Toward the Holistic Investigation of the Connecticut River Valley

TOWARD THE HOLISTIC INVESTIGATION OF THE CONNECTICUT RIVER VALLEY

John Worrell

Historic Archaeologist
Old Sturbridge Village
Sturbridge, Massachusetts 01566

These comments should be recognized as being of the oral genre. That's what "discussant" implies. But, as Boyle's Law states, gas expands to fill the space available. Discussants constantly verify Boyle's Law. So the writing is done for purposes of control. But the genre remains oral - and the notes remain just notes. It is my intent, therefore, that this will further progressive discussion. It makes no pretension of being a concluding word on any part of the valuable conversation initiated above.

It is a delight to be asked to comment on studies about which one is not expected to be an expert! It takes the pressure off. I have deferred to Frank McManamon for the authoritative word on pre-historic researches, and to demographers, ethographers, ecologists, statisticians, etc., for matters in their respective specializations. But, it is at that point that the buck-passing stops, and the buck itself turns back on me with a vengeance. To do historical archaeology demands a comprehension of each of these specialized disciplines and of their methodologies. It is imperative that the acquaintance be suffient for me to recognize the perils and potentials they bring to any research I may be doing. And most importantly, I must implement a research design having sufficient comprehension and integrity to involve their diversity responsibly, from inception through interpretation. Although hackneyed by dilettantish misapplication, the appropriate term for the investigative methodology to which I refer is "holistic."

Archaeology long has suffered from sins of omission: the failure to integrate - or even to employ - a variety of disciplinary resources which can best inform its problems. Shallow or unilinear research objectives have resulted, forcing limited and narrow strategies and techniques. Such, of course, could only produce conclusions which were partial, in every sense of that word. Recently, however, as archaeology "has come of age" and become more responsible in the breadth and control of its strategy, we have also witnessed an unfortunate increase in the incidence of sins of commission. By this, I mean the too-ready application of diverse disciplinary tactics before their respective methodologies were mastered and their limitations were recognized. We have too often caught a vision of the potential for our research that resides in a perspective or a technique which has been honed in another disciplinary context, only to carry it into battle and emerge claiming a conquest, whereas those who know it empirically see only our impropriety. To our good fortune, nevertheless, responsible holistic research designs are being employed with increasing propriety and with better controlled results.

To iron the biases out of an interpretation requires that its testing come from as many independent avenues as possible. To shape the independent investigations into a single cogent set of hypotheses, and to move on to the intelligent refinement of the probabilities, it is a prodigious task of integration. But it is the only responsible procedure.

This symposium, with its diversity of studies, presents a graphic demonstration of the spectrum of research components upon which broader

investigations as well as those having different problem-foci can draw. As such, it is programmatic. The papers display both the assets and the liabilities - the potentials and the risks - involved in problem-oriented research. The Connecticut River Valley Research Project may well be one of those enterprises whose totality far exceeds the sum of its parts. (I can say this objectively because I am not now nor have I ever been a card-carrying member of the University of Massachusetts nor of this project!)

Perhaps I may be able to clarify the value of multiple perspectives by a simplistic illustration derived from a situation related to one of the studies in this symposium. It involves two inventories for the Asa Knight Store from Dummerston, Vermont. The establishment, its social and economic contexts, physical situation and material culture were researched by a team from Old Sturbridge Village. Suzanne Spencer-Wood did the archaeological investigation of the site. As her paper demonstrates, an integration of research methods and perspectives can elucidate and provide control for considering general problems beyond those usually perceived. While that research project has furthered several areas of historical knowledge as carried forward by Spencer-Wood and the Sturbridge research team, it provided unintentional instruction in method as well. The after-the-fact coordination of some of the independent researches made integration a great deal more difficult. To fail to involve the various methodologies at the problem-formulation stage is to program-in frustration. So that particular project taught a series of lessons on how to - and how not to - instigate comprehensive research designs. Sometimes more is learned by trial and <u>error</u> than by trial and success. But that's another story, and not the <u>simple</u> illustration I intended.

Back to the two available store inventories: the first was done by Knight himself. He came out of his office, began with ceramics which was the commodity closest at hand, and moved by sequential category around his entire store. The second was done in probate at Knight's death. The parties doing the inventory entered the front door, began with the first item there (textiles), and moved on around the store. It is informative to note what they respectively saw and how they saw it and described it. Analysis of either inventory provides a sort of stop-action survey of the contents of that particular rural store. But by far the more exciting understanding derives from the comparison of the two. The combined perspectives, properly analyzed, inform us about respective responsibilities of the inventoriers. The divergence in their concerns, priorities and cognitive associations is something that can come only from approaching the hard data with that comparative recognition. Patterns in the organization of the materials present provide clues to what was important for the early 19th century establishment and the community it served. But the perceptions of those artifacts <u>and</u> their organization as presented to us by the multiple observers tell the most about intangibles and process.

I see in this a useful parable. Like all parables, its utility is in the eye of the beholder. Participants in this symposium have come out of different doors appearing in the Connecticut Valley at different

times, inspecting different factors often from different methodological perspectives. It is informative to compare what was seen and not seen by the respective observers and their methodologies. The symposium itself furnished the comparative base for determining what approaches may be most productive in application to our own specific problems, while the individual studies supply superb cases in point.

However, it is in the blending of the various research methodologies, and in the cumulation of the information that the fullest potential resides. Therein lies a quite different kind of value for plotting the trajectories coursed by complex interactions of social and ecological phenomena. The dynamic nature of that which we inspect makes multi-lineal research mandatory. And the diversity of well formulated approaches provides the appropriate check and balance system. It is not just that each can contribute quantitatively to the better rounded picture. Rather, my chief concern is with their reciprocity - and the holistic potential provided is qualitatively different from that of any single perspective. Control is further provided as one methodology informs another of what is not a tenable hypothesis on grounds other than those immediately available to a linear research design.

The primary potential for integration in these varied researches lies more in intent than design. They display an open recognition of the value of others' methods. It is of utmost urgency that the specific dimensions of any problem must intrinsically determine what avenues and resources are employed toward its denouement. No single method nor model can be pre-supposed. Likewise, just because a methodology has demonstrated its validity in a specified sector does not license its applicability to other kinds of data without sufficient testing. For example, excavation archaeology could not have produced the type of information needed by Richard Meindl, Helena Temkin-Greener or Alan McArdle. At least it would have been exceedingly cumbersome to get even limited information and to test it from that source. On the other hand, the most direct route to the vital information sought by Thorbahn and Mrozowski and by Spencer-Wood was through examination of archaeologically-derived materials.

There are two edges to this sword: first, the independence of respective methodologies must be maintained. Otherwise the research designs will be ill-fit. We've suffered through too many borrowed models which force data to squeeze into a picture over which there is no adequate control. It is frustrating to get answers for which there is no fit in our questions, but the supreme embarrassment is in failing to recognize that they don't fit! The second side is how to integrate the independent researches into a single, authentic multi-dimensional picture - perhaps "drama" would be a more apt metaphor than "picture," since it is process (change, motion) that we are attempting to catch. Unsynthesized data has no real interpretive value until it is fitted into a pattern which has both adaptability and potential for verification. This is as true of a model processed to sterility by a computer as it is of a simple descriptive typology of ceramics.

Factors controlling variability, deviation or idiosyncracy in the material and in the historical record are far more complicated than our standard patterning procedures can tolerate or control. I sometimes get the chilling feeling that we are caught in the same sort of myopia that aroused the "New Archaeology" movement. We've just dressed up our shortcomings with respectable jargon and moved them into the high-rent district. While far more sophisticated, our assumptions are often scarcely less conclusive in explaining function, chronology and the dynamics of change than were those employed by Sir Flinders Petrie at the end of the last century when such explanations were derived from gross observations on architecture, ceramics and thick bands of ash!

End of sermon. Altar call. What do we do about it? Primarily, we take what we think we are doing right and well, and we devise procedures for reciprocating with what others are doing well. We look to other researches being carried out in similar situations (other sites having comparable research questions), and also in those methodologically dissimilar but with common spatial, temporal or problem concerns (e.g., demographers, archaeologists, natural scientists, ethnographers, social historians, ecologists each working their own sets of hypotheses through on a centralized region - which is what we have in this symposium). The liason thus effected does more than provide new avenues to information. Even more importantly, the newly invoked perspectives provide entire additional sets of checks on our own inferences. This adds immeasurably to control and to reliability in our conclusions.

Now, I turn to brief responses to individual papers. I'll pass over all the pre-historic papers given in the morning session at this juncture. Frank McManamon has provided incisive critique for them. But I do want to point out that holistic investigation called for by my preceding comments would be severely disadvantaged without thorough attention to the problems exemplified by those studies. Our conventions of segregation - even the sacred ones like pre-historic and historic - are concessions to convenience for our limited human minds. They bear no more necessary correspondence to natural law and to what is out there to be discovered than do the arbitrary catalogues of academic disciplines. No event, natural or social, ever occurred exclusively in an academic discipline.

For purposes of informing my own work, these studies fall into three categories: direct analogues are found in the two papers in historical archaeology. Methodological issues are informed by all of them. The demographic papers provide useful background, as do both historical and prehistorical ecological studies. I see all of these studies in both sessions, however, as having their greatest value in being programmatic for further testing, refinement, and - most of all - integration.

Alan McArdle zeroes in on a specific community and the factors determining its growth and movement. His problem is framed so as to capitalize on the data available from a controlled information source. Karl Finison's paper is even more narrowly specified to a single farm and a specific factor - energy expenditure. Both demonstrate the

precarious nature of such research in two ways: first, the introduction of a small variation in a key factor - not to mention the introduction of an unprocessed variable - would alter the quantifications, hence the conclusions, exponentially. For example, in Finison's case, a slight variation in technique, tool design or individual dexterity of those inspected could have dramatic inferential consequences. McArdle's data is likewise heavily dependent on assumptions standardized by topographic constraints and the cultural priorities of a specific group. But each is cautiously aware that such is the name of his game. I am only reiterating the necessity for scrupulous caution in constructing a comprehensive research strategy. The second concern is that these two studies are "site-specific" in a sense. In demography no less than in archaeology this introduces a question of propriety. How far may we generalize from their well-developed conclusions? That caveat noted, nevertheless, the big point is that each has gone into the battle, securely armed with a methodology that fits the task, and has reached some impressive probabilities. That's what I mean by programmatic. We are presented here with sound bases for application elsewhere - in other contexts, having some different variables. These can mutually instruct and be tested by other approaches to the cultural milieu in which they reside. The problems themselves have extensive ramifications beyond the unique situation researched.

A similar case presents itself in the paper by Rich Meindl and Helena Temkin-Greener. It is an impressively appropriate application of a method to a problem. I'm always impressed by thoughtfully controlled presentations on topics about which I'm not competent to comment. Still, I'd like to have a bit more information on specific categories within the gross cohorts and age groups they examine. The influence of various status components and the interaction of forces occasioned by the dissolution of extended families raise processual questions. Then, I wonder about the typicality of the sub-class sampled, maternal health factors, the possibly malignant reciprocity occasioned by extreme closeness of persons of great age variance, etc. But, again, the point is that one carefully articulated problem has been thoroughly researched according to circumscribed technical controls. At least, we can now see with much greater specification where to turn for refinement, corroboration and elaboration of the broadened question and the context into which it fits. Those are, indeed, questions of change through time. A unilinear tracement of change, however, provides only a baseline or trajectory from which to work on the interactive systems which ideally hold the potential to tell the complete story. What it calls for is the isolation of other possible variables and the definition of further methodological perspectives to isolate and test the conceivable weaknesses of approach or conclusion. The integrity of the study is illustrated by the fact that Meindl and Temkin-Greener pointed us to most of those gaps.

The other two historical papers really fire me up to say a <u>lot</u> more than I should - and, fortunately for you, - than I have time to.

I am especially appreciative of the approach taken by Thorbahn and Mrozowski, because of their effective handling of multilineal resources.

Our Industrial Village Project at Old Sturbridge Village has led us through the myriad pitfalls and provisions involved in synergistic methodology. Their study takes a large, intrepid step into the valley of the unknown along those lines. Only a systemic perspective can lead us beyond simplistic categories in examining a pervasive and durative set of phenomena such as those usually casually cast as the "Industrial Revolution." I wince at stereotypes, whether epochal, causal or methodological. But it cannot be overlooked that there was a difference, in degree and kind, accompanying the early nineteenth century onslaught on technology. It was attended by an integrated mind-set shift that was both cause and effect of gross alterations in the socio-economic fabric of that period. Some of the sociological shifts of the "industrial revolution" outstrip the technological ones. Some of those elements in transit are quantifiable. Some are only indirectly quantum-reflective, and there is an uneven time lag in the changes reflected by different cultural indicators.

A simple model of the sort that may work well in predicting change through time in general, suffers here from a gear-shift (quantum jump) that is irregularly felt but is the nub of the change. Not only do the hierarchies of a weighted system of variables shift, but new ones are rapidly introduced in such a situation. This may be seen variantly by community, by site-type, by individual cultural component studied or in the eccentricity of a single family. Some are more conservative in certain factors than in others. (Never underestimate the power of a stubborn New England Yankee to autocratically defy your model!) I feel obliged at this point to insert a warning against expecting any given site - especially any rural domestic site - to conform to a predictive model. If it does, so much the better. But rural Yankee individualism is a factor to be reckoned with. And it is a variable for which we have yet to devise a suitable model. I believe this caution to be worth extending to all site investigations. Generalized models cannot be "proven" from specific sites, certainly not from those of the early 19th century. This further warns us to beware of arbitrary categorizations such as "economic vs. ecological". Ecosystemic analysis includes economy as a cluster of reciprocating factors. The study by Thorbahn and Mrozowski recognizes this much better than some of its binary rhetoric suggests. While it talks of "ecological" versus "economic" causal factors, it is clear that no such narrow tack is taken by the study. The recognition of multiplex factors of change is implicit throughout. I think that despite the recognition that one, usual unilinear causative interpretation doesn't work, the paper in some places suffers by appearing to substitute another in its stead.

I would likewise have felt more secure seeing the results of further research on the respective productivity of areas abandoned by period. Probably earlier-abandoned ones were never as good. There possibly lurks a telling correlation between the sequence of settlement and the sequence of abandonment. This is in no way to deny the conclusions given. It is merely a call to tighten up the argument.

I've taken time to go into these critical comments not because this is a weak study, but because it is a good one! It integrates resources and directs them to solving processual problems. Again, it is programmatic, in that it gives solid raw material having broad potential for simplification and verification. And it also flings a lightly concealed gauntlet at some less appropriate interpretive media whose application to these problems we have all too frequently witnessed.

Finally, Suzanne Spencer-Wood's paper provides me with the most intense feelings of all - and they are excitingly ambiguous. In many respects, it is the most ambitious research of the lot. That's both the good news and the bad news of it. In some respects, the problem as formulated may be too ambitious for the data available. It has some of the characteristics of those "sins of commission" that I alluded to earlier.

I don't want any criticism or question I might raise to discourage the breadth and large-problem focus the present study exhibits. This is what we should be addressing. My questions bear on how we do it, and how much we can expect how soon! Having seen this paper now through three drafts, I am most gratified to witness the degree to which Spencer-Wood is herself aware of that problem and the efforts that are continuing toward its resolution. The study therefore doubles as a testimony to methodological accountability.

To have visibility, a research design must utilize available data optimally. This is not necessarily the same as maximally. One which expects more than the data can bear is of no greater value than one which expects or inspects too little - and may be more dangerous.

Suzanne's objectives are proper and commendable. And it's truly exciting to see an archaeologist not afraid to go out and grab a live bear by the tail! It is a fine example of the sort of dynamic questions that we have frequently been reluctant to confront archaeologically. It does not suffer from traditional site-internal myopia. But I question whether the quantity and variety of information utilized is capable of sustaining it in this instance. I just wonder if a loose assortment of marked glass can divulge patterns so comprehensive. I don't doubt that it can be one resource, and an indicative one. I believe her research design to be suggestive - valuably so - so long as it makes no pretense of being conclusive. Spencer-Wood recognizes those limitations, presenting conclusions in the form of testable hypotheses derived from her evidence.

Some of the perplexities involved in broad-problem orientation are illustrated in this paper. Variability in time, as well as in other situations (e.g., ethnic or economic stratification) may not allow equal quantification of data. Lack of equal data may reduce control below levels of reliability. For example, is there any provision in the system for assessing the importance of unmarked bottles, or at least respective quantities thereof? This might be an essential data source. Probably the most telling factor in the ratio suggested is the proximity of the

of the site to available resources. If no one locally is manufacturing a given bottled commodity then the only option is non-local or none. The appearance of utilizing unequal resources produces suspect conclusions. An opened ended problem is generated by the inclusion of a different category in the data base of a single site. Table glass at Dummerston and not at others provides such an uncontrolled source.

Some further complexities over which control is not made clear include: time variability, technological innovation (i.e., that accompanying the shift from aerobic to non-aerobic storage), trade complexes, appropriateness of distance gradients used for analysis, different kinds of glass bottles, and the specific function of those used at respective sites. Canning jars and medicine bottles and table glass cannot be equally compared on the basis that they are all glass. So a system must be devised to isolate comparable aspects. A similar question resolves around the functional and geographical unevenness of sites. Can they be used to support a single model base? This highlights the peril of utilizing data from sites dug for reasons other than this problem would ideally select.

A model that assumes constant variables must demonstrate their constancy. But that almost prohibits so comprehensive a problem formulation as this study attempts. I am not encouraging a return to narrow isolation in archaeological strategy. Attacking broad cultural questions with an appropriately comprehensive research design is the responsible and challenging course we must take. I am, however, calling attention to the unevenness of data, especially that derived from investigations not so broadly conceived or controlled. Data may be objective, neutral and benign. But its acquisition and classification is not. The latter responds significantly to the questions asked in calling it forth. The use of appropriate information obligates keenest discernment.

Again, I've just done what I didn't intend to do. I've picked apart the paper, perhaps giving the illusion that I question its validity. Let me set the record straight quickly, by re-asserting that the problem that Spencer-Wood is confronting involves just the kind of vitally informative complex that we should be tackling but have too long avoided. However, I'm anticipating some of the lines of criticism that we all have to face when we attempt to do responsible, problem-directed research. From the "devil's advocate" position, I question whether the information available and utilized is adequate to sustain the impressive methodology demanded by the first part of the paper. So, the conclusions remain inconclusive. But they are highly suggestive in several respects. And, these are respects that are programmatic for further research along similar as well as different lines as Spencer-Wood correctly articulates. And that is the most imposing contribution of this paper. The problem is strongly formulated. The framework in which an integrated research must be conducted is laid out. One limited resource having high controllabiltiy, is utilized. Now we can be optimistic that it will be joined by researches into other commodities and by differing methodologies in a truly multilineal investigation.

A frequent shortcoming in any problem-oriented research is the tendency, if not the necessity, to consider a limited number of variables. The situation is exacerbated horribly by another frequent shortcoming: to think that such a limited consideration is capable of producing something absolute or conclusive. The search for "universal" laws is not absolute. If human nature/society can and does change, then any proposed law is suspect. But if change is a fact, it occurs in response to such a magnitude of causes, and with such heavy reluctance, the principles (nomothetic) can have a strong probabilistic value. Probabalism is actually our modus operandi. The goal is confirmation of the viability of a hypothesis, and the elimination of those that are not viable. The more independent research tools that we master, the more rapid and secure this "weeding-out" process can be. The principal asset to historical - as compared with prehistorical - archaeology is the fuller set of resources for isolating and demonstrating our uncertainties. And that is our first and most essential research task in problem-solving.

Archaeology, as the other disciplines exemplified in this symposium, has a strong experimental line to it. That is a source of self-correction, of potential for integration, - of integrity in both usual senses of that term. Even the most imposing conclusions, from the best controlled studies, producing the most convincing presentations, should be properly viewed as experimental. They beg to be tested in other contexts, by other proven criteria. Any useful understanding of our past-present continuum is cumulative. The more perspectives that can be brought to bear on any facet, the higher the probability of correct conclusions, and the more confidence we can have in our own interpretation and in its predictive capacity.