Research and Resource Management Priorities for Northeast Historical Archaeology: A Plan for the Comman Man

RESEARCH AND RESOURCE MANAGEMENT PRIORITIES FOR NORTHEAST HISTORICAL ARCHAEOLOGY: A PLAN FOR THE COMMAN MAN

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Historical Archaeology suffers a notable bias which has tended to warp our perception of cultural heritage and to prejudice both research and preservation priorities. It is an understandable and sometimes unavoidable bias, to which the American Northeast is a principal subscriber. We now have, however, the opportunity and the historical assets to become a primary force in its correction.

The bias is the inevitable by-product of our natural fascination with superlatives (biggest, finest, first, most) and with the sensational (unique or bizarre or culturally symbolic events, episodes, persons and material features). History has endowed the Northeast with an inordinate amount of the extraordinary. Hence, it is no surprise to find that our research and preservation concerns have been largely limited to things urban, early, unique and of high status. Legitimate reasons of interest, better identification and urgency have abetted this fixation. Documentation proliferates around the highly visible components of history and their material accoutrements, making the bias self-reinforcing. The limited sphere from which the preponderance of information has been produced has warped any sense of balance in the assessment of our cultural heritage. Therefore, everything has come to be interpreted and evaluated in line with solid data and artifacts whose validity can only be demonstrated in the cultural extremities of history.

The other side of the coin, however, is opportunity. The "silent majority" of more commonplace incidences of historical process have

precipitated physical evidences broadly throughout the Northeastern landscape. And economic shifts have minimized subsequent disturbances of the residue, especially in those remote areas and later periods which have received the least attention to date. They are also sectors of prime potential for the implementation of newer techniques, such as those employed by environmental studies, which can derive kinds of information that are likely to be lost between the cracks of traditional research. The time has come for a shift in research and preservation priorities from things prominent and evident to features that are remote, hidden and more representative.

Cultural resource management decisions will need to be made with primary regard for the system in which any feature operated historically. We need to begin evaluating the spaces between the architectural components, thinking of communities rather than just sites, and taking note of secondary and support features as much as of the primary ones to which they relate. With the exception of privies, functional features of a domestic site have not excited archaeologists very much. But the barn may have about as much to divulge as does the house, may have suffered less subsequent alteration, and may be more worthy of preserving. And as a non-productive tax burden, it is likely to be far more acutely endangered at present. A similar plea of respective relationship may be made for warehouses and shops to mills, for tenant quarters and secondary-function areas to farmsteads, and for paupers' huts to center villages.

This realignment of priorities bears a variety of important implications for cultural resource assessment (e.g., by physical setting, by period, by site-type). I shall generalize what I see to be the primary priorities. To be too specific would be to predict out of hand the value ranking of components for a given community. And a primary claim that I would make is that the cultural development of each community <u>as community</u> be the major determinative for the cultural resource management of its components. Similarly, the community--or neighborhood that functioned as the social and economic unit--must become the focus of research.

In terms of geography, more attention needs to be devoted to the hill country and highland communities. Topographically, every small and intermediate stream in New England that has not been subsequently completely altered is probably a primary information source, and every larger one that centralizaed industrialization passed by certainly is. Every period is important, of course, but the major chronological priorities for research and preservation vary greatly by region, community and site. It is generally far more useful to look at stages in the development of a given community than at calendars when making judgements regarding cultural importance. However, by and large in this region, the period falling roughly between the Revolution and the Civil War is the neglected stepchild of preservationists, historians and archaeologists. It is also the one whose resources are currently the most heavily distressed and perhaps the one bearing the greatest potential. With regard to type, it is surely apparent by now that I am making a case for rural and proto-industrial communities as the most

promising and neglected resource. The agrarian neighborhood system provides the parameters in which I think research and preservation decisions need to be made. Spatial variables, the alteration of the landscape and the placement of communal-use facilities are all factors of primary importance that are notable by their absence in the site-oriented criteria normally employed. Farmsteads, and not necessarily the most distinguished or the earliest, deserve more attention. Their arrangement, their supporting functional features (e.g., barns, storage facilities for food and ice, workshops, systems of domestic hydrology, and secondary economic activities) and vernacularized effects all stand in need of the fullest protection and investigation. These bear information otherwise sporadically documented at best. Low technology industries also require major attention. Redware potteries, blacksmithing, brickmaking, wheelwrighting and construction trades are examples of industries that are more or less known from urban concentrations, but whose fit into rural economic communities at varying stages of development is presently only guessed at. Small, early and adaptive waterpower sites, especially those in remote areas with low water flow, are a further source of otherwise inaccessable information. They join the low-technology enterprises in forming the necessary precursor to American industrial economy. For every success recorded at such a site, there are hundreds of trials and sites that provide the essential data for balanced understanding which remain unrecorded. Much of that corrective information is still available, largely undisturbed, usually below the ground.

I should affirm that I am unaware of the importance of continuing to preserve the urban and unique sites. Religious, commercial and public structures and the residences of prominent persons will continue to receive deserved attention. Particularly, I am also aware of the critical position of industrial sites and centers. While I am personally not prepared to make a categorical division between "historical" and "industrial" archaeology, I do recognize the unique clustering of specializations that is necessary for the latter. It encompasses a large, technology-oriented area of investigation. Therefore, I shall leave it to those more directly charged with their responsibility to comment on industrial sites, except to make two observations. First, I think priority decisions for those site complexes likewise should be made in the context of the intrinsic system and community to which they subscribed, not shortchanging secondary functional components. Second, I contend that small scale industries and proto-industrial communities should first be evaluated individually and independently, not according to criteria derived out of our knowledge of developed industrial centers.

Research strategies for the more diffuse and less visible components of our cultural history have yet to be developed cogently. Lumping together sites because they are "eighteenth century" or "colonial" may eclipse more relevant distinctions than it enlightens. There is no single time line that can be plugged into the calendar to assist us in isolating important and neglected or imperiled features. To impose any rigid chronological hierarchy across the board would be contrary to our knowledge and would violate the peculiar history of any community or site. Irregularities of development by region and community would impose

exceptions overwhelming the rule. One simple model however, seems to apply for the great majority of New England communities in any region: they move from frontier organization through successive periods of agrarian-communal and proto-industrial structures into urban industrial systems. Some, to be sure, enter the scene at such late date as to have only partial or abbreviated early stages, or more usually, to build off those stages undertaken nearby. Some never reach the last stage, being arrested in the proto-industrial phase and passing from viability as economies radically shifted. Or, rarely, they modified the agrarian social structure sufficiently to endure into recent times as an agricultural adjunct to the modern economic complex. Nevertheless, this general model of development does seem to be broadly applicable and adaptable to most communities established in the Northeast during the first 250 years of settlement.

Focusing on the developmental model rather than simple periodization allows research questions and material importance priorities to be framed more specifically. Rejecting a unilinear time trajectory allows us to recognize the interplay of multi-dimensional mitigating factors. In this way differing stages in the developmental model held coevally by separate but partially interacting communities can be appropriately understood. In another instance, the material residue of an agrarian community and that of a neighboring mill village coexisting in the early nineteenth century would be compared using a different set of criteria than would be employed in the comparison of that same agrarian community with one elsewhere that held a similar stage of community organization to its own a century earlier. Both of these dimensions need factoring out within the same geographic region. One recognizes chronological significances horizontally, the other vertically. Both see the community as the central focus for assessing significance, and move in concentric rings of consequence in determining the degree of importance of a particular component such as a farmstead, mill dam, scove kiln or tenement.

Similar features must be compared at a variety of levels, therefore, and not simply on the basis of superficial stylizations. The essential orientation is the pattern of development of the community and the position within the system held by any functional component at any stage. Only then is it appropriate to ask for generalized, regional cultural comparisons. Some of the considerations that will vary by time, locality and developmental stage, and which need sharpening in order for us to determine significance, include: structure of economic organization, internal and external mechanisms for meeting community needs, stages of technological advance, degrees of neighborhood autonomy or self-sufficiency, feedback from areas more advanced on the developmental line, respective time periods spent by the unit in question at each stage of the developmental model, resource availability and changes therein, and the number of similar entities extant within the locality. This list is uneven and no claim for completeness is intended. It is offered to illustrate the types of considerations that need to be made in assessing significance. They are probably of greater utility, however, than those of uniqueness, prominence and style normally applied from a site-specific, architecture-dictated perspective.

It will be recognized immediately that the position taken in this paper is idealistic. But it intends to suggest a bridge to the practical. Research and preservation goals correlate better in the ideal than in practice it seems. It is easier to formulate a strategy and marshal support for the preservation of that which is obvious and well-known. Research, on the other hand, is most needed in areas that are less evident and less tangible. Management of historic cultural resources involves both. Passing irretrievably and without notice are such historical resources as those that inform about spatial orientation, landscape alteration, biotic change, everyday lifeways. A systematic attempt to canvass all organizations, academic institutions and persons who may have some of this information tucked away in esoteric files would be a step in the right direction. Major public education about those ephemeral aspects of our heritage would be another. By turning our research and preservation attentions to rural areas--those that have remained agricultural, communities bypassed by major development thrusts, uplands overlooked in the historical and economic fascination with the industrial centers to which they were oriented--we may yet have time to correct the imbalance.

The priorities will remain, and should remain, local. Not only is the historical community the essential unit to which to address our research questions, but the existing political community is the one whose interests are most to the fore and most usually in conflict. Legitimate conflicts of legitimate rights will not be evaporated by even the most enlightened program of historical and archaeological investigation and management. But without such a plan, many resources will be lost by default. And much of what is most important may even pass unrecognized. Switching priorities from the spectacular to the typical will make it possible to identify areas of significance not presently considered and to have an appropriately broad base from which to evaluate even the prominent ones. The criterion of uniqueness itself may be given a new dimension once we learn what is typical, because the "typical" is rapidly being rendered unique by the lack of concern for preserving the vernacular or the system of which a structure was a part. Only by such broadened recognition can responsible decisions be made regarding what is most in need of investigation; what bears the most information potential; when to preserve, even to restore or to reconstruct; when to inventory, document and allow to pass from the scene; and when, justifiably, to ignore.