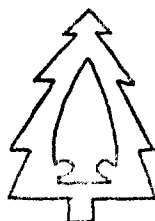


MAINE ARCHAEOLOGICAL SOCIETY



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LUNCH

Bring your lunch and visit with us
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Executive Board Meeting

1:30 p.m.

Business Meeting - Election of Officers

Reports

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Speakers

Mr. & Mrs. Millard Camp will present a program on
Pemaquid and archaeological activity at this
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DUES ARE DUE

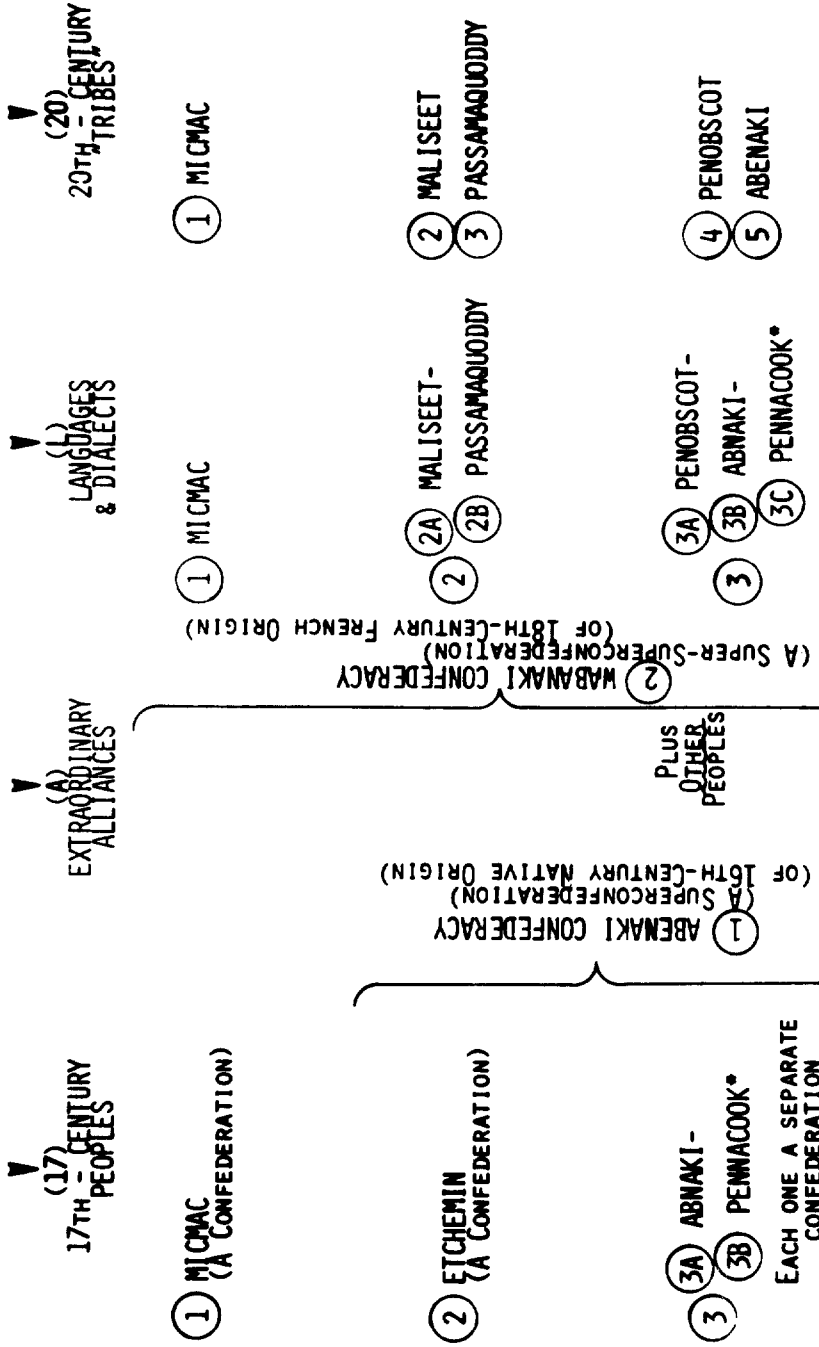
Our year ended with September 30, so it is time to once
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THE WABAWAKI OF THE NORTH-ATLANTIC SLOPE CULTURE AREA

(HUNTING-FISHING-GATHERING BASIC; MAIZE HORTICULTURE SUBSIDIARY WHERE PRESENT)



*NOT ALL OF THE PEOPLES OF THE PENNACOOK CONFEDERACY ARE MEANT TO BE INCLUDED IN EITHER ALLIANCES 1 & 2 OR IN LANGUAGE 3

OBSERVATIONS CONCERNING AN ETHNOHISTORICAL
TAXONOMY OF THE WABANAKI ALGONKIAN AMERINDS

2289

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Before starting my explanation of the taxonomic chart on the preceding page, I first want to discuss some major academic issues involved in the study of the Wabanaki Algonkian Amerinds, or of any other peoples--issues which inevitably color our attempts at interpretation of their cultures and their histories. The methodologies of both anthropology and history must be utilized in our interpretation attempts, and this combined endeavor is called ethnohistory; it is a sort of alloy discipline of greater strength and with fewer shortcomings than either of its parental disciplines, yet it, too, is far from being trouble-free. Much of our data was poorly gathered long ago, or was carelessly garbled since, and we are now totally unable to make up for these past mistakes; past research concerns differed from those of the present, and frequently we cannot find answers to our new questions in the best of the old data, or find meaningful relevance in the old concerns; not even the most sophisticated methodology today ever can overcome these deficiencies and differences of the past. However, modern ethnohistorians are more aware of the dangers inherent in the idiosyncracies of each of the parent subjects than are many practitioners of either "straight" anthropology or "straight" history in their standard, classic forms. Ethnohistorians tend to be innovators, not deference-paying tradition-followers and ancestor-worshippers; they tend to be quite eclectic in their selection of what they consider to be the most useful theories and methods of both parental disciplines, and tend to be quite devastatingly

critical of the work of many of the "saints" and local giants of both parent subjects. Frequently they play the role of the little boy who, alone, dares to remark aloud that the emperor is not wearing the finest of new clothes at all, but is, instead, stark naked; and such a role is often just as necessary as it is unpopular. Ethnohistorians realize that the links between anthropology and history are as essential as they are inevitable, and they try both to make the most of the alliance and to correct for the isolationist trends of some members of each discipline. Some ethnohistorians today have anthropology training backgrounds, others have history training backgrounds, and a few are trained in backgrounds of both subjects, serially.

The great contemporary British social anthropologist E. E. Evans-Pritchard, in his 1961 essay ANTHROPOLOGY AND HISTORY, accepts the dictum of an earlier essayist that "anthropology must choose between being history and being nothing", meaning that time is a basic and essential dimension in which all human experience occurs. Yet many British social anthropologists, until recently at least, have been openly ahistorical if not anti-historical, and have presented their structural-functional analytical descriptions of exotic cultures in only one time plane, the "now" (i.e., they present synchronic ethnographies, not diachronic ones). And many American anthropologists have long perpetuated a most unreal, if delightfully simplifying, mythical concept called the "ethnographic present"--meaning a description of an exotic culture as it existed prior to devastating Westernizing influences' spoiling its independent integrity, the assumed date of reference ranging in the case of Amerinds (short for American Indians) from seventeenth to nineteenth century,

depending upon the "tribe" under consideration--as if to imply that "that's when the Micmac were really the Micmac." (One could as well ask, "When were, or are, the American people really American?", and if we could agree on a date, then go about describing American culture at that time as a once-and-for-all ethnography of ourselves!) Clearly, one who ignores time wastes time--and opportunity.

Evans-Pritchard (1961) qualifies his remarks, however: "I accept the dictum, though only if it can also be reserved--history must choose between being ... anthropology or being nothing," meaning that very sophisticated structural-functional analyses of social and cultural affairs must be built into any description of temporal events. I could not agree more with Evans-Pritchard, both about that and about a closely related matter. A particular frustration which I have encountered repeatedly in the works of many historians, both old and new, is a blatant lack of concern--almost as if it was beneath their dignity--with attempting the accurate, specific, identification of non-white-race ethnic groups. When a modern historian states only that "the Indians" attacked and besieged Wells in 1692, and then goes on to state immediately thereafter the most precise gory details of their torture of captive James Diamond, I feel that he is letting the tail wag the dog. What Indians were these, and from where? Obviously angry ones. But angry at whom: Diamond? Wells? Englishmen in general? And why so angry? Perhaps for very good reason. And if we were told what Indians as precisely as we were told about their act of torture, we might even be able to push on to learn: first, about a very interesting specific structuring of prior social relations between these particular Indians and certain neighboring Englishmen; second, about an attitude of general structuring of reciprocity in social relations between the two

groups (if only at the very most general level of any and all enemies of the French being considered enemies of Indians allied to the French); and third, about some of the functions fulfilled by torturing. But we are denied all this potential knowledge by the monitoring of our modern chronicler, who, indeed, seems to doubt by implication that the Indians could act as rational human beings, albeit in terms of their own cultural norms. Indians--any Indians--attacked and tortured only with a motive, and an ethnohistorian might just be able to learn much, much, more about the larger meanings of what went on at Wells in 1692, by carefully re-analyzing the old documents thereon. If the facts are there, they should be used to present a deeper, more meaningful, and less narrow-minded history-in-the-round. If these facts are not there, in the documents, their non-availability should be remarked upon and sincerely lamented. Clearly, one who ignores the "savage" side of American colonial affairs tells only a flat, biased, "white man's history"--either by intent or by default.

Having illustrated Evans-Pritchard's (1961) general indictments--of anthropology for too often excluding temporal analysis, and of history for too often excluding sociocultural structural-functional analysis--with flagrant (yet frequent) examples of my own choosing, I now turn to some problems common to both parent disciplines and to ethnohistory as well. Solutions to these problems may be unavailable at this stage of human knowledge, but mere awareness of the existence of such crevasses is a vital first step toward avoiding their potential dangers. In general, these can be called problems of relativity, and they can cause many misinterpretations because they so easily usher in unintended biases.

By virtue of their being a part of the here and the now, modern cultural anthropologists (also called ethnologists) and historians are doomed never to be able to see things exactly as they "really" are in the there (the ethnologists' plight) or as they "really" were in the then (the historians' plight); and, since ethnohistorians deal with both the there and the then, they are doubly damned. Most modern scholars are well aware of these relativity problems, yet some scholars still seem to be less explicitly concerned with them than I feel that they should be, considering that much of the general public has not become as aware of the easy errors caused by these problems as the scholars have. The ethnological concept of cultural relativity is both the explanation of our perceptual limitations and the guideline for expanding our perspective--i.e., it is both our master and our servant. We must realize that too often we can view only from our own contexts, willy-nilly, the exotic present or past things and events that always we can understand best only in their own contexts. Awareness and frank, public, admission of this type of relativity problem is not only essential but easy, and the least we can expect from scholars while they are trying to devise better ways of solving these problems.

In ethnology, ever-more-sophisticated attempts (too numerous to list here) at gaining the "inside view" of present-day exotic cultures are being developed, but some of these attempts have been charged--perhaps quite rightfully--with being exercises in self-delusion and hocus-pocus. Since many of these attempts look to synchronic descriptive linguistics as their model, and are intended only for present-time studies, it probably will be some time before their meaningful successful modification for diachronic studies can be achieved. However, Charles Hudson, an anthropologically-trained

American ethnohistorian, has started forward in this direction by distinguishing the "EMICS" from the "ETICS" in culture-history. EMICS and ETICS are terms (and concepts) borrowed directly from linguistic analysis; as Hudson (1966) uses them, the EMICS are the FOLK HISTORY (the insider's, subjective, frequently self-praising version of a society's past); the ETICS are the ETHNOHISTORY (the outsider's, more-nearly-objective, analytically-oriented version of a society's past); both, together, must be taken into consideration for a well-rounded perspective, because human beings do act in terms of inside assumptions and perceptions, however "irrational" these may seem to outsiders.

Coupled with the type of relativity problem just discussed is another--that of terminology. Terms, too, are relative to place and time. Our analytic terms should be useful to us, to impart our messages in the here and the now--certainly not at the expense of doing known injustice to the there and the then, or to prior analytic usage, but whenever new terms are in order. An example of a good "new" (i.e., non-aboriginal) term is ALGONKIAN (sometimes spelled ALGONQUIAN), meaning the largest language family of native North America--comparable to the INDO-EUROPEAN language family of Eurasia. The term was coined from the name of one of the individual member languages therein: ALGONKIN (or ALGONQUIN). When used as intended--as the name of a language family--it is a most meaningful term, but some persons persist in referring erroneously to "the Algonkian nation", as if the term meant a political state, which it most decidedly does not. This error is directly comparable to

saying the "Indo-European nation", and indirectly comparable to calling a cat "a dog"; certainly it is far from useful. As much care should be taken in the use of meaningful terms as in the coining of them. The extension of the linguistic term ALGONKIAN into cultural affairs can be done cautiously, however, and we frequently hear of "Algonkian peoples" and "Algonkian cultures"--because, since language is the chief vehicle of culture, linguistically-related peoples and cultures frequently share many similarities and much history. An excellent review of scholarly knowledge about Algonkian peoples and cultures is presented by the greatest contemporary American, historically-concerned, social anthropologist, Fred Eggan (1967), in an article of the utmost importance to anyone studying any aspect of Algonkian affairs. The ALGONQUIAN LANGUAGE FAMILY is treated succinctly by American linguistic anthropologists Carl and Florence Voegelin (1964) in their orientational article.

Perhaps the most important ramification of relativity problems in general is the following place-time description difficulty. In any taxonomy or typology or set of terms intended to describe the past in general, some violation of the truth (meaning, of course, whatever we choose to define as "the truth"--and that tends to vary with increasing knowledge) is inevitable. There is no such thing as "the past in general"; rather there are many temporally-specific pasts. Indeed, there are as many PASTS as there were PRESENTS! Any given territory, like any given floor in a hotel, has had very different occupants at different times, and for very different reasons. Also, there are many spatially-specific aspects of the past. Like an amoeba, any viable society contracts, expands, and otherwise changes shape in its land-use adaptations over time. So, the single-shot map of NATIVE TRIBES OF NORTH AMERICA, prepared by

Alfred Louis Kroeber (1939b)--the dean of American anthropologists at the time of his death in 1960--although it is a must for every North Americanist, and is the product of serious research by a titan of anthropology, is also a travesty of "the truth" regarding time and space, because very many of the "tribes" shown on this continent-wide map never coexisted, or, if they were contemporaries, they would not have recognized the spatial boundaries that Kroeber assigns them and their neighbors. Yet even though Kroeber knew all this, he made the map anyway; and even though I know these negative points about it, I still applaud the map, and Kroeber for making it, because it is useful to have this map to help to begin to order our data.

Native affairs alone brought about inevitable changes of who's who and where which frequently perplex the ethnohistorian, because of no "documentation" other than artifacts and oral traditions. Add to this the chaos brought by European contacts before the earliest rational accounts were written by explorers, traders, missionaries, soldiers, and colonists--frequently from competing European nations, using different languages, and very often in severe disagreement with each other not only over details but in basic statements as well--and our view of "the past" as it "really" was becomes even more illusory. The European impact was so overwhelming to the Amerinds, and their adaptations so far-reaching, that even our attempts at group identification along the moving frontier are highly problematical, as anthropologically-trained Canadian ethnohistorian Ted Brassler (1968:261) has pointed out:

Whatever unit [meaning both proper-noun
group names and common-noun political

entity types] we try to define, all of them prove to have been phases in a continual process of restructuring the native societies, in response to situations requiring cooperative efforts to reduce internal strain and external stress. xxx Directly connected with this process of formation and reformation we observe a progressive loss of cultural distinction between tribal groups.

Dynamic processes make static terms look rather pitiful!

Now, having completed my remarks on some of the problems confronting ethnohistorians in general, as part of my discussion of some major academic issues coloring our attempts at interpretation, I can start to focus more specifically on the Wabanaki Algonkian Amerinds. The geographical area with which I am dealing in the taxonomic chart on page 2 could be called either the Sub-St. Lawrence Peninsula, or the Maritime Peninsula, or even the Wabanaki Peninsula. Kroeber (1939a) calls it the "North Atlantic Slope Culture Area." It consists of Sub-St. Lawrence Canada (three and a fraction provinces) and Northern New England plus some of Massachusetts (three and a fraction states). The time period under consideration is from before 1600 to the present. Anthropologically-trained American ethnohistorian Bernard Hoffman (1955) has mapped** the "Tribal Distribution" on our peninsula for "Before 1600" and for "In 1700"; and his article describes: the evolution of the old groupings into the new, for those who stayed put; the moves, space-shifts, and clear-outs of others; and the complete disappearance, from both shores of the St. Lawrence River, of the Kwedech, or Laurentian Iroquoians, whose

departure (sometime between c.1540 and c.1600--Cartier found them there, Champlain did not!) left this Sub-St. Lawrence Peninsula wholly to Amerind peoples who spoke languages of the Algonkian language family.

These peoples, on this peninsula, I call three synonymous names: 1) NORTH-ATLANTIC-SLOPE ALGONKIANS; 2) EAST-NORTH-EAST ALGONKIANS; and 3) WABANAKI ALGONKIANS. The first term is Kroeber's, and it implies that the Amerind peoples of this area shared both a culture area (an area of similar cultures) and a language family (a family of related languages). The second term is a necessary construct of my own making, because I have found repeatedly that if one says just "Northeastern", one's hearers too easily think of the Montagnais or Naskapi of the Labrador Peninsula. The third term can be shortened to just WABANAKI, implying Wabanaki peoples. Wabanaki is translated as "Dawnlanders", and means to me, as it has to anthropologically-trained American ethnohistorians Ernest Dodge (1957) and Dean Snow (1968), to name but a couple of scholars, five living "tribes"--MICMAC, MALISEET, PASSAMAQUODDY, PENOBSCOT, ABENAKI--and one now-transformed group--PENNACOOK--for a total of six separate peoples, the autonomous heydays of which certainly never coincided in the Kroeber map territories assigned to them, if at all!

This term Wabanaki, although it is a favorite term of mine, is disliked by some linguistic experts, who would prefer it to be rendered as Abenaki. In my usage, however, Abenaki means a smaller entity than Wabanaki, and Abnaki means an even smaller entity than Abenaki (see chart on page 2). If I abandoned my three terms for the linguistic experts' one term, I would have

to add sub-script size-numbers to distinguish which definition I meant, in order to attain the same level of meaningful usefulness already inherent in my use of three variant spellings and pronunciations. Perhaps the sub-script size-numbers idea is less linguistically barbarous--although far from aboriginal, at best. Linguistic barbarities of some sort seem to me to be inevitable, and indeed quite necessary for the sake of clarity. Words, after all, are only tools, not gods. Clarity, on the other hand, should merit our perpetual reverence, lest we subvert our own scholastic efforts. All three variants do exist, and all three "sizes" need to be indicated, so I merely couple them up for efficiency's sake.

The taxonomic chart on page 2 is, of course, an oversimplification of complex things and events. To diagram anything is to oversimplify it, but three centuries of Wabanaki history is particularly prone to this violation. Earliest-contact accounts of Wabanaki politics depict a very few "overlords", each with his cluster of "homage-paying" sagamores of local riverine and coastal territories. Alliances united, and feuds separated, ever-changing greater or lesser power blocs. War, murders, pestilence, and famine followed increasing European contact; then European trade, missionization, immigration, and international rivalries further altered native sociopolitical structures and territorial usages. Eventually Americans and Canadians continued the pressures started by Europeans, and realigned, isolated, reservation "bands" resulted. A minimal "explanation" of the chart now follows. I cite few references because most of my statements are synthesized from a variety of sources.

The MICMAC appear on the chart to be a study in still-life, which is not true at all, except relative to the other peoples

charted, term-wise. The 17th-Century MICMAC (17.1) evolved into the 20th-Century MICMAC (20.1), and they speak MICMAC (L.1)--one of many separate languages of the Algonkian language family. They participated in the WABANAKI Confederacy (A.2), an interesting if incomplete account of which can be found in an article by cultural anthropologist Frank Speck (1915)--but it is a very incomplete account, even in the listing of membership, and well illustrates field-ethnographer Speck's annoying frequent lack of interest in ethnohistorical matters. MICMAC reserves today are scattered throughout their aboriginal territory, farthest north and east of all Wabanaki peoples--Gaspé Quebec, eastern New Brunswick, Prince Edward Island, all of Nova Scotia (including Cape Breton Island). Some MICMAC even occupied southern Newfoundland. Aboriginally they may never have done maize horticulture, and, therefore, their settlement patterns were geared to the seasonal movement required by their hunting-fishing-gathering economy; they are at the opposite end of the Wabanaki spectrum, ecologically and culturally, from the much more sedentary, horticultural, PENNACOOK. Among the very first of all Amerinds to be contacted by Europeans, the MICMAC early became allies of the French who colonized Acadia.

The ETCHEMIN (17.2) either developed into, or at least were replaced by, today's MALISEET (20.2) and PASSAMAQUODDY (20.3), who speak, respectively, the MALISEET dialect (L.2A) and the PASSAMAQUODDY dialect (L.2B) of the language (L.2) called MALISEET-PASSAMAQUODDY. Today the MALISEET reserves are centered in the St. John River Valley of New Brunswick, and the 'QUODDY reservations are located on the bay and lakes of the easternmost corner of Maine. But in the first decade and a half of

the 1600's, the greatest ETCHEMIN overlord, Bashaba, lived on the Penobscot River, in what now is (and is shown as on Kroeber's map) the heart of PENOBSCOT territory, while his authority spread to the Saco River and his influence extended far into ABNAKI and PENNACOOK lands. Later, Bashaba's successors (including Madockawando) continued the ETCHEMIN overlordship, but of ever-less-vast domains. In the last decade of the 1600's, Englishman John Gyles lived as a captive of the MALISEET on the St. John River, helping them to hunt, fish, gather, and grow maize, under French "protection" and influence. Sometimes the ETCHEMIN and MICMAC were at war with each other (as ca 1615, when the MICMAC killed Bashaba), and at other times they were peacefully sharing hunting grounds. Their overall cultures were and are very similar in many ways. Fewer hostile periods may have marked the relations of the ETCHEMIN with the ABNAKI and the PENNACOOK, because of closer, or more frequent, alliance ties, called the ABENAKI Confederacy (A.1). Cultural similarities of ETCHEMIN and ABNAKI were great, also; but, as with a spectrum, the ETCHEMIN in the middle were more similar to both the MICMAC and the ABNAKI on each side than the MICMAC and ABNAKI were to each other.

To the southwest of the ETCHEMIN lived peoples of two separate confederations--the ABNAKI (17.3A) and the PENNACOOK (17.3B)--which phased into each other, but differently at different times. Likewise, their alliances with their neighbors varied over time, but it is probably safe to say that always in the past three centuries there has been something of a common-cause unity among at least portions of ETCHEMIN, ABNAKI, and PENNACOOK (or their descendants) that could be called "an ABENAKI Confederacy" of some sort. This "concept of unity" was described well by a Wabanaki college student, now Andrea Bear Nicholas, who knew both its EMICS and its

ETICS (Bear MS). It seems better, at least for our rather general purposes of gross taxonomy, to consider the ABNAKI and the PENNACOOK together, and there is no term better than the hyphenated one (17.3), ABNAKI-PENNACOOK, which shows their distinctness in their togetherness. First epidemics and then English determination to get the land without its native occupants on it caused both ABNAKI and PENNACOOK to remove and regroup before truly adequate accounts could be made of their aboriginal affairs, and, once started, their removing and regrouping became habitual, until we simply cannot tell village names from band names from tribe names, synonyms from different names, etc. For example, in the HANDBOOK OF AMERICAN INDIANS (Hodge 1907-10), ACCOMINTA and OSSIPEE are called both ABNAKI tribes and PENNACOOK villages. Also, long ago, someone started equating the term SOKOKI with the (to us) nameless SACO RIVER INDIANS, and others blindly followed the leader; however, anthropologically-oriented Canadian ethnohistorian Gordon Day (1965) has shown that SOKOKI is not a synonym for the SACO RIVER INDIANS at all, but for the SQUAKHEAG of the Northfield, Massachusetts area of the Connecticut River; so now every time one sees the term SOKOKI, its meaning must be checked by context, or some bad mistakes will continue to be compounded.

Most of the ABNAKI (17.3A) are long gone from their former homelands throughout most of Northern New England, the exception being today's PENOBSCOT (20.4) of Maine, whose history is treated in some detail by self-trained ethnohistorian Fannie Hardy Eckstorm (1945:especially 73-83). If there ever was any such thing as a "typical" ABNAKI "tribe", it probably was the KENNEBEC, or NORRIDGEWOCK. They, like the PEQUAKET and others, were

exterminated or driven from their lands by New Englanders during the Abenaki War of 1722-26--the only "French & Indian" War with no European counterpart. Some displaced ABNAKI went to live in devastated and vacant Old Town (where cleared ABNAKI maize-gardens lay idle). Today's PENOBSCOT are the descendants of these (and perhaps other) refugees; those few of them who still "talk Indian" speak PENOBSCOT (L.3A), which is a dialect of the language (L.3) called PENOBSCOT-ABNAKI-PENNACOOK. Most of the forced-out ABNAKI fled to Canada to join the host of other exiles, largely ABNAKI and PENNA-COOK, already at ST. FRANCIS, and later conflicts would see them joined there by still others. Indeed, today's ST. FRANCIS ABENAKI (20.5) are a composite people, generated from the heterogeneous refugee Amerinds of all the French and Indian Wars. ST. FRANCIS is located between Sorel and Drummondville, Quebec, on the site of the old French mission village (and raid-organizing base) destroyed by Rogers' Rangers in 1759. Because of their pluralistic origins, their native tongue shows traces of both the ABNAKI (L.3B) and the PENNACOOK (L.3C) dialects of the PENOBSCOT-ABNAKI-PENNACOOK language (L.3).

The PENNACOOK (17.3B), once centered in the Merrimac River Valley of southern New Hampshire and northern Massachusetts, no longer exist anywhere as such--and neither does the pure PENNACOOK dialect (L.3C) of the PENOBSCOT-ABNAKI-PENNACOOK language (L.3). Perhaps the largest number of PENNACOOK descendants in any one place today can be found among the ST. FRANCIS ABENAKI (20.5). The PENNACOOK started fleeing to French mission villages in Canada, and to ABNAKI and Mahican settlements, during and after King Phillip's War (1675-1678), because, even though some PENNACOOK were not involved in the war, vengeful New Englanders started practicing

genocide and indiscriminate enslavement of "savages" in general. These policies of the English colonists were directly responsible for the following century of anti-British hostilities by the Wabanaki peoples; indiscriminate slaughter was returned with interest from then on, by both sides, and only the French profited, if anyone did. But before this started, Passaconaway and his son Wonalancet organized the PENNACOOK Confederacy, with their own PENACOOK tribe as the leading member. This confederacy eventually included some tribes much less like the ABNAKI and much more like the Southern New England Algonkians, having a more sedentary horticultural economy, elm-bark houses, dugout canoes, and language more akin to Mahican. Some of these traits were possessed to a degree by all PENNACOOK, and even by some ABNAKI, but not to this extreme extent of excluding more northerly traits, such as were exhibited by the MICMAC. The PENNACOOK were at the opposite end of the Wabanaki spectrum, ecologically and culturally, from the MICMAC, and were next in line to the Southern New England Algonkians, whom they quite naturally somewhat resembled.

For each of the peoples, old and new, on the chart, detailed lists stating the names and locations of the various divisions (districts, tribes, bands, and villages) may be found in two Smithsonian Bureau of American Ethnology publications--HANDBOOK OF AMERICAN INDIANS NORTH OF MEXICO (Hodge 1907-10) and THE INDIAN TRIBES OF NORTH AMERICA (Swanton 1952). For the MICMAC alone, Hoffman's (MS:Ch.VII) unpublished dissertation THE HISTORICAL ETHNOGRAPHY OF THE MICMAC OF THE 16TH & 17TH CENTURIES provides a detailed list. Sister Mary Celeste Leger's (1929:159-163) monograph THE CATHOLIC INDIAN MISSIONS

IN MAINE: 1611-1820 presents a list for the ABENAKI Confederacy (A.1). And Frederick Johnson's (1940) article "THE INDIANS OF NEW HAMPSHIRE" lists PENNACOOK divisions. These lists (and any and all others), like Kroeber's map, should be studied with both appreciation and wariness, and then the careful student should study the sources himself and make up his own mind--because (and it should not come as too great a surprise, after all my preceding remarks) these lists are not in agreement. Especially is this so for the ABNAKI and PENNACOOK lists, because of the early and continual removals and regroupings of the constituent elements of these two confederations.

The greater the degree of detail one desires about the pasts (plural!) the greater the aggravation one suffers over ambiguities, unknowns, and mistakes, and the more prominent becomes the role of personal opinion in deciding the unclear situations (which abound!). Over time, all these problems are compounded, and the would-be interpreter becomes ever more confounded. No source or interpreter is exclusively correct, but some sources and interpretations obviously appear to be better than others, and some are just plain wrong. Sources and interpretations must be weighed carefully. But, our criteria for judging old sources and old interpretations are all too often only our own, and we are biased, even if only relative to the present. The errors and biases of the pasts may or may not be apprehensible and comprehensible in the present, anymore than the data of the exotic pasts. The ethnohistorian, the historian, and the ethnographer alike have accepted the great challenge, from their positions in the here and the now, of attempting to make more nearly orderly the inherent chaos of differences found in the there and the then, the then, and the there, respectively. Alike they share the practically impossible task of attempting to make the

exotic become readily understandable, first to themselves, then to their audiences. Even their taxonomies, let alone their "explanations" suffer from being stretched tautly across the credibility gap. Yet if their task is practically impossible, it also is impractically fascinating, both to the practitioners and to their public, and therein lies its reward!

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*I am much indebted to Dr. David C. Switzer (Associate Professor of History, Plymouth State College of the University of New Hampshire) for his constructive criticism of an earlier version of this paper. However, he is in no way responsible for any errors or other negative aspects of the present version.

**The maps on page 1 of this paper are based upon Hoffman (1955:70-71), but I have revised them to conform with other inputs. Particularly, I am led to believe that map "B" better represents tribal distributions and names for "CA 1725" than for "IN 1700."

NOTES FROM THE ARCHAEOLOGY LAB

The 1973 field season was a busy one for the archaeologists and students at UMO. It began in June and, weather permitting, will continue until late October. Projects included a field school, survey, and excavation

Field school was held in the Machias Bay area from 18 June thru 27 July, directed by Robert G. MacKay, assistant Jean T. MacKay. We based at the UMM Dorms and investigated six different sites.

From a training standpoint it was a successful session, the variety in the several sites gave the students a broader experience than has been possible in the past. From a research angle it was less favorable. Shore erosion has taken its toll and extensive pot-holing made it difficult in some sites to get in a two-meters square in undisturbed soil. However, some interesting material was recovered and several large hearths were exposed.

In June, Dave Sanger accompanied a team of Quaternary scientists to the Debert site in Nova Scotia for the purpose of taking another lake sediment core in order to get more specific data on the environment during the paleo-Indian occupation around 10,700 years ago. Dan Livingston, of Duke University, will be responsible for producing a pollen analysis diagram and Robert Stuckenrath of the Smithsonian Institution will provide the radiocarbon dates so that the pollen record can be tied to the occupation. The work is being done on contract with the National Museums of Canada and supervised for the University of Maine by Harold Borns. The Debert site was in danger of being destroyed by a new industrial complex, but we are pleased to report that the site will be set aside as a protected area within the industrial complex and that it is further under the protection of the Canadian Historic Sites Commission. This is our only known paleo-Indian site in the Northeast still maintaining the potential for further study and it is most important that the site be protected for as long as possible.

Later in June, Sanger continued his work in Passamaquoddy Bay with a survey of Grand Manan Island and surrounding islands. Unfortunately, the entire week was

a foggy one, and little was accomplished, except that sites known to have existed in relatively recent years have now been eroded away. Assisting with the survey was Mary Jo Sanger.

In July, Sanger, assisted by Norman Guerrete of Fort Kent, spent three weeks in a survey of the upper St. John and Allagash area assessing the possibilities of a more detailed survey. They found the area from Grand Falls, New Brunswick to Fort Kent relatively unproductive, but above Fort Kent there are some sites. Certainly, any hydro-electric program in the upper St. John should include some detailed survey.

During the month of August the important Hirundo site was excavated. Again we are pleased to acknowledge the help of the National Geographic Society. At times we had a crew of 16 students working in the field and processing in the lab. In an attempt to speed up the recovery we resorted to washing with water, an experiment which was only partially successful. Many of the crew members were from the field school. During the dig we not only recovered a better sample of the sited (we now have dug an estimated three percent), but were able to obtain a better idea of the stratigraphy. It would seem that the earliest artifacts rested on the glacial till, although we do not associate the age of the artifacts with that geological event. As Pushaw Stream continued to overflow its banks during high water periods silts were deposited on the eroded till surface. Man settled at the site, beginning perhaps as far back as 7000 years. A preliminary report has been submitted to the National Geographic Society and for publication to the fine new journal "Man in the Northeast". It will also be available in another form (see below). Work will continue at the site this fall and analysis will consume much time this winter. During the third week in August Bob MacKay supervised a few MAS members at Hirundo, and these lucky people got a feeling for the difficulties encountered at the site.

In accordance with new legislation regarding zoning, John Palter and Richard Bonenfant spent two months on the coast conducting perhaps the most detailed survey ever undertaken on the Maine coast. They got bogged down in Frenchman's Bay

where the large number of sites consumed a great deal of their time. New sites were discovered and added to the roster and many of the older ones, reported by Moorehead and workers pre-Moorehead, were visited and assessed. The survey was sponsored by Ford Foundation and State of Maine funding. A report on the survey and recommendations are in preparation. We hope that this kind of systematic survey and assessment of the State's archaeological resources can be continued in future summers.

Finally, we are busy preparing a series of reprints of articles pertaining to Maine prehistory. These should be available before the end of the year and at a very reasonable cost. Included with the reprints will be a set of artifact plates for reference purposes. As more articles on Maine prehistory are published we will make these available in low cost reprint form. A circular giving prices, etc. will be sent around to the membership when the reprints are available.

"Prehistoric Burial Places in Maine" C. C. Willoughby. 1898

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