

1980

## Regional Research Design--A Necessity

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Dekin, Albert A. Jr., "Regional Research Design--A Necessity" (1980). *Research Report 19: Proceedings of the Conference on Northeastern Archaeology*. 4.

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## REGIONAL RESEARCH DESIGN--A NECESSITY

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We can understand a lot about formal modelling and formal research design by building model railroads. For instance, what sorts of problems have my son and I had in building a model railroad system?

1. Missing parts.
2. Broken parts.
3. Parts which vary too much to fit together properly.
4. Different manufacturers produce components at different scales.
5. We lack the proper tools to make the model work (both knowledge and equipment).
6. We cannot decide just what we want to do (our model design is incomplete).

In our region, we need a Research Design for all the reasons my son and I need a layout design.

At the present time, things just don't go together and our continuing efforts are building neither regional models nor regional understandings. There seem to be four potential sources of guidance available to assist us in this matter. These are the experiences of the Southwestern Archaeological Research Group (SARG), the Southeast Archaeological Conference, the relevant state historic preservation

plans, and those areas where a particular institution has dominated research to the extent that a coherent research design might be expected to exist. However, none of these sources can provide us with an operational example of a research design which works and which provides us with guidance in building regional models of subsistence/settlement systems.

Generally speaking, a research design contains the following elements.

1. Statement of Problem.
2. Statement of Research Objectives.
3. Statement of Guiding Theory, or Paradigm.
4. Statement of Relevant Data Requirements (variables, observations, measurements, categories, observation-states, etc.)
5. Statement of Hypotheses for Exploration or Evaluation.
6. Statement of Data-Structure Expectations (test implications).
7. Prescription of Appropriate Methodology (sampling, observation, etc.).
8. Prescription of Appropriate Field and Analytic Techniques.

At the intellectual level, the problem which we propose to address is finding answers to the what, where, who, when, how and why of past human behaviors, as evidenced by the tangible data preserved in archaeological deposits. The answers to these questions will result from more specific inquiries aimed broadly at examining the spatial and temporal structure of human behavior. Inquiries guided by both broad intellectual goals, and specific management and administrative goals.

At the operational level, the following objectives are equally applicable to intellectual or management goals, as they exist in the Northeast.

1. To maximize the return from our research efforts, both pure and applied, so that the resources made available from numerous sources are not wasted.
2. To create increasingly sophisticated models of human behaviors at all social, spatial and temporal scales of analysis. To increase the public accountability of archaeologists for their use of resources, both monetary and cultural.

However, these goals and objectives are relatively easy to formulate and to support in the Northeast. Where we find difficulty is at "lower" and more specific levels of operation where the application of method and

technique is required. This was seen by Jeffrey Dean as the major problem which SARG failed to face and which was the major reason why the SARG research design was not entirely effective in accomplishing its overall objectives.

A research design, especially at the regional level, must be capable of being implemented by a wide range of contributors, and this requires specification of specific methods and techniques. No regional research design has ever done this and all have failed because of it.

Recent efforts in this direction have dealt with "problem domains" which consist largely of collections of areal research problems. These are "open ended" collections which cannot reach closure, as they represent the state of the research art at a particular point in time. They have focused attention on contemporary regional intellectual and management interests. Such formulations are a necessary step in regional research design, but our biggest failure has been our unwillingness to follow through with prescriptions of the specific methods, techniques and observations which are to be made in approaching these problem domains. This is essential to insure that the data collected are relevant and applicable to these problem domains.

We have also failed to maintain appropriate scales of observation and measurement as we attempt to relate variables to test the hypotheses which form the core of research design. Regional hypotheses require regional data, acquired in a directly comparable manner, using methods and techniques which are prescribed to insure such comparability of results. Fine scale data become idiosyncratic and useless for regional hypotheses if they are not collected explicitly to fit into a larger integrated research design.

As part of an effort to determine what we know, the following is a list of some things which we do not know well enough.

1. What are the appropriate analytic units (site, episode, component, activity area, etc.) which are behaviorally meaningful and empirically justified?
2. At what spatial and temporal scales are these unknown units relevant?
3. What are optimal, reliable and compatible methods to discover, observe and categorize these analytical units?
4. What are necessary and specific field and reporting techniques to make appropriate observations on data relevant to defining these analytical units?
5. How can our field and analytic technique be improved to allow measurement at "higher scales" of measurement (e.g., from nominal to ordinal to interval to ratio)?

As an example of this problem, we can take our own research design

for highway survey at SUNY Binghamton. Initially, it was designed to use the "site" as the major analytic unit, with the following site "types:" camp, hamlet, village, workshop and burial site. However, it soon became apparent that these units of analysis were not operationally defined in a replicable manner and that data categorized in this system were not comparable. With the change from corridor survey to site investigation, we adjusted our scale of concern to those analytic units which comprised sites, viewing definable clusters of artifacts as episodes of occupation. These were, therefore, the "building blocks" of sites. Rather obviously, if the integrity of analytic units at higher levels is not based on sound analysis of empirically valid units defined at lower levels, such higher level units are meaningless for a regional research design. The definition of behaviorally-meaningful units within "sites" (e.g., episodes) is only now in a developmental stage, since comparable research is non-existent.

So where do we go from here, since we can't stay where we are. I recommend that we work together to thrash-out a North-East Research Design, the acronym for which could be NERD. This would involve a statement of problems, of research objectives, of problem domains, of data expectations, and of standard techniques of observations for building a comparable data base. Through the exchange and consolidation of comparable data, we would be capable of reaching a critical mass from which to derive useful analyses and models (in this vein, perhaps this conference and workshops could be called Critical MASS, I ). Such an effort, however, raises additional problems and potential criticisms such as those which follow.

1. An integrated research design leads to stagnation of research technique and interest and entrenchment of method and research domain.
2. The role of creativity in research design and operation is reduced or eliminated.
3. An integrated research design requires practioners to do not what they please but what others want them to do, forcing conformity on archaeologists.

A commitment to such a regional research design is an indication that regional archaeological research is a science and not an art. It is also an indication that we can no longer tolerate complete anarchy in research design and that professionalization will require standardization of results. As Dean Snow indicated, the alternatives to professionalization in archaeology are as unacceptable as the alternatives to professionalization in dentistry. There will always be a role for research and development (R&D), but not everyone will do it all the time.

As tangible steps in this direction, I would suggest the following.

1. Through a series of conferences and workshops, we should draft a set of standards for research results (note, explicitly this is a set of standards for results and not standard personnel

requirements nor standard research procedures). These should establish standards for reliability, with standard error estimates, etc. By example, when the goal of a survey is to locate all significant resources, the level of reliability of the results must be stated. To do this, we must know the parameters of the resources which we seek.

2. Through a series of meetings with regional archaeologists, state preservation planners, and federal cultural resource managers we should develop standard federal and state site and survey reporting requirements to insure the regional comparability of cultural resources data. Standard reporting requirements are common in many areas of federal involvement, such as health care, business and environmental protection, and we are remiss in not insisting on common reportage in a region as small as the Northeast. The compliance mechanism may be made operational through agencies which manage federal monies and through the federal preservation planning and funding procedure.
3. These conferences and workshops can be funded through existing planning procedures, using the contributed time and effort of archaeologists in the region to produce a "match" of federal survey and planning moneys for the respective State Historic Preservation Offices. With cooperation among the respective state offices, a "round robin" of such conferences could be initiated within the next fiscal year, and we could be well on the way towards accomplishing a meaningful and operational regional research design by 1980. If successful, these efforts could serve as pilot programs which reflect the state of the art.

As the title of this paper implies, the development of an operational regional research design for the Northeast is no longer a luxury. We cannot continue to consume extensive resources, both monetary and archaeological, without providing for their contribution to overall research goals. If we cannot establish a plan for the development of such a design and a time-table for the generation of meaningful results, then we can no longer justify our activities as being in the public interest and the hope for "public archaeology" is without foundation. If this occurs, then those of us with a public conscience may decide that the continued subsidization of non-productive archaeology is not in the best interests of historic preservation. The call is out for archaeologists in the Northeast to either put-up or shut-up, and the time for action is at hand.