transformation in cultural complexity, from an egalitarian to a ranked society in Fried's terms. Just as in the egalitarian case, the formal definition only hints at a complex web of interacting institutions and effects.

Accepting that the Northwest Coast situation is one of cultural complexity does not mean that this sort of society is a necessary stage in cultural evolution from the egalitarian level. I think it is a common development, as archaeological examples such as the Natufan in late Pre-Neolithic times in Palestine indicate, but I do not claim it is the only route to cultural complexity. A possible alternative route is suggested by V. Miller's paper on the Micmac (this volume), via political rather than social organizational developments.

Explaining the development of the ranked society from an egalitarian one is the goal of this paper. How did this development take place? Why did it occur? What society would willingly go from a situation where everyone is considered in some way to be equal and

thus worthwhile, to one in which only some could have high status, where a large part of the society is placed beyond the pale, and where much of these distinctions are made on the basis of birth? The answer to this last question is that there was not much "choice" in the matter contrary to what workers such as Diener (1980) would argue. A rank society is not only abhorrent to modern ethics but is also economically (or biologically -- or ecologically) inefficient. Thus many traditional prime movers cannot be the main "causes" of this transformation, at least not on a gross level. The notion of intensification, however, combined with the nature of resource location can be used to describe a setting where this transformation would be all but inevitable.

INTENSIFICATION

Intensification is a word which means different things to different people, but is one that, like "function" or "adaptation" stands for a crucial concept, though one difficult to define unambiguously. Intensification is frequently used in the sense of "relating to a method designed to increase productivity by the expenditure of more capital and labor rather than by increase in scope" (Webster 1974:601). Intensification is also used, I think, to include increase in scope as well. While this definition would include my idea of intensification, a better description of the process I envisage is:

A process where at time T_0, an amount of time (or capita) X is spent on an activity to give a production or result of Z. The rate of return R (productivity or efficiency) is X divided by Z. At time T_1 the amount of time spent is now X + something and the return is also Z + something but the *rate* of return is now R + something. Thus an intensification process is one in which as more time is spent on an activity, not only is the amount of return or production increased but so is its efficiency.

This view of intensification might be conceived as a two way positive feedback loop. The first loop is one where more time is being spent and thus a greater return is occurring on successive cycles. The second loop is one where through time successive changes in the organization of resource exploitation increase the rate of return making the overall process more efficient through time. So intensification, as used here, means increasing investment, production and efficiency.

A now classic example of this kind of model is that of Flannery (1968) on the origin of maize agriculture in Meso-America. Here, as

more time is spent planting and harvesting the precursor of maize (apparently teosinte), progressively more efficient forms are developed, which leads to spending more time developing better forms until the amount of time, the amount of return, and the efficiency are all great. Thus maize became a staple of the diet of Meso-Americans through intensification. Clearly this is only part, but an important part, of the explanation of the origins of maize agriculture.

This same general "intensification process" has been suggested as a major mechanism of culture change by Buckley (1967). Wood and Matson (1973) have presented it as a mechanism of change when one is dealing with internally initiated change. In this more general form the second positive loop is preceded by some sort of mechanism creating varieties of organization and some of these varieties are selected by the culture system. In the specific intensification case these varieties will vary in efficiency and the more efficient varieties will be selected for "reproduction," everything else being equal.

It is in this last statement that we can see how the intensification process leads to greater efficiency. This point of including efficiency within the definition of intensification has been a bone of contention to others, who argue, like Boserup (1965), that through time, intensification can lead to decreased productivity or efficiency. Efficiency here increases in the short run or at a given level of production. If increased levels of production develop, the proper comparison for efficiency is with an equivalent level of production of one or a group of subsistence activities.

Boserup (1965) uses intensification to refer to increased cropping of a set piece of land and thus a more "efficient" use of land (more return per unit of land). Boserup suggests that in terms of output per labor unit, this process is one that goes towards efficiency, or lower rates, although this point is disputed. Whether Boserup's version of intensification would fit the present model would depend on the price given to land, if, in fact, output per labor hour does decrease. If it does not, then it does fit nicely.

The notion of efficiency also is called into question on another front, that of the definition offered by Earle (1980) and Christenson (1981). They define efficiency as marginal costs, that is, the addition to total cost caused by the addition of one unit of production (Earle 1980:8). This definition of efficiency is justified by its being theoretically the optimal evaluative cost unit when there is no major initial cost. The cost measure can be very different from the average unit cost measure used here. I doubt that intensification occurs without extensive initial costs.

I expect that through time one would progress from procurement technologies with low initial costs, like those modelled by Earle and Christenson, to one with high initial costs, as seen in intensified technologies.

Efficiency, then, is subject to a number of definitions which have greater or lesser utility depending on the situation. Problems with efficiency are not eliminated by clearly defining it, as here, by cost per unit, as then what are the appropriate costs becomes the question. Further, the question of what is the appropriate unit of comparison is also present.

In what way can intensification processes be said to become more efficient? As argued above, for any given level of output, an intensified procurement process will increase in efficiency over time. This should be true if only the most obvious cost in hunting and gathering societies, time, is included. It has been suggested that if this is so, then we could visualize a system in which the output remained the same but the labor costs decrease. I think this situation is possible, but it is not one that leads to intensification as the returns remain constant, so that the importance of that activity remains relatively constant. So, given a set level of output, which appears to be the proper means of comparison, intensification results in gains in efficiency or productivity.

The other part of our definition of intensification is the increase in scale. Here gains in efficiency can again be found by comparing cost per unit at a set level, but at the level found at the larger scale. Thus at the time T_1, Z + return is found; if we pushed the technology at time T_0 to Z + return we would find the cost at time T_0 to be higher. In this sense, also, efficiency must clearly increase, everything else being equal.

Actually this last case would probably not occur, as the system at T_0 could not be pushed to the higher level of outputs. The real comparison would be with other alternative procurement schemes, and as intensification occurs, its relative efficiency must increase compared to the alternatives or intensification would not continue. Instead, the other alternatives would be followed.

These arguments are beside the point in that they implicitly assume that the cost per unit will actually increase through time. There is little evidence to suggest that this does occur in intensification processes, but then there is little evidence either way as discussion of Boserup's work has shown.

A fundamental problem with using economic models is that it is

difficult to incorporate changes in processes as well as in choice. Thus it is relatively easy to maximize some variable in a static situation, as one does with linear programming, and it is also relatively easy to discuss changes in processes, such as the "learning curve" but difficult to deal with both kinds of events in an inter-related model. Intensification is a process which includes both sorts of events, increasing efficiency and a selection for increasing scope (and a selection against other subsistence activities) by maximizing costs in a more synchronic fashion.

One Northwest Coast subsistence activity that appears to fit this model is pelagic sealing on the west coast of Vancouver Island. Gay Calvert has recently completed a dissertation concerning this area (Calvert 1980). Ethnographically pelagic, or deep sea, hunting of fur seals was an important part of the subsistence pattern. Today fur seals are migratory and stay well off the coast. One can imagine a process in which at first only a few fur seals wandering near the coast are taken with primitive water craft and equipment. As time goes on and more experience is obtained, so are better water craft, more specialized tools and more open ocean seals. At this point a more specialized technology is developed, one with greater initial costs, but one much more efficient at obtaining fur seals. Now that hunting fur seals in the open ocean has become an extensive activity other resource procurement activities must have been cut back (the scheduling conflict of Flannery (1968)). As pelagic fur seal hunting becomes more intensive, possibly other sea mammals are taken ("embedded" in Binford's (1979) terminology). This intensification of fur seal mammal hunting has taken place in the last 1000 years according to Calvert (1980).

A different situation occurred at the Chatham Islands off New Zealand where similar seals were hunted (Sutton and Marshall 1980; Sutton 1982). Here, the only rookeries were on rocks immediately off shore, unlike the situation on the Northwest Coast, where the fur seal rookeries, at least today, are far off in the Bering Sea on the Pribilofs. While the limitations on Vancouver Island were technological, at least in the short run, as at that procurement level the inhabitants could not make serious inroads into the fur seal propulation, on the Chatham Islands the situation was otherwise. Over-hunting is almost immediately a real threat to continuing production. Moreover, technology is not a limitation, as only a low level is needed to get to the off shore islands and harvest the seals.

Sutton (1982) does refer to an intensification of seal hunting on the Chatham Islands. Here he is describing settlement pattern

changes where the settlements become located near seal rookeries and how seals become a more important part of the diet. While this amount of "intensification" occurs, intensification does not continue because of the easily reached limits of production of the local rookeries. In this situation continued intensification leads, in the long run, to less return and to lower efficiency. The long term return is governed by the production of the rookeries and not the technology or organization of the hunters. Even though the resources, the environments and the technologies were similar on the Chatham Islands and the West Coast of Vancouver Island, in one case continued intensification is possible, in the other, it is not. As the Chatham Islands are some 1000 km from New Zealand, the earliest inhabitants had an impressive seagoing technology. Perhaps because high technology is not necessary to hunt seals on the Chatham Islands, Sutton found that devolution occurred, at least in technology.

These two brief sketches present a key idea about intensification; it can only occur in certain circumstances where the nature of the resource allows it. If a society were to vote on intensifying the use of a set resource (and I doubt that this sort of choice ever occurred), unless the nature of the resource was such that, given the appropriate technology, more returns for less costs per units could occur, intensification would not take place. The nature of the resource exploited limits the amount of intensification that can take place.

INTENSIFICATION ON THE NORTHWEST COAST

Beyond doubt the key resource in the Northwest was salmon. This is not only attested to by the ethnographies, but also by demonstrations of strong relationships between amount of salmon and ethnographic population estimates by Sneed (1971) for the interior and by Donald and Mitchell (1975) for the central coast. Salmon were exploited, both extensively and intensively, on the Northwest Coast.

Salmon have been exploited in the Pacific Northwest since Old Cordilleran Culture times (6000-8000 B.P.) with remains being found both at The Dalles on the Columbia River (Cressman *et al.* 1960) and at the Glenrose Cannery Site on the Fraser River (Matson 1976) during this time. Other attributes of the ethnographic Northwest Coast pattern are much later, however, ca. 2500 B.P. (Matson 1981a) and there is little evidence of the intensive exploitation seen ethnographically extending back to Old Cordilleran times.

I have argued that for the coast, the drying and storage technology may have been a more important prerequisite for large scale use of salmon than the procuring technology (Matson 1976, 1981a). Schalk (1977:230-231) has argued that a storage strategy is more important to the north. Since Schalk's argument is based on the shorter period of availability to the north, Burley (1979b) has extended it to upstream portions of major rivers, where availability would be similar to further north and where diverse coastal resources are unavailable. In this situation with abundant but temporally limited salmon resources, the addition of storage is seen as promoting sedentism and specialization (1979b:138). Intensification of salmon procurement along the coast would then follow only later. These ideas will be discussed in more detail below.

The basic model for salmon intensification is very simple. As more experience is gained with using salmon, and as salmon getting and storing technology becomes more developed, salmon fishing becomes both more efficient and more important. What is not so simple is under what conditions this situation is allowed to continue. In addition the sources of salmon getting and storage technology and the interaction of salmon resource exploitation and other subsistence activities, or seasonality and scheduling in Flannery's terms (1968), must be examined.

Turning to these additional factors, salmon producing and storing technology has been thought by some to develop first in interior situations. In this view necessity is the mother of invention. As ably pointed out by Schalk (1977), where runs are short, if one is to use salmon resources extensively, one must have an elaborate technology to catch the salmon and dry them before they spoil. In interior, northern areas, this is the situation, as documented for recent times by Hudson (1980).

As Schalk has pointed out, in the far north and in the interior in the middle north (45°-55°N) in the upstream portions runs are not long enough or reliable enough to serve as a foundation for a complex, semi-sedentary society (1977:242).

Burley's scheme has the locus for development of intensification occurring 100 miles upstream on the Fraser River. Here, at the first constriction of the river, numerous kinds of salmon would be available, without the greatly restricted availability and reliability found further upstream (1979b:138). Further, the fish could be obtained by minimal technology, that of the dip net. The well known canyon winds and less clouded skies would make drying the catch a less difficult proposition than at the river mouth.

While I have no doubt that salmon were first exploited in small streams, I have doubts that either scenario is at all correct. The technology for dealing with small interior streams cannot be easily transferred to the coast. Ethnographically, and, as far as we can tell, archaeologically, the technology is quite different, with storage pits ("cache pits") and dip nets found along interior streams of moderate size but not on the coast. Not only is the technology different, but so are the conditions. These include not only the larger, slower moving streams, but also the length of the runs, the makeup of the fish, and the weather conditions. The combination of a cooler, moister climate and fish with a much higher oil content means that the drying process is much more difficult along the coast. So the problems and solutions differ between coastal and interior regions, notwithstanding which ones were solved first.

The results of salmon intenstification on the coast and interior differ as well. While intensification works for awhile in the interior, blocking streams with weirs or other "efficient" fishing techniques can cause a reduction in production, and the cyclic fluctuations make heavy reliance impossible for all years. The much greater numbers and kinds of anadromous fish in the lower reaches of the major streams and the absence of river blocking fishing techniques meant that neither of these problems were important on the coast.

In Burley's suggested Fraser Canyon scheme, the problems of year to year fluctuations in fish and over-fishing would not exist. But as Burley (1979b:138) notes the canyon is essentially an "interior locale" and the dip net (and interior style processing techniques) are used. Thus if salmon intensification occurred first at this location we would expect a significant time lag before the technological transformations needed to transport it to the coast could be developed. The mechanisms for such transfer to the coast are not developed by Burley, but I think some credible ones could be easily developed.

Burley (1979b:139) argues that ground slate knives may be correlated with intensive salmon processing (for drying) and that their presence in the Eayem Phase in the Fraser Canyon is evidence for early intensification of salmon in the canyon area. I find the widely used argument of ground slate knives and extensive use of fish worthy of serious consideration but the archaeological presence of it at earlier times unconvincing. The ground slate knife fragments found in Eayem Phase do not have clearly documented provenience in print (Borden 1968b:14-15) and are from poorly documented layers adjacent to a large pithouse of the multicomponent

Esilao site. The actual pieces informally attributed to the Eayem phase are small, further suggesting the possiblities of mixing. More importantly, the much less confused Eayem component of the Mauer site, also in the same general area, lacks any such ground slate knives, and is much better documented (Le Clair 1976). Until ground slate knives are unequivocally found in Eayem context, this part of Burley's argument is invalid. In short, Burley's interesting "hybrid" scheme with interior technology but coastal abundance at this point fails to have archaeological support.

In sum then, the coastal technology cannot be imported directly from elsewhere and there does not seem to be any reason to believe salmon intensification took place earlier in the interior. While there is a long history of salmon use on the coast, crucial aspects of the ethnographic pattern were missing until the last 3000 years -- whether or not the storage aspects are as important as I have suggested (Matson 1976, 1981a). What exactly were the developments on the coast cannot be currently determined with precision, but the following model is offered as a reasonable possibility.

The first inhabitants of the southern B.C. coast were clearly oriented to large mammal hunting and belonged to that widespread tradition which has been called Old Cordilleran Culture (Matson 1976, 1981a; C. Carlson 1979). The Old Cordilleran subsistence pattern clearly included salmon fishing. This is verified by locational and seasonal information (late summer-early fall) at the Milliken site (Borden 1975) and by fish remains along the Snake River (Bense 1972), along the Columbia (Cressman 1960), and at the mouth of the Fraser River (Matson 1976, 1981a).

At the mouth of the Fraser, the Glenrose site reveals a spring and early summer occupation with the excellent seasonal markers of eulachon and sticklebacks both being present (Casteel 1976b, Matson 1976). While fish (and a few shellfish) remains were present in this component, deer and elk (wapiti) faunal remains were definitely dominant.

For Old Cordilleran groups living close to the coast we can suggest the following seasonal round. From non-coastal locations they would move to the coast in late spring to harvest eulachon, sticklebacks and other fish, as well as some shellfish. Elk, deer and seals would also be sought. In summer salmon could be obtained in local small streams with a simple technology. It is possible that trips would be made in late summer into the interior where salmon is more accessible with a low level of technology. In the fall, hunting of large mammals would be important and winter sites would probably occur inland in the vicinity of large wintering

ungulates. In late spring the cycle would being again. A family-band dichotomy probably existed with a low population density and little status differentiation.

How did this basal use of salmon with little or no storage develop into the ethnographic situation? The next stage, as seen in the St. Mungo phase component at the Glenrose site (4500-3300 B.P.) (Matson 1976, 1981a) is one of more extensive use of coastal resources, particularly of shellfish and salmon. The previously used resources were all still being exploited but the seasonal evidence suggests that the coast was exploited at various times of the year, rathern than spring and early summer. Since shellfish are available during the winter with a low level of technology (no storage needed) I would expect that they would attract people to the coast during this most difficult season, and we do have evidence of use of shellfish during this season in this component (Ham 1976; Matson 1981a). Extensive remains of shell are found during this time, creating a shell midden, although the lack of ground slate knives would seem to indicate an absence of an intensive storage technology for salmon.

Concurrent with this change would be a relative reduction of large land mammals, at least during the winters. One would expect the generalized fishing technology to become more efficient as more experience accumulated. The greater amounts of time spent near good fishing areas would encourage the development of more specialized and higher initial cost technologies. It may be that gill net fishing (Suttles 1951) or drag (bag) net fishing (J. Kew 1976) was invented at this time. There is no evidence for extensively occupied winter villages, large habitation structures, or non-egalitarian society during the St. Mungo phase. From this broad scale hunting, gathering, and fishing subsistence base, emphasizing coastal resources, the ethnographic subsistence pattern developed through focussing on salmon.

The switch from large land mammals to other small and more numerous resources is, of course, not unique to the Northwest Coast. The development of the archaic in the New World and the mesolithic in the Old show this trend to be near worldwide in extent. The reasons for this trend are detailed in Hayden (1981) which is partly reveiwed below and partly extended by Matson (1981b). This pervasive development is a precondition to intensification but obviously is not in any way an explanation of it.

Since the carrying capacity for large land mammals is low in this area, and since shellfish can be overcollected and difficult to obtain in dark, stormy winters, it is no surprise that salmon became

the resource that was emphasized. Even before an efficient storage technology was developed salmon could be an important resource in an area such as the Gulf of Georgia where they are available for a large part of the year, as suggested by Schalk (1977:229). It was not, however until efficient ways of getting large numbers during the peaks of the runs and storing them were developed that large, dense populations and extensive winter villages could develop.

The curing and storing technology consists of important but unspectacular ways of butchering, cutting, spreading, smoking and storing fish under conditions which were often adverse. The procuring technology included reef net fishing in which a large, semi-fixed net was suspended in front of a reef between two canoes. This method, as J. Kew (1976) points out, has obvious similarities with the more primitive drag net, which was suspended from two canoes in a river. A single reef net position could involve over a dozen individuals to obtain the fish while others processed them on shore (Suttles 1951).

The reasons for successful intensification of salmon fishing on the coastal rivers are clear. The numbers of fish were too large to be seriously interferred with at this level of technology and the runs were long and reliable enough so that large numbers could be processed once the curing and storage technology was developed. The large numbers of fish existed because the fish had to swim through the lower reaches of the river to get to their upstream spawning grounds, and the length and reliability of the runs a result of different races and species going through the lower reaches of the river at slightly different times and having different peak years. The intensification process allowed denser, and probably more sedentary populations. As put so ably by Hayden (1981) increasing population density is essentially a one way gate.

In Hayden's model population density is determined by a trade off between "*the cost of physical suffering* every so many years and *the cost of maintaining population controls*" (1981:522). The trend is to minimize the effects of resource stress whenever possible, which has the effect of increasing population density. Population density can only be decreased by high costs in either (or both) directions. In Hayden's view (and mine) population densities are within sight of the "carrying capacity" on bad years and maintain themselves there *even though the limiting density is subject to change as the carrying capacity changes through technological innovation* (or environmental changes). (Environmental changes could be caused by climatic changes or be the result of human activities, such as over-fishing.) Hayden terms this the "resource-stress model" and would argue that what is being maximized is resource

reliability and that increasing population density is only a consequence of this maximization, or so I infer. Thus increasing population density would result from these changes in subsistence and settlement patterns which coincided with changes in social organization.

SOCIAL ORGANIZATIONAL IMPLICATIONS

Schalk (1977:236-237) argues that the amount of control in social organization where anadromous fish resources are important should increase where availability is limited and year to year fluctuations are great. He quotes Suttles (1968) to the effect that formal social organization is more developed towards the north as support of his argument. The situation is reversed from what he expects, however, when one compares coastal and interior groups in the Northwest. Why is it along the coast that the more complex, more rigid social organization occurs, along with relatively less fluctuation and greater availability of salmon resources?

I think the answer has to do with ownership of resources. In the interior ownership is weakly developed and access to most resources is freely available. Along the coast we find ownership of most important resources and even stretches of beaches. Ownership arises when a resource is important, reliable, and localized and when the society is relatively sedentary.

If a resource is not reliable and fluctuates, control will not usually be important. Since such a resource is not predictable, it is not efficient for the local group to exercise control over it, since much or most of the time sufficient return would not occur. This would be particularly so if access to other such resources occurs in other areas.

On the other hand, if a resource is reliable, control over it is made economic by the consistent return and since it is predictable, technology can be developed to fully utilize it.

Looking at these aspects of resource natures from a slightly different but complementary perspective, for an unreliable resource the most important aspect is whether or not it will occur in abundant amounts. For a reliable resource this is not important, but access to it is. If a resource is not localized, access is assured, but if it is localized, access may not be certain. Access to a reliable resource is only important if it is abundant. Therefore efforts at controlling access would be expected if the resource is abundant, reliable and localized, but not otherwise.

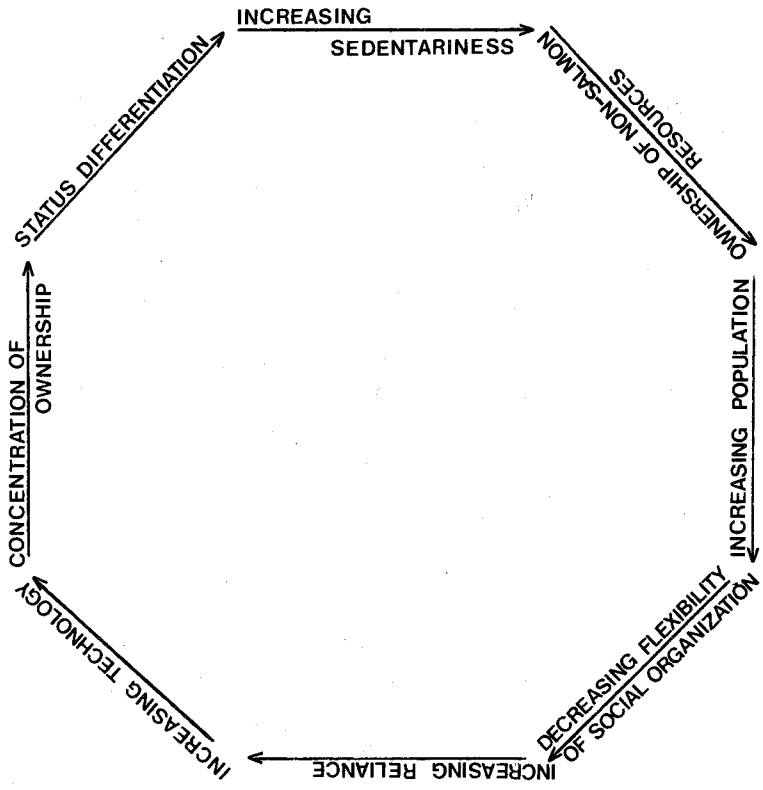

Figure 1. Situation in Interior Salmon Streams.

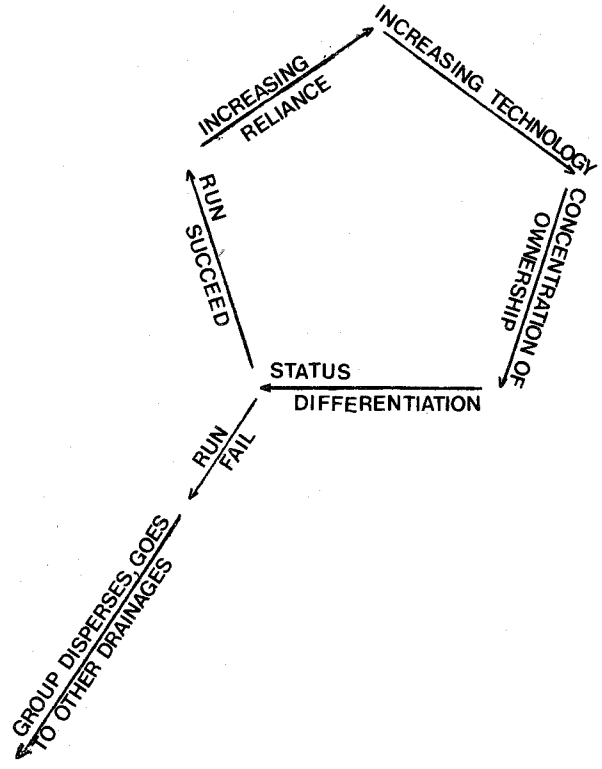

Figure 2. Situation on Coastal Portions of Large Salmon Streams.

Effective control can only be exercised if the local group is relatively sedentary near the resource. Otherwise another group could appear at the location earlier in season, if it is seasonal, or just use the resource sometime while the original group was not present, if it is not seasonal.

In a situation where resources do fluctuate so that they cannot be depended on, the centrifugal cycle of more dependence and ownership is broken each time the resource fails, no matter how localized and abundant the resource is when it does occur. Thus in Hudson's (1980) example, when the run failed, the Carrier groups broke up and went as families to visit groups having access to other river systems (Figure 1) which had different runs. If this is apt to happen every few years, we can see how society would be flexible and ownership would not be important in spite of an impressive technology and reliance on salmon.

Schalk (1977:237) argues that as seasonality in productivity increases, periods of resource availability decrease, and year to year fluctuations increase, specialization upon anadromous fish becomes more difficult and, eventually, an impossibility. The Carrier described by Hudson may be close to this limit. Elsewhere Schalk (1977:230) suggests that upstream groups should have more generalized forms of subsistence. He does not, as far as I can see, explain how groups become more and more rigid as the resource becomes available during smaller parts of the year and, then, suddenly, become more generalized and less rigid. Hudson's example suggests how rigidity was not possible in such conditions, in spite of specialization.

In contrast, along the coast, where failures such as this were much less frequent, dependence and ownership would increase without any levelling mechanisms. A resource location such as a reef net site is too small to be shared by a large group, and too few existed to allow free access, so ownership by a smaller group (household?) was inevitable. In contrast with the interior, where almost all other resources beside salmon are widely scattered, other resources along the coast are typically located at specific places along the shore. With the higher population density and shorter seasonal rounds allowed by the salmon intensification, control of these resources by members of local groups was both possible and feasible. Once the most important resource locations are "owned" it is easy to see how all localized resource locations would soon be controlled (Figure 2).

In contrast with the interior situation, where few resources were owned, the coastal social organization became much less

flexible. First, flexibility on the same scale was not needed (i.e., local salmon resources could not fail and so the option of joining groups with different salmon resources would be unnecessary) and secondly, most of the other important resources would be owned and thus could not be used by someone who was not well integrated into the local group. Once resources were owned, inequities relating to access and inheritance would occur as well as changes in technology which would affect the relative worth of the resource locations. The most extreme case may be reef net locations, where the owners hired captains and crews for a part of the catch, with the owners not participating in the actual exploitation of the fish (Suttles 1951:161-162). Specialization here occurred at several levels, reef net fisherman (crew), reef net captain and reef net owner. The end result of such inequities would be a ranked society such as seen ethnographically.

While it is relatively straightforward to explain why the upstream interior groups did not develop culturally complex societies, it is more difficult to deal with groups along the main streams but downstream enough so that long, large and reliable runs occurred, such as the Fraser Canyon area. Here the resource is surely reliable enough and relatively unaffected by fishing, so why not a complex ranked society? I think the answer has to do with a lack of localization of resources or a need for high technology. If a complex system of weirs is used and can be used only at certain parts of the river, as reported by Hudson, resource and/or technological control will develop if the reliability and importance is there.

Along the downstream parts of the Fraser River the technology used was the simple dip net, which does not require a complex social organization, as one might argue something like a reef net does (Schalk 1977). But more importantly, dip net locations are not localized but extend up and down the river banks. Further, other important resources such as deer, elk, berries and root crops are also widely dispersed. In such a situation if social inequities develop over access to the best dip netting stations, the solution for a family that does not have good access to the best stations is simple, move upstream or down and use the best unused one. In a sense one trades poor access to the best locations for good access to not-so-good locations.

Along some rivers, such as the Skeena, culturally complex groups did inhabit the lower reaches. It would be interesting to examine these rivers and compare them with other rivers where this did not occur. It may well be that this was not a primary development but occurred secondly to developments along the coast.

Intensification of salmon fishing on the Northwest Coast can be seen to have several different important aspects. It was stable, allowing feedback trends to continue. These included increasing technology, specialization of labor force, increasing importance, and increasing ownership. Salmon resources were also inexhaustible so that increasing production did not have an important effect on run size (survival rates from egg to fingerlings are thought to be the most important factor). The trends of increasing ownership of and increasing importance of salmon in a large part brought about the inegalitarian ascribe aspects of society so noted on the Northwest Coast. The increasing importance, or increasing production, also allowed the high population density and large settlements seen ethnographically on the Northwest Coast.

Several aspects should be pointed out here. The first is that not all groups had access to the same resources or to the same reliability of resources. The complex situation pointed out by Suttles (1968) for the Salish is probably functionally equivalent to the interior flexibility. So trade, potlatches and widespread kin networks evened out the fluctuations on the coast instead of the wholesale splitting up and moving of groups as in the interior where the fluctuations were greater.

This view about the linked nature of sedentariness, ranked society and important, reliable resources is not unique to me. Others have pointed out that, in general, hunters and gatherers need to have a reliable, large scale resource to become sedentary and that the ownership or control of such resources, if inherited below the level of the community, would create inequalities in status differentiation of kin groups. The more detailed description above of the Northwest Coast case can be thought of as an example of this general process. A similar view of Northwest Coast developments has been expressed by Sutton (1982).

In any disinterested inquiry it is important that ideas about the nature of things be evaluated in a non-tautological fashion. What evidence is now available that bears on these ideas? What observations might be made in the future to confirm or falsify this model?

The core of the model is that ranked society, sedentariness and large scale use of salmon resources should all be tied tightly together and that, initially, one should not occur without the other two. It is important to note that once such a system is in existence, successful variants that no longer have all the attributes of the original will become established. For example, we can see areas on the fringe of really successful salmon producing

areas, whether acculturated to a sedentary, ranked society, or offshoots of it, finding ways of making up salmon shortfalls through some other process. In time this new variant would spread through the area in which it is a potentially successful adaptation and variants of it also would develop. Hayden's "Resource Stress" model explains why the new variant would spread. The general mechanism is that suggested by Wood and Matson (1973). So the crucial point is that the *initial* occurrence has all three factors.

In the Gulf of Georgia area, the Marpole culture is widely thought to be the first archaeological manifestation of a sedendary, ranked society. Although the evidence for this is indirect there are a number of lines of evidence, and those who have investigated this question are in agreement (Borden 1970; Mitchell 1971:54; Matson 1976:304, 1981:85; Burley 1979b:134, 1980b:60). So the Marpole culture ought to show evidence of being dependent on salmon, at least in its initial stages, to a greater extent than earlier or later cultures.

A number of lines of evidence suggest that this is the case. Mitchell (1971:52) states that "From the distribution of the Marpole sites it seems the subsistence of the type was closely identified with the major fish runs of the Fraser River, and it is probable that a mainstay of the economy was the salmon runs, perhaps even to a greater extent than was true of the Gulf of Georgia type." Burley (1980b:43, 45) shows that the mean distribution of the Marpole sites with early radiocarbon dates is in the Fraser Delta, suggesting that the culture originated there. Matson, Ludowicz and Boyd (1980) have shown that all typologically early Marpole sites are clustered in the Fraser Delta. So it does appear that the Marpole culture developed adjacent to the lower Fraser River, one of the most important salmon sources in the Pacific.

Ham in his dissertation (1982) has reviewed Gulf of Georgia archaeology and concluded that salmon were most important in the Marpole culture with earlier and later periods showing less emphasis on salmon. His evidence is admittedly not as good as one would like, but is at least partially independent of, and a confirmation of, the above distributional evidence.

The present archaeological evidence, then, is in accord with the model, but by no means can be said to have confirmed it. Falsification is much easier to do in a definite manner, and findings of sedentariness, with or without ranking, in the little-understood Locarno Beach culture which immediately precedes Marpole, would certainly do so. Other findings in Locarno, such as ranking with sedentariness, or ranking with sedentariness but

without an emphasis on salmon would also do. In fact findings of any one of sedentariness, ranking or large scale use of salmon, or any combination, except for the joint occurrence of all three together, would falsify this model. The presently known distribution of Locarno Beach sites, however, is not centered on the Fraser Delta, which is mildly supportive of this model.

The more general model might be examined by careful cross-cultural comparison of hunters and gatherers which have ranked societies, or by archaeological investigations elsewhere of such groups. Since I have argued that the nature of the resource that is intensified is all important, one might look at an area that has resources that are apparently similar to those found on the Northwest Coast but that does not have comparable cultural complexity, and ask why not. Such a case, appears, at least initially, to be the Micmac on the east coast.

THE NORTHEAST COAST, THE MICMAC

In contrast to the Northwest Coast, cultural complexity in the Northeast was relatively low. Why did the situation occur? The Micmac were too far north to growm maize, but had access to resources broadly similar to those available on the Northwest Coast. The Micmac were contacted very early and were greatly influenced by this contact so that the ethnographic records are much less reliable than those for the Northwest. Hoffman (1955) has made an attempt to gather such information as exists together, and the following account relies heavily on his material.

Many questions remain about the basic attributes of the Micmac that make comparisons difficult. For instance according to some the population density was much lower, possibly one tenth, than that on the Northwest Coast (Hoffman 1955:230) while others (Miller 1976, 1980a) suggest comparable densities. The size of local communities is likewise contentious. The seasonal dispersion of the population is reversed from that of the Northwest Coast, with summertime villages and winter dispersion. The summer aggregations might average around 60 persons (Hoffman, population size after contact, 3000; 45 known villages) or have 200 or more individuals (Bock 1978:109) according to different workers. For the purposes of this paper I will assume that the estimates that are most like those for the Northwest Coast are correct. In this way we may be certain such differences as remain did exist in precontact times. So the population density and maximum community size appear to be similar, although the seasons of aggregation are reversed.

Many of the important resources were similar in both areas. Sea mammals, fish and shellfish were all important to the Micmac. During the spring, smelt, herring, flounder, sturgeon and salmon were all taken, along with shellfish. In the summer, a number of sea fish were obtained; in fall, salmon, herring and brook trout were taken. In the winter smelt, tom cod, seals, eels, and walrus were obtained along the coast; beaver, moose and caribou, inland (Hoffman 1955:153). The period of maximum dispersion was during winter when small groups were inland hunting, but Hoffman suggests that this winter inland pattern may have developed in response to the fur trade (1955:233-236). Even if this was so, the summer villages still dispersed in the fall to smaller settlements in areas along the coast and small rivers. Burley (1981a) suggests that riverine locations near the coast may have been occupied during fall and winter while a wide variety of resources were exploited.

Other aspects of the Micmac culture also differ from the Northwest Coast. While there does appear to be some complexity in terms of political organization, the highly ranked society with ownership of resources found on the Northwest Coast is absent. There is little evidence of sedentariness. The summertime villages were not occupied for long and differ greatly from the Northwest Coast winter villages. The only status position that appears to have an ascribed component is that of chief. I think this is due to warfare and a reflection of a more widespread Woodland pattern (see Miller, this symposium), and not an important part of everyday life in Micmac society. In most other aspects, except for elaborate feasts and the summertime aggregations, the Micmac are close to the basal pattern of hunters and gatherers, discussed earlier. If Miller is correct, they did have a high population density, but this does not by itself lead to a ranked or sedentary society. Following Martin (1973), areas with rich resources can be expected to have more hunting and gathering bands rather than larger or more sedentary bands, although one would also expect the total distance travelled in the seasonal round to be less. In general, then, the Micmac contrast strongly with the Northwest Coast.

The explanation for this similarity of resources and differences in cultural complexity lies in the Micmac lack of intensification of resource utilization. As described, the Micmac show similarities with the St. Mungo archaeological phase described previously; intensive use of coastal resources during a variety of seasons but lacking intensive use of a single resource. When an intensification process occurs, the time allotted to processing the intensified resources interferes with processing other resources available only at that time. The switch from one resource to the other will only occur if the first resource is giving more return

than the second (leaving aside such obvious exceptions as nutrients, important ceremonial purposes, etc.). So after intensification occurs there will be a reduction in range of activities (again leaving aside specilization which can increase the range of activities by having different people do different things). The decrease in range of activities and increase in yield leads to increased sedentarism and to the possiblity of increased complexity.

Why did this intensification not occur in the Micmac? I think the most obvious answer is that the salmon in the east is not as abundant as the Pacific salmon. The Atlantic salmon (*Salmo salar*) is a member of the same genus as the western steelhead and has a life history which more closely approaches that of the steelhead than the generically different Pacific salmon (Scott and Crossman 1973:192-197). The numbers involved, while greater than those for steelhead, are but a small fraction of those for the Pacific salmon (compare for instance, Dymond 1963:389 to Scott and Crossman 1973:171 for the sockeye). The absolute abundance of the Atlantic salmon appears to be somewhere between one hundredth and one tenth of the Pacific salmon, and in spite of Rostlund's statement to the contrary (1952:26), there is little doubt that it always has been so. Further, the river systems in the east that have salmon runs are small, making the east-west differences greater. While the major streams in the west gain reliability of runs through having different species and races of salmon, in the east there is only one species and the small size of the streams makes the probability of different races existing remote.

To sum up, as it was put to me by one familiar with both types of salmon, but professionally involved with Pacific salmonids, "There are no Fraser Rivers in the east." To which I might add, no Marpole-like cultures, either.

Rostlund also makes a claim for more fish per square mile of land in the east than in the west (1952:52) but this is spread out over a variety of types of fish. If so, this density of fish might help to explain the density of Micmac as argued by Miller but does not give the preconditions of a single, large, reliable resource which I have argued is necessary for the development of a sedentary, ranked society on a foraging base. In the Micmac territory there appears to have been no single resource that was significantly more important than the Atlantic salmon.

CONCLUSIONS

It has long been held that the developments seen in the Northwest Coast culture area have been related to the resources available, but the exact relationship has not been spelled out nor tested in detail. The model presented here is a special case of both a more general model of culture change and a more general model of the relationships between sedentarism, ranking and resource base for hunters and gatherers. The evidence that we have today is in line with expectations generated from the intensification model. Further, the model appears to be successful in explaining the lack of Northwest Coast-like developments in the Northeast coast.

I do not think that the resource intensification route is the only one possible to cultural complexity. As suggested earlier, I think it is a common one, but there are other routes and other forms of complexity, such as seen in the development of political complexity seen in the Micmac. The intensification model needs further development; there are a number of features left blank at this time. Clearly, for ownership to be successful, sedentarism, as argued above, allows one to look after the resource, but only if it is localized. If the resource is diffuse, it would be difficult to control. What kind of limits are needed for control in terms of geographical or seasonal spread are not stated. Further, the mechanisms of ownership or control are not specified. Yet even in this initial formulation the model is sufficiently complete to enable us to generate a series of expectations, and the examination of this certainly supports further efforts to develop and test these ideas.

Acknowledgements:

This paper is really fourth in a series of attempts to understand the development of the Northwest Coast culture (Matson 1974; 1976; 1981a). There have been many who have helped me in these attempts; I am grateful for their assistance.

The late Wilson Duff gave generously of his time at an earlier stage, Michael Kew has always been helpful, Thomas Northcote has been a wealth of information on fish and fishing, and Wayne Suttles has been a consistent source of inspiration. The Anthropology 520 seminar in 1978-79 provided the source of the substantive model presented here and Douglas Sutton gave a very interesting lecture on the Chatham Islands in 1980 which stimulated some of the ideas presented here and he has been working along parallel lines.

Ronald Nash and David Burley have been informative about Micmac archaeology, and Burley's work on the Marpole has been a source of stimulation. Virginia Miller has been particularly generous of her time and thoughts on the Micmac, and David Aberle has not only been a rigorous critic but an important source of ideas as well. Brian Hayden has always been a source of simulation and I am grateful for his careful analysis of an earlier draft. Bill Lipe provided his usual careful critical and precise comment as well. Finally John Wood provided some of the background and a lot of inspiration for this continuing work.

SETTLEMENT LOCATION DETERMINANTS: AN EXPLORATION OF SOME NORTHWEST COAST DATA

Philip M. Hobler

The relevance of the following study of west coast data to east coast archaeology at the moment lies more in method than in conclusions. The work constitutes a first step in probing the logic of settlement location on the central portion of the Northwest Coast. Perhaps ultimately this logic can be stated in the form of an equation which, minimizing historical factors, will take into consideration several elements. These should include attributes of the locality itself such as flat ground, drainage, availability of potable water, suitability for landing watercraft, and accessability for inter-regional trade. Also included should be proximity to food resources such as land and sea mammals, migratory and non-migratory birds, intertidal bivalves, bottom fish, and especially the anadromous fish. On the Northwest Coast one would expect that the anadromous salmon would have been of paramount importance in determining settlement location. However, the following study seems to show that on the Central Coast the massive food potential of the runs of spawning salmon has little specific local effect on archaeological site distributions.

From a distance of some 5000 km it is difficult to say what implications these findings may have for the study of East Coast settlement distributions. One would not expect that the relative importance of the various food resources on the two coasts would be exactly the same. Shellfish availability on the two coasts, although not yet systematically measured, may well be similar. One

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suspects that the cod and other bottomfish, extant in immense numbers on the Northeast Coast, may actually have been more available for harvest given aboriginal watercraft and tackle in the protected calmer waters of the Northwest Coast. Certainly the aboriginal salmon fishery on the Northwest Coast far outweighs in food value the fish harvest possible with similar technological devices on the Northeast Coast.

For the archaeologist on either coast the significance of food remains found in archaeological sites seems to be getting more and more difficult to assess as zooarchaeologists become more deeply mired in problems of sampling and as ethno-archaeologists discover the innumerable ways that dietary habits can get translated into archaeological remains. Perhaps subsistence in the archaeological record can be better studied by taking a wider view of food resources over an entire region. When looked at in this way it is clear that foods are rarely uniformly distributed. Archaeological surveys show that evidence of human use of a region is also unevenly distributed. The question asked in this study is: to what extent does variation in resource distribution relate to variation in the distribution of archaeological sites?

In the last decade an extensive, if not particularly impressive, archaeological and ethnological literature has accumulated around the theme of "catchment analysis." In the author's opinion, catchment studies are most profitably applied to groups of fixed location, such as farmers, and to peoples whose food storage and preservation practices are so weakly developed that the food obtained on a given day is usually eaten on that day. Simplistic catchment studies seem to work least well with peoples having efficient long range transportation, a long-term food storage capability, and a custom of inter-group exchange that can vastly broaden access to regional resources. These three traits, of course, are characteristic of the Northwest Coast. Taking them into consideration one might well predict that significant resources could be found outside of easy commuting distance of archaeological site concentrations. Thus, the potential for conventional "catchment" type studies on the Northwest Coast may be limited. But how can this be tested? One approach is to look at the resource base in the vicinity of a single large site. Croes and Hackenberger (1981) at the Hoko River site have been conducting a meticulous assessment of the marine and terrestrial resources within a one day commuting distance. At the Crescent Beach site Ham (1981, pers. com.) has been surveying the complex intertidal resources on the adjoining beach for comparison with his excavated samples. These are important studies but both focus only on single sites and thus cannot take full cognizance of seasonal transhumance, trade and storage.

On the Central Coast we have an archaeological survey sample that is large and reasonably complete. Unfortunately we have few quantifiable measures of the distributions of aboriginal food resources in this region. In fact, on both the east and west coasts the quality of biological inventories and assessments lags far behind that of archaeological survey. It would be most desirable to relate the full complex of food resources throughout the whole central coast study area to the total archaeological site distribution. But, with the exception of the salmon, the scarcity of biological data makes this impossible. Coastal ethnographies emphasize the preminence of salmon in coastal subsistence. The contemporary commercial fishing industry on the west coast is heavily dependent upon this valuable resource. For this reason federal agencies have been keeping detailed records of spawning streams. In fact, spawning conts for 46 salmon streams on the Central Coast have been recorded over the last 20-40 years. The number of individuals of each salmon species has been tallied each year for each stream and entered into voluminous stream catalogues. Thus, we have available on a region-wide basis detailed and reliable data albeit for only a single resource.

To address the question of the relationship between the distribution of archaeological sites and the distribution of this resource I began with the analysis of the fishery data from the inner or eastern half of the central B.C. coast, essentially the area eastward from Fisher Channel (Fig. 1). An assumption is that the relative productivity of the various streams today is similar to what it was in late prehistoric times. The analysis asks this question: to what degree are the number and kind of archaeological features in the vicinity of salmon spawning streams correlated with the modern food productivity of those streams? The potential food yield of a stream is figured by calculating a mean tonnage for each stream. This is done by multiplying the mean number of fish of each of the five salmon species as counted over a 20-40 year period by average weight for each species.

The "vicinity" of each stream is defined as a 5 km radius. This amounts to a paddle of one or two hours in a canoe or a walk taking about the same time, in other words, the range within which it is possible to travel out, collect the resource, and return in one day.

Correlation coefficients relating these mean salmon weights to the number of archaeological features within a 5 km radius of each of the counted streams on the eastern portion of the Central Coast were computed (Table 1).

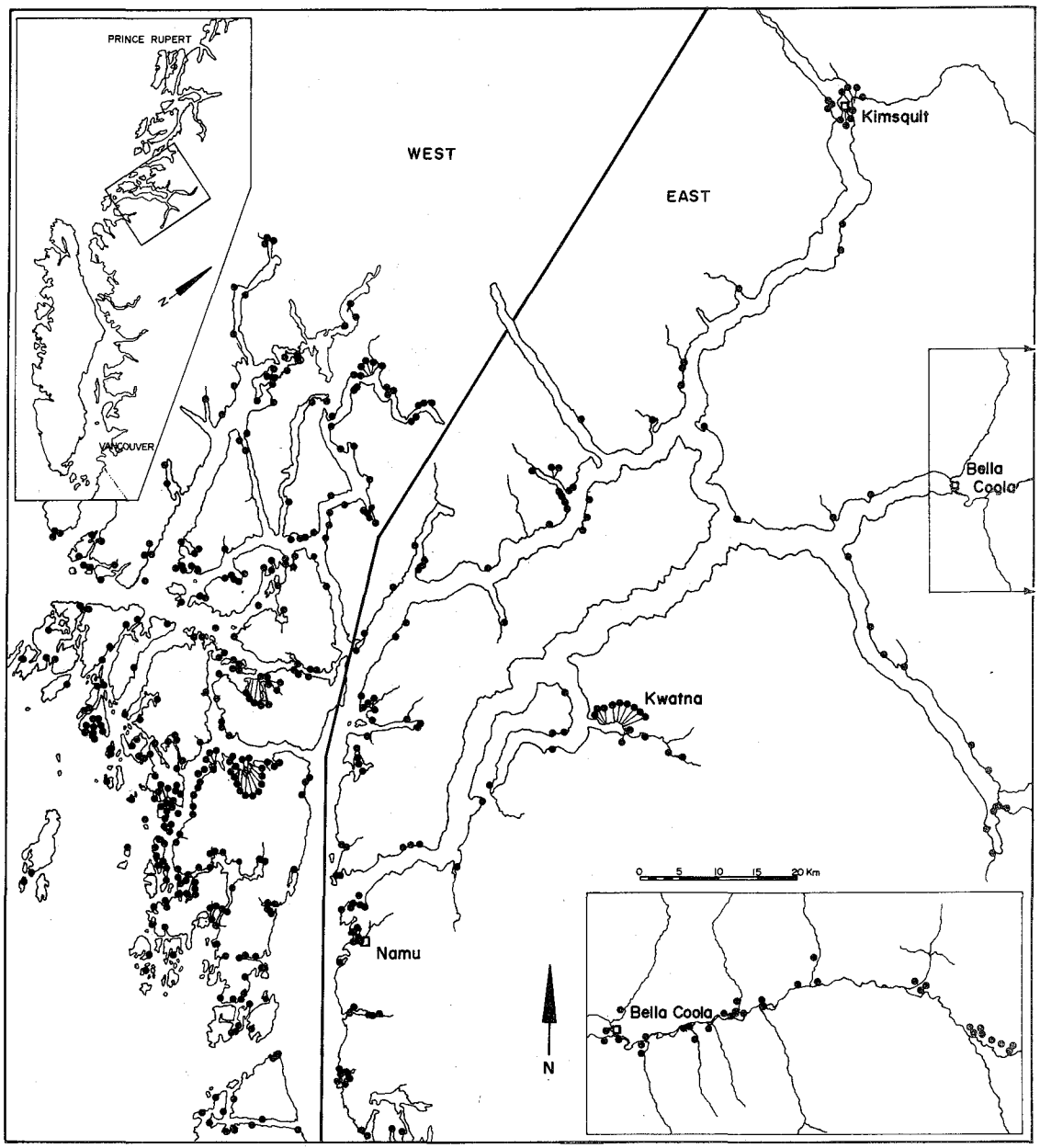

Figure 1. Archaeological sites on the central portion of the British Columbia coast. Eastern and Western halves of the study area are indicated.

Table 1. Correlation coefficients showing the degree to which the numbers of archaeological features in the vicinity of salmon spawning streams varies with the food productivity of those streams as measured by the mean tonnage of fish returning to spawn.

Archaeological Features	Mean Tonnage East	West
Pictographs	.39	-.13
Middens	.27	-.23
Burials	.01	-.07
Fish traps	.72	-.04
Historic artifacts	.23	.01
Wooden structures	-.07	-.05
Intertidal artifacts	.06	.05
Surface artifacts	.72	-.16
House pits	.63	-.13
Other architecture	.29	-.16
Petroglyphs	.75	-.15
Total Features	.52	-.23

It was not surprising to see that these figures indicate moderate positive correlations suggesting some "attraction" effect of salmon upon settlement. A second look brought into focus two reasons for doubting this interpretation. The eastern portion of the central coast is characterized by high terrain relief (Fig. 2). In the fjord landscape most of the shoreline is steep and beachless. Almost all of it is unsuitable for camps or villages. Localities suitable for settlement are largely limited to the deltas and lower reaches of rivers. Thus, areas suitable for settlement are quite fortuitously associated with salmon spawning streams. Probably an even more important factor contributing to the spuriously high correlation are the rich oolachin runs in the inner coast rivers. The oolachin arrive almost six months after the salmon have gone from the rivers and thus provide a major and timely replenishment of food stocks. Haggarty has recently emphasized that resource diversity is the key to understanding nootkan settlement concentrations on Vancouver Island (Haggarty and Inglis 1981). I would further add that in the Bella Coola area resource timing and duration are of similar if not greater importance. Thus, on the inner coast the apparent correlation between salmon tonnage is spawning streams and the number of associated archaeological features in fact reflects the combined effects of terrain and the presence of *two* major food resources occurring in the same place but six months apart.

Figure 2. View along Dean Channel on the central portion of the British Columbia coast. The fjord landscape predominates along the eastern or inner portions of this part of the coast and is characterized by steep terrain and the near absence of beaches or other land forms suitable for habitation.

On the western (Bella Bella) portion of the central coast we are free of the complicating factors of terrain and availability of oolachin in the spring. The terrain on the west is much flatter (Fig. 3). Localities suitable for settlement are ubiquitous and are not limited to the vicinity of salmon spawning streams. By expanding the analysis to include the western area, the survey sample is also greatly increased (Fig. 1). Calculating Person's *r* values relating archaeological features and salmon tonnage for streams in this area we get results showing no correlation at all or even a slight negative relationship (Table 1). How is this rather surprising result to be explained? Certainly Pomeroy's exhaustive analysis of the historical and ethnographical records for the western portion of the Central Coast has shown that when aboriginal band territories can be reconstructed and plotted on maps no territory is complete without at least one good salmon spawning stream (Pomeroy 1980). The work of Donald and Mitchell (1975) has also substantiated the importance of access to productive spawning streams in determining a band's status. Yet, the correlations fairly clearly show that, when factors of terrain can be held constant, there is little statistical evidence of a relationship between the amount of harvestable salmon in a stream and the number of archaeological features in its vicinity. Certainly this does not mean that the salmon were an unimportant food resource. Rather it shows that when other factors are held constant, immediate proximity to salmon spawning streams was not an important determinant of site

Figure 3. Fish trap site on Troup Passage on the outer or western portion of the Central Coast of British Columbia. In this area of low terrain relief localities suitable for habitation are numerous.

location. Why should this be? It must be remembered that the duration of many of the spawning runs is actually quite brief. Often it was possible to harvest a significant portion of a group's annual salmon needs in only a few weeks. The small streamside camps resulting from this activity tend to have low archaeological visibility. In addition the relatively brief stays by small groups would also result in fewer non-occupational archaeological features such as rock art.

If the salmon resource is not responsible for dictating specific intra-regional archaeological site distributions what is? Do these results mean that within a region (or within a band's territory) another resource or combination of resources properly weighted will constitute the determinative part of the settlement location equation? Or is it all influenced by something apart from resources such as accessibility for trade or other more complex historical factors?

By way of conclusion there should be an important lesson in this. If one were doing archaeological research on the central coast employing a simplistic "catchment" approach and operating in an ethnographic vacuum the fact that the intra-regional distribution of archaeological features does not seem to show any tendency to reflect variations in distribution of salmon might well result in a conclusion that salmon were not an important food resource. Only the richness of the ethnographic record can prevent such a error and can show the limitation of simplistic interpretative schemes.

Acknowledgements: The author wishes to express appreciation to Morley Farwell of the Canada Department of Fisheries and Oceans for making spawning data and other information available.

CULTURAL COMPLEXITY AND EVOLUTION IN THE DEVELOPMENT OF COASTAL ADAPTATIONS AMONG THE MICMAC AND COAST SALISH

David V. Burley

INTRODUCTION

The northeast and northwest coasts of North America, in many respects, share a highly analogous geographic and physical environment with regards to human adaptations. This includes both similarities in exploitable resources as well as concomitant restraints such as cyclical fluctuations in the anadromous salmon and environmental stress caused by sea level shifts. Yet despite the recognized similarities, there developed cultural systems so diverse that one finds it difficult to identify even a small number of overlapping characteristics. On the northeastern coastline we find an indigenous population that, with a few exceptions, are typically considered to be hunters and gatherers operating on the composite band level of social organization with all of its implications (see Steward 1955; Service 1962). On the other hand, the northwest coast populations, while still considered to be hunters and gatherers, had achieved a level of cultural complexity bordering on the "chiefdom" level. This includes what Fladmark (1975) has labelled an extravagance in art, architecture and social organization along with an extremely sophisticated technology for resource procurement. How did such variability occur and what are its implications for the archaeological study of coastal systems?

Central to the following paper is the principle that, while such differences do exist, each is an adaptive response to a regional

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ecological milieu. In this regard, one cannot simply pursue a straightforward reductionist philosophy and argue that one element or its absence in the geographic environment is the fundamental basis for an evolution of Northwest Coast complexity as opposed to northeastern egalitarian band organizations. Rather, each adaptation must be viewed within its regional context over time. Such being the case, each of the coastal adaptations is independently modelled in subsequent sections. The ethnographic populations upon which this paper will focus are the Micmac of the eastern Maritime provinces and the Coast Salish of the Gulf of Georgia region (Figure 1). Both groups, it may be argued, are analogous in their composite exploitation of maritime, lacustrine and riverine resources. They are, in effect, first fishers and only then hunters and gatherers.

Micmac Adaptations

The Micmac of the Canadian Maritime provinces are possibly one of the best and earliest ethnographically documented groups along the eastern seaboard (see Hoffman 1955; Wallis and Wallis 1955). From the early ethnographies (i.e., Biard 1959; LeClercq 1910), we find an adaptation characterized by composite band organization, utilization of both coastal and riverine ecozones and, finally, an assigned winter hunting territorial complex. Hoffman (1955) has questioned the efficacy of this adaptation within its regional context and argued it to be a result of modifications induced by the fur trade. Using the available ethnohistoric data, he argues for a more extensive maritime orientation with a brief inland hunting period during the winter. Recently, I have questioned this interpretation on the basis of coastal resource availability and the known distribution of archaeological sites (Burley 1980a). In turn, the data appear to suggest a precontact subsistence pattern which was neither coastal nor interior but one highly attuned to resource diversity in several econiches. Included here would be a much greater emphasis on the riverine ecozone within the Micmac subsistence strategy (Figure 2). This model can be characterized as a generalized hunting and gathering adaptation whereby no single resource is seen to dominate subsistence pursuits and, hence, restrict settlement pattern mobility. In an ecological sense, this pattern would approximate a fine grained exploitation.

The regional context in which the Micmac are found has an overall similarity in resource diversity. However, it is important to emphasize that considerable variation does exist in the density of those resources as they occur throughout the Maritime area. Because of this, one must be extremely cautious in applying a single subsistence strategy model. For example, if we assume that

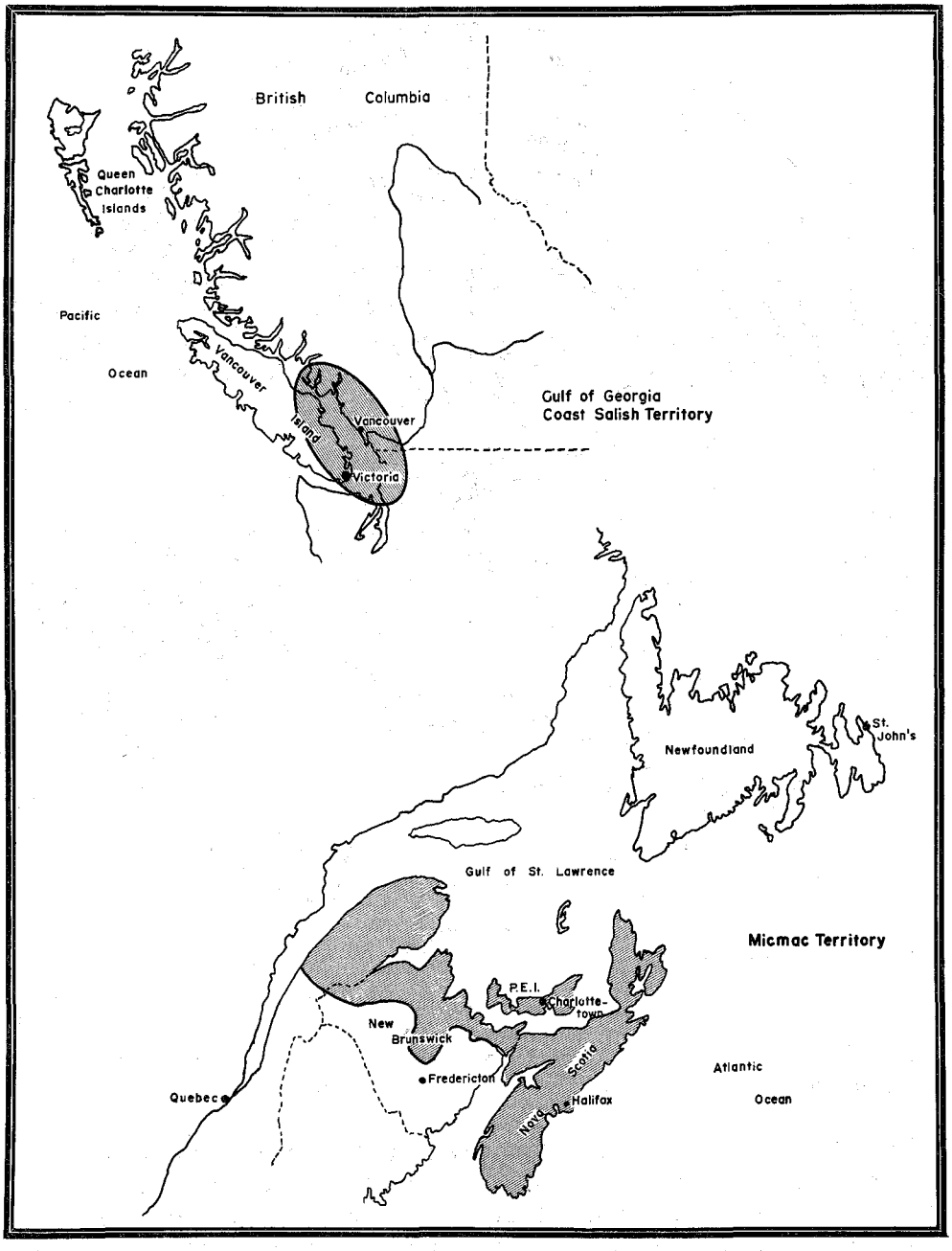

Figure 1. Micmac and Gulf of Georgia Coast Salish territorial boundaries.

commercial catch statistics for the Atlantic salmon are a reflection of individual watershed densities (see Huntsman 1931) then it is possible to argue for considerable variation in abundance. A similar case may also be made for shellfish resources (Burley 1980a; Newcombe 1936). Because few studies have looked at potential differences in localized adaptations, it is difficult to project exactly where these differences may be. Nash (1980a) has suggested that at least for the Cape Breton area a concentration on eels may

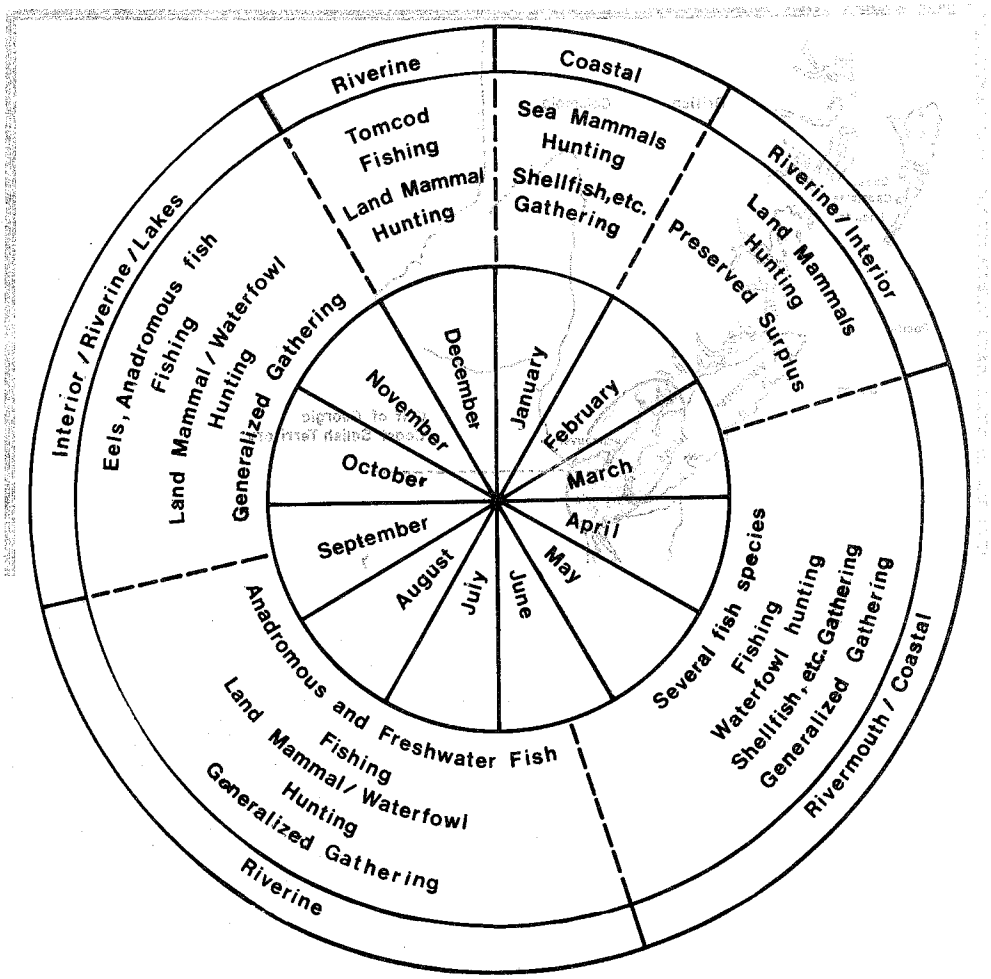

Figure 2. Precontact Micmac settlement pattern cycle.

have attained a much greater importance than that usually attributed to the anadromous species.

Despite the fact that intraregional variations most probably did exist, there is no evidence to imply that the generalized character of the adaptation differed to any considerable degree. We must ask, therefore, what are the supportive mechanisms behind that adaptation? As Hoffman (1955) intimates, clearly one of the most important considerations must be the great range of diversity in the available resource base. In addition, I would further suggest that the nature of those resources, their distribution across the landscape and seasonal variations in productivity climaxes would actively select for the generalized hunting and gathering adaptation (also see Christianson 1979).

As I have noted, the Micmac seemed to have had a strong riverine/lacustrine orientation. This is not overly surprising considering that a minimum of eight anadromous fish (salmon,

alewife, shad, smelt, sturgeon, striped bass and white and yellow perch), another that might be considered partially anadromous (tomcod) and one that is catadromous (eel) are present. Spawning runs for each are varied throughout the year with a number being present during the cold weather period (tomcod, eel and smelt). It is also of note that the spring run of Atlantic salmon (*Salmo salar*) can be procured from holding ponds in interior locales during the summer and early fall. While I do not want to give the impression that these resources totally dominated Micmac economic activities, they most certainly were an important element in the overall subsistence strategy. This resource base was crosscut by a variety of other available food species which, in many cases, could be conjunctively exploited from a central place. For example, a rivermouth/coastal locale in the early spring allowed for, in addition to the procurement of several anadromous fish, an exploitation of coastal foreshore resources and the interception of northward migrating waterfowl (see Biard 1959:81). Perhaps the only period where the Micmac larder was severely reduced was that of late winter where a greater reliance on land mammal resources was necessitated. This, however, could be offset through preserved surpluses from the summer and fall.

The presence of a preservation and storage technology, while yet to be proven in the archaeological record, does have a basis in the ethnohistoric and ethnographic literature. Christian LeClercq (1910:110), for example, makes mention of both drying and smoking in meat and fish processing. LeClercq's observations document the years between 1675 and 1687 (Christianson 1979:85). Similarly, Speck and Dexter (1951) note the presence of smoked salmon, shad, herring and gaspereau while Wallis and Wallis (1955:61, 64, 251) provide various references to food preservation. The importance accorded to food storage is another matter. Despite the fact that they are probably overstated, the comments of Father Pierre Biard could be taken as a reflection of the Micmac perspective on storage and future needs. He states:

> This nation takes little care for the future, but, like all the other Americans, enjoys the present; they are not urged on to work except by present necessity.... If you tell them that they will be hungry in the Winter: *Endriex*, they will answer you, 'It is the same to us, we shall stand it well enough: we spend seven and eight days, even ten sometimes, without eating anything, yet we do not die.' Nevertheless, if they are by themselves and where they may safely listen to their wives (for women are everywhere better managers), they will sometimes make storehouses for the Winter, where they will keep smoked

> meats, roots, shelled acorns, peas, beans, or prunes bought from us, etc.
>
> (Biard 1959:107).

Baird's observations are succinctly supported by LeClercq (1910:110) who concluded that "They (the Micmac) are convinced that fifteen to twenty lumps of meat, or of fish, dried or cured, in the smoke, are more than enough to support them for the space of five to six months." In light of these statements, I would conclude that food storage was but a minor effort limited to providing dietary supplements for a short winter period. The Micmac environment appears to have been viewed as one of steady state abundance within the Micmac adaptation.

To hypothesize on the development of this adaptation, one must turn to the evolutionary context of regional prehistory. For the Maritime provinces this is no simple task. From an archaeological perspective, it may be one of the poorest documented areas on the eastern seaboard and the limited knowledge that is present shows an extremely complex pattern that poses more questions than answers. Nevertheless, these data in conjunction with those from surrounding areas, do allow some speculation on general trends.

Snow's (1980) recent synthesis of New England prehistory effectively reviews the current state of knowledge of cultural sequences and changing adaptations in those areas immediately south of the Micmac occupied territory. Speaking of northern Maine and, by implication, the Maritime provinces he posits a tundra based paleo-Indian population with a localized adaptation on large game animals and a "free wandering" settlement pattern strategy. With the local geography being dramatically transformed into dense forest regimes predominated by white pine after 10,000 B.P., Snow among a host of others (Sanger 1979b; Ritchie and Funk 1971; Fitting 1968) postulates a severely reduced carrying capacity which would have necessitated drastic alterations to the adaptive pattern. This transition would require a shift to a more diffuse or fine grained adaptation and of necessity, a "restricted wandering" strategy (Snow 1980:171). Tuck (1974) characterizes this time as a period of "settling in." With but a few exceptions, (Sanger 1979c; Benmouyal 1976; Tuck and McGhee 1976), all of which occur outside of the Maritimes, we lack firm archaeological data to accept or reject such a hypothesis. After circa 5000 B.P. (see Sanger 1979a), the forests of northern Maine are seen to be dominated by various hardwoods. In turn, the regional carrying capacity is increased and, not unsurprisingly, we have what appears to be a more intensive, albeit complex prehistoric occupation documented in the archaeological record.

The picture is complex because of the conflicting views held by regional archaeologists and the virtual absence of controlled excavation on period occupation sites in the Maritime provinces. Wright (1972) has proposed that, at least for the interior highlands, we can expect to find elements of his Shield Archaic (also see Sanger 1971a). Coterminous with the Shield Archaic (but see Tuck 1975c) and more or less adapted to a coastal/riverine setting is a regional variation of the Laurential tradition (Sanger 1973) with its associated Moorehead burial complex. On the other hand, Tuck (1978) suggests this regional variation to be virtually identical to that of cultural developments in Newfoundland and Labrador and defines it as the Maritime Archaic tradition. However, while the Maritime Archaic would be present along the Fundy Bay coastline of New Brunswick, all other areas including the northeastern shore would be occupied by populations of his Lake Forest Archaic tradition (ibid). Until more data are in, it may be somewhat premature to argue for the validity of one scheme over any other. Nevertheless, each of these concepts does propose an adaptation that exploits a range of localized resources including the anadromous fish runs on a seasonal basis. Greater reliance on land and sea mammal hunting and possibly some specialization on swordfish (Sanger 1975) seems to differentiate the pattern from those of the later precontact era.

The terminal archaic presents an equally complex setting in the archaeological record of the Maritimes. Even so, I along with others (Allen 1981:133) believe the evidence is *slowly* coming to the fore to possibly extend Sanger's (1975; also see Snow 1980:245) and Bourque's (1975) hypothesis of a late archaic population replacement or assimilation by peoples associated with the Susquehanna tradition (but see Tuck 1975c). Sanger (1975:72) has proposed such a model to account for not only stylistic changes in a few artifact types, but drastic shifts in the subsistence, settlement, technological and mortuary subsystems. Moreover, he is able to relate it to rapid changes in the marine ecology of the Gulf of Maine and the development of foreshore resources as well as a transition in the forest regimes to increasing conifer densities. The Susquehanna movement is seen to be an expansion by groups already adapted to the new environment while the population being replaced or assimilated would be subjected to considerable environmental stress requiring drastic adaptive alterations. Snow (1980:248) also notes that the linguistic split between Micmac and other eastern Algonquian languages may be tied to the terminal Archaic.

Whether or not the dislocation model proves to be true has extreme implications for determining the origins of the Micmac precontact adaptive pattern. Susquehanna subsistence patterns appear to have been a highly diffusive exploitative strategy with,

possibly, a more intensive use of the soft shelled clam (Bourque 1975) and a greater concentration on migratory fish species as found in the primary tributaries (Turnbaugh 1975; also see Snow, 1980:247-248). This pattern with its decreasing emphasis on marine resources and increasing exploitation of lacustrine and riverine species includes all of the basics in the previously defined subsistence model. Once in place, it undoubtedly underwent alterations to account for localized variations in the resource base. Nevertheless, the basic principles, including a supportive egalitarian social organization, are suggested as being present.

Arguing that the Susquehanna tradition is a precursor of the Micmac peoples implies a cultural continuity from circa 3500 B.P. up to contact. This, in fact, may not be the situation. For example, in northeastern New Brunswick there can be little dispute that the Augustine mound had an association with Adena developments (Turnbull 1980). Such being the case, Allen (1981:144) has argued there to be an infiltration of Adena related peoples by 2600 B.P. This infiltration is seen to intensify over time. Subsequently, she also proposes that by 2100 B.P. this population was actively interacting with a second immigrating group from the south and west. Characterized by "Lagoon and/or Rossville" projectile points, these peoples brought with them a subsistence pattern heavily dependent upon shellfish. An *in situ* ontogenous cultural development is proposed for the post 2100 B.P. period.

That Adena and later influences are present cannot be disputed. These, however, might equally be explained by a regionalized participation in a broad based exchange network (see Snow 1980:268) supported by the widespread Adena mortuary complex. Whatever the case may be, there exists little evidence to suggest a radically altered subsistence strategy for these groups. The generalized pattern is suggested to have not only been maintained, but continued to be maintained up to the protohistoric period.

Because the Micmac seem to have been linguistically isolated to the point of a separate grammatical evolution (Goddard 1978:76) from that of Maliseet-Passamaquoddy, the linguistic data may be taken to support a relatively segregated existence to those on its borders for possibly as much as 2000 years. A large part of this segregation might well be related to the facts of geography. For example, assuming that post archaic prehistoric populations are concentrated in areas adjacent to at least intermediate sized tributaries, the northwestern shore of Fundy Bay (from the Saint John to Petticodiac Rivers) would have been poorly suited for concentrated settlement. In addition, since Nova Scotia and Prince Edward Island are in themselves virtual geographic isolates, a

sustained interface between proto Micmac and other groups would be primarily restricted to interior northeastern New Brunswick and, possibly, portions of the Gaspé Bay. With the exception of the latter, I would suggest that because of different adaptations, interior/riverine versus coastal/riverine, there may have been only limited and formalized interaction. In essence, there may not have been two populations competing for the same set of ecological resources but, instead, two side by side adaptations geared toward differing environmental regimes.

To summarize my views on the precontact Micmac adaptation, I have argued that contrary to the historic pattern, it can be characterized as one well suited to its environmental resource base. It was a generalized subsistence pattern which, because of the anadromous fish species, had a strong riverine focus. It is hypothesized that at least the major elements of this adaptation were in place by the terminal archaic period and only minor changes occurred up to the time of contact. These changes would be the result of variations in the localized resource base, fluctuations over time in the density of those resources and, perhaps, environmental stress caused by rising sea levels (see Grant 1970). Population expansion up to the regional carrying capacity limit is also taken to be a given.

GULF OF GEORGIA COAST SALISH ADAPTATION

The Coast Salish adaptation, as found within the Gulf of Georgia region, can be considered as one variation of the general Northwest Coast pattern. Based on an intensive use of the salmon resource and a semisedentary settlement pattern, the ethnographic population had many characteristics normally associated with a chiefdom level of cultural development yet remained on a hunting and gathering sphere in their economic structure. Ascribed ranking, surplus production beyond subsistence needs, wealth accumulation and highly developed artistic and ceremonial traditions are but a few of the seemingly aberrent traits. Although it is probable that considerable culture change had occurred with white contact (Burley 1980b; Collins 1974), the ethnographic model of a specialized economic adaptation can be considered appropriate for the late prehistoric period.

Because of variations in localized resource availability and differing fishing strategies (Mitchell 1971), a single subsistence-settlement pattern description for the Gulf of Georgia Coast Salish would be inappropriate. The specialized concentration on the salmon resource along with an efficient preservation and storage technology, nevertheless, is found throughout. This adaptation

generally allowed for a highly dense population (e.g., Sneed 1971; M. Kew 1976) and is considered to be a focal point for most other cultural developments (Suttles 1968; Schalk 1977). It is considered to be a good example of coarse grained exploitation where one resource has a disproportionate use *vis a vis* the total range of those which are potentially available. This is not to mean that there was a neglect of other available resources. Rather, it simply argues for a subsistence adaptation that is dominated by the procurement of a single resource which, in turn, is central to a settlement pattern strategy.

The Coast Salish variant, as I have argued elsewhere (Burley 1979b, 1980b), can be recognized in the archaeological record back to the Marpole culture type of circa 400 B.C. Prior to this time, including the Mayne and St. Mungo phases as well as the Locarno Beach culture type, notable differences are present. In particular, there is a distinct lack of evidence for an ascribed ranking system as illustrated in the mortuary system; there does not appear to have been a use of the large multifamilied plank house nor are large village aggregates apparent; and, finally, there appears to have been a slightly varied economic focus with, perhaps, a greater concentration on maritime resources (see Boucher 1976; Borden 1968b; Carlson 1970b; Burley 1979b). These factors have led me to hypothesize a more generalized adaptation lacking intensive specialization on the anadramous salmon (Burley 1979b, 1980b). Concomitant with this adaptation would be "... smaller, more frequently mobile bands of egalitarian hunters and gatherers" (Burley 1979b:135; also see Matson 1976:303). In essence, while salmon may have been important in the seasonal round it would neither dominate resource scheduling nor would a massive surplus have been procured for the maintenance of a sedentary winter village population. Because of this, the regional carrying capacity would have been lowered and we can expect a significantly lower population density.

In pondering the potential explanations behind the transition of this generalized adaptation to the more specialized form, I was unable to find any internal cultural mechanisms which could account for such a drastic modification or, as Matson (1981c) has argued, the beginnings of intensification. This, of course, is based upon the belief that population size in a hunting and gathering society will remain at an equilibrium point below the carrying capacity level as implied in the Micmac adaptation. Hence, population pressure stimulating technological change cannot be used as an independent causal agent. At this point, it was necessary to look at external factors in the environment. Because of what appeared to be evidence in the archaeological record for discontinuity between

Marpole and earlier manifestations (Burley and Beattie 1977; Burley 1980b), it was suggested that the critical balance between population size and resource carrying capacity was upset by an influx or infiltration of peoples from the Hope/Yale locality. It is of note that the culture historical sequence in the latter locale has an abrupt discontinuity from the Baldwin to the Skamel phase at exactly the same time as Marpole is recognized on the Fraser delta (Borden 1968b). Finally, the population involved in this movement was considered to have had an adaptation already geared toward intensified salmon procurement and storage (for an explanation of the rationale see Burley 1979b:138-139). At no point, however, was it suggested that the Northwest Coast cultural pattern nor even the total technological inventory, arrived fully intact. To the contrary, most of the cultural developments associated with the Marpole culture type were seen to be *in situ* coastal adaptations (Burley 1980b:74).

In the past, I have placed considerable emphasis on the development and/or knowledge of a storage technology as the evolutionary threshold for Northwest Coast developments (also see Schalk 1977). While, obviously, such knowledge is important, it is the stimulus behind storage practises which must be given consideration. It must have some immediate and recognized adaptive advantage. The Hope/Yale locality is suggested to be better suited ecologically for storage intensification than adjacent coastal areas. For adapting populations, there are two primary resource groups which could serve as a focal point in subsistence strategies. These are the anadromous salmon and the larger terrestrial mammals. Lacking the diversity of the coastal zone, and in particular the marine and foreshore fauna, the anadromous species would form a significant proportion of the potentially available resource base. Moreover, changing forest regimes from a lodge pole pine dominated post-glacial to the coast-forest biome of 3000 years ago (Mathewes 1973) may have dramatically reduced the land mammal population density (Mitchell 1971:12) thus requiring even greater concentration on salmon procurement. The gains to be accrued in salmon storage are therefore considerable.

To summarize this argument, I would first suggest that the technological knowledge for food preservation by drying is not considerable and, undoubtedly, was known and probably practised by both coastal and upriver groups in pre Marpole times. The stimuli for intensive storage, however, are more greatly pronounced in the latter and would select for salmon specialization.

Provided that the above hypothesis is true, in addition to the population movement suggested earlier, we must still explain the

evolutionary development of the Northwest Coast cultural pattern. Assuming that it is basically a coastal adaptation, I have previously presented a speculative model in which the principle mechanisms involved in this development are outlined (Fig. 3) (Burley 1980b:71-73; also see Schalk 1977; Langdon 1976). The following is a summary review of that model.

At circa 400 B.C. or slightly earlier, we find a population at the mouth of the Fraser River who, for better or worse, have a subsistence adaptation that includes intensified salmon storage for winter consumption. Because the technological requirements for catching and preserving a surplus of salmon in the area require a more cooperative effort than may have been necessary in the Hope/Yale locality, a greater complexity in labor organization is anticipated. The preceding argument is based on the individual dip net and drying strategy of ethnographic Fraser Canyon groups as opposed to a small stream weir or drag net exploitation required at the Fraser River mouth. Complexity in labor organization I hold to be the key principle in all further developments. First and foremost, it would select for the origins of a centralized head or big man. This leader could serve a number of group functions including regulation of labor expenditures, control over resource locales and redistribution of productivity. Second, and of equal importance, a greater cooperative effort requires an expansion in the size of the productive unit. In turn, we can anticipate a shift from a nuclear family based social organization to that focused on an extended family dependent upon ties in the male line. Extended families would also promote the acceptance of a larger multifamilied residence type.

Here it is important to consider the supportive nature of the ecological milieu in this development. Cyclical fluctuations in salmon escapement are a well documented occurrence in the Gulf of Georgia region (M. Kew 1976). In the Hope/Yale locale, to maintain a population equilibrium a group must keep its density at or below carrying capacity level in the lowest productive year in the anadromous cycle. The situation on the coast is somewhat different. In low productivity years, it is possible to exploit other steady state coastal resources to offset insufficient surpluses. In particular, the abundant foreshore fauna are seen to be an important factor which would allow for a population rise beyond that supported by salmon alone. Moreover, foreshore resources are considered to be low risk and, possibly, would select for the maintenance of formerly unproductive population members (see Pearlman 1980; Yesner 1980a). In combination with the need for a single locale for preserved surpluses, the semisedentary winter settlement strategy would be a direct outgrowth.

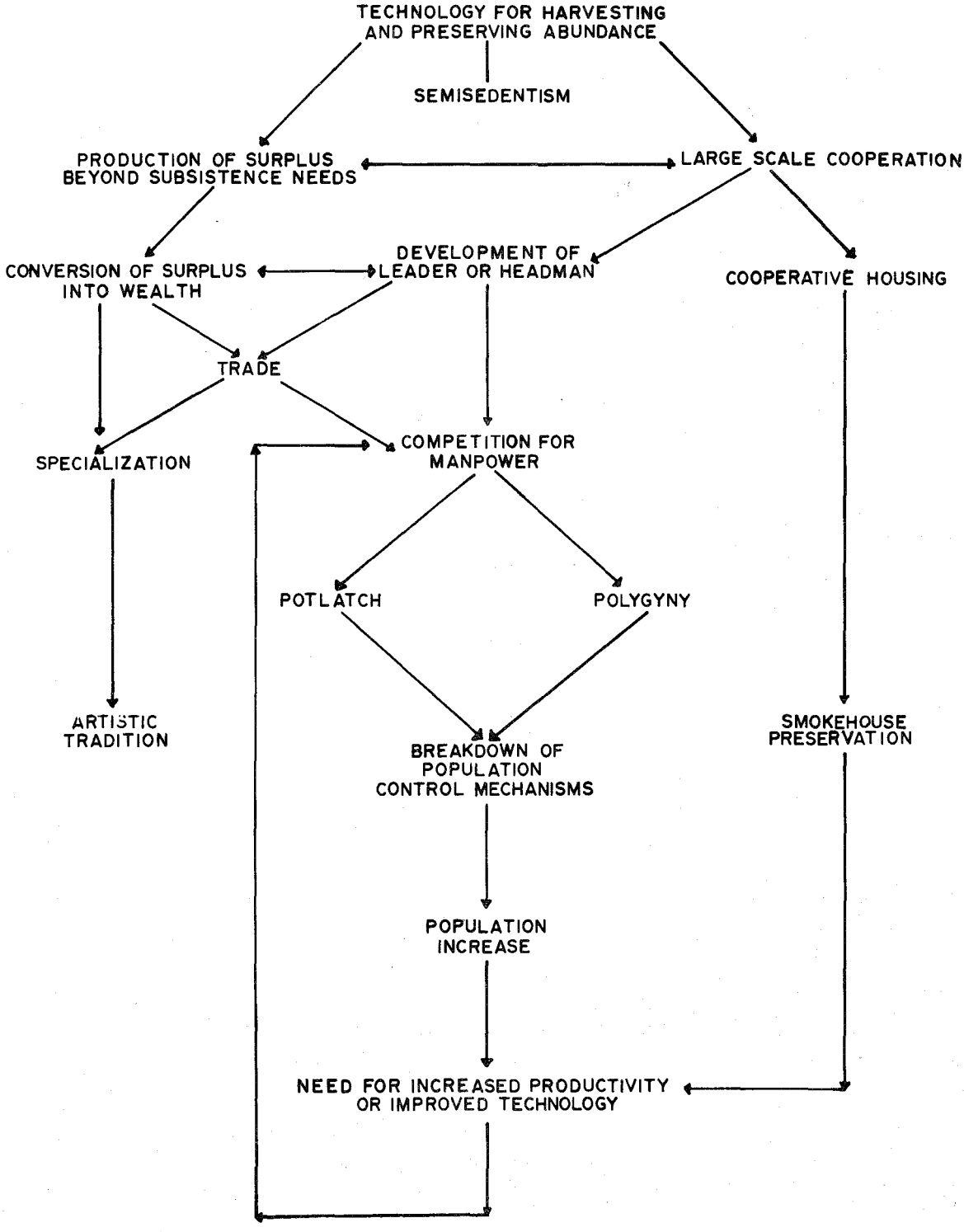

Figure 3. Schematic illustration for the development of a Gulf of Georgia variant of the Northwest Coast cultural pattern.

Each of the preceding developments allows for some increase in population density. In a high year in the quadrennial cycle, however, there would be a salmon surplus well beyond that needed for subsistence. When this happens, it is possible to project the expansion of intraregional exchange networks particularly in nonutilitarian wealth items. Because the group leader controls the surplus production, the accumulation of nonsubsistence wealth would also be a consequence. In turn, this provides at least a visual basis for social stratification and provides greater incentives for surplus production. As Matson (1981c) postulates, it is at this time that we should anticipate a positive feedback loop which selects for increased technological capacity, greater population density and eventually, the full development of the Northwest Coast pattern. Populations grow not because it is their nature to grow. Instead, I would argue that growth, in this particular case, was being selected for by several traits intricately tied into the development of a stratified social organization.

Because the Northwest Coast culture pattern is not simply restricted to the Gulf of Georgia area, one has to question the validity of this model *vis a vis* adjacent cultural developments. Beyond the incentives for salmon procurement and storage, I would hold that the general principles involved in the evolutionary development would be highly analogous (see also Schalk 1977; Langdon 1976). In the Gulf of Georgia, it has been argued that the generalized hunting and gathering pattern was superceded by a population with an intact specialized salmon adaptation as selected for in an upriver setting. The reasons behind such a transition in other coastal locations remain to be determined.

CONCLUSIONS

Implicitly, I have attempted to emphasize throughout the preceding discussions that in both the case of the Micmac and the Gulf of Georgia Coast Salish we are dealing with independent adaptations that must be viewed as such. Each can be characterized as an ecological population which, as defined by Kirch (1980:111), constitutes a "... group of interacting individuals that (1) are confronted by the same set of environmental challenges or selection pressures; (2) regularly transmit and share adaptive information among themselves; and (3) share the same patterns of behavioral response to environment." Environment in the ecological sense refers to the total set of physical and social factors within a regional milieu. Environmental stress, therefore, may result from not only perturbations in the physical environment but also consists of external pressures from competing ecological populations.

The Micmac adaptive strategy has been characterized as a generalized or fine grained exploitative pattern. It takes into account a great diversity in resource availability at variable periods throughout the year. The Micmac environment could be characterized as having several productive subsistence surges which are cross cut by a number of steady state resources. A settlement pattern strategy attuned to the interception of these surges but also geared toward conjunctive exploitation of the total resource base appears to have been the result. Some storage to supplement short periods of low productivity was part of this adaptation. Finally, I have argued that the basis for this adaptation was present in the terminal Archaic and, with minor shifts to accommodate environmental stress, persisted up to a precontact period.

In a pre-Marpole period in the Gulf of Georgia, I would also argue for a generalized adaptation. While obviously differing from that of the Micmac, the basic character would be analogous. A frequently mobile settlement pattern could take advantage of several potential subsistence resources at differing temporal intervals throughout the yearly cycle. Again a storage technology may have been present to offset periodic shortages but, without an intensive procurement of salmon, it too would appear to have been operative along the lines of the Micmac preservation strategy. In the Hope/Yale locality, however, a less diverse resource base would select for a specialized adaptation on salmon. A population movement sometime prior to 400 B.C. is argued to have brought this adaptation to the mouth of the Fraser River and subsequently underwent a series of revolutionary changes leading towards the Northwest Coast cultural pattern.

Comparing the developments of the Micmac and Coast Salish certainly brings into focus the entire question of unchecked population growth as a stimulant behind culture change. For example, if one simply views the development of complex technologies and, ultimately, complex social organization on the Northwest Coast as the result of independent population growth over time, then an explanation as to why such developments did not occur in the Maritime provinces must be forthcoming. In this view, I would argue that a Northwest Coast like specialized adaptation is viable in the Maritimes. However, rather than an intensive concentration on a single resource, it would have to be developed on the total range of anadromous, partially anadromous and catadromous species. I have argued that the Micmac maintained a hunting and gathering adaptation which, presumably, was kept at or below carrying capacity level within a given set of technological parameters. While population growth undoubtedly occurred, it is argued that densities within any localized milieu are kept in an equilibrium balance with the

environment. Although the specifics for the Micmac have yet to be researched, any number of culturally induced population control mechanisms operative on the group level could maintain this context (see Kirch 1980; Hayden 1975; Birdsell 1968). In the Gulf of Georgia region, such population control mechanisms appear to have been severely relaxed or dropped from the cultural system altogether. (The result, of course, is a high population density to resource ratio.) The reasons behind this transition I would suggest to be intricately involved in the origins of a stratified social organization and its outward visibility, wealth accumulation. In essence, the conversion of surplus production to wealth selects for greater surplus production and, thus, greater population size. Increased technological complexity to maintain greater population densities would be an outgrowth.

In conclusion, it is apparent that a great deal of problem oriented archaeological research is needed in both the Micmac and Gulf of Georgia regions to support the preceding statements. Even if this research were to prove either or both of the hypothesized developments totally invalid, I firmly believe that future comparative papers must take into account the various independent ecological factors influencing regional adaptations. Only through such an approach will it be possible to gain a full understanding of the mechanisms behind the cultural evolution of coastal peoples.

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TOWARDS A GENERAL MODEL OF THE EVOLUTION OF RANKING AMONG FORAGERS

Kenneth M. Ames

INTRODUCTION

Several recent papers and presentations (Ames 1981; Burley 1979b, 1980b; Langdon 1976; Matson this volume; Schalk 1977) have dealt directly or indirectly with the development of rank societies on the Northwest Coast. Burley and Langdon attempt to explain the development of particular archaeological or historic cultures, Marpole and Nootka respectively, while I attempt, through a general, processual model, to link the development of ranking on the coast to the development of ranked and stratified societies anywhere. Schalk's discussion of the issue is somewhat tangential to his model of salmon as a resource on the coast. Matson sees ranking evolving in conjunction with sedentism and subsistence intensification. Other studies by Fladmark (1975) and Borden (1975) treat what these authors see as necessary preconditions for these developments, but not with their actual course.

In my paper (Ames 1981), I proposed that ranking evolves because, under the appropriate conditions, it provides improved monitoring of the environment and improved responses to environmental shifts through hierarchical information flow. Flexible adaptations were described in terms of systemic resilience (*sensu* Hollings 1973:17) and rank societies as stable (*sensu, op. cit.*) systems, with less systemic flexibility in the face of environmental shifts. In a resilient system, subsystems or variables

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may assume a wide range of values and the system persists, while in a stable system survival depends upon the ability of systemic variables to maintain a limited set of values despite environmental shifts. Thus a stable system is one which maintains its internal coherence and form and is relatively inflexible.

I argued that limitations were placed upon early, resilient adaptations on the coast by the regional specialization in salmon fishing and by circumscription. Food storage was seen as a critical element in the salmon specialization. Circumscription was used as a rubric essentially for locational constraints imposed by the distribution of resources, but which caused competition between human groups, leading to warfare. The procurement and storage of salmon was seen as imposing a need for social organizational complexity and leadership. The processes bringing ranking into being were population growth, sedentism and promotion.

My intention here is to link that model to more general models and theories of the development of hierarchial societies and of cultural evolution. Implicit in the orginal model is the idea that the processes involved are universal processes which may vary in their intensity, scale and duration in particular historical contexts, but that hierarchical societies on the coast are explainable by the same body of theory employed to explain the rise of the classic Maya, states on the Peruvian coast or Bronze Age chiefdoms in southern England. This conviction also seems to underlay at least the work of Schalk and Matson. My discussion will emphasize the ideas of resilience and stability, how they provide a framework for discussing change and a basis for linking that framework to more general theory.

In this discussion I will draw upon cultural evolutionary theory and historical materialism. By cultural evolution I mean the kind of theory being outlined by, among others, Durham (1976) and Dunnell (1980). This theoretical approach is the more congenial, at least to Americanists, while archaeological applications of historical materialism seem primarily limited to Europe and Britain (e.g., Friedmann and Rowlands 1977; Tilley 1981). A detailed review of both these classes of theory is far beyond the scope of this paper, and, with regard to historical materialism, the competence of this writer. However, there are certain basic tenets of historical materialism which seem critical to any theory of cultural evolution. Among these are what Godelier (cited in Friedman 1974:417-449) terms inter-systematic and intra-systematic contradictions. However, before pursuing this point, several other issues must be discussed.

The linkage between cultural evolution on the one hand and resilience and stability on the other will be made by use of the

concept of adaptive strategy as it has recently been employed by Cordell and Plog (1979), Bettinger (1978) and Jochim (1981). The major emphasis in the paper will be on making that connection. When that has been done, the discussion will turn first to Godelier's contradictions and then, in conclusion, to what all this may tell us about the development of hierarchies among foragers in general and on North America's western and eastern coasts specifically. It is first necessary to outline what I feel to be archaeology's role in building theories of cultural evolution.

HISTORY, HETEREOGENEITY AND SCALE

Much of the recent archaeological work on foragers (e.g., Binford 1978, 1980; R. Gould 1980a) rests upon the assumption that there are general, or global processes at work shaping forager adaptations and that these apply to any group. It is sometimes further assumed that these processes are not only universal but ahistorical as well, i.e., they not only do not show local and regional variation in tempo and rate, but do not show temporal variation either. (I am not implying that either Binford or Gould are ahistorical; they are not.) Trigger (1978a) attributes this assumption to archaeologists viewing archaeology as a generalizing or nomothetic social science and misunderstanding the role of historical explanation in archaeology. He sees archaeological explanation as having four goals: 1) generalize about the nature of culture and human behavior; 2) to test existing theories; 3) to explore interconnections between existing bodies of theory; and, 4) to explain the past (Trigger 1978a:51). He sees number four as the major goal.

Goals number two and three involve applying social (and natural) science theory to explain historical events. He sees archaeology then as a particularizing, or idiographic, science, not generating theory. Social science theory, in his view, can only be generated studying living peoples. He uses paleontology as his role model for archaeology, arguing that paleontology cannot generate evolutionary theory, but does test it. However, there have been serious efforts to change paleontology into a nomothetic science (S.J. Gould 1980; Ayala and Dobzhansky 1974), with the result of forcing a serious reconsideration of evolutionary theory (Eldridge and Gould 1972; Gould and Eldridge 1977). Evolutionary theory based upon shortrun observations of modern experimental data was not accounting for patterns observed in the fossil record, according to some paleontologists. They have therefore formulated a theory based upon the fossil evidence to be tested both on experimental and paleontological data, where the issue currently rests.

Here then is the role of archaeological explanation, to formulate and test general theories of cultural evolution against the historical data that are the archaeologists' particular province. The goals are twofold and equal in importance: to generate theory and to explain the past. None of this is new. What is important here, however, is the emphasis upon explaining the past; explaining particular historical sequences or events is an important an archaeological goal as constructing nomothetic theories. Further, the nomothetic theories themselves must take history into account.

Much of current explanation in archaeology relies on the concept of adaptation (Dunnell 1980). Winterhalter's (1980) criticisms of environmental analysis are applicable to common anthropological usage of adaptation:

1. there seems to be little realization that adaptations reflect the action of past environments. Thus present, or ethnographic present, behaviors are only partially the product of present circumstances. In other words, adaptations display time lag.

2. Ethnographic descriptions are usually based upon short term observations and thus the adaptations described are treated as though they are static, lacking any dynamism. (Archaeological interpretations based upon such descriptions reconstruct a static past.)

3. Environmentally caused cultural changes can get overlooked. (This is less a problem for archaeology.)

4. Typological environmental classifications and descriptions mask critical environmental variation.

5. Much of cultural ecological theory is ahistorical, as a result, in part, of points 1-3.

6. The idea of adaptations is misunderstood by anthropologists in any case.

According to Winterhalter, the emphasis should be upon environmental analyses which stress the dynamic nature of environments, i.e., with temporal and spatial variability. In his view, and that taken here, "Ecological adaptations result from historical processes in natural ecosystems which have as their most important characteristics temporal variance and spatial heterogeneity" (Winterhalter 1980:136). Our understanding of these processes must be based upon the physical limitations of events, of rates, magnitudes and distances of real environments. What is required then is detailed

and careful application of general theory to particular historical sequences. There does have to be, on the other hand, a general theory to apply, and archaeology is as capable of generating it as is paleontology.

ADAPTATION AND STRATEGIES

I have suggested elsewhere (Ames 1979, 1981) that cultural adaptations on the northwest coast, or anywhere for that matter, can be analyzed using two polar types defined by Holling (1973): resilient and stable systems. These are heuristic devices, and do not represent any real system, but systems may be described in these terms. As heuristic devices, these types offer certain advantages (Ames 1979:223, 1981:792), one of which is that adaptive success is measured on some basis other than survival or population growth. Rather, adaptive success is measured using the persistence of relationships among systemic variables, including population size. Thus change, evolutionary change, occurs when relationships among variables change, break down, restructure, etc.

There are two questions to be answered here; first, what are the systemic variables being discussed, and second, what are the ramifications of the definition of evolution implicit in the statement that evolutionary change occurs when relationships among variables change?

The first question may seem to be the old question of what cultural elements are important for understanding cultural evolution, the answers to which usually approximate Steward's "culture core" (Steward 1955) or White's layer cake model of culture (White 1959), that is, technological, subsistence and economic variables are the most important. This view is not adequate. From an evolutionary standpoint it is now evident that many traits are adaptively neutral, possibly even certain subsistence activities, though that is less likely than that traits associated with ritual may be neutral. However, traits associated with ritual can be adaptive; these ritual traits may be part of what historical materialists call the relations of production, that is, "those social relationships which dominate (i.e., determine the economic rationality of) the material process of production in given technological conditions at a given stage of development of the forces production" (Friedman 1974:946). That is to say, that social and ideological variables are as important in determining the form of a particular adaptation as are subsistence variables. While certainly not a new insight, this important point frequently gets forgotten. In any case, variables which are adaptively neutral or

adaptively positive will have to be discovered with each instance, though theory can predict what general classes of variables to examine. I am going to discuss these variables in terms of strategies or solutions to adaptive problems.

> It is self evident that any living species, at least any species not about to become extinct, has a valid set of solutions for certain basic ecological, or biological problems. It must secure food to replenish its energy store, must have a place to live, and a method to reproduce and thus perpetuate its genes in succeeding generations.
>
> (Dobzhansky 1974:323, cited in Jochim 1981:14).

Particular solutions are strategies in the terminology used here. As Jochim (1981) notes, this definition with its insistence upon genetic perpetuation raises problems for the analysis of cultural evolution. These problems will be discussed below. The basis or level of these solutions is not specified, thus it may be individuals, populations, societies, etc. (c.f. McCay 1981:357). It follows from this, as has already been mentioned, that there may be cultural elements which are not solutions to environmental problems, thus they are not strategies. They may, however, limit the choice of available strategies (e.g., McGovern 1980). At this point it is necessary to turn to the second question, the implications of the statement that evolution is changes in the relationships pertaining between variables.

This view of cultural evolution is consonant with what Dunnell (1980) has termed the transformational view of cultural evolution, i.e., that systems are transformed and become increasingly complex. They are not replaced. This view is perhaps most clearly expressed by Buckley's concept of morphogenesis (Buckley 1967). Dunnell criticizes this view in his excellent paper on cultural evolution. A statement of his criticisms is essential. In Dunnell's view:

> Evolution is a particular framework for explaining change as differential persistence of variability ... evolution views change as a selective and not as a transformational process. Variability is ... discrete. Change is accomplished by alteration in the frequency of variants rather than alterations in the form of a ... variant.
>
> (Dunnell 1980:38)

From an anthropological viewpoint, this is a radical view of change, which stems from Dunnell's close adherence to the idea, dominant in biology, that natural selection acts upon individuals (phenotypes) and that evolution can be withnessed in changes in gene frequencies

across generations. Dunnell is not alone among anthropologists in this approach (e.g., Durham 1976; Jochim 1981). Dunnell does not, however, offer a suggestion as to what the discrete unit of cultural variability is, nor how it arises. This is not a criticism since that problem is a extraordinarily complex one.

It is now clear, for instance, that genes themselves are not the unitary, discrete units of inheritance envisioned by Mendel. Genes may be pleiotropic, affecting many traits, or polytropic, many genes producing a "single" phenotypic trait such as skin color. Thus selection acts not only upon genes, but on the relationships pertaining between genes. As genes alter in frequency, their relationships change as does the resultant organism, sometimes in unforeseen ways. Thus it would be simplistic in this case to argue, for example, that cultural evolution proceeds as the adaptive strategies employed by a cultural tradition change frequencies, some becoming dominant, others rare or disappearing. If selection acts upon human behavior in the manner envisioned by Dunnell, it acts upon sets of "solutions to adaptive problems," upon constellations of strategies.

What I am going to suggest is that cultural evolution proceeds in a manner reminiscent of species selection (Stanley 1979). Before introducing this concept, the following *caveats* are in order. First, this is a controversial theory in biology, and is propably not held by the majority of biologists. Secondly, there arises the inevitable problems when theories are transferred from one discipline to another; that is, they do not always translate well, and thirdly, I am not sure the species concept is in any way applicable to con-specific human groups. I am not advocating species selection, but believe it can provide useful insights into cultural change. In Stanley's view, (1979:191) species selection is natural selection among reproductive isolates, species, and therefore among different adaptations. It is this aspect that I want to emphasize. The source of ultimate variation remains the individual, but the level of major evolutionary change is the species. (Stanley carefully distinguishes species selection from Wynne-Edwards' (1962) now moribund concept of group selection.) Species selection is selection among sets of adaptive strategies. The concept also recognizes that many traits may be neutral, and these become irrelevant to natural selection, and that many of the processes of speciation are random and unpredictable. I am suggesting here that variations, new strategies, arise from individual human actors, but ther success depends upon natural selection among constellations of strategies. Species evolution proceeds by the processes of speciation and extinction. We may say that cultural evolution proceeds by innovation and failure.

I cannot pursue the issue here but the general notion of species selection may obviate the apparent problem of individual selection versus group selection ("the altruism problem") (c.f. Durham 1976) with regards to human groups. Dunnell (1980:66) offers a similar suggestion, though he restricts it to complex societies. He suggests selection shifts from individuals to groups as human groups become large. I suggest it always works on groups (though not exclusively).

To summarize at this point, I have argued (1) that the internal analytical variables are adaptive strategies, (2) that these adaptive strageties are not limited to subsistence or economics, but include the relationships of production and (3) that change is the result of natural selection among discrete constellations of these solutions. To say that change is "the result of natural selection" does not explain anything. The next section discusses that issue.

DYNAMICS OF CHANGE

According to Alland, there are three levels of selection:

> The first selection will occur by virtue of the system itself. Emitted traits are limited by the characteristics of the system, by its genetic and cultural elements. The second selection will occur as emitted traits are accepted or rejected on the basis of systemic properties and will depend on how well a trait fits a particular structure. The third selection will occur as a result of interaction between the system and the environment.
>
> (Alland 1975:69)

Variations do not arise from whole cloth, they arise from the particular potential of the system. Oysters do not suddenly begin to talk. Secondly, variations must fit what is already there, the variables and relationships already existing, and then thirdly, natural selection as it is usually understood operates.

However, it is implicit in this view that adaptive systems are well integrated wholes. They are not: they are compromises, and a trait will be positively selected if its positive benefits even marginally outweigh its negative effects. Different strategies may accomplish their ends, but have undesirable or conflicting side effects.

Alland cites Morris Goldman's notion that organic systems are both internally and externally adapted. "Internal adaptation represents the coherence ... of the system ... External adaptation

... the goodness of fit between the system and its environment" (1975:69).

Thus adaptation involves accommodation to intra-systemic and inter-system constraints. It is useful here to introduce Godelier's concepts of inter-systemic and intra-systemic contradictions, as discussed by Friedman.

> ... (intra-systemic) contraditions are within a structure ... between systematically self contradictory aspects of a social relation. Inter-systemic contradictions are those that exist *between* structures (emphasis Friedman's) ... This relationship is one of mutual constraint ... it is analogous to mutually limiting functions in systems or equations ... The *functions are autonomous, but the range of values which they can take is limited by the other functions*. (emphasis mine)
>
> (Friedman 1974:447).

In the terminology used here, intra-systemic contradictions are those within an adaptive strategy, or closely tied set of strategies. Thus the accumulation of radioactive waste is an internal contradiction within the strategy of atomic energy. Inter-systemic contradictions are between strategies which are functionally independent, but which constrain each other. These are both internal contradictions, within a single adaptive system. They might be better termed intra-subsystemic and inter-subsystemic, but those terms are overly cumbersome. I do not wish to introduce or belabor jargon, but to emphasize the importance of Goldman's internal adaptations, and that the internal order of a system is a function both of internal order and the external order. That is to say, the internal order depends upon the internal coherence of variables, the relationships among variables and the selective pressures on variables. Changes proceed, in my view, from either internal inconsistencies, or changes in external selective pressure. As Winterhalter (1980) points out, adaptations display time lag. Different strategies within a single system will, since they are each solutions to different environmental problems, display differing degrees of lag. Thus inter-systemic contradictions are inevitable, and in extreme cases these internal contradictions require their own solutions. The relationship between the system and its environment, and the system's internal relationships are seen here as semi-autonomous sources of variation and change. Cultural selection (the cultural equivalent of species selection) should operate then on both internal and external adaptation. At this point, the discussion will return to a consideration of stable and resilient systems and then conclude with a consideration of social hierarchies among foragers.

STABLE AND RESILIENT SYSTEMS

In resilient systems, a variety of strategies may be employed to meet external adaptive problems without generating major internal contradictions. The strategies themselves may contain contradictions (intra-systemic contradictions), but there should be few contradictions between strategies (inter-systemic contradiction). When problems arise, solutions should be simple and straightforward. Internal disputes may be resolved by someone moving to another group, for example. Because of the lack of internal contradictions, these systems are capable of persisting for long periods. However, they are not in internal equilibria. Resilient systems persist through their internal fluidity and their capacity to assimilate environmental extremes. Thus these systems accommodate both external and internal hetereogeneity.

Stable systems cannot accommodate external hetereogeneity. They are complex systems which adjust to external variation by maintaining an internal equilibrium. If equilibrium is not maintained, then contradictions inherent in the system will be exacerbated, and this should result in the development of greater internal complexity to solve those problems. These systems, unless the environment is stable, may not persist for long periods because of their difficulty in maintaining internal coherence.

It follows from this that selection in unstable conditions should favor increasing resilience at the cost of less internal complexity, or increasing stability (with greater internal complexity), at the cost of persistence. In the earlier paper I specified five kinds of change (Ames 1981): 1) a resilient system adapting; 2) a stable system adapting; 3) a resilient system becoming, or being replaced by, a stable system; 4) *vice versa*; and, 5) systemic extinction. The key question becomes, under what conditions will selection favor increasing simplicity and under what conditions increasing complexity. According to Stanley's model of species selection, new variants (species) will arise more or less at random, and unpredictably, i.e., one cannot know where or when speciation will occur. Speciation events are, in his view, experiments, some of which fail, some which succeed. These experiments must be within permissible bounds (Alland's first level of selection). Because of this conservatism, and long term selective pressure (directional selection, [Grant 1963:237]) trends may result, but these are not inherent, or progressive trends. In the same way, we may expect cultural experiments with either increasing complexity or simplicity, and trends resulting from cultural conservatism and directional selection. Whether these trends are transformational or replacive is not critical to my argument.

If selection favors either complexity or simplicity, can we specify events in which both have happened? Can we predict under what conditions complexity will be favored and when simplicity will be favored? The answer to both questions is yes. The prehistory of portions of central California is an example of a historical sequence where shifts in conditions have favored first complexity, then simplicity and then again complexity (Moratto, King and Woolfender 1978). The changes of house patterns reported by Fitzhugh (1980) may reflect alterating selection for internal simplicity and complexity. The second question is discussed in the next section.

SOCIAL HIERARCHIES AMONG FORAGERS

I would argue that social hierarchies will arise wherever and whenever internal or external circumstances increase internal contradictions or internal flexibility is not possible, i.e., whenever circumstances constrain the capacity of the system to adapt internally. At this juncture a system can no longer adjust to external conditions by altering internal values and therefore strategies which maintain internal values in the face of external changes will be positively selected. Ranking is such a strategy.

Demographic change, whether growth or changes in dispersion, new technology, sedentism where it has not previously been practical, subsistence intensification or specialization are all factors which can produce contradictions. Increasing conflict, either internal (e.g., Tilly 1981) or external (e.g., Carniero 1981) are symptomatic of the contradictions and exacerbate them. None of these are new suggestions. However, their effects and the contradictions they can cause may not be generally anticipated. McGovern's (1980) fascinating study of Greenland Norse is an excellent example of inter-systemic contradictions limiting a society's capacity to respond, and ultimately causing the society's physical extinction. Jochim's (1979b) simulation of mesolithic German subsistence patterns shows that technical innovations can require additional strategies to deal with problems caused by the initial innovation. We, of course, are surrounded by examples of this in our daily lives; the automobile is one excellent case. Conversely some of these may not always be sufficient to cause change. Thus sedentism may produce no significant social changes (e.g., Fagan 1978). Matson (this volume) may be correct in arguing that a combination of sedentism and resource intensification will produce complex societies. However, on the Columbia Plateau, there appears to be both partial sedentism and resource intensification, with, at present, no important evidence of social complexity similar to that of Northwest Coast societies. There are probably critical

thresholds below which contradictions are not serious, above which change occurs.

I do not think that the west coast or east coast (Miller this volume) or coastal societies in general are special cases requiring special theories (e.g., Yesner 1980a). Rather, I think that Kehoe (1981) will be borne out and we will discover that aboriginal social organization was everywhere more complex than we now realize. Ranked societies were unstable and areas will have been like the Sacramento Valley, with temporal shifts between ranking and egalitarian systems.

OVERVIEW

David Sanger

Anthropologists, generally lacking the means to test their theories by controlled laboratory experiments, have traditionally employed contrasting data sets. At a time when multiple society cross-cultural evaluations are possible, it is interesting to reflect on the dualistic nature of this symposium -- the Northwest Coast vs the Northeast Coast. Why were the former peoples organized into complex, strongly linear, stratified tribal segments, whilst the latter evinced relatively egalitarian, less complex, structures largely devoid of ascribed statuses and ranking? The Northeast Algonkians generally behave as hunters and gatherers should, according to Service (1979) and others interested in placing societies on an evolutionary ladder. That the Northwest Coast hunting and gathering societies did not conform has long been a source of theoretical embarassment. What, then, are the merits of comparing apples with oranges?

A critical feature of the comparative approach is the need for comparable phenomena and comparable base-line data. If we can agree that Northwest Coast and Northeast Coast societies had a major maritime component in their lifestyle, then we have a comparable phenomenon aspect -- the adaptation of hunter/gatherer societies to marine ecosystems. Both ecosystems are rich and varied in humanly consumable resources. The base-line data that are being compared are problematic.

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In neither area are the archaeological sequences well known. When Northwest Coast data are used they are derived largely from the Fraser Delta, although Hobler's data set (this volume) is not. In this group of papers the prehistoric data from the Northeast are derived largely from the Maine coast with some Maritimes Provinces information. Although the Fraser Delta sequence may be better developed in several important ways, there are still a great many unanswered questions pivotal to issues of developing social complexity.

Ethnographically, the contrast in base-line data is strong. Nowhere in the Northeast coastal zone do we have the wealth of documentation seen for the major groups of the Northwest Coast. Miller's novel approach (this volume) is unconvincing given the 17th century dislocations, the vagaries of the documentation, and her methodology. Unfortunately, the diverse sources used to characterize the Micmac cannot stand the burden placed on them by the developing body of mid-range theory. In short, the results of the comparative exercise are generally disappointing due to the incomparability of the data sets.

This conclusion does not imply that nothing has been gained -- quite the contrary. In a body of literature dominated by such marginal hunter/gatherers such as the Bushman, Shoshone, Inuit, etc., it is important to grapple with those societies that place higher on the evolutionary scale of social complexity. By taking the diachronic view, the otherwise anomolous Northwest Coast groups may assume an important role in theory building as we try to understand the mechanisms whereby the societies attained their ethnographic characters. The Northwest Coast ethnography, so rich in detail, affords an opportunity to generate hypotheses that can be tested with archaeological methods. For example, salmon on the Northwest Coast have always dominated the thinking of anthropologists. Did it also dominate the native peoples in pre-contact times? The answer must come from a detailed analysis of bones in the middens. Is there in fact an intensification on this single resource? Some research suggests this is not in fact the case (C. Carlson 1982).

Whitlam's (this volume) MNE model also offers the basis for an empirical examination of the evolution of societies on both coasts. Like so many good ideas it cannot be tested with our current archaeological data base. However, because of its emphasis on seasonality of scheduling the excellent preservation in east and in west coast shell middens could provide the requisite data. Demographic factors may be far more difficult to handle, unfortunately. An adequate test of this model, like the "intensification" model, requires a research design that specifically eschews the normative approach found in much

current archaeological thinking (Sanger 1982). As we move away from an emphasis on cultural-historical models we will be compelled to evolve different concepts of how culture works, and data generated under one model will have to be massaged to fit the new.

Several anthropologists have noted that the impact of European contact has been to establish marginally placed huntng and gathering societies and also to create societies that may evolve towards the lower end of the bandstate continuum. The Northeast hunter/gatherers may be such an example, but what about the Northwest Coast groups? Is there a possibility that European contact actually fostered a greater emphasis on ranking through status affirmation and hereditary rights to resources?

There are a number of archaeologists who hope to evolve a body of method and theory specifically geared to maritime adapted hunting/gathering societies. On the one side, there are those who would argue that no special body of theory is needed to explain maritime hunter/gatherers. On the other, the focus on maritime ecosystems is sufficient to warrant special examination (Osborne 1977; Perlman 1980; Yesner 1980). From these broadly-ranging reviews we can see that there may be certain features of maritime adaptations that are different from purely terrestrial adaptations.

It can be shown that the kind of maritime focus seen on the Northwest Coast and in the Northeast does not occur in all maritime zones in the world. Both of these areas share with a handful of others rich inshore ecosystems where high plankton growth sets the platform for high biomass levels. Although efficiency may be lost through the number of trophic levels, the gain comes through the trade of quantity for inefficiency. The convergence of plankton consuming animals that can convert the biomass to protein and fat provides man with a super abundance of food if he can capture these "converters" and, if necessary, store them for leaner times. The ability of the Northwest Coast peoples to store food is, as several contributors to this symposium have pointed out, the capstone to a procurement system that maximizes on high quality and high quantity resources that are relatively predictable. It must be remembered, however, that the same storage technology was applied to animals other than salmon, and it is clear from shell midden faunal analyses that extensive (diffuse) subsistence patterns existed on both coasts.

A challenge for archaeologists working with maritime hunter/gatherers is the reconstruction of the marine conditions. Here the terrestrial hunter/gatherer specialists have an edge both in the available techniques and in the comparative analogues. It is encouraging to see in this volume how the interface between

archaeology and oceanography is closing that gap. As ever, it is a matter of learning enough about the marine ecosystem to ask the right questions. Once some useful characterizations of paleo-marine conditions are ascertained we can then proceed from the particularistic through an examination of various models purporting to explain human behavior.

The varied resources available to maritime hunter/gatherers create options that purely terrestrial hunter/gatherers may not enjoy. Most of the former benefit from access to the sea at the front door and the land at the back. Whilst orienting themselves to a marine schedule, the contributions from the terrestrial ecosystems are not insignificant. Under such conditions the plethora of choices may lessen the utility of some of the ecologically founded models that make predictions based upon ideas of least risk and effort. Site catchment and related models based on walking time and effort of carrying prey have to be drastically altered to accommodate water transport. In short, although maritime hunter/gatherers are still hunter/gatherers, there may well be benefits to generating behavioral models that more accurately reflect their physical environment. Such models, of course, should ideally lead to a better understanding of their social environment.

Finally, it is the nature of our discipline to proceed from the particularistic to the general, and from the model to the hypothesis in a fashion that often appears ideosyncratic. Symposia like this one serve a useful function in that scholars who rarely communicate with each other have done so, and regional specialists have been encouraged to look beyond their estuary.

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