Prehistoric Land-Use in the Green Mountains: A View from the National Forest

by David M. Lacy

Abstract

Vermont's mountainous terrain is the last major environmental zone to have its role and significance in prehistoric land-use systems remain obscured by myth and misconception. There is, however, little reason to doubt that people have lived, worked, played, worshipped and died in the mountains through time and, as elsewhere, have left archaeological evidence of their activities. In the mid-1980s, the National Forest created an opportunity for its archaeology program to challenge the myth of a non-existent, or archaeologically invisible, Green Mountain prehistory. In the years since, field work has led to the discovery of several new sites. These discoveries, and our on-going dialogue with the Abenaki Research Project, have led to a new appreciation of the possible breadth of land-use patterns in Vermont's past.

Introduction

Myths are a necessary and pervasive part of how we deal with the world. They help explain the unknown and unknowable, provide justifications for individual and collective actions, and generally wrap reality in packages our ideological taste buds find palatable. Archaeologists, consciously and unconsciously, investigate and occasionally create myths about the past. Society consumes some of these myths when they become part of the culture histories and evolutionary stories we fashion from our revealing, yet incomplete, data sets.

The venerable myth of an under-occupied prehistoric Vermont, for example, shaped the way New Englanders looked at this state's early land-use and land-tenure for generations (see Day 1965; Haviland and Power 1981; Calloway 1984; Thomas 1986; among others; also see the article by McLaughlin and Thomas in this volume). This perception, in turn, conspired with other forces to prolong the modern alienation of Native Americans in Vermont (Haviland and Power 1981; Calloway 1990). While archaeologists have been among the contributors to the recent demise of this myth at a general level, it has demonstrated a long half-life even within our ranks when applied to the uplands and mountains, and has therefore helped sculpt the shape of the distribution of known archaeological sites in the region by skewing the definition of where sites are likely to occur.

This paper addresses three interconnected myths or misconceptions about prehistoric site locations and land-use patterns that I confronted when I started working in the Green Mountains in the mid-1980s. The issues discussed have relevance, I think, to both the history and the future of archaeology in Vermont because these myths were hale and hearty when the Vermont Archaeological Society began but are (we hope) on their death beds as we mark the Society's 25th anniversary. In their simplest form they state:

1. "Native Americans didn't use the mountains of Vermont;"

2. "Even if they did, the resulting sites would be insignificant, low-density scatters;" and

3. "Limits on money and methods prevent us from reliably detecting those kinds of sites."

These statements have myth-like qualities in that they contain elements that are believable assertions about the unknown (i.e., Native use of the mountains); went largely unchallenged by the relevant audience(s); are conveniently explanatory (thus justifying how/where we do archaeology); and are ideologically embedded in Euro-American cultural and economic value systems. They are, in addition, misconceptions because they are demonstrably false. And yet they have persisted among us both in the way we go about the business of archaeology and in the kinds of stories that we tell to our main audience: the public. In my attempt to shed some new light on these myths, this paper proceeds from the general level (the distribution and nature of sites), to the local (observations

Plate 1. *A Green Mountain view looking southwest from Bromley Mountain: topographic peaks, but archaeological "valleys?"*

about the Green Mountains), to the ground (implementation and results of some testing).

Peaks and Valleys in the Distribution of Archaeological Materials

At a general level, archaeology's traditional focus on large-scale multi-component sites made it easy to think that site discovery was a fairly simple matter of applying gross presence/absence tests across our sampling universe: either the pyramid/temple/city/fort/pueblo/henge/cairn was there or it wasn't. This was a perfectly good way of doing business as long as we were focussing on developing chronologies and typologies (or collecting art objects) and were able to think of sites as concrete, bounded, finite units of analysis (or containers full of archaeological "goodies").

As the scope of archaeology evolved to incorporate investigation of a broader range of economic classes and social behaviors represented in the past (as well as the articulation of cultural and natural realms), the definition of "sites" — and our methods for finding them and documenting their distribution — changed. Basically, it proved more difficult to archaeologically detect enough specific sites to let us see the general land-use patterns of pastoral nomads, land-poor peasants, the under-class of hierarchical societies in general, or — as in Vermont — the dispersed, low-profile portions of hunter-gatherer annual cycles.

Since our definition of what constitutes a site is often subjective (or at least highly variable), and our sampling methods have yet to yield a statistically valid model for their distribution (in part because we have a hard time defining them), a useful alternative formulation of the problem is to presume that *archaeological materials are distributed continuously, if unevenly, across the landscape* (Wobst 1983; Lacy 1986). Since the archaeological record is largely the result of human behavior, we should also assume that the unevenness of the distribution is non-random. It follows that there would be archaeologically detectable (and theoretically predictable) peaks and valleys within the distribution of materials across any given landscape — but given the proper scale, there are no truly empty places (just some very large distances between artifacts and/or features). Using this concept as a baseline, "site" boundaries become more permeable and vague, and are defined by an arbitrary decision about what density of materials constitutes a meaningful unit of analysis.

In the extreme, distributional "peaks" will be areas with large, dense, contiguous clusters of cultural material.

ological deserts, since the space between archaeological objects and features (or even clusters of such) may be greater than the resolution of our standard methods of detection.

Under these conditions, our presence/absence tests do not have much credibility or meaning unless they are geared toward detecting a specific range of shapes, sizes and densities of material clusters. We need to be more specific about what we are looking for (or, conversely, what we are willing to miss) with our testing strategies; even if we cannot use powerful statistics to express ourselves, narrative assessments would be a good start. In other words, we won't find many sites in the thin part of the distribution ("valleys") if we dig a skinny shovel test pit every 25 meters.

Now, in most places where the landscape has topographic relief, the odds are good that the peaks of the archaeological distributions are seen to have a direct and negative correlation with elevation. This view has certainly been prevalent in New England for a long time. Thus, topographic valleys are seen as distributional peaks, and topographic peaks are projected as archaeological valleys.

This correlation has some merit in terms of the sheer volume of archaeological "stuff" per acre (or square mile or whatever), but it does not reflect the probable number or significance of sites (or "meaningful clusters of material") in a given zone. A trap we can all too easily fall into is to suppose that the highly visible clusters we can identify in the lowlands and river valleys are substantially more numerous than the smaller, less visible clusters in the uplands. We simply don't know that to be true; moreover, it is highly unlikely in areas where most of the archaeological record is alleged to have been generated by people who spent the majority of the year in relatively small, mobile bands. While both the overall density of materials and numbers of large sites per acre in the traditional "high potential" zones is no doubt higher than elsewhere, the reality is that these "high potential" zones represent just a small part (10% maximum?) of the landbase in the state. Thus (as long as I insist on dealing in extreme scenarios), if we prioritize the delineation and protection of zones with a"high potential" for yielding rich, dense, stratified sites at the expense of the rest of the state's landbase, we virtually ignore the vast majority of sites (i.e., those located on the remaining 90% of the landbase).

What's worse (at least from a research point of view) is that it is an easy next step to assume that the rich lowland sites we protect are simply big versions of thin upland sites (Bender 1986). If that were the case, then lowland sites might indeed be inherently more interesting and significant because there is generally more to them, and the potentially time-consuming, labor intensive search for the less-obvious higher elevation sites could be avoided.

However, we have yet to establish data bases allowing us to compare the range of site functions, or seasons or eras of occupation, for example, between upland and lowland sites (but see Frink, Knoblock and Baker in this volume). If we graph/plot the location of different kinds of sites (e.g., contact villages, kill sites, lithic procurement workshops, vision quest spots), or functionally similar sites at different seasons, their distributions would have different shapes and slopes. If taken as a whole, however, the distributions would tend to level out (i.e., they might overlap somewhat but would not duplicate one another). Collector activity and CRM surveys may well have reinforced the dominance of just one of several possible graphs/slopes by focussing on, for example, artifact-rich seasonal fishing villages. We may also find that the population of upland sites reflects a high proportion of single or limited component events — thereby offering more analytically robust opportunities than the more complex/disturbed deposits often associated with larger sites (Thomas 1986; Dewar 1986).

Expanding the Archaeology of the Original Vermonters

Shifting from the general to a more personal and local level, my suburban Boston upbringing included a world view assumption that most everyone who could do so lived on flat, well-drained, and reasonably low-lying places — with the occasional ski bum, affluent, eccentric, fire tower or missile silo perched atop the local drumlin. My subsequent indoctrination into the prehistoric archaeology of New England did very little to alter this assumption: if the traditional syntheses of our archaeological heritage were any indication, the vast majority of Native Americans must have shared my suburban sensibilities.

Between the time of my first education in the archaeology of New England and the time I started working in Vermont, however, I had worked with colleagues at the University of Massachusetts on a project entitled "A Retrospective Assessment of Archaeological Survey Contracts in Massachusetts, 1970-1979" (Dincauze, et al. 1980). This study taught us, among other things, that the distribution of

known "sites" (yes, those loosely-bounded, poorly-defined, unstandardized clusters of archaeological materials) was a direct function of the number and intensity of surveys done in an area (Hasenstab and Lacy 1984) — a point which is still not widely acknowledged.

I was, therefore, primed to ask some questions bout areas which were previously thought to be archaeologically "marginal" by the time I came to the Green Mountains. I observed that the National Forest had a rich natural resource base, was in proximity to areas of known archaeological sensitivity, had reasonably good soil preservation, and was positioned strategically *vis a vis* major transportation corridors (see Jackson 1929) and river drainages. Yet my literature review, background research and interviews with archaeologists resulted in a striking absence of known sites — or even previous archaeological inquiry. (In 1984 the inventory of known prehistoric sites within the more than 350,000 acres of the National Forest consisted of three unverified "find spots" [Casjens 1978; Loring n.d.].) In retrospect, the only reference I can recall finding for a successful site survey near the Forest, but not on the Otter Creek, was Thomas et al.'s 1982 Ball Mountain Lake report which located a single component Late Woodland site along the West River (Jamaica, VT) at just under 900' elevation. (See Table 1 for comparison with on-Forest site elevations.)

In addition to seeming illogical (i.e., the site inventory didn't even fit society's stereotypes of "can-do," all-knowing, one-with-nature Native American behavior) this state of affairs ran counter to a growing number of precedents elsewhere in North America which showed that there were significant populations of archaeological sites at higher elevations (e.g., Wright, Bender and Reeve 1980, and Bender 1986 [in Wyoming/Jackson Hole]; Benedict 1981 [Colorado Rockies]; Bettinger and Thomas n.d. [ranges around the Great Basin]), as well as in the geographically and/or geologically related environs of the Appalachians (Ayers 1976 [Blue Ridge Mountains]; Bass 1977 [Great Smoky Mountains]; Mathis and Crow [North Carolina]; Barber 1984 [various Southeast locales]; Sassaman 1986 [South Carolina Piedmont]) and, potentially, right next door in New Hampshire's White Mountains (Cassedy 1986). These and other sources represented ample evidence, then, from comparable areas to suggest that hunting, trapping, gathering, travel, refuge, lithic procurement, and sacred or ritual activities were all reasonable activities to have taken place here in Vermont as well.

The most obvious reason for the apparent dearth of recorded knowledge about archaeological resources in the Green Mountains was that no one had looked very hard or very often. Ironically, archaeologists — known to the public for our activities in remote and exotic locales — had not been attracted to Vermont's National Forest with the same intensity as tourists. Since they had not, the Forest remained archaeological *terra incognita*.

In fairness, I should acknowledge that a few archaeologists working in or near Vermont shared and encouraged my enthusiasm (to varying degrees) about the potential for sites in the mountains, but none had been in a position to operationalize these expectations in the field. Regardless, in order to justify my request to Forest Service management to fund a prehistoric site survey program, I needed to identify why no one had gone before me (in effect, why I thought the inherited wisdom, or myths, about the lack of sites in the mountains was unfounded). I concluded that the primary reasons were to be found in the social and economic context in which we do archaeology (Lacy 1985). Briefly:

(a) prior to the advent of professional Cultural Resource Management (CRM), most sites were discovered through soil disturbance associated with farming (i.e., plowed fields) at lower elevations in alluvial deposits;

(b) our 20th century, Euro-American view of the landscape did not encourage a search since the mountains were not "civilized" places to live or farm; [but prehistoric economic systems would not have generated stratified land-use patterns parallel to our own anyway (see Sassaman 1986)];

(c) archaeologists had not (or not often) consulted with Native Americans about their understandings of past land use in the mountains [so our lack of knowledge was based in part on a culturally derived myopia (see Note 2)];

(d) the development of culture histories and typologies favored the identification of large, dense, stratified deposits characteristic of seasonal riverine gathering spots [which in turn influenced field survey methodology]; and

(e) CRM followed in the geographic footsteps of earlier site discoveries (since that's where the work was) and therefore reinforced predictive models which were based on already existing data — which takes us back to the beginning of this sequence.

Table 1. Distribution and Result of Survey Units by Elevation

Elevation (feet) Above Sea Level	Site	Description/Context
700	yes	Otter Creek; 2 hearths/biface/flakes
760	yes	Otter Creek; flakes/hammerstone
950	no	West Branch floodplain
950	no	White River floodplain
1000	no	Hancock Branch floodplain
1000	no	Michigan Brook
1250	no	upland pond A, #2
1250	no	upland pond A, #3
1250	yes	upland pond A, #4; flakes
1300	no	upland pond A, #1
1300	no	upper Middlebury River
1300	no	Texas Falls
1700	no	branch Middlebury River headwaters
1900	yes	upper Deerfield River
1900	yes	upland pond B; flakes/bifaces/blanks
2100	yes	beaver pond; 2 points/knife/flake
2240	yes	upland pond C; 1 point/flakes
2350	no	knoll overlooking upland pond D

In sum, the pattern of site discovery in Vermont (as elsewhere in northern New England and New York) was an artifact of the interplay between the money, ideology, science, population density, and systematic soil disturbances characteristic of modern Euro-American history — which, for the most part, excludes the mountains.

Prospecting for Evidence of a Green Mountain Prehistory

Given the assumption of a continuous but unevenly (non-randomly) clustered distribution of significant archaeological materials across the landscape; the rich resource base and strategic location of the Green Mountains; archaeological precedents for use of the mountains elsewhere in North America; and a socio-economic argument for why no one had yet focussed on this large area of our state, the next challenge was to design and implement a search strategy within the National Forest. While the long-term goal was (and still is) to generate a predictive model for site locations, the short-term goal was a somewhat humbler task: to test the "null hypothesis" that significant sites were not commonly occurring phenomena in the mountains — or even rare and unusual events (Lacy 1988). Thus, the two basic questions were where and how to look.

In contrast to a mini-myth that has arisen recently to the effect that I have a uniquely insightful model for site locations in the mountains, my practical guidelines for where to look were for the most part traditional site locational criteria (i.e., reasonably level, well-drained areas with access to fresh water, strategic travelways, unique landmarks, and/or rich or unusual natural resources), with the following caveats:

(a) eliminate elevation as a predictive factor in selecting areas (this is, after all, the main feature of the myth: mountains are high places);

(b) reduce, the size/scale of the areas being considered (e.g., half-acre spots are just as likely to contain interesting sites as extensive plains);

(c) downplay soils as predictive factors, except as gross indicators (e.g., don't trust that old correlations between site locations and soil types, based as they are on distributions in alluvial contexts, have anything to say about site distributions in the mountains);

Plate 2. *One of several upland ponds (i.e., above ca. 1500' elevation) within the National Forest which hold promise for evidence of prehistoric use or occupation.*

(d) stratify ruthlessly (i.e., eliminate "low" and "moderate" zones from consideration); and

(e) freely use intuition in the field to determine "hot spots" to test within the general area you identified based on general environmental criteria (this includes consideration of such things as view-sheds, personal comfort, evidence of modern day preferences, etc.).

Second, having located areas based on these guidelines, I needed to establish *how* to test an area with appropriate intensity given the nature, size and density of the sites likely to occur in the area. I chose to cluster three 40-cm square shovel test pits (stps) every ten meters within a 100 x 100 m "Survey Unit" grid. This level of intensity reduced both the diameter of, and the density of materials within, clusters confidently detectable through the use of stps (this intuitively logical observation is quantifiable through modelling; my work was informed by Hasenstab 1986). Small diameter clusters of materials would not fall between the grid intervals; low artifact densities at any test point had a good chance of being detected by the stp clusters; and Survey Units subsumed both the phenomena we wished to observe (all or parts of a site) and the small-scale topographic variability characteristic of the mountainous terrain. [Though I wasn't aware of it at the time, this was essentially the same line of reasoning which resulted in the State's standardized 8 meter stp interval.] An additional consideration was that testing within this Survey Unit could usually be completed in a reasonable amount of time.

Since Survey Units were placed subjectively and selectively in areas considered to be "hot" (in keeping with the guidelines listed above), and vast areas of the potential "sampling universe" (the 350,000-acre National Forest) were ignored, the strategy should be considered "prospecting" rather than sampling (Wobst 1983). The initial intent, after all, was to demonstrate that sites existed and were detectable, not that we could document their general distribution. (Thus, even as I tried to make the data base more representative, I may well have perpetuated untested assumptions about site locations which the next generation of archaeologists will have to debunk.) The benefits derived from prospecting intensively in small areas, however, are the increased likelihood of site encounter and a more rigorous definition of archaeological "empty spaces" (i.e., at least we can know with certainty that clusters of material smaller and less dense than a certain arbitrary but calculable figure are not present).

Table 2. Distribution of All Known Sites on the Forest by Elevation

Elevation (feet) Above Sea Level	Description
700	flakes, bifaces, hearths on Otter Creek knoll;
750	chert scraper; oral history accounts, on White River;
760	flakes & hammerstone in Otter Creek floodplain;
850	non-diagnostic quartz point find on trib. to Otter Creek;
1240	2 Woodland dug-out canoes in upland Pond A;
1250	flakes found, collector has points; shore of Pond A;
1270	"nutting stone" find spot, opposite shore of Pond A;
1400	pestle found and reported along upper White River;
1650	small lithic scatter found during "106" testing;
1700	find spot reported to the State DHP;
1750	large enigmatic cairns; possibly Native American;
1890	quartzite quarry: flakes/bifaces/blanks & more;
1900	flakes in floodplain of upper Deerfield River;
1900	flakes along outlet from upland Pond B;
1920	flakes/tools/hearths on terrace of upland Pond B;
2100	Madison point, knife, flakes on edge of beaver pond;
2200	apparent multi-acre encampment by large wetland;
2200	arch. & oral history indicates "traditional use" site;
2210	3 scrapers found on shore near beaver pond complex;
2240	Levanna point, flakes by outlet of upland Pond C;
2250	non-diagnostic point find spot on beach of Pond C;
2250	quartzite knife find spot along upper Deerfield River;
2600	flakes of various materials on east side of Pond D;
2610	primarily quartzite flakes on west side of Pond D;
2740	mountain top lithic scatter, collector has biface;
2830	same mountain: different site, different vista;
3000+	based on location, vistas, headwaters, and other criteria, and confirmed through oral histories, many mountains have the potential to be sacred and/or traditional use sites.

Results

Between 1985 and 1988, 18 Survey Units were completed by part-time Forest Service employees (including the author) and occasional volunteers. Seven of these Units (or ca. 40%) yielded prehistoric archaeological materials (see Table 1). Elevations of sites ranged from 700' to 2240' above sea level, with no apparent clustering in or preference for lower elevations. So, while elevation may indeed inhibit or constrain land-use or other behaviors, the archaeological record did not demonstrate that very well in this instance.

This testing program was effectively ended in 1988, when a more standardized, project-based approach was adopted — in effect, a development reflecting the success of the Survey in demonstrating that there were indeed sites to be found on the National Forest [see Note 1 for a 1993 status update on the FS Heritage Program]. Since then, additional sites have been found or reported — some through testing, some accidentally, some by virtue of raised awareness on the part of non-archaeologists, and some through our relationship with the Abenaki Research Project (see Note 2). These sites run the descriptive gamut from small "lithic scatters" to a kilometer-long quarry, isolated biface find spots, to a multi-acre high elevation encampment. Table 2 is a list of all known sites and find spots by elevation.

Conclusion

What, then, of the three myths I highlighted at the beginning of this paper? First, and most obviously, we have evidence that Native Americans did use the mountains of Vermont, perhaps extensively, during prehistory. The myth that they did not do so persisted until recently because of the history of social and economic forces that shape why, how and where we do archaeology. In a related vein, we should be cautioned against relying too heavily on known site inventories as indexes of how prehistoric people used the landscape. Given our biased sample of the region it is possible that this distribution reflects as much about how 20th century folks have been using and thinking about the landscape as it does about people in prehistory. This should have serious implications for archaeologists doing environmental review (although recognizing the implications and being able to do anything constructive about them in the short term may be two entirely different propositions).

Secondly, we have evidence that sites in the uplands are not all small, low-density scatters (e.g., Lacy 1987; and see Table 2). But even if most of them are, there is no good reason to suppose that they are simply clones of the large low-land sites, nor that they would be insignificant based solely on their size or material richness. We need site-specific analyses of a number of sites from a range of environments and elevations before we can address this issue completely. Funding and conducting analysis on sites discovered within the National Forest remains a challenge in the present economic environment, but we are obligated and committed to following through on the sites we have identified to date.

Third, a standardization of methods for site detection in the forested landscape of New England is one of the by-products of the CRM boom of the 1970s and 1980s (Lacy and Hasenstab 1983; Dincauze and Lacy 1985). This took place, for the most part, away from the uplands and mountains in areas which were more likely to contain large, high-density sites. Given the demonstrably broad range of variability in the density and distribution of archaeological materials, we should not expect that one strategy can yield equally sensitive results in all environments. Having said that, however, Vermont should be applauded for having adopted and mandated effective test pit intervals early on in its regulatory history — intervals which should be sensitive to detecting even small diameter sites.

As we refine our approaches further, and apply this interval across a broader range of environments, we should experiment with the size of the test pits dug at each point on our grids or transects, varying them as our expectations of site type, size and associated artifact densities vary. Since, as a general rule, postglacial soil formation rates (and therefore maximum possible depth of cultural deposits) decreases with elevation and distance from active alluvial contexts, the additional cost of wider test units in some of the ostensibly "marginal" upland environments should be offset by their shallow profiles. The suggestion, therefore, that the discovery cost for low-density upland sites is significantly more than that of sites in other contexts, or that our methods are not adequate to detect them, is hard to sustain (although a good argument could be made that the cumulative cost of documenting the broad, population-level patterns of these sites could be astronomical given the huge acreage involved).

Finally, I think it would be ironic if we supposed that our present inventory gave us an accurate reading on hunter-gatherer behavioral systems (an implicit goal in most of New England prehistoric archaeology) if the small, dispersed sites likely to have been characteristic of these peoples' living patterns for the majority of the year are not an integral part of our data sets — or even how we think about modelling the distribution of potential sites. Our present inventory is likely top-heavy with aggregation points reflecting some seasonal and functional sub-set of those systems. I think we should recognize that there is potentially a whole population of uninvestigated sites out there which could introduce significant new variability into our interpretations. Since most of Vermont and northern New England's prehistory is the story of various hunter-gatherer economies, it may well be these "valleys" of the distributional continuum that we need to start studying in order to achieve a more complete understanding of regional systems and broad-scale land-use patterns.

Notes

1. Status of Heritage Resource Management on the Forest:

Having laid out some rather idealistic rhetoric, it would be fair to ask how this information has affected practical management of Heritage resources on the National Forest over the last few years. My "inside" assessment is generally positive, but there are several lifetimes of work left to do:

• Awareness of the existence and importance of archaeological sites, Native American heritage and traditional use issues, and relevant laws and regulations affecting how our organization does business has become relatively good among Forest employees, but suffers from the turnover of personnel and the wide range of other resource concerns competing for attention. This awareness has resulted in the discovery of at least one significant site and the protection of several others from unanticipated impacts.

• Funding for the Heritage Resource program varies from year to year but has included funding for at least one full-time assistant archaeologist for the last 4 years (a dramatic change from the early 198Os when there was but one part-time archaeologist). That level of funding may be in jeopardy as we anticipate Service-wide budget and program cuts beginning in Fiscal 1994, but to date has been in place (at least in part) due to the positive results of our testing program.

• Methodologically, we have adopted the State's standard 8 meter interval for testing. We have not experimented in any systematic way with varying test pit sizes, although we are the logical candidates to do so. We conduct more field testing now than we ever did before initiation of the Surveys reported on in this paper, but much less than while this prototype was funded. Despite an increased budget for our program, the Forest Service is institutionally biased toward "avoidance;" so (ironically) once we were able to demonstrate that sites did indeed exist "out there," the route of least resistance/cost turned out to be re-design of projects more often than conducting archaeological testing. Thus, we spend more time doing environmental review and field reconnaissance, and less time digging test pits. (And an old lament: while we have had success in identifying sites, we also have a serious backlog in terms of analysis, evaluation and reporting.)

• We have developed a landmark Partnership with the Abenaki Research Project (ARP). Our cooperative agreement has as its primary goal the proper management of sites relating to Native American heritage and traditional use. The working relationship provides ARP personnel with the opportunity to visit, evaluate and help develop management strategies for sites discovered on the Forest, and for them to identify areas of concern within proposed Forest Service projects of various types. Most of the ARP time involved in this process is covered under a reimburseable, matching Cost Share Agreement. Not all sites identified fit the usual definition of "archaeological" site since they may have no artifactual component and, therefore, would not readily have come to the attention of an archaeologist.

• As this paper was being written (August 1993), the Heritage Program received word that our prehistoric and historic site inventory was to be digitized and incorporated into the Forest's Geographic Information System (GIS) data base — a computerized mapping system. This is exciting news in terms of the power of this tool to provide analytical results, help add sophistication to our management recommendations, and facilitate providing information to others.

• Finally, the recent (1992) advent of an "Ecosystem Management" philosophy/directive within the Forest Service holds the as-yet-unfulfilled promise of true interdisciplinary research into the best management practices for lands controlled by the Forest. A heavy emphasis on the kinds of information that archaeology might provide would be logical (e.g., past land use, paleo-environments, biotic population trends, catastrophic vs. incremental changes, etc.), and may offer another kind of applied utility (and source of support) for the program.

2. There is yet another myth which I did not touch on here (but see Lacy 1989): the notion that relations between archaeologists and Native Americans are inherently contentious, that we mix like oil and water. This is relevant to the subject of this paper because if we choose to pursue knowledge about the use of the mountains we may be opening a door to a set of sites with a disproportionately high percentage of sacred and traditional use components.

My experiences in Vermont so far show that this myth of "us versus them" is analogous to the one I originally confronted: that is, if taken at face value, the traditional relationships between archaeologists and Native Americans would seem to have about the same odds of resulting in a healthy dialogue as the state's collector- and CRM-based site inventory has of predicting a wealth of prehistoric sites in the mountains. Yet, as this paper has tried to demonstrate about the archaeological record, there's probably more to the situation than meets the eye. I think that if we take the time to assess the goals and values we share, and think about who (if anyone) benefits when a wedge is driven between us, that the actual breadth of common ground is striking (as one working example, I would cite Lacy, Moody and Bruchac's 1992 report to the National Park Service in regard to the re-

route of the Appalachian Trail in the Pico/Killington region). The establishment of positive working relationships can serve all of us — and the resource — well in the long run.

Acknowledgements

I presented a shorter version of this paper at a joint meeting of the Vermont and New Hampshire Archeological Societies in Enfield, N.H. (Lacy 1991). Thanks go to the people at the National Forest for their support of the Heritage program; to the archaeologists who have worked with me there (most recently and especially Shelley Hight and Eric Bowman); to the members of the Abenaki Research Project, past and present, for their interest and trust; and to Barbara, Jameson and Mackenzie, who help me maintain some perspective on how and where archaeology fits in the larger scheme of things.

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