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PREHISTORY, MODELS AND ECOLOGICAL ANTHROPOLOGY
IN THE MIDDLE CONNECTICUT RIVER VALLEY

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This commentary benefitted from the review of drafts of the papers. I thank the authors and organizer for this courtesy. As my comments indicate, I found the papers provocative and full of merit. I hope the authors and readers will accept my comments as an attempt at constructive criticism.

Curran's paper reports on her field work to collect data in order to test a "predictor model" of cultural change. She argues that four relatively rapid environmental changes occurred during the late glacial and early post-glacial time period. She hypothesizes that these changes caused four different episodes of behavioral change in the "adaptive strategy" of human populations.

Curran has accepted several formidable, but interesting, problems at a series of levels. The environmental reconstruction for which she argues and its chronology are a problem on the road to solution (Curran and Dincauze 1977).

Her insistence in defining the biomass, by which she seems to mean the capacity of an environment for supporting a human population, is a problem less likely to be solved. Hayden (1975), among others, has criticized the use of this concept for human adaptive systems. Other anthropologists also have argued that explanations of human adaptations based upon carrying capacity, or even energy flows, can be simplistic and misleading (Vayda and McCay 1975:246; Ammerman 1975:26).

Another problem she has chosen is the reconstruction of the human adaptive systems during the different environmental periods. There are two aspects of this problem. First is the isolation and dating of distinct behavioral events represented within the site. Curran's program of computer mapping, soil analysis and carbon dating will, I hope, result in isolation and dating of these episodes. The more intractable, and perhaps unsolvable, problem will be the reconstruction of an adaptive system of any period from archeological remains limited to a 9 x 6 m. area. Systems are "... regularly interacting and interdependent components forming a unified whole" (Odum 1971:4). In this case, if one or more of the kinds of behavior which were parts of the adaptive system are not represented within the excavated area, reconstruction will not be possible. If important behavior occurred only at other sites within the settlement patterns of the site's former occupants, they will not be represented by the record within the excavated area. Even if all the relevant behaviors did occur within the site, they might not be represented within the excavated portion of it. Finally, if the excavated area does contain archeological data representing all the relevant behaviors, it will be a real mess to interpret. All kinds of behavior, including ritual and ceremony, can be part of an adaptive system (Rappaport 1968, 1971; Little and Morren 1976; Hardesty 1975:23-31). Archaeologists have dealt mostly with the reconstruction of technology and resource procurement behavior. Few examples of the reconstruction of other kinds of behavior from archeological remains exist. However, it is encouraging that Curran recognizes the potential adaptive importance
of behavior not directly involved with resource procurement. Her dis-
cussion of the reproductive requirements of populations (Wobst 1974)
and the effect of these upon social relations among human groups indi-
cates this awareness.

Once the dating, isolating, reconstruction and correlation is com-
pleted, Curran has the task of explaining the relationship between human
adaptive system(s) and the environmental shifts. Her comments about the
importance of biomass and a correlation between environmental and behav-
ioral shifts imply an explanation grounded in environmental possibilism.
I hope that as lower level problems are solved, other hypotheses are
developed to explain observed behavioral variation. These hypotheses
can then be tested and the one which affirms its test implications best
selected as the presently most suitable explanation.

In his paper, Mulholland develops a model designed to explain some
of the prehistoric behavioral changes which, it is implied, occurred
during the Archaic time period. Increases in population size, fluctu-
ations in diet breadth, expanding settlement patterns and the development
of territoriality are mentioned. The model explains these developments
as human behavioral responses to changes in the natural environmental
"resource potential" because of forest succession development during the
Archaic time period.

The model presents a scenario of behavioral variation and offers an
explanation of the variation:

1. The initial setting, following deglaciation, is southern
New England where critical subsistence resources, defined
as vegetable foods in this model, are numerous and diverse
but nonclustered. Human groups are dispersed widely to
exploit this resource base.

2. As forest succession developed, some subsistence resources
increased while others decreased. This increased the
density and predictability of some resources. Human groups
responded by clustering more and increasing in size. How-
ever, the loss of some resources also occurred and reduced
the variation in subsistence patterns.

3. Their reliance upon a limited number of subsistence resources
caused the human groups to be increasingly concerned about the
availability of the resources, thus leading to the development
of territoriality.

4. Further population increases occurred because of the need
to defend the territory and the continuing increase in
resource density.

5. Increased population finally caused shortages of resources
and created a need for "niche expansion," that is, the
exploitation of new resources.
6. Niche expansion led to the change in settlement pattern to exploit new resources.

This is an interesting general model of the evolution of human adaptive systems in a changing forest environment. This model includes a reciprocal relationship between environment and culture since it allows the human groups to modify their environment and not simply respond to it. It also considers human behavior not directly involved in resource extraction, for example, territoriality, as part of the adaptive system.

The model also has some troublesome characteristics and assumptions. It is unilinear, that is, a range of behavioral responses to some of the crises caused by environmental change and past adaptations are not considered. But different responses might have been possible, for example, a group faced with an increased reliance upon an abundant, but more limited number of resources has options other than territoriality, such as trading networks, to secure their resources. Also, human groups need not automatically increase their size because a subsistence resource increases (Cowgill 1975).

The assumption that decreasing resource diversity leads to increasing subsistence efficiency, and its corollary that subsistence efficiency is reduced when a wide range of resources are exploited, might not be true under some conditions of scheduling, resource abundance and resource availability. For example, through division of labor a group might exploit fish, mammal and plant food sources in a small area where all these resources concentrate with greater efficiency than the exploitation of a single resource by everyone. Also, the universal appropriateness of subsistence efficiency as an implied goal of all human subsistence systems is questionable. Characteristics of one or more resources might make them more desirable than others and worth the inefficiency necessary to procure them.

The selection of plant resources as the critical subsistence resource is probably a mistake for the Northeast. This is not to say that plant foods were unimportant, only that other resources - notably fish and large terrestrial mammals - were also important subsistence resources. Densities of vegetable, fish and mammal resources do not necessarily correlate positively with each other. For example, if a climax forest is the maximum density for plant resources, it is negatively correlated with white tailed deer abundance which is greatest in open forest.

The choice of existing pollen diagrams to designate plant resource density and abundance also might be misleading. Pollen diagrams usually do not distinguish species useful for human subsistence from others. They do not contain information on very many of the plants used for subsistence by human groups in the Northeast.

Mulholland notes that the test of his model is quite preliminary and I agree. The archeological data used to test the model is riddled with unknown bias. Very little sound information about site distributions in
the Northeast exists for any prehistoric period. The term "site" is often used to describe all kinds of archeological manifestations from isolated finds to multicomponent sites. A tremendous leap of faith and imagination is required to equate all of these "site" phenomena.

In their paper, Moore and Root investigate the constraints upon subsistence behavior and settlement patterns in prehistoric eastern North America. They note that anadromous fish have been mentioned as a subsistence resource during the Archaic and Woodland time periods, but that they have not often been considered a critical or dominant resource. Their paper proposes that anadromous fish were an abundant and predictable major subsistence resource of prehistoric groups in the middle Connecticut River Valley.

Spatial variations in the availability of the resource are focused upon. Moore and Root conclude that stream basins with relatively high ranks according to the Strahler method provide a more consistently productive spawning ground for anadromous fish than lower rank basins. The consistency of the spawning grounds of the higher rank basins is based upon a mathematical model which assumes that numerous local environmental fluctuations will cause the productivity of spawning beds in low rank basins to vary widely from year to year. This would cause the number of fish returning to the basin spawning beds to vary and would make the resource less predictive. Higher rank basins, which incorporate a number of the lower rank basins, should not be subject to these fluctuations since local environmental variations within the basin would cancel each other out. An independent, nonmathematical analysis of the structure of an anadromous fish resource (Schalk 1977:7-19), incorporating much information from fisheries management research, supports this conclusion. The additional benefit of Moore and Root's model is the possibility of quantifying the resource potential of drainage basins. This will allow more exact comparisons between basins or larger spatial units regarding their potential for anadromous fish.

Moore and Root conclude that the higher rank stream segments would have provided the best locations from which to catch fish since a reliable number of fish would pass through them on their journey to various spawning grounds in lower rank streams. This, they say, probably presented some technological and organizational problems for the exploitation of the wide and deep higher rank stream segments. They suggest that the utilization of natural narrow points in the stream channel, such as falls and rapids, would have provided a solution to this problem. However, the procurement of the resource need not have been limited to these kinds of locations. Sixteenth and seventeenth century observers report an apparently successful exploitation of anadromous fish by the Montagnais, a Native American group living in the lower St. Lawrence River Valley and its surroundings (McMenamon 1975:58-9). In the St. Lawrence itself, the Montagnais were able to take large numbers of fish. The fish were speared from boats or, in shallow water, taken in nets and weirs and caught in tidal traps (McManamon 1975:42-3). Stone fish weirs, possibly prehistoric, discovered in the Potomac River indicate
that the artificial constriction of a wide waterway is possible without high technology (Strandberg and Tomlinson 1964).

Moore and Root next test their model using the distribution of known and reported prehistoric sites from Franklin County, Massachusetts. They compare the distribution of known sites with a series of hypothetical distributions. One of the hypothetical distributions is predicted by the pattern strength of anadromous fish resource per stream basin. The known and reported site distribution comes closest to corresponding with this distribution. However, according to their chi-square test, none of the hypothesized distributions correspond very closely to the known site distribution. On the other hand, a correlation coefficient of .595 was obtained for the drainage basin weightings and distribution of known sites. This is not an extremely strong score, but indicates some correlation.

Moore and Root note the weaknesses of the known and reported site data which they have used to test their model. Comments similar to those already made about this data need not be repeated but here also apply.

The resource potential of anadromous fish has been overlooked in favor of large mammal and plant resources throughout the East. My own limited ethno-historic research in the Northeast indicates that anadromous and catadromous species were heavily exploited and provided the major subsistence resource during the seasons they were available (McManamon 1975). The quantification of the anadromous fish resource potential of drainage basins is an important contribution. It could provide an objective way of comparing the attractiveness of different areas to past human groups if anadromous fish really were a critical resource which dictated site location. This would also make it possible to predict some site locations accurately, thus reducing the time and money necessary for site discovery.

The model could be improved in at least two ways. The percentage of rank one streams with spawning beds and the number of spawning beds found in other rank stream segments, as well as the number of different low rank streams within high rank basins can be estimated and will increase the accuracy of the prediction of resource potential for any given basin and high rank segment. Also, the magnitude of the anadromous, and catadromous, fish resource should be documented and the spatial and temporal availability of specific species incorporated into the model.

Like Mulholland, Ulrich's brief paper also presents a general model of prehistoric human adaptation, in this case, for the Late Archaic and Early Woodland time period. This includes the most recent end of the period to which Mulholland's model applies.

The model suggests that human adaptation during the Late Archaic/Early Woodland period was not open to change. Ulrich calls the human adaptation a mature subsystem of a mature ecosystem. His intention is to investigate how and why this adaptation was changed by horticulture. Ulrich argues that instability was necessary to have caused the acceptance
of horticulture by the already mature human adaptation. Three classes of instability are mentioned: extra-systemic, inter-systemic and intra-systemic, but immigration is also a kind of population change and is better defined as inter-systemic, or extra-systemic within this classification. Ulrich should, as he suggests he will, expand considerably the kinds of specific sources of instability, and their likely archeological correlates for which he will test at the Indian Crossing site. In the present paper these sources and the correlates are not well developed, as Ulrich acknowledges. The basic assumption of an ecosystem as a real unit of nature also should be further investigated. Most ecologists seem to regard and use the term to describe a heuristic or analytical construct without assuming it is a natural unit (Colinvaux 1973).

The model of hunter-gatherer adaptation in the Northeast as a relatively static and stable pattern of behavior is common though not well tested. Investigators in ecology and ecological anthropology have begun recently to investigate the concept of maturity or stability in ecological systems (e.g., Colinvaux 1973; Ammerman 1975; Vayda and McCay 1975). Closer scrutiny has uncovered interesting fluctuations in systems, and mechanisms for coping with variations, which imply that stability, defined as no significant fluctuations, is less common than supposed. The stability of the Late Archaic/Early Woodland adaptation is an assumption which Ulrich will not be testing through his investigation. One cannot explore all related problems in a single investigation. However, I am certain Ulrich would agree that it is important for his model to understand the Late Archaic/Early Woodland human adaptation much better than we now do.

Conclusions

I shall address three general topics:

1. the assumptions of the models or hypotheses presented in these papers

2. the importance of test implications in hypothesis testing; and

3. sources of data for hypothesis testing.

Some of the proposed models or hypotheses could profit from additional support or specification of their assumptions, for example, the correlation of human behavioral change with environmental change and Mulholland's model of human behavioral response to change in resource patterning. Moore and Root could support their assumptions regarding the desirable characteristics of an anadromous fish resource by including more information about the abundance and availability of the species.

Test implications are statements about the expected characteristics of data if a stated hypothesis is true. The statements are a critical logical link between the hypothesis and the data. All of the papers
discuss test implications or the kinds of data from which implications might be drawn. One of the values of using models and hypothesis testing is the opportunity to identify the kinds of data which is needed for testing a hypothesis. This allows greater precision in data collection since only the data necessary to solve the problem at hand need be collected. Time, energy, money and archeological resources should be conserved when test implications are carefully devised prior to data collection. I recommend highly the intellectual exercises through which these authors have gone to develop their hypotheses and test implications. Not only do the papers provide exciting ideas about past human adaptations and their origins, but also they did not require any activity which destroyed the archeological record. Specifications of the kinds of archeological data needed to test the ideas are included, so precise future data collections can be undertaken. Energy expended by archeologists in these intellectual exercises will result in more and better results than energy used in projects undertaken with a minimum amount of prior hypothesis generation.

It is noteworthy that all the papers aim to explain an aspect of human behavior with a single variable, either environmental fluctuation or resource availability. This is part of the nature of models. They are simplifications of reality through which we hope to identify critical variables. We ought not to expect complete correlations between a model of human behavior and our reconstruction of actual behavior. In fact, the way in which an accurate reconstruction differs from a model is of greater use and importance. The differences point to other relevant variables and help measure the extent to which the model variable affects behavior. All of these papers could profit from such a consideration of their test results.

This brings me to my final topic, the available sources of data. I perceive three:

1. existing reconstructions of behavior,
2. extant raw data, especially collections, and
3. data still "in the bank," the archeological record.

All of these sources have some bias in them which must be understood in order to use them properly. The existing reconstructions of prehistoric human behavior in the Northeast are based upon relatively little analysis of a few sites with little information about site distributions or settlement systems beyond a single site. Therefore, when Ulrich and others perceive a mature cultural system during the Late Archaic/Early Woodland, and Mulholland territoriality in the Late Archaic, they are really hypothesizing that these phenomena existed. Test implications for and testing of these hypotheses is needed.

Extant raw data, especially known site distributions and collections, are the sources which archeologists should use next to test their hypotheses as Moore and Root have done. Bias here comes from spatial and
material sampling problems. Not all the sites are represented, nor are all the materials from those sites which are represented. Moore and Root have used the known site distribution in testing their hypothesis. A next step could be to examine collections from these sites for evidence of fishing and other activities, chronology and recurrent use, among other things. Examining the extant collections can, at best, answer the questions. At worst, it can help to specify the kinds of data necessary from new data collection.

The in-ground archeological record also is a biased record of past cultural phenomenon. Therefore, it behooves archeologists to know the kinds of data which they need to answer their questions and identify the bias in their data. Curran's multi-stage investigation with input and feedback from data already collected and Ulrich's preliminary specification of the data necessary regarding features at the Indian Crossing site enhances their ability to collect data relevant to their problem solutions.

It must be stressed that the research reflected by these papers must not cease because a model or hypothesis has been confirmed by severely biased data. To the extent that it does, the ideas presented will be less useful and have less impact or a deleterious effect upon our understanding of Northeastern prehistory and human ecology. These models and hypotheses need to be refined and tested further. Assumptions must be supported, or rejected with suitable modifications in the hypotheses. Well thought out test implications usable for investigation of extant data, and, if necessary, new data, must be generated and tested. It is my greatest hope that these comments will be useful for the authors in their continuing research and repay them in kind, if not quantity.
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