

Dawnland Diaspora: Wabanaki Dynamics for Survival

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INTRODUCTION

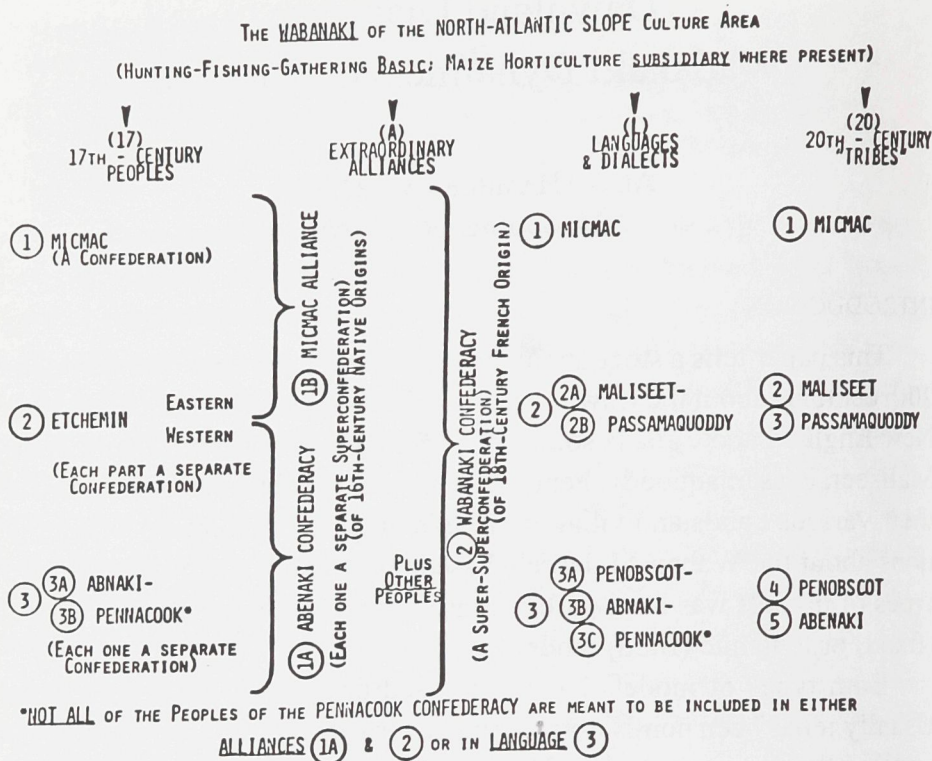
This paper tells a story about the stories that have been told during the 20th century, about the Dawnlanders — the Wabanaki peoples of northern New England and Canada south of the St. Lawrence River: the Micmac, Maliseet, Passamaquoddy, Penobscot, Abenaki, and Pennacook, each with their various bands and villages. It tells of quite different basic assumptions about the Wabanaki, largely based upon which one of two different types of models was used (either consciously or unconsciously): a static (fixed) or dynamic (fluid) model.

Both types of models have been used throughout the 20th century. Usually it has been non-Natives using historical records who have plied or implied the dynamic model. Natives relying on date-free oral traditions have tended to use the static model. However, these are merely trends, not requirements nor even political beliefs (except occasionally), as will be noted later.

As a diagrammatic concrete example of these abstract terms, consider my own use of a dynamic model. Geographic and ethnic fluidity over time are illustrated in the accompanying maps and chart — by changes both between maps A and B, and between the left and right columns on the chart.

William H. Mechling (1917:274) was one of the first anthropologists to recommend using historical documents in ethnological research about the Wabanaki peoples. Fannie Hardy Eckstorm (1919) was among the first historians studying the Wabanaki past to be truly concerned with their political organization and changing ethnic groupings. Mechling's and Eckstorm's combined ethnological-historical approach requires a dynamic model, with fluid assumptions as its basis.

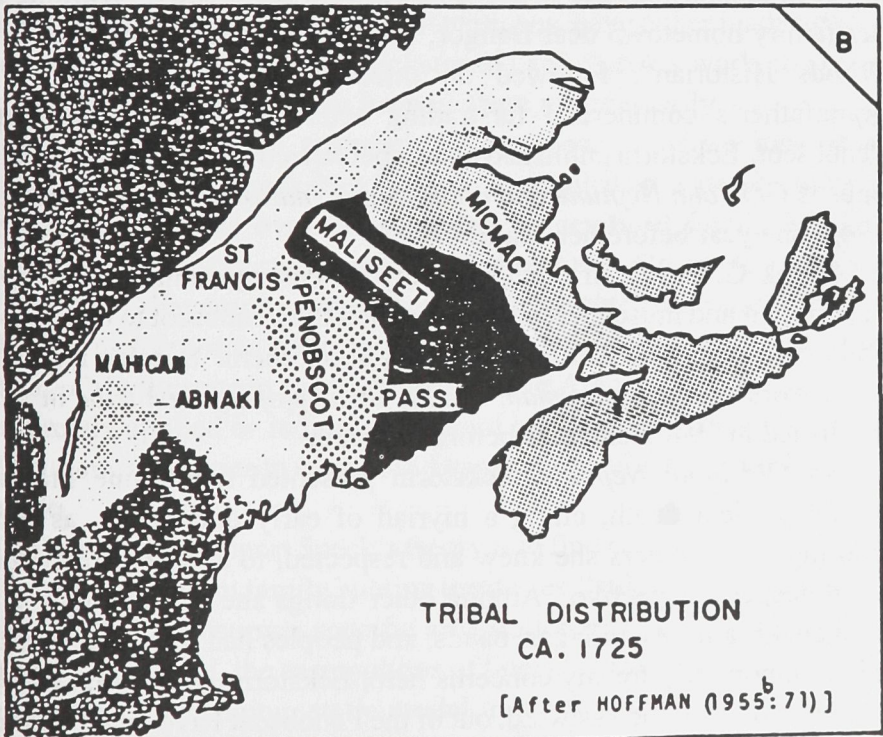
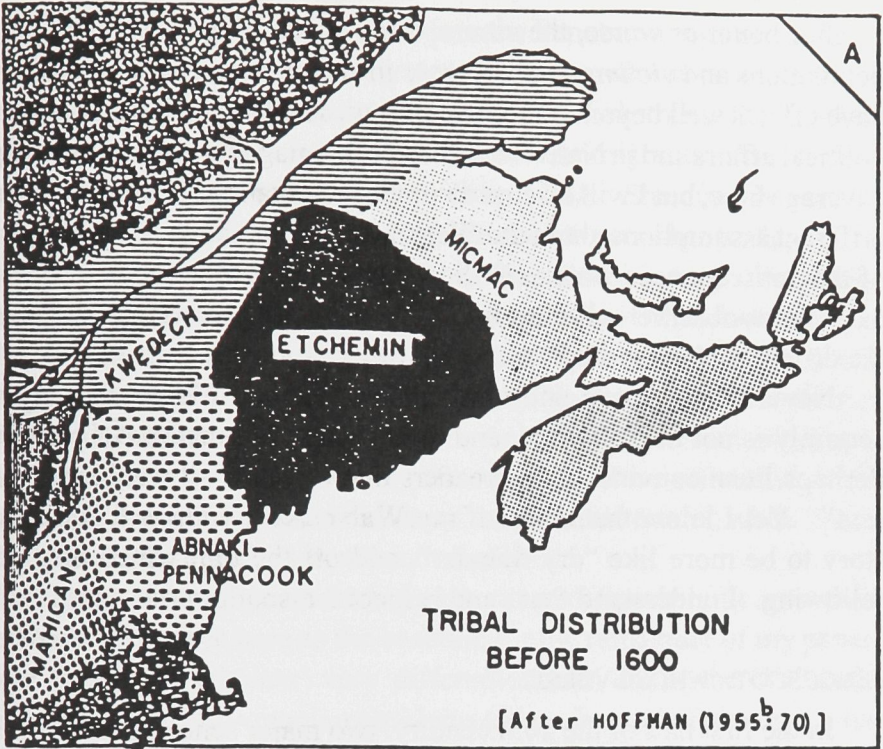
Quite contrarily, however, some prominent anthropologists and historians have plied their respective trades in such separation from the other that only a static model with its fixed assumptions is relevant, either



by intent or by default. This paper discusses both fluid and fixed choices and the consequences thereof, not only in theory but in practice.

On the theoretical side, consider, for example, assumptions about identifying Wabanaki ethnicity in time and place. In static assumptions of *who-where-when*, today's Wabanaki occupants of an area are considered simply to be the same ethnic people as yesterday's, and the same ethnic label is applied to both times' groups. The dynamic approach posits more complex factors of demographic change; therefore today's Wabanaki occupants of an area may be at least a continually different ethnic mix than yesterday's, if not an entirely different people.

For another example, consider assumptions about encounter outcomes. In extreme static assumptions, the Wabanaki are depicted simply as affected victims of white conquest — military and/or religious. The dynamic extremes credit the Wabanaki with complex heroic parrying of white thrusts of many sorts. Clearly these very different assumptions beget very different stories told about the Wabanaki in general and about some Wabanaki peoples in particular.



For better or worse, the assumptions of *unchanging* versus *changing* populations and *victims* versus *heroes* in encounters take on substance and have effects well beyond the scope of pure scholarship, both in public and political affairs and in Native American self-images. I attempt no complete coverage here, but I will discuss a number of situations in which there were different assumptions about the Wabanaki peoples, and will describe some of the consequences which resulted, from the perspective of a long-term participant-observer who has both affected and been affected by some of the different stories told.

Nevertheless, as an anthropological relativist, I know that my story certainly is not the only one, and that “truths” are matters of perception. Perhaps I can convince many readers that I’m not “all wet” but “right as rain”. Yet I know that some of my Wabanaki friends will consider my story to be more like “dry thunder”, and off the trail which they advise following. I understand that, and respect it also.

PENOBSCOT: WHOSE VERSION?

In the first half of the 20th century, two major scholars of the Wabanaki each emphasized the Penobscot, but very differently. Working from her family hometown near Bangor, Fannie Hardy Eckstorm, the “Maine Woods Historian”, followed up intellectually on her father’s and grandfather’s commercial fur-trading activities with the neighboring Penobscot. Eckstorm published many specialized articles, but her magnum opus is *Old John Neptune and other Maine Indian shamans*, published in 1945, one year before her death.

Frank G. Speck of the University of Pennsylvania’s anthropology department and museum repeatedly visited the Penobscot to do fieldwork, and published many specialized papers about them. Speck’s major summary work is *Penobscot man: the life history of a forest tribe in Maine*, published in 1940, ten years before his death.

In *Old John Neptune*, Eckstorm presented her Maine Indians in ethnohistorical depth, citing a myriad of early documents, as well as quoting native elders she knew and respected, to add oral traditions for even deeper perspective. Among other things she outlined how certain Wabanaki families, villages, bands, and peoples had relocated over time. Most importantly for my concerns here, Eckstorm relates that Etchemin people moved to the eastward, out of the Penobscot River and Bay region;

Abenaki people moved from the west, into the Penobscot region; and part of the (Etchemin) Neptune family returned from the east, to become Abenaki leaders in the Penobscot region. To Eckstorm, the modern Penobscot Indians were basically Abenaki immigrants into former Etchemin territory in the first quarter of the 1700s. She cited both early Euro-American documents and Native oral traditions as her basis.

In *Penobscot man*, Speck described his Penobscot in a detailed broad-ranging field ethnography; but, despite his subtitle of “life history”, it is *timeless* — omitting historical depth. For reasons discussed below, Speck implied a stationary occupancy of Penobscot River and Bay. Surely this was just a contrasting style, but its different assumptions eventually made it a conflicting story. Insofar as they are concerned (or not) with the *time* dimension, Eckstorm mothered the use of the dynamic model, while Speck fathered the static model, in Penobscot studies.

The basic assumptions which Eckstorm and Speck built into their respective works serve here as the baseline for the remainder of my paper, because these two scholars’ very different stories about the Wabanaki became the basis of still-ongoing problems today. Instead of blending what should be only complementary findings to gain a bigger brighter picture, some later scholars, including Native Americans, have either inadequately connected or have misconnected Eckstorm’s and Speck’s work, to short-circuit us into the dark and/or to keep us there unnecessarily.

Speck’s non-historical approach makes sense — if we look at it historically. Earlier, Speck had become embroiled in a debate among several anthropologists over the supposed longevity of family hunting territories throughout northeastern North America. Others (e.g. Eggan 1967, Feit 1991) have summarized this debate, in which Speck argued that family hunting territories were pre-Columbian in origin — i.e., before the intrusion of the European fur trade into Native American affairs. Speck either directly applied or indirectly implied his theory of pre-Columbian family hunting territories to various Indian peoples and areas, including the Wabanaki.

The best way to support Speck’s theory is to find (or at least to assume) “traditional” but current family hunting territories, “still” used by a specific people who “still” occupy a specific area stationarily, since a very early “long ago”. In a word, the assumptions of Speck’s theory *required* him to use a timeless, unchanging, static model in his fieldwork reporting. To

bring in *time* is to bring in *changes*, which tend to emphasize variables, not constants, at least in Euro-Americans' thinking, and Native American continuity with tradition may be disrupted thereby. Speck very likely considered this point carefully indeed, especially in regard to highly important land usage relationships.

Nevertheless, it was Eckstorm's ethnohistorical approach that was emulated when two major scholarly works about the Micmac appeared in 1955. Wilson D. and Ruth Sawtell Wallis used early documents to give integral depth to their 515-page general study, *The Micmac Indians of eastern Canada*. Bernard G. Hoffman's 839-page doctoral dissertation, "The historical ethnography of the Micmac of the 16th and 17th centuries", was an ethnohistorical tour de force — with a valuable spin-off article in *Ethnohistory* (Hoffman 1955b) which even maps Eckstorm's scenario of Abenaki movement into former Etchemin territory in the Penobscot region. Clearly, Wabanaki studies in the post-Eckstorm/Speck era started out with a dynamic model in mind; indeed, by then, North American ethnology in general was largely so inclined.

In 1974, I completed my doctoral dissertation on Wabanaki political leadership in 17th-century encounter dynamics with Europeans, summarized in two Algonquian Conference papers (Morrison 1976 and 1991). Like Eckstorm and Hoffman before me, my most basic assumption was ever-changing reconfigurations of the Wabanaki peoples. Yet I soon found myself (*and* Eckstorm and Hoffman) to be in repeated discord with Dean R. Snow, whose main assumption was based squarely on Speck's old static model of the Penobscot.

Most discordant of all was the issue of believing (as I did) or not believing (as Snow did not) the early documents' statements as to the ethnicity of two of the greatest 17th-century Wabanaki sagamores (chiefs), Bashaba (died *c.* 1615) and Madockawando (died 1698), and their people. The early records clearly call them Etchemin (and Madockawando was later more specifically called Maliseet — the Maliseet and Passamaquoddy being today's descendants of the Etchemin). I have used the term *Western Etchemin* for those west of Mt. Desert Island. Snow collapsed the earlier Western Etchemin with the recent Penobscot into "Eastern Abenaki", not only in an *Ethnohistory* article (Snow 1976) but also in his chapter in the Smithsonian *Handbook of North American Indians* (Snow 1978). I tried to focus attention on this discrepancy, seeking scholarly solution of the

problem if possible (Morrison 1978); later, I clarified the matter myself, unexpectedly, as related below.

What may seem truly trivial, thus far, suddenly took on a meaningful life of its own. The Maine Indian land claims case had gradually emerged during the mid-1970s, and both lawyers and politicians sought scholarly assistance. In the 1970s, one of the best criteria to win or lose a land claims case in an American court was to prove or disprove “exclusive occupancy” from “time immemorial”. Of course, Speck’s old static model seemed made to order for the Indian team, so Snow became the anthropological consultant for the plaintiffs. For the defendants’ side, Eckstorm’s dynamic model of population movements was equally useful, so I, as its current disciple, was drafted by the State of Maine’s team.

Fate had put me into the supposedly “anti”-Wabanaki camp, but both I and my research partner James D. Wherry greatly hoped for some sort of *pro*-Indian outcome. Fortunately, the U.S. Congress arranged a legislative settlement in the waning days of the Carter administration, so the Maine Indian land claims case never went to court. This was lucky for the Wabanaki in two ways. First, Wherry quickly went to work for the Houlton band of Maliseet, whom he was able to get included in the 1980 congressional settlement; for this applied ethnohistorical research, Wherry received the Praxis Award from the Washington Association of Professional Anthropologists. Second, my immediate contribution was the rediscovery of interesting forgotten information which would have soured the court case considerably. In carefully reviewing all possible research articles, I learned that Speck had recanted his old static model of the Penobscot, by agreeing with Eckstorm’s population movements model.

In 1947, three years before his own death and one year after Eckstorm’s death, Speck published two reviews of Eckstorm’s *Old John Neptune* (1945). In one of them (Speck 1947a) Speck only hints at a change of mind from his old static model of Penobscot country, but in the other review Speck clearly states his agreement with Eckstorm in words that could be her own, but are indeed his:

Carefully weighed reasoning... applied to the problems of identity of the people designated as “Etchemins”... by early writers indicates that the[y]... were resident about Penobscot bay at the time of first European contact and subsequently drifted eastward to where they now dwell under the names of Passamaquoddy and Malecite. [Speck 1947b:285]

Speck made no comment regarding the negative consequences to his

own works that his agreement with Eckstorm causes. Yet clearly if a single ethnic group did *not* occupy the Penobscot region continuously from time immemorial, then modern Penobscot family hunting territories cannot be pre-Columbian by default. (To this negative observation I must add that lawyers waving Speck's book review would have shot down at least the Penobscot land claim, and possibly also that of the Passamaquoddy, if these cases had gone to court for settlement instead of to Congress.) Apparently no one before me (Morrison 1980) had published any comment about Speck's unexpected consideration of the ethnohistorical perspective. We may never know his reasons, but Harvey A. Feit (personal communication, 1989) has told me that in his later years Speck also softened his stand on some issues related to family hunting territories among Northern Algonquian peoples.

Certainly I am very pleased that my rediscovery of Speck's agreement with Eckstorm did not hurt the Maine Indians' cause. Quite the opposite — a decade later (1990) my rediscovery helped the Aroostook band of Micmac gain congressional approval for federal recognition and retroactive inclusion in the 1980 Maine Indian land claims settlement, because by then the Eckstorm dynamic model of population movements had finally come of age in practice as well as theory. The requirement of demonstrating exclusive occupancy from time immemorial had eased towards showing shared usage before white takeover.

DIASPORA IN *ALL* DIRECTIONS

The years following 1980 brought remarkable development of both dynamic models and their related assumptions about the Wabanaki. Previously, many scholars — even Speck (1928) himself — had discussed *northern* movements and reconfigurations of Wabanaki peoples, mostly Abenaki and Pennacook bands, but only pathologically — in wounded retreat to Canada, to multi-ethnic refugee/French-missionary villages, from wherever they formerly had lived. Movements to the north were described, but none in other directions. The reason given was simply English pushing, or French pulling, or both together. Other Wabanaki peoples were presented as staying put and shrinking in place into ever-smaller enclaves. These stories implied a state of limbo, whether or not it was meant.

Finally scholars started to look beyond Eurocentric negative assumptions of forced *dislocations* to see adaptive *relocations* as well. Certainly

these moves were not always voluntary; Iroquois raids and European diseases and wars were frequent goads. But just as certainly they were not always forced. Kinship and marriage ties and a variety of opportunities (including the adventure of travel to visit other places and people) guided the moves, whether forced or voluntary. These guiding factors already seem to have been active before European settlement, and were unquestionably older than the outbreak of Wabanaki–English hostilities in 1675.

Seasonal environmental-use patterns of population regroupings and movements undoubtedly set the Wabanaki social rhythm. Yet the flexibility and fluidity of Wabanaki social organization and political leadership set the tone for voluntary obedience to a sagamore and voluntary inclusion in a Wabanaki community (see Morrison 1976). With intermarried relatives spread virtually throughout the Dawnland, Wabanaki residence options were both open-ended and structured. Whether the stimulus was danger or desire, Wabanaki peoples, bands, villages, families, and individuals could (and seemingly did) move more easily and more often than cultural outsiders then or now can easily grasp. And moving meant intermingling, which set up even more opportunities for future moves. All this is what I mean by the term *diaspora* — dispersion, for whatever reasons — and it applies equally to *all* directions, although at different times a single direction may prevail. Indeed, these adaptive relocations still continue today: by the second half of the 20th century, Wabanaki enclaves had developed in several northeastern cities (Guillemin 1975).

The annual Algonquian Conferences have been the forum for several scholars to develop dynamic models, either of Wabanaki population movements or of Wabanaki encounter outcomes, or even of both together. I consider the following to be some ethnohistorical milestones in dynamic developments in Wabanaki studies.

Bruce J. Bourque (1989) showed as never before the complexities involved in labeling and affiliating some peoples and places of the past, and called for an end to static models thereof. Bourque and Whitehead (1994) discussed the importance of the Micmac in redistributing European trade goods all across the Gulf of Maine, and in raiding which complicated the issues of who was where when, and (especially) how they were allied.

Harald E. L. Prins (1986) discussed the presence of Micmac and Maliseet in the St. Lawrence River valley, where no static model could

account for their presence. His paper was part of the successful case for retroactively including the Aroostook band of Micmac in the land claims settlement in Maine — where, also, a static model could not account for their presence. Prins (1992) showed that the supposedly Maliseet community of Meductic was multi-ethnic in both its culture and its people.

David L. Ghéré's paper on the "disappearance" of the Abenaki in western Maine (1993) told of the abandonment of large villages for smaller social groupings, leading the settled-community-minded English wrongly to assume an Abenaki withdrawal to Canada. Yet it was only the Abenaki village structures that disappeared, not the regrouped Abenaki, who moved around but not away.

Evan Haefeli and Kevin Sweeney (1994) did for an individual leader what Prins had done for the Meductic community. Their "Wattanummon's world: personal and tribal identity in the Algonquian diaspora c.1660–1712" traced the continual inter-ethnic reaffiliations of an originally Pennacook man on the move. This paper seems to contain the first scholarly use of the term *diaspora* for the dispersals of the Wabanaki and their Algonquian neighbors.

Mention of two other recent participants in Algonquian Conferences also should be included here for their relevant doctoral dissertation research — using dynamic models on the cutting edge of Wabanaki studies, extending the work of the other scholars just mentioned.

Ann Morrison [Spinney] (1997) pointed to examples of cultural resistance in her study of Passamaquoddy songs and dances: heroic parrying of the intruders' thrusts, by music! Her dissertation argues that Passamaquoddy musical creativity, including multiple uses of traditional melodies and use of outside melodies, has served to maintain their song tradition, rather than to destroy it. She was inspired to couch her findings in terms of *resistance* by Prins's Algonquian Conference paper on Micmac "neo-traditions" (1995), and his book *The Mi'kmaq: resistance, accommodation, and cultural survival* (Prins 1996), which is the most comprehensive recent story told about a Wabanaki people by a non-Native scholar.

David Stewart-Smith (personal communication, 1997) is attempting to retrace the Pennacook trail to apparent extinction by connecting whatever dots he can find in early records, starting by reconstructing kinship and marriage networks of the known leaders' families, and mapping their extents of influence, to assess the directions Pennacook bands went and

why. A large New England people as important as the Pennacook–Pawtucket alliance may be expected to have left lots of tracks, but what and where await finding. Among his greatest inspirations are Ghere’s model of “disappearance” through regrouping, Prins’s discussion of community heterogenization, and Haefeli and Sweeney’s example of the ethnic chameleon Wattanummon, who started out in the Pawtucket homeland on his, and his people’s, diaspora.

CONCLUSION

Humanistic understanding certainly should temper knowledge-just-for-knowing, lest the “mad scientist” monster prevail in making “paper Indians” more important than live ones. Yet no matter how well intentioned, the interface of the intellectual freedom of scholarship with the integrity and desires of Native Americans is always highly volatile, because it is as necessary as it is dangerous. Recognition by white governments and Indian land claims are the twin issues best showing both the necessity and the danger, because in them the stakes are highest. There are both benefits and costs, both real and imagined, to both scholars and Native Americans.

Staying constant to my belief in the dynamic model of Wabanaki population movements, I have been, at different times, on both sides of important Indian issues, and felt first the pain and later the pleasure of Wabanaki responses — negatively, in 1978, when I was screamed at in a busy hotel lobby for being on the “wrong” side with the “wrong” idea, and positively, in 1990, when I was bear-hugged under the U.S. Capitol dome for being on the “right” side with the “right” idea. It was the very same idea in both cases, but I never felt professionally lower, or higher, respectively, than I did on those two memorable occasions.

Certainly I want to continue applying my own scholarship “positively”, if I can. However, in what I research next, it may be that I again please one group and displease another, this time simultaneously. I personally consider that *both* groups involved merit classification as “Indians”, but I understand that this issue may be divisive. Again, also, a dynamic model and a static model compete.

Currently I again live in the one area I have longest wanted to study: western Maine, where the Abenaki did not “disappear” but hid in plain sight, often accepting the label of “French-Canadian”. Friends of mine here, who claim to have Maine Abenaki heritage (which often also means

having connections with the St. Francis/Odanak Abenaki in southern Quebec) are now attempting to organize to attain some form of recognition for their group. I have heard, both from some of those friends and from people affiliated with already recognized Wabanaki groups, that this attempt of the newcomers is being challenged by some individuals among the already recognized Wabanaki peoples. “They aren’t real Indians” is the reason given for discriminating against the Maine Abenaki claimants.

That same “reason” I have heard before, elsewhere. The Aroostook Micmac were left out of the 1980 Maine settlement of recognition and land claims, because no scholars at that time had prepared a case for their inclusion. In 1990 the Micmac case was ready, and was presented to Congress. The Bureau of Indian Affairs objected, but was overridden. However, the Micmac bill failed to pass in 1990 because of objections from one congressman, from a western state with a large population of Native Americans. Both objections centered on the Micmac supposedly not being *real-enough* Indians to be recognized officially. In 1991 the bill was reintroduced, and this time the Aroostook Micmac succeeded in winning both recognition and land.

Will the Maine Abenaki ever even stand a chance of group recognition? Will other Wabanaki become more, or less, discriminatory against them if they do, or don’t? The outcome of the longstanding bid for recognition by the Abenaki in Vermont may influence the answer to both of those questions.

Social discrimination is an ugly social problem, made uglier when intramural. Perhaps the ugliest discrimination of all can occur within families and between individuals — if and when their overall group *does* gain white-governmental recognition — using the mathematical discrimination of “blood-quantum” criteria. This is a double-edged sword that can cut off the Native American future while it guards the doorway for the present in-group. Perhaps the “mad scientist” monster already prevails in the blood-quantum countdown. The idea of a “pure race” unfortunately has outlived Hitler, being applied now to Native Americans. Blood-quantum criteria could easily be used for an equivalent of concentration camps, as the “ultimate solution” to a supposed racial/ethnic “problem”.

The apparent key to Wabanaki survival — from Iroquois raids, European diseases and wars, and social marginalization ever since — seems to be the Dawnlanders’ flexible adaptive patterns of social organiza-

tion and residence that truly deserve the name *diaspora*. Their maintenance of relative freedom depended upon the resulting social intermingling among the various Wabanaki peoples, neighboring Native American peoples, *and* friends of other “races”, for marriage partners, political and military leadership, and economic opportunities. Yet success in survival by diaspora is the very antithesis of success in survival by blood-quantum; sooner or later, to pass the former test is to fail the latter one. “Racial purity” requirements seem to me to be something best left behind, as one of the very most mistaken static models from the past, as we enter the 21st century.

This paper has told the story of my own vision-quest into Wabanaki studies — a journey not alone but increasingly in good company with like-minded story-tellers. We think, now, that we are on the right trail to the Wabanaki past, not “all wet” but “right as rain”, with only a little “dry-thunder”. Yet, others before have thought that, too, and we have taken them to task for it. The talking-chiefs of the future, white as well as red, may decide to class us with David Ingram of Elizabethan times, who trekked out of the Wabanaki woods and told fanciful tales which few were willing to believe. Only time will tell; we all are subject to *that* most commonplace dynamic model of relativity and relevance.

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