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**CULTURAL RESOURCE SIGNIFICANCE EVALUATION:  
PROCEEDINGS OF A U.S. ARMY CORPS OF ENGINEERS WORKSHOP  
3-4 OCTOBER 1994  
VICKSBURG, MISSISSIPPI**

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## PREFACE

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The Waterways Experiment Station principal investigator for this work unit was Dr. Frederick L. Briuer. The Institute for Water Resources co-principal investigator was Mr. Darrell Nolton. The Workshop Proceedings were edited and compiled by Drs. Frederick L. Briuer and Clay Mathers. Mr. Gary A. Hebler, a contract student at WES, also contributed to the preparation, editing and compiling of the manuscripts.

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## EXECUTIVE SUMMARY

This publication consists of a set of papers from a Corps of Engineers sponsored workshop on the subject of significance evaluation in cultural resources management held 3-4 October, 1994 at the U.S. Army Engineer Waterways Experiment Station, Vicksburg Mississippi. The purpose of the workshop was to solicit early input and advice on how best to proceed with plans for the research project described in the lead paper of these proceedings. Each participant was invited to prepare a written version of their presentations. Before proceeding with a regional demonstration model based on actual data from a Corps of Engineers District, it was thought important to receive as much input as possible from the particular perspective of those in the field who have been grappling with the legal requirements of significance evaluation programs. Progress toward achieving the goals of the overall project is discussed in a lead paper that also summarizes and comments on each workshop paper in view of research results and input received from products that have already been distributed in hard copy and on the Internet World Wide Web.



**TOWARD A HOLISTIC  
APPROACH TO SIGNIFICANCE EVALUATION:  
INTRODUCTION AND COMMENTS ON THE WORKSHOP PAPERS**

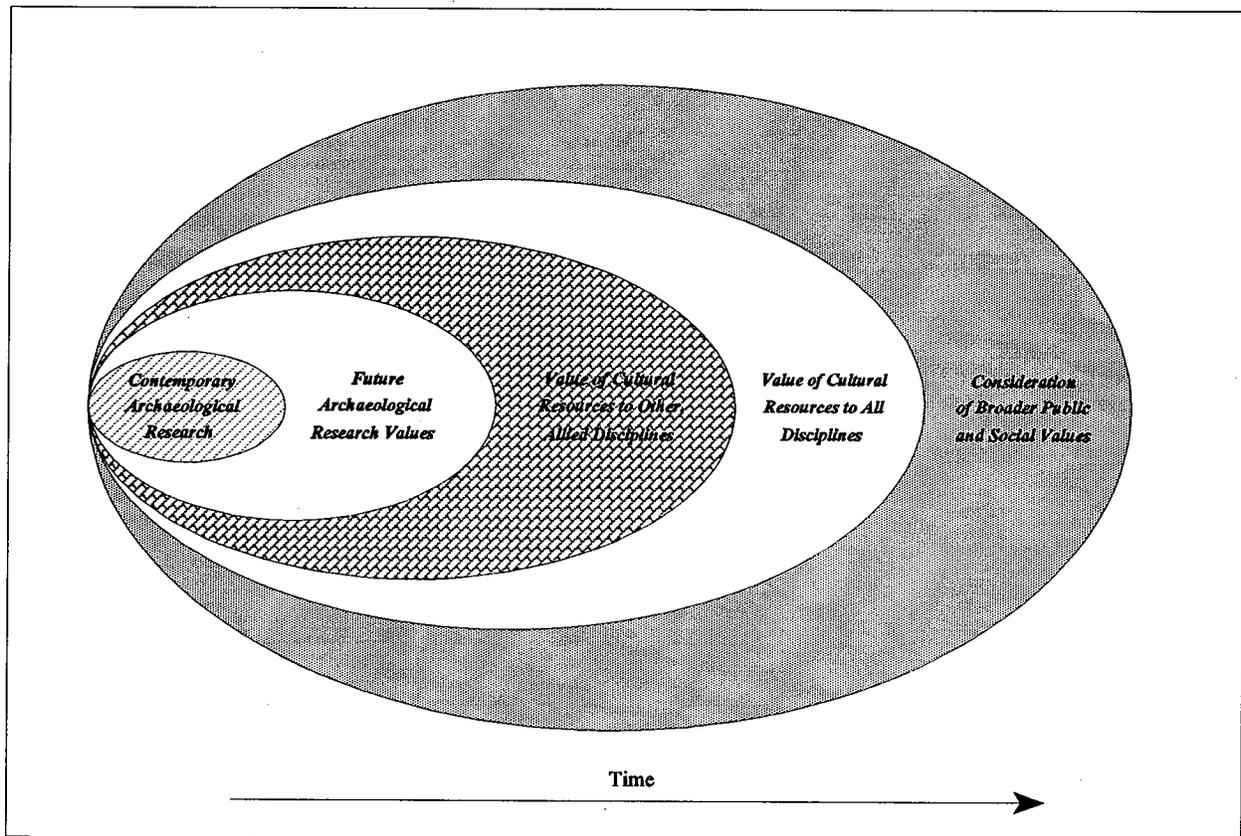
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**ABSTRACT**

This paper introduces the set of five workshop papers and briefly discusses the Evaluation of Environmental Investments Research Program (EEIRP) and the present work unit. The opening paper also includes a description of the objectives and products of the work unit, both completed and in progress. The workshop papers are introduced by summarizing some of their most important features, particularly as these relate to research results to date.

With the passage of several landmark bills in the U.S. Congress in the late 1960s and early 1970s, the concept of significance emerged as a major focus of attention within the cultural resource management (CRM) community. In the last 25 years, the idea of significance has been addressed by U.S. archaeologists in a wide range of thematic contexts and from a multitude of different perspectives. The nature of these discussions, and the theoretical/methodological issues which underlie them, have shifted considerably over time as the concept of significance has been continuously redefined and expanded (see *Figure 1*). Fueled by the emergence of new research questions, as well as the shifting social, economic and political priorities of the nation, the frames of reference for understanding cultural resource significance have grown from a rather narrow, intra-disciplinary focus, to a more expansive and inclusive one.



**Figure 1: Expanding Domains of Significance Evaluation**

On the positive side, this expanded frame of reference has brought fresh insights to cultural resource managers and archaeologists, and has encouraged more widespread use of new techniques, ideas and approaches. While the idea of holistic resource management seems both attractive and logical, the more sobering task has been to develop methods and strategies that can *operationalize* these broader definitions of significance, particularly in the day-to-day world of field investigation, planning and management. The current climate of financial constriction, long-term budgetary uncertainty and political decentralization has added a further dimension of complexity to the challenging debate about how to redefine and evaluate cultural resource significance.

The papers included in this volume represent an effort to address a range of contemporary issues surrounding the definition and evaluation of significance, particularly as they affect cultural resource managers in the U.S. Army Corps of Engineers. These workshop proceedings form part of a larger research effort that will be briefly described below along with a brief introduction of the workshop papers with comments on key points of articulation with the goals and products of the overall research project.

## RESEARCH PROJECT BACKGROUND

The Evaluation of Environmental Investments Research Program. Publication of the proceedings from this workshop on cultural resource significance evaluation was made possible with funding from the *Evaluation of Environmental Investments Research Program*. This direct allotted US Army Corps of Engineers research program is jointly managed by the Institute for Water Resources (IWR), Fort Belvoir, Virginia and the U.S. Army Engineer Waterways Experiment Station, Vicksburg, MS. The research program consists of a number of interdisciplinary work units concerned with the protection and restoration of environmental (both cultural and natural) resources. The EEIRP program recognizes the current lack of accepted methods for assessing the efficiency and effectiveness of strategies to protect and restore environmental resources. Consequently, methodologies are required to help identify the most desirable management option, or combination of options, for accomplishing required environmental objectives. Furthermore, techniques are needed that will help allocate limited resources in a prioritized fashion in order to accomplish a wide variety of environmental objectives.

As an interdisciplinary research program, the EEIRP has recognized that cultural resources are an integral part of the total population of resources that must be managed by the Corps of Engineers. Grappling with the significance of cultural resources is in many ways similar and no less demanding than addressing the significance of other (natural) resources. The inclusion of a work unit in the EEIRP specifically dealing with cultural resource significance reflects the growing awareness of the need to develop cost effective and integrated strategies for more holistic management. By approaching resource management in this broader and more inclusive fashion, it will be possible to design projects that provide a single, more integrated investment (e.g., increasing efficiency by sharing both the burdens of cost, as well as transferable research results, among all interested parties).

## OBJECTIVE EVALUATION OF CULTURAL RESOURCES

Problem: The concept of National Register significance applied to cultural resources set forth in public law is very broad and purposely comprehensive. The legal definition of significance, particularly with respect to National Register criteria, has resulted in a prevalent practice of using very subjective, often idiosyncratic rationale for evaluating significance on a site-by-site basis. More objective and resourceful procedures are needed as alternatives to conventional, piecemeal evaluations. Procedures that allow an evaluator to consider any particular site in the broader, more holistic context of local, regional and national populations of sites should be encouraged. Until more objective approaches are developed, there is little assurance that we are making the wisest use of our limited financial resources to comply with our long range cultural resource management and stewardship responsibilities. Developing significance evaluation strategies that exploit new methods, emerging technologies and high quality, reliable information not readily available in the past, presents an opportunity to see the forests and not just the trees when making the critical and difficult decisions about the future management of rapidly dwindling cultural resources.

**Objectives:** The principle aims of this research are: (1) conduct a review and evaluation of the literature on, and practice of, cultural resource evaluation, to be performed in conjunction with a workshop of interested Corps archaeologists (the latter was designed to solicit practical input from evaluation practitioners prior to developing a demonstration strategy), (2) develop a pilot procedure for employing a more explicit and holistic approach to cultural resource evaluation, (3) field test the model at a select demonstration site using an appropriate suite of research and information management tools and, (4) distribute results of the demonstration, database, and objective evaluation procedures to the field in the form of a technical report with procedural guidance. It needs to be made very clear that the objectives of this research are meant to complement existing Federal guidance regarding significance evaluation. The goal is to encourage new and innovative management strategies regarding significance evaluation that are entirely compatible and in compliance with all current Federal laws and regulations. This is not to say that existing federal guidance regarding significance evaluation represents the last words on the subject of exactly how federal managers should proceed with their responsibilities. This should always be considered a subject of ongoing research and development.

**Benefits:** It is anticipated that the most valuable product of this work will be a set of formal and explicit methods for evaluating cultural resources that can serve as recommendations for consideration, adoption, revision or rejection depending upon the particular needs and circumstances that each manager confronts in the field. Assessment categories that minimize subjectivity and unnecessary duplication have enormous potential for financial savings and for reducing the number of marginally important and redundant cultural resources currently requiring substantial financial investment. Explicit evaluation criteria have the advantage of providing a common frame of reference with which to compare phenomena over a wide area. Adopting a broader *population* approach to site evaluation will better assure that limited funding sources are used to protect, preserve and mitigate adverse effects to the most representative sample of cultural resources. Furthermore, the use of specific, well-defined assessment criteria ensures the results of evaluations are easily understood, replicable and supported by clear, defensible arguments. An implicit assumption in this research is that a concerted effort is needed for greater professional accountability in making significance evaluations.

## **THE WORKSHOP**

A two-day workshop was held at the US Army Engineer Waterways Experiment Station Vicksburg Mississippi on 3 and 4 October 1994. The purpose of the meeting was to bring together a panel of cultural resource managers and other closely related specialists to describe and openly discuss the EEIRP work unit plans dealing with the objective evaluation of cultural resource significance. The workshop idea grew out of an earlier meeting of Corps of Engineers archaeologists who had met informally at the 1994 Society for American Archaeology meeting to discuss the research project.

The workshop schedule and agenda including a list of attendees are included in appendix A. Attendees giving presentations at the workshop included Dr. Frederick L. Briuer, Director for the Center For Cultural Site Preservation Technology at the Waterways Experiment Station. Dr. Briuer along with Mr. Darrell Nolton (IWR) are co-principal investigators for the work unit on the objective evaluation of cultural resources. Dr. Briuer also served as workshop organizer and moderator. Other attendees giving presentations were several archaeologists from the US Army Corps of Engineers including Dr. John Schelberg, Albuquerque District; Ms. Carroll Kleinhans, North Central Division; Mr. Robert (Skipper) Scott, Forth Worth District; and Dr. Robert Maslowski, Huntington District. Mr. Horace Foxhall, historic architect and manager for the Center for Historic Architectural Expertise, Seattle District, gave an excellent oral presentation on problems of architectural significance evaluation, but was unable to provide a manuscript for inclusion in the Workshop Proceedings.

Participants giving presentations from outside the Corps of Engineers included Dr. W. Fredrick Limp, Director of the Center For Advanced Spatial Technology, University of Arkansas, Fayetteville and Mr. Evan Peacock archaeologist with the US Forest Service, Tombigbee Ranger District. Dr. Limp's presentation focused on recommended strategies for evaluation that were based on investments in comprehensive and rigorous regional data sets that lend themselves particularly well to the use of new and emerging technologies especially geographic information systems as efficient analytical tools for cultural resource managers. Unfortunately Dr. Limp was also unable to provide the editors a manuscript of his presentation.

Contributions to the significance workshop were meant to complement a parallel research effort which is now completed. This research was devoted to an historical review and annotated bibliography of the significance concept in the peer reviewed professional literature (Briuer and Mathers 1996). This report, in hard copy is currently available, upon request, for as long as supplies last. Alternatively, Internet users, can download an FTP file version of "Trends and Patterns in Cultural Resource Significance: An Historical Perspective and Annotated Bibliography" through the World Wide Web Home Page for the Center For Cultural Site Preservation Technology (URL <http://www.wes.army.mil/el/ccspt.html>). Having undertaken an historical analysis of the definitions, ideas and methods used by archaeologists and cultural resource managers to address significance in the past two and one-half decades, it was clearly important to move on to assess more contemporary approaches to this issue. Apart from hosting the significance workshop and attending other relevant conferences and symposia, other attempts to monitor current ideas and *practice* consist of:

- (a) disseminating results and pursuing information/feedback via the Internet/WWW, *and*
- (b) conducting a sampling survey of a stratified random sample of Corps of Engineers projects throughout the nation that would describe the practice of significance evaluation based primarily on information from unpublished sources generally not represented in the peer reviewed literature.

The final products remaining from this research will include reports describing the results of the regional model of significance evaluation for a large region of Central New Mexico characterized by a high density of diverse cultural resources and the availability of interdisciplinary information complementary to a wealth of archaeological information available in an automated format through the New Mexico site files system. In addition, the final product from this research project will include a description of the practice of significance evaluation based on a survey of a representative sample of Corps of Engineer projects.

The following discussion of workshop papers briefly describes the salient features discussed by each author. Key points are summarized especially as they relate to major issues and concepts resulting from the analysis and discussion of historic trends in the peer reviewed literature on the subject of significance evaluation. Besides looking at each paper in terms of their distinctive features an attempt has been made to describe their similarities and commonalities.

## **THE WORKSHOP PAPERS**

### **What Happened To Stewardship (Carroll Kleinhans)**

In this paper, Kleinhans, in a very thoughtful and deliberate fashion, notes an overwhelming emphasis in terms of time, energy and resources devoted to compliance with provisions of section 106 of the National Historic Preservation Act that has unfortunately resulted in placing the concept of effective long term stewardship on a back burner. A lockstep pattern of compliance with separate legal requirements has led to the generation of massive amounts of disparate, descriptive information about cultural resources but very little regional synthesis of any real research utility to science or the general public.

Instead of continuing to treat CRM as a set of discreet compliance events resulting in a great deal of duplication, redundancy and wasted effort, Kleinhans recommends a more seamless strategy with management activity seen more as an ongoing and iterative process. Kleinhans discusses for instance, investment in studies that would produce useful information that would encourage a regional perspective transcending the usual site specific evaluation of cultural resources. This could be better accomplished if cultural resource managers were more resourceful in exploiting available information as well as new and emerging technologies readily available to them. These would include the use of more diverse and comprehensive interdisciplinary information as well as more efficient information management techniques and other analytical tools such as geographic information systems. The latter being especially appropriate for developing regional perspectives that transcend individual sites as well as promote analytical power and pattern recognition hardly possible using conventional evaluation procedures. Kleinhans argues that a shift towards regional research objectives is actually more cost effective than conventional individual site centered evaluations that have resulted in a great deal of useless and inaccessible information.

In championing a greater emphasis on stewardship, Kleinhans also points out the fallacy of assuming that cultural resources are being responsibly managed in the absence of effective and realistic monitoring programs and formal efforts to better understand ongoing impact processes in all their potential manifestations, as if “no news was good news as long as we are in compliance with section 106 for those sites adversely effected by a federal undertaking.” Her amplified discussion clearly supports the concept of viewing the evaluation of cultural resources as an iterative process of resourceful information acquisition and refinement rather than a one time event designed to decide the fate of large numbers of resources on a very superficial informational basis.

Each occasion to revisit a site ought to be seen as a research opportunity to upgrade our knowledge about the region and the site, including its physical condition and threats from impact processes. Some of these impacts, if better understood, will be more cost effective to mitigate through site preservation and protection rather than conventional, destructive data recovery excavations. Without an attempt to develop and continually refine regional research designs and exploit new information and research opportunities inherent in long term stewardship responsibilities, there will be little assurance that traditional evaluation procedures will result in achieving the purported goal of generating knowledge and assuring a future preservation sample of maximum use to science and the general public.

#### **Site Significance in the Context of Regions, Research Designs, and Redundancy (John Schelberg Ph.D.)**

After systematically reviewing the published literature on significance evaluation in this work unit, Schelberg’s paper was found to offer one of the very few discussions of the topic that is actually based on real archaeological field and analytical data. Schelberg does a superb job of illustrating the principle that significance arguments must be relevant to well thought out, executable, problem oriented research designs, preferably framed in a regional context. He uses an example of archaeological research undertaken in a relatively small region of New Mexico characterized by a large number of small, low density lithic scatters.

Questions about redundancy and research importance, in Schelberg’s view are not self evident and must be defined in terms of a commitment to asking and answering interesting, cutting edge questions that reflect the research needs of the region. Schelberg throws in fascinating and surprising examples of why we need to question even what appears to be obvious and be very cautious about assuming non-significance. Even small, numerous, low density lithic scatters, so often considered derisively as redundant and devoid of research potential, were found upon closer examination, using a concerted research effort involving a suite of relative and absolute dating techniques, to yield some rather surprising results. The answers generated from this CRM project contribute in very important ways to a better understanding of the archaeological variability of the region, illustrating the point that the harder you look the more you learn and what has appeared to be obvious to many simply does not hold up to rigorous examination.

Schelberg's paper provides directions for those cultural resource managers who have been frustrated by the difficulty of operationalizing a research design for relatively small project areas. Schelberg's paper should encourage cultural resource managers to be more actively involved in both the research as well as the development of thoughtful research designs consisting of answerable questions logically drawn from more general theoretical issues that can be executed at any number of small scattered projects. Schelberg cautions that the casual treatment of significance as some sort of self-evident, a-theoretical exercise based on very superficial information is resulting in irreparable loss of information. One cannot but wonder at the loss of information that has already occurred from the plethora of CRM evaluation projects characterized by little or no research commitment on the part of the managers or their contractors.

### **Significance: A View From Corps Regulatory (Robert F. Scott IV)**

Scott discusses significance evaluation from a broader perspective than others in the workshop. In addition to the evaluation of the significance of cultural resources, Scott quite correctly raises the separate issue of assessing the significance of impacts and discusses the various laws and regulations that require managers to evaluate impacts from federal actions. Scott's perspective is that of an administrator or manager concerned about the real pressures of processing an extraordinary number of very short fused permitting actions usually involving very small areas where nevertheless, cultural resources must be considered. Much of his discussion concerns the pressures involved in a large permitting program where clients press for decisions favoring the insignificance of resources while archaeologists tend often to take the opposite position.

This paper focuses on concerns for finding acceptable strategies that do not cause controversial, time consuming and expensive administrative delays. Although one can easily empathize with these real world concerns, it does leave open the legitimate question of how the Corps of Engineers 404 Permitting program can ever move away from idiosyncratic site specific evaluations and begin to incorporate problem oriented regional research designs into its program. It would be interesting to get input from other Corps districts on how they handle Section 404 Permits and whether or not anything like the approach demonstrated by Schelberg in New Mexico has ever been successfully implemented. Hypothetically, there ought to be no reason why a set of well thought out and answerable research questions could not guide the research associated with the permitting of a large number of small federal actions in a region and still make meaningful contributions to theoretically substantive knowledge (Butler, 1987:827).

### **Significance In Cultural Resources Management: An Ohio Valley Example (Robert F. Maslowski, Ph.D.)**

Written from the point of view of an archaeologist who has provided direction and program continuity for the Huntington District for over twenty years, the writer has a comprehensive knowledge of the archaeological record and experience with the resource management issues of the

region that is uncommon considering the normal turn over in archaeologists positions throughout the Corps of Engineers. One of the obvious beneficial manifestations of this program continuity is the incremental development of a thorough District wide bibliography and database on cultural resources in the area of Kentucky, West Virginia and Ohio. Bibliographic control over all cultural resource management projects ought to be a basic information investment for every district. Unfortunately some districts are far better off than others in this respect. Without this comprehensive district bibliography, unnecessary duplication of effort will be inevitable, as each contract project reinvents the wheel without taking into consideration and building upon previous research efforts. This also means that the opportunity to standardize data collection and develop an integrated regional research design is lost. A strong case can certainly be made for centralized orchestration of resources needed to implement a successful research and cultural resource evaluation program.

One of the problems that Maslowski has experienced with executing problem oriented research designs as the basis for significance evaluation efforts for large regions is essentially a function of the short fused nature of typical site evaluation projects in his district. Maslowski offers some sensible recommendations as alternatives to overly ambitious research designs that are simply not executable in an unrealistic time frame and where expectations of recovered data may never occur. His paper also addresses an issue that is rarely discussed in the professional literature on significance. He sees both positive and negative features in the concept of attempting to be more explicit by developing quantitative evaluation criteria. The concept of using quantitative evaluation criteria was not well received with at least one State Historic Preservation Officer and interestingly, the opportunity to improve the attempt has never again presented itself.

Ironically, his experience suggests that it has been far easier to deal with regional and national significance than local significance. This would seem to make it all the harder to exercise caution with low density sites as both Maslowski and Schelberg have suggested. Maslowski like Kleinhans also questions the tendency of liberal determinations of no adverse effect for low density sites without a serious consideration of the potential for site preservation and protection alternatives. This would seem to offer all the more reason for evaluating sites within the larger framework of regional variability and comparative preservation potential. A formal consideration of site formation and impact processes ought to be included as an integral part of any regional research design. Answers to both kinds of questions are not easily attained without appropriate research effort and many archaeologists still do not consider such questions real archaeology, when in fact the research needed to convincingly answer such questions is just as demanding as traditional archaeological research.

### **Some Comments On Significance And Federal Archaeology (Evan Peacock)**

As an outsider, Peacock brought the refreshing perspective of a Forest Service archaeologist to the workshop. Obviously there are some similarities between the two agencies, both are responsible for large land holdings and a wide variety of impact processes affecting huge numbers of cultural resources. Although the Corps does have a timbering program, it in no way approaches the magnitude of timber harvesting in the Department of Agriculture. Therefore, the significance

evaluation recommendations put forth by Peacock are to a large extent a function in some very real differences in agency mission, that often allows Forest Service archaeologists the option to emphasize a flag and avoid management strategy. There is no way, for example, to flag and avoid thousands of archaeological sites adversely affected by severe erosion problems occurring along thousands of miles of Corps lakes reservoirs. Nevertheless, Peacock's paper anticipates in some form of discussion more of the twenty-one concepts brought out in Briuer and Mathers (1996) than any of the other papers in this work shop. Secondly, the stridency of Peacock's major argument makes his paper the most provocative in the set.

Peacock takes exception to the primacy of problem oriented research as the best evaluation strategy primarily because this involves the unnecessary destruction of the archaeological record with no assurances whatsoever that future research needs can be accommodated. At best, state-of-the-art research cannot anticipate future research potential in view of our inability to ask and answer questions that no one has even begun to suspect could be answered or in view of rapidly changing methods, techniques and theories that will result in entirely new research directions and possibilities in the future. Dunnell (1984) probably made the most eloquent argument for this position and Briuer and Mathers (1996) also discuss the distinct limitations of problem oriented research designs for significance evaluation.

Instead of evaluation programs routinely relying on destructive excavation, Peacock suggests instead that sites be found, recorded, avoided and representative samples be put aside in preserves. In addition Federal evaluation efforts should minimize subsurface excavation and maximize the use of nondestructive or low impact technologies as well as exploit new interdisciplinary information and analytical techniques such as geographic information systems. All these recommendations have been considered in professional literature in one way or the other.

Just about everyone in the workshop or in their papers has suggested the need for establishing representative samples. Therefore, it is not so much what is said as what is not said in Peacock's approach that raises important concerns begging attention. For example, how does one establish a representative sample for preservation purposes? The results of research in this work unit concerning this concept suggest that it is far more easily talked about than implemented. It has to be a complex and difficult task. It is not a straightforward simple exercise in sampling space and classifying cultural resources in obvious categories but is a research endeavor in and of itself. It will require resources in high quality, reliable information that at present do not exist but must be creatively developed. It will require large complex inventories that will involve multifaceted information as well as new methods and techniques, not to mention a research commitment and expertise in order to make it all happen. Just saying that establishing representative samples and set aside areas for long term preservation is preferable to the ongoing destruction of the archaeological record in massive testing programs will not make it happen.

There is a disconcerting sense of schism implicit in both Peacock's and Dunnell's views that seems to suggest that academic archaeologists alone do problem oriented research while cultural resource managers survey, inventory, gather sufficient information to flag and avoid sites and then establish

representative samples for long term preservation, all without doing any research. It can be argued that creating new knowledge, new techniques, new methods, grappling with the cutting edge, questioning the obvious, contributing to substantive and theoretical knowledge or whatever else one wants to call it, **is research**, and it should be both basic and applied and must be incorporated in cultural resource management lest the discipline degenerate into a stagnant and unquestioning exercise in simply following obvious legal compliance procedures.

### **Concluding Remarks**

Taken together, the workshop papers have collectively anticipated in some fashion most of the major themes or concepts recognized in the review of the published literature on significance evaluation (Briuer and Mathers, 1996). In these papers there is common concern for cost effectiveness and development of evaluation strategies that avoid redundancy and wasted resources. There is also concern about the goal of establishing representative samples for long term preservation for better assuring that a healthy sample of cultural resources will be available for future scientific and public appreciation purposes. At least half of the authors have suggested the need for a regional, more holistic perspective if representative samples and set aside strategies are to be successfully implemented. There seems to be a healthy skepticism running through these papers about conventional, inflexible evaluation strategies that do not recognize the dynamic nature of evaluations and the obvious relationships between research and social values to significance.

Several of the authors have raised issues associated with problem oriented research. Finally, it is interesting that as a group, compared to the group of peer reviewed publications, the authors in this set of workshop papers have expressed an even stronger and more frequently stated concern about redundancy issues, the need for representative samples and the criticality of exploiting non-destructive, non-intrusive as well as new and emerging technologies that was not as frequently stated in the review of published literature.

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**THE SIGNIFICANCE WORKSHOP MANUSCRIPTS**

## **WHAT HAPPENED TO STEWARDSHIP?**

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### **ABSTRACT**

The 1992 amendments to the National Historic Preservation Act, and subsequent re-writing of 36 CFR 800, invite examination of the results of nearly thirty years of compliance with Section 106 of the Act. The process has been exceedingly efficient in gathering data but not in fostering consistent stewardship, the principle underlying all federal preservation law.

### **INTRODUCTION**

With the passage of the Antiquities Act of 1906, the federal government recognized stewardship responsibilities toward historic and prehistoric ruins or monuments on federal lands. Subsequent federal legislation expanded the stewardship role. Activities associated with stewardship are now commonly referred to as cultural resource management and include inventory, assessment, recording, curation and preservation. The importance of the Antiquities Act has been overshadowed by that of the National Historic Preservation Act, as amended, and more specifically, Section 106 of the Act and its promulgating regulation, 36 CFR 800. Stewardship, however, remains the rationale for a federal interest in locating, recording, and protecting properties which contribute to the national heritage.

As a legal term, stewardship implies management in another's best interest. In the absence of competing values, stewardship of the national heritage might equate simply with long-term preservation. However, Federal interest in large public construction projects throughout this century introduced new priorities to the long list of Federal responsibilities promoting the public good, pitting in situ preservation against land use change.

### **LIMITS OF THE SECTION 106 PROCESS**

Section 106 of the National Historic Preservation Act, as amended, integrates preservation concerns with public land management and construction goals. As Butler (1987) points out, the Section 106 process outlined by 36 CFR 800 is a management tool. It is not archaeology, and, in hindsight, may not be good stewardship.

As a management tool, the Section 106 process is elegantly simple, easily communicated, and has proven its efficiency in thousands of federal decisions related to federal construction, licensing, and permitting. The Section 106 process is a progression of negotiated if-then decisions which may stop

at one of several points after which the resource receives no further consideration. Section 106 provides a seamless flow of management decisions and responsibilities relative to the impacts of a single project. It is concerned with mitigating project impact to significant resources only, project by project. Despite common usage, mitigation of impact can include a wide array of actions, not just excavation. Section 106, however, does not specifically provide for what happens to either sites or data once project specific mitigation is complete.

The final stages of stewardship are controlled by a series of unrelated regulations, only two of which originate from the National Historic Preservation Act. This lack of cohesive control allows the separation and dispersal of data, and application of variable priorities to post-construction stewardship concerns. For example, curation of federally-owned and administered archaeological collections is authorized by Sections 101 and 110 of the National Historic Preservation Act, the Reservoir Salvage Act and the Archaeological Resources Protection Act. Curation responsibilities are defined under 36 CFR 79, rather than 36 CFR 800. A separate regulation directs how agencies make reports of archaeological investigations available to the public. Still another directs how agencies treat their own records. And finally, a separate set of regulations control how resources on Federal lands are treated, following project construction.

Because agencies are organized by action missions synonymous with constant forward momentum, funding to complete the stewardship cycle declines following the project construction stage. Recognition and completion of stewardship tasks are typically the responsibility of agency offices which had no role in collecting the initial data or defining the importance of the resource.

The issue of adequate and consistent curation of federal collections required nearly a decade of effort to receive agency recognition. Similar attention is being brought to the gray literature by professionals attempting to use investigation reports generated by the 106 process. There is little recognition yet that the reams of agency correspondence and contracting files are an integral part of the archaeological record. Add to this class of information the dispute records of the Advisory Council on Historic Preservation, and the records of all State Historic Preservation Officers. Aging data bases used to make preservation decisions could also be considered part of the archaeological record. And, finally, there is a wealth of information on site destruction processes which is not being put to scientific use to protect remaining sites or standardize the understanding of impact.

### **IMPACTS OF THE SECTION 106 PROCESS**

The Section 106 decision process spawned a data gathering industry. Terms such as cultural resource, historic property, sacred place and traditional cultural property were coined in response to widening appreciation of property types contributing to our national heritage. The industry changed the way North American archaeology is taught. Tasks and responsibilities associated with cultural resource management require additional training and experience beyond traditional graduate training. Historic archaeology, industrial archaeology and architectural history have grown into separate, definable fields. Dependence upon other disciplines, such as history and geomorphology, is now commonplace. Membership data from the Society for American Archaeology suggest that the majority of archaeologists now find employment outside the academic arena (SAA 1994:11-12).

And, finally, the Section 106 process has created status niches separating academic, contract and federal archaeologists.

Through the National Register of Historic Places, the Historic American Building Survey, Historic American Engineering Record and the negotiated mitigation process, Section 106 has fostered the 'stockpiling' of a tremendous number of sites, structures or collections which represent the scientific value of properties since destroyed. The industry has been so effective in meeting management goals, it has outstripped its ability to integrate data collected into a cohesive regional record easily recoverable for public or professional use. The response to the data recovered and saved has not been as great as the response to the directive to seek and record data.

### WEAKNESSES IN THE SYSTEM

The stresses which the volume of information collected place on the combined academic-professional-agency system are indicative of weaknesses needing correction.

The Role of Agencies in Preservation Decisions. Because the Section 106 process is a series of compromises and negotiations, some of which are embedded within agency culture, the effect of agency decisions and biases on the archaeological record is seldom recognized except by the agency archaeologist. Federal managers, not all of whom are archaeologists or preservation minded, have a great deal of influence in each decision: what gets surveyed; who conducts the survey (in-house versus by contract); whether price or technical capability will determine contractor selection; the time of year work is conducted; monetary limitations affecting sample size, choice of methodology, and the number of analytical samples collected; the length of time which elapses between survey and site testing or data recovery; acceptance or rejection of contractor significance assessments; acceptance or rejection of mitigation options, etc. All of these decisions affect archaeological interpretation. The lack of recognition of the role of the federal archaeologist and agency supervisory chain in the shaping of the archaeological data base will eventually affect historical analysis of twentieth century contributions to North American archaeology.

The Fate of Sites Determined Eligible to the National Register of Historic Places. A troubling by-product of the Section 106 process is the large number of sites found eligible to, but not listed on, the National Register of Historic Places. The system provides no companion register for these sites. It is impossible not to notice the bias toward architectural properties in weekly Federal Register listings of sites being considered for inclusion in the National Register. For the many times we repeat that archaeological sites are a non-renewable resource, we proceed with a No Adverse Effect determination to excavate a site in the way of construction. The process also allows the agency to assume that if sites determined eligible to the Register are avoided by federal action, nothing further will happen to them. While significant sites avoided by a project are to be accorded the same protection as National Register listed sites, they typically receive no further attention until there is renewed federal interest in the acreage they occupy. The process ignores the obvious. Because of the unexplored but suspected scientific value of sites determined eligible to the Register, these sites constitute the true archaeological legacy we leave to the future.

There is currently no Congressional support for protection of resources identified with Federal funding, but located on private lands excised from project easements in order to avoid impact. Federal agencies, however, have the regulatory authority to protect, stabilize, interpret and otherwise manage sites within Federal project easements or on fee lands, but frequently fail to exercise that authority when responsible expenditure of public funds is defined as non-expenditure of public funds (Grosser 1991). Stewardship of known sites requires tracking land use change in site vicinities, physical or remote monitoring to identify natural or induced environmental changes, and pro-active preservation or restoration if new impacts are identified.

Syntheses. Agencies supposedly collect data for future public use, but are slow to recognize detailed analysis or synthesis as economical ingredients of resource management. Despite the presence of State Plans to aid in recognition of research issues and data gaps, Section 106 inventories are conducted project-by-project, allowing agencies to ignore application of consistent field methodologies in any particular region. Research designs may vary contract by contract. Similarly, the level of analysis built into individual contracts is usually only sufficient to address the immediate issues of significance. The reports of investigation produced to satisfy the Section 106 process do not suffice as regional syntheses. Agencies are beginning to apply analytical techniques such as Geographic Information Systems (GIS) to the assessment of probable impact. GIS is ideally suited to incorporating large, complex sets of existing information and identifying meaningful patterns within them. As used by agencies, however, such tools are asked to perform only the most superficial of pattern recognition - i.e. presence or absence categories.

While construction agencies typically do not accept research as a Federal responsibility, analysis and restructuring of data into a more accessible form is compatible with the stewardship role. Certainly, the scattered data bases and reports which now characterize cultural resource management are not efficient. If anything, they could be interpreted as wasteful. Neither agencies nor the profession make sufficient use of the data already collected. Indeed, agencies have little idea who is using the data now being curated or the reports produced. Redundancy in data collection can not be reasonably discussed until we have satisfied the need for synthesis.

One practical solution is to consider syntheses in the administrative array of reasonable mitigation alternatives. The cost of periodic syntheses probably equates well to the cost of excavating a single site. Syntheses are also a logical vehicle for addressing the projected research contributions and importance of those sites found eligible to the National Register for which immediate impacts were previously avoided.

## **ROLE OF PUBLIC OPINION**

As our appreciation increases for the array of resource classes which contribute to the national heritage, we become more aware of the role of the non-archaeological public in the significance assessment and stewardship process, as expressed through the comments of State Historic Preservation Officers, the Advisory Council on Historic Preservation, and guidance prepared by the National Park Service. The National Park Service has taken up the banner of public outreach and the Advisory Council encourages increasingly creative forms of mitigative action which interpret

significant properties for the public. As efforts are made to expand public appreciation for the relevance of heritage preservation, it is imperative that the quest for cheaper and more public-oriented mitigation measures not endanger the stability and preservation of more venerable repositories, such as HABS-HAER.

We find ourselves in an interesting period when definitions of the public good and historic values are being discussed and questioned by the non-archaeological public. The cultural resource management industry fostered the shift from prehistorians assessing the significance of all resources found during survey, to the more rigorous multi-disciplinary approach now in place. At the same time, the profession has had to adjust to a shift in emphasis from Federal archaeology for archaeologists to Federal archaeology for the public. We are presently experiencing an attitudinal shift from the assumption that all excavation is acceptable, to weighing excavation against in-place preservation.

Preservation-related disciplines have assumed that they educate and shape public appreciation for the past. However, from time to time limitations to public appreciation, tolerance or willingness to dedicate resources to preservation issues are expressed powerfully for the public through Congressional action. The definition of what constitutes the public interest, and hence the stewardship responsibility, will continue to be refined and shaped by the competition of public priorities. The public, through the auspices of the academic community, State Historic Preservation Officers and the Advisory Council on Historic Preservation, have a great deal of influence on the quality of agency sponsored work. The time has come to review the role of research, analysis and the application of new research tools as fundamental and inseparable elements of effective resource management.

## CONCLUSIONS

If one could re-write Section 106, after nearly thirty years of application, one might want to incorporate all stages and activities associated with stewardship into one seamless, umbrella section of the Act. Federal attention has been so focused upon Section 106 compliance, that other stewardship activities have suffered in the competition for Federal dollars, time and labor. While it is unrealistic to expect an elevation of the importance of stewardship in public opinion, ignoring any aspect of the responsibility ignores a public trust.

Agencies need to reevaluate their perception of rigorous synthesis as a luxury. In tandem with GIS technology, syntheses are a cost effective means of analyzing cumulative impacts and identifying with greater precision the need for further investigation in any given project area. Agencies have been aware for some time that there are only so many new, large projects left to construct. Smaller projects and changing agency missions will control expenditure and manpower resources available for preservation in the future. The time is approaching when project-by-project and single agency perspectives must give way to regional, multi-agency approaches based upon sound analysis and compilation of regional data.

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## **SITE SIGNIFICANCE IN THE CONTEXT OF REGIONS, RESEARCH DESIGNS, AND REDUNDANCY**

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### **ABSTRACT**

Archaeological site significance is most usefully determined by: (1) assessing the data requirements of the surrounding region, and (2) exploring and expanding the data potential of that region. A research design focusing on the corpus of material likely to be recovered is necessary. However, such a design is not a guarantee of success since data collection is relative to the questions being asked. If new questions are not introduced into research, we run the risk of confirming what we know and failing to detect important variability. Not considering categories of sites due to assumed nonsignificance will hinder our understanding of past adaptations.

### **INTRODUCTION**

The topic of site and data significance involves several related subjects. This summary paper focuses on research designs, regional context, and redundancy. Federal archaeologists must improve their abilities to make the most of scarce economic and cultural resources. One means of improving the overall quality of federal projects is to continually improve research designs by ensuring correspondence to the scale and details of the undertaking. Cookbook models designed to test grand schemes probably will fail. Even the best and most appropriate research design can only contribute so much to a project. Numerous factors conspire against a successful undertaking. However, one factor which can be controlled is maintaining a large pool of sites for research through carefully considered eligibility determinations. While other factors are not the major concern of this paper they are intimately interrelated and have a direct bearing on the outcome. It is important to maintain a holistic perspective. Successful projects do not just happen.

The challenge to the agency archaeologist is to balance the competing concerns of management, archaeology, and reality in order to avoid the same repetitious, descriptive, culture-historic reports. While the regional approach is not viable (as it is usually conceived) for most Corps' projects, it is nevertheless possible to consider issues germane to the region, what has been done, and would be possible to do.

### **ARCHAEOLOGY -- THE THEORY**

In one of his earliest articles, Consideration of an Archaeological Research Design, Binford (1972)

was concerned with the necessity of testing hypotheses relating to human behavior. He briefly discusses several additional topics including the now familiar notions of the regional approach, culture process, and sampling. Most of the paper addresses sampling but some other thoughts bear repeating, especially the importance of cultural process; the futility of enumerating culture traits; and the study of past culture systems requiring the determination of both extent and cultural and ecological components. It is necessary to isolate the conditions and mechanisms through which cultures change; the most expeditious approach is one which is regional in scope and executed in the context of a viable research design (Binford 1972:135-148).

A research project designed to isolate and define the content, structure, and range of a cultural system, together with its ecological relationships, is most productive when regions are the unit of investigation. The regional approach involves a detailed and systematic study of the area reasonably expected to have supported the cultural system. The extent of a region is relative to the adaptation of the cultural system under study. For example, more complex societies generally cover greater ecological ranges and enter into more complex extra societal interactions (1972:137). Binford's statements about regions and research designs were written in 1964 yet are similar to more recent articles (e.g., Butler 1987; Lynott 1981; Raab 1977). Generally a research design is a framework in which strategies for achieving research goals are planned, operationalized, and evaluated. Archaeologists quibble over details and argue induction verses deduction; however, there appears to be broad general agreement concerning components of research designs.

A review of national and regional archaeological journals failed to produce more than a few articles addressing the topic of research designs. Most are concerned with the gamut of issues relating to cultural resource management including a variety of criticisms of Federal archaeology, money, contracting, standards, etc. From the journals, one is left with the impression that many problems were resolved as the field of cultural resource management developed and matured, agencies hired archaeologists, standards for personnel and the work were raised, and research designs were incorporated into the projects.

It appears that as the years pass and public archaeology continues, the individuals who do the work are not those writing about research design problems. Fearing that I was failing to find articles devoted to research designs, I conferred with several State Historic Preservation Offices, state archaeologists, and the Interagency Archaeological Services (IAS) in Denver. No one could recall any recent articles on research designs. IAS advised that one of their main concerns when reviewing a research design is fulfillment of the requirements in the Advisory Council's Handbook (1980:24-26). The tone of the IAS' response to my suggestion that people must be doing research designs rather than writing about them was - *"of course you fool, everyone knows that."*

In a 1987 article concerning significance and other frustrations in cultural resource management, Butler (1987:827) noted that a colleague once asked him why he even wanted to write about these problems: *"Much of what you want to say has been said before and, with few exceptions, most archaeologists are not listening."* Butler indicates that, in one form or another, problems remain with contractors, Federal archaeologists, and agencies.

The 1966 National Historic Preservation Act did not assume that significance could be a matter of

rigid, objective measurement. It specifically encouraged the recognition of locally significant historic resources which provide a community with a sense of past and place. The historic value will be some combination of public sentiment and rigorous, but also subjective, professional assessment. The passage of time is necessary prior to declaring something 'historic'. To be a tool for public administration requires that the National Register not include properties of transient value or interest. Of necessity, properties nominated to the National Register should possess enduring value for their historical associations, appearance, or information potential (Bulletin 22:3). Bulletins 15 and 22 are concerned with actually nominating a property to the Register. We are more concerned with a property's eligibility in order that research may be undertaken and knowing that once the work is completed the property may not be eligible because most or all of it has disappeared. The motivations are very different - one is to aid long-term preservation; the other is a step in a bureaucratic process which results in the properties destruction.

Gordon Willey's Viru Valley project is often considered an impetus to settlement system analysis, especially as the region became an analytical unit. Settlement system analysis attempts to understand all categories of sites addressed through some type of probability sampling. Sampling has been stressed in an attempt to overcome archaeological proclivities to work at the biggest, the most impressive, the oldest, or the richest sites. With settlement system analysis and the detailed, albeit localized, bodies of site data resulting from CRM related surveys, archaeologists began to grapple with defining a region which approximated that of the cultural adaptations (e.g., hunting and gathering or agriculture) being studied. Material identification and sourcing of stone, shell, ceramic temper, turquoise, iron, or trees along with faunal habitat, and prehistoric diet were all studied for the dual purpose of gaining material-specific information and attempting to specify points or areas of origin. Overlapping these analyses were the development of models of trade, exchange, and interaction. Borders and boundaries were studied and disputed. Economic anthropology became a subdiscipline, and ethnoarchaeologists studied the activities of living communities in attempts to better understand the variables associated with such tasks as craft production, subsistence, and transportation. Explicitly stated or not, these investigations and theory building exercises were undertaken in order to more accurately define the region.

## **ARCHAEOLOGY -- THE REALITY**

If everyone is in general agreement about the construction, content, and use of research designs, regions, and perhaps significance, what are the remaining problems and why is it so difficult to conduct a project that makes a contribution beyond description and culture history? Many impediments can be subsumed under the broad headings of those endemic to the Federal agency in need of a compliance project, the mechanisms of Federal procurement by competitive contracting, and those originating within the contracting firms.

One factor relevant to research designs, significance, and the regional approach is the amount of land actually owned or under the responsibility of an agency. A major land holding agency is in a more advantageous position to initiate projects where the research design is regional in focus. Forest Service and Bureau of Land Management holdings are a closer approximation to aboriginal use areas than are the small segments of land adjacent to a river administered by the Bureau of Reclamation or

the Corps of Engineers. While the Albuquerque District covers a relatively large area (New Mexico, southeastern Colorado, and extreme southwestern Texas), it maintains only nine dams. The archaeological work that occurs tends to be conducted in incremental bands around a lake. These situations of restricted land management require a focused research design that cannot effectively address the overriding broad spectrum research topics such as the development of agriculture or complex societies.

Even a focused research design is difficult to develop when little is known about the archaeology in an area. The first order of business may be to conduct a survey to provide a good quality descriptive data base. We have undertaken survey and testing for the Air Force and Department of Energy in several areas of New Mexico where basic descriptive data was considered a major contribution simply because of the lack of previous systematic work. Significance is easier to deal with in this case - when little is known everything is significant, especially if a possibility for dating exists. For example, in the 26,000 square miles of southeastern New Mexico there were 15 independently dated sites or the equivalent of one dated site for each 1,800 square miles. For comparison, this would be the equivalent of writing the archaeological overview for Delaware with one dated site for use in the interpretation (Stuart and Gauthier 1981:283).

It may be useful to operationalize the archaeological concept of 'region' in two apparently contrasting styles/manners. One is the region as a well known and defined cultural unit (e.g. the Hohokam), which represents the bulk of the published material within a given geographic area. The second is the region as a less well known entity which represents a source of research questions and data gaps to be addressed by archaeologists during Federal undertakings. Taken together, these two concepts of an archaeological region form the 'frame of reference' within which site significance can be addressed.

The archaeological sites within a COE slice of the region contain portions of settlement systems. The types of sites will, in all probability, be different from temporal period to temporal period or as contrasted by adaptation - hunting and gathering or agriculture. Therefore, it is necessary to evaluate the sites with respect to their own temporal periods and adaptation within the context of regional research issues. The results of comparing like to like will be valid and produce better science than simply excavating some sites for the sake of doing work. The analysis of the local material can be evaluated against the existing regional data thereby resulting in a great deal more analysis for very little extra money. With this expanded data base, evaluation of theoretical models is actually possible unlike the situation when one or a few sites have been excavated.

The suggestion that agency archaeologists are confined only to the agencies land is spurious with respect to creation of theoretical models and evaluation of results. The National Register itself is predicated on the word "context" (e.g., Bulletins 15 and 22). It is the region as defined by the settlement system which provides the context with which to assess an agencies sites and the results of any undertaking. The actual size of the agencies project and its attendant cost is a much more realistic determinant of the practicality of the regional approach than is the culturally artificial boundary fence. Since significance is very broadly formulated as an operating construct, it is not difficult to create viable arguments for mitigation. Assuming that the project area was reliably surveyed (when, how, and by whom was the survey completed) and using settlement systems as a

vehicle, the potential for sites to contribute information can be weighed against the regional data base.

By consulting the state plan; overviews and other reports; SHPO; and federal, private, and academic archaeologists who have worked in the area, viable research questions can be developed which are concerned with the region and commensurate with the sites to be mitigated. What categories of sites or issues have been underemphasized in the area is useful to know as are regionally common explanations for the archaeological record. It is also important to stratify the sites in the project area by time and/or adaptation and employ research questions related to each category. They may be different issues. The big picture research questions such as the development of agriculture or complexity should serve as an umbrella to derive more focused and possibly attainable results. Given the destructive nature of mitigation and the rare opportunity for excavation, several research issues should be considered for each temporal period.

### ABIQUIU - AN EXAMPLE

In 1982 Congress permitted a 20-foot increase in the level of the permanent water storage at Abiquiu Dam in north central New Mexico. The mitigative measures for this new Federal undertaking addressed both research issues and future management needs at 61 eligible sites in 1984 and 1985. One portion of the project was establishing and verifying an obsidian hydration dating curve; we could then collect and date obsidian from the remaining sites in the project. This would allow us to target sites from particular temporal periods or to focus on sites from under-represented periods in the event of future projects.

Sites within the project area spanned 10,000 years of prehistoric and historic use and occupation; sites from most temporal periods would be impacted by the rising water. In setting up the scope of work, we established research issues specific to each major period. Several recurrent goals cross-cut the periods in order to focus the project towards the broad questions of time, adaptation, and ethnicity, that is who was using the area, when, how, and why. By number, the single most frequent site was the lithic scatter (70%). Of these, 66.7% were undiagnostic. In New Mexico, the undiagnostic lithic scatter is probably the single most frequently recorded site type. What, if any, treatment they should receive during mitigation is frequently debated and they are frequently ignored, especially if structural sites or sites with diagnostics are also present. The Abiquiu lithic sites could represent debris left by any of the following groups: Paleo-Indian, Archaic, Anasazi, Jicarilla Apache, Comanche, Ute, Tewa, Navajo and/or Hispanic.

One focus of the field work was the collection of material for dating, with emphasis given to proveniences suitable for crossdating. A total of 45 radiocarbon dates from 12 sites, 9 thermoluminescence dates from 8 sites (9 additional thermoluminescence samples dated geological events), and 782 obsidian hydration dates from 61 sites were reported (Bertram et al. 1989; Lord et al. n.d.). Twelve dendrochronological samples were submitted but all were pinyon and none dated due to the absence of a refined calibration for this area. The latter was unfortunate since dates based on this relatively accurate technique may have clarified several spatially associated but conflicting dates based on other techniques. Ceramic cross dates came from 21 sites and projectile point cross dates came from most sites. Multiple obsidian hydration dates, that is from two to four dates per

individual artifact, were recorded on 45 artifacts in an attempt to address such questions as use-life and prehistoric recycling.

The extent to which all of the sites were multicomponent had been completely misunderstood. Virtually every site was used during three or more of the cultural periods occurring in north central New Mexico. For example, at site LA 25328 seven collection units produced 72 chronometric dates including Early (4,800 BC), Middle (3,000 and 2,500 BC), and Late Archaic (75 BC); Basketmaker (A.D. 100, 400, and 600); numerous dates between A.D. 0 and 850; and Puebloan dates between A.D. 750 and 1250. A number of dates indicating recycling were obtained from this site. One interesting feature was a cache of Archaic-period flakes; the nonrecycled flakes dated from the late Archaic while the recycled flakes indicated much earlier use (6,200 BP). For this site, 7 or 8 occupational periods occurred within three distinct collection grids, and each of the seven collection grids were found to have more than one occupation. The seven obsidian projectile points consistently dated earlier than the bulk of the associated tools and debitage, suggesting the collection and reuse of earlier points by later occupants.

Many more sites dated to the Pueblo Periods than previously believed. Sites which lack traditionally diagnostic Puebloan artifacts are typically overlooked during survey and during analysis if there is no method to date the material. The Pueblo-period lithic scatters were recorded as Archaic or undiagnostic. Points which are typologically Archaic clearly date to the Pueblo periods. This makes sense if one considers the functional basis for large points - hunting large mammals. High altitude hunting, presumably for deer, elk, and mountain sheep, by Puebloans living in the very large sites dozens of miles down the Chama River Valley occurred in the Abiquiu area.

To test whether large points are only attributable to the Archaic, recycled, or made by later groups, multiple obsidian hydration samples were taken from single artifacts. Tool damage and recycling were ubiquitous in the Abiquiu assemblages. Extreme care is required to ensure that rinds to be dated pertain to a specific knapping or behavioral event. If one is interested in manufacture then the haft should be sampled. Reworked or broken edges should be sampled separately. The archaeologist must indicate to the hydration analyst where and how many samples should be taken from a single artifact. Simply getting a date may not be particularly useful. For example, a point which is typologically older may be dated to a younger period if a broken, damaged, or reworked portion is sampled. The usual response is that the date is incorrect. The date, in fact, may be accurate for the event being sampled (e.g., reworking) but not for the manufacturing event that created the morphology upon which the typology is based.

The project was conceptualized from a site-based perspective; however, the distribution of the lithic material in the Abiquiu area may be more effectively studied from the perspective of artifacts and activities distributed across the landscape. The results of the obsidian hydration study indicate that much of the lithic debris resulted from thousands of years of overlapping behavioral events, including prehistoric recycling.

It is painfully clear that only one or two dates from a site (and especially these sites) are woefully inadequate. Had we chosen to ignore or simply collect representative samples from the generally undiagnostic lithic scatters because everyone knows that they produce little information, we would

have confirmed what everyone knows. The question of how such a large number of dates from any one area can be justified leads to the issue of how many are enough or redundant, a recurring topic of concern. Butler (1987:823-824) suggests that levels of redundancy can best be evaluated within the context of research designs based on current theoretical and substantive knowledge (1987:823-824) However, redundancy, and for that matter significance, are relative to the paradigm or conceptual scheme under which the project is approached. For example, if an analysis of several discrete lithic scatters were undertaken from a site based perspective in a settlement system analysis then one or a few dates may be sufficient and 782 might be redundant. If, however, the same project were approached from a nonsite or locality basis where small clusters of lithics may represent separate noncontemporaneous events then it may be necessary to date each cluster and 782 would not be redundant.

Redundancy cannot be realistically discussed or understood until a problem has been defined and a reliable body of data, which can be used in the evaluative process, exists. The issue is more than simply having a research design based on current theoretical and substantive knowledge. There are several paradigms competing at any given time and it is not always possible to use data collected under one research design to evaluate other paradigms. Approaching each project from the same basic perspective (e.g., undiagnostic lithic scatters provide no information or all Pueblo Indians were always sedentary) and with the same goals is counter productive, not scientific, and redundant from the outset. Research problems must be designed to be compatible with the archaeological remains in a given location. Significance, at least in the short run, must be construed in a manner which accommodates our lack of reliable information. If significance is treated in too casual a manner, a category of sites may be excluded from a subsequently carefully crafted research program.

## CONCLUSIONS

A declaration of site significance rings with finality concerning eligibility to the National Register of Historic Places and any subsequent expenditure of Federal funds. The future of a site rests with its potential contributions to the archaeological data base and its elucidation of issues relating to culture, adaptation, and behavior. Assessing the data gaps of the surrounding region and devising a research design are critically important to a project's success. Equally as important, however, is questioning apparently fundamental assumptions. All categories of sites must be investigated. Should site significance not be seriously considered then the population of available sites may be unnecessarily restricted resulting in a project with familiar, but inaccurate, conclusions. Notwithstanding declining funding, significance is an issue which requires careful and considered attention due to its importance in the cultural resource management process and in its real world implications.

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**SIGNIFICANCE:  
A VIEW FROM CORPS REGULATORY**

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**ABSTRACT**

The definition of significance is widely and variously construed throughout the technical and legal literature of federal cultural resources management. While the National Environmental Policy Act has attempted to qualify and define levels of significance, no similar procedure exists for cultural resources. The Corps of Engineers Regulatory program largely processes small, short duration projects on non-federal land. Significance arguments set out in public law (i.e. under 36 CFR 60.4 - particularly criterion d) that are not further clarified, are likely to cause processing delays and problematic treatment of historic properties. Using additional criteria developed from federal guidelines, laws, and academic sources, it is suggested that judgements of significance can be more refined at earlier stages in a project's development. This procedure has direct benefits to a program such as the Corps Regulatory program that factor processing time and economic issues into permit decisions.

**INTRODUCTION**

'Significance' is possibly the most used, misused, and abused word in the federal government, much less cultural resources work. The word has the power to mobilize an Environmental Impact Statement under the National Environmental Policy Act, or trigger Section 106 and 110 review processes under the National Historic Preservation Act. Interestingly, it does not appear within the Archeological Resources Protection Act (ARPA). This fact is made more curious by ARPA's statutory authority in contrast to the processual requirements of the National Register review and compliance.

According to Webster's dictionary, in its nominal form, significance is defined as:

1. (i) importance, consequence
2. (i) meaning, import
3. (i) the quality of being important or meaningful

In its adjectival form, 'significant', changes its meaning slightly and is characterized as:

1. (ii) important, of consequence

2. (ii) having or expressing a meaning
3. (ii) having a special, secret, or disguised meaning

As a noun, the word can be used to essentially define itself. In the adjectival form it eventually assumes the (somewhat amusing) form of a cryptic game in a Tom Clancy novel. Unfortunately it is this third definition (i.e. 3. (i)) that often drives the significance and national register processes in cultural resources. Even when filtered through the necessarily vague criteria in 36 CFR 60.4 (particularly criterion d), the term significance appears seven times under 'Criteria for Evaluation'.

Any attempt to define resource significance should begin with the National Environmental Policy Act (NEPA), the cornerstone of modern federal environmental legislation and model for much state environmental legislation. At some level, all federal cultural resources work, whether through environmental impact statements (EIS's), environmental assessments (EA's), historic property management plans (HPMP's), or the widely criticized 33 CFR 320.5 (Appendix C), are linked to NEPA. Significance of the impact of a federal undertaking is the trigger that begins the full NEPA process. For this reason, NEPA offers a very thorough discussion of the processes involved in judging and defining specific major issues that will be the focus of the federal government's investment of time and resources.

Considering the potential effect of an EIS, deciding what is and is not significant is sobering. One EIS undertaken in the Ft. Worth District in 1986 still guides development along the Trinity River corridor between Dallas and Ft. Worth, Texas. Its influence is in the tens of millions of dollars in terms of private development project design and redesign, as well as the benefit of slowing the environmental degradation of the area.

To this end, NEPA addresses significance in three succinct discussions (FR, vol. 43, No. 230, Wednesday November 29, 1978, parts 1502.1, 1502.2, and 1508.27). Paraphrasing two of these, EIS' must be analytical, and impacts *"shall be discussed in proportion to the significance"*. A discussion of the term "significantly" begins, *"as used in NEPA requires consideration of both context and intensity."* Context is defined as *"the significance of an action must be analyzed in several contexts"*. *"Significant varies with the setting of the proposed action."* It continues with "intensity", defined as *"the severity of the impact"*, including the knowledge that impacts may be *"both beneficial and adverse"*. Eventually, it encompasses effects, the uniqueness of an area, cumulative impacts, the degree of uncertain or unknown risks, and of course, cultural resources.

Discussions of context and effect within NEPA resemble the familiar paths of 36 CFR 60.4 and the Secretary of the Interior's Standards and Guidelines (FR, Vol. 48, No. 190, Thursday, September 29, 1983). However, NEPA gives credence to more variables, as well an understanding of proportional impacts. The Secretary of the Interior's Standards and Guidelines (SIG), as well as the bulk of the literature on significance within cultural resources, is guided by the analytical process based on the synthesis of historic contexts and the constant refinement of the research designs they spawn. The 'Standards of Evaluation' (p.44723) approach significance with a very straightforward statement.

I. *Criteria for significance should focus on historic, archeological, architectural, engineering and cultural values (resulting in a) statement of the minimum information necessary to evaluate*

*(these sites) against criteria.*

- II. *Properties are evaluated using a historic context that identifies significant patterns that properties represent, and defines expected property types against which individual properties may be compared.*
- III. *The evaluation process and inventory of significant properties represent a dynamic list.*
- IV. *This information shall be made available to the public.*

Synthesis, identification of objectives, needs, and expected results, and methods drive this familiar process. The SIG lay out the process in detail, attempting to address significance synthetically and dynamically. This links directly to 36 CFR 60.4 and the introduction to the criteria:

***"The quality of significance in American history, architecture, archeology, engineering, and culture is present in districts, sites, buildings, structures, and objects that possess integrity of location, design, setting, materials, workmanship, feeling, and association..."***

The term "integrity" is used here to further qualify and assess significance. Otherwise, significance largely defines itself, or is a subjective qualifier of importance throughout the a-d criteria. Significance defines National Register of Historic Places (NRHP) eligibility, and the machinations of the process become the reality of our day-to-day jobs in compliance with federal law. At this point significance shifts to physical site potential as a research design or treatment issue.

A dynamic of significance that has rarely been explored in cultural resources is the feedback between the 'Section 106 process' (used here to include all actions under Section 106, 110, 36 CFR 800, and Appendix C) and significance. Decisions on effect (including adverse, not adverse, and no effect) have tremendous bearing on the ultimate fate of the historical resources that we are charged to judge. A decision to agree upon an eligibility determination (in other words, agreeing that it is significant) carries a miniature version of NEPA responsibility, invoking review, time commitments, money commitments, protection responsibility, and the potential for any number of exasperating adventures into costs, delay, and disagreements with everyone involved. This becomes particularly evident when the Section 106 process becomes subject to pressure from applicants (who would benefit from non-eligible, non-significant resources associated with their projects), or political involvement exponentially slows reviews at commenting agencies. Worse, contractors have the incentive to invoke significance, as the prospect of additional work on marginal resources is hard to resist and easily argued into the vast gray area between significant and insignificant, eligible and ineligible.

The Corps of Engineers regulatory program is particularly vulnerable to such arguments. Under Section 404 of the Clean Water Act and Section 10 of the Rivers and Harbors Act of 1899, the Regulatory program is endowed with its own regulations (33 CFR 320-330 and other associated regulations), its own time limitations for processing permits, and its own budget. It is one of the few areas of government where jurisdiction is directly applied to private property. For this reason, significance is a word that has serious NEPA connotations, and significant sites may invoke financial

and processual impacts far beyond the scope of the permitted impact. In case the potential complications inherent in the program are not enough, the Clean Water Act regulations contain their own compliance mechanism for cultural resources under 33 CFR 325, Appendix C. Commonly referred to as simply 'Appendix C', these regulations address the complex problems of applying Regulatory and the Clean Water Act to cultural resources. The differences between Appendix C and 36 CFR 800 have led to the apocryphal tales of Appendix C's transgressions and a running feud between the Corps of Engineers and the commenting agencies.

The nature of the average permitted project and the resources it affects brings the problems with current significance determinations to a head. Permits generally affect a small area (many are in the tenth of an acre range) in areas in, or immediately adjacent to, waterways and isolated water bodies (such as playas). When sites are identified, their importance is often disproportional to the size of the project. Since the responsibility to undertake compliance work will fall on the applicant, costs and time delays can become factors in the project from the applicant's perspective. Time delays also affect Regulatory project managers who have time limits built into the law for processing applications, and must ultimately decide the scope of the government's jurisdiction, as well as the disposition of the applicant's project (to issue, deny, or modify the permit). Two events, then, affect significance judgements: limited project scope (limiting resources or jurisdictional areas), and small, isolated, or marginal cultural resources.

Butler (1987) addresses precisely this type of issue when encountering these gray areas. Butler emphasizes that no areas are truly unknown, and that proper application of theoretical and substantive knowledge (TSK) should result in research design issues that address gaps in HPMP's and state contexts. In this same vein, he continues that federal agencies should develop research design issues in advance for such classes of repetitive site types (like lithic scatters, burned-rock middens, tenant farms, etc.). Ultimately, most of the literature on significance comes to rely on familiar related solutions of more synthesis, better research designs, and dynamic context development.

The problem remains that state historic contexts often tend to be either incomplete, absurdly complex, or too general for meaningful adaptation to small scale projects. Research designs tend to be the opposite of these documents; long winded and excruciatingly detailed pleas from the contractors to not be shamed into doing less than the Absolute Science that the resource deserves. In-house federal development of research design issues may not be widely possible in jobs such as Regulatory where time to undertake development of them is limited by the day-to-day issues of review and compliance. The regulated public is not normally anxious to fund pure research levels of inquiry, and often is left stunned at the length of Section 106 process and the cost of the most basic levels of survey, testing, or mitigation. The academic vacuum created between these two extremes spawns the most passionately specious arguments under the 60.4 criteria. When contractor interests become further tangled in the unholy embraces of financial pressures, personal research interests, and political pressure, the best intentions of significance determinations and TSK evaporate.

In regulatory, processing times of sixty to one hundred twenty days for an individual permit (as opposed to nationwide and general permits which have shorter processing times) is part of every project manager's performance indicators. Within the cultural resources compliance process, this amount of time may equal approximately one rewritten research design and two exchanges of letters

between the Corps and SHPO. In the regulatory program, protracted cultural resources arguments over significance and treatment problems are common and send the process into chaos. Any disagreements on eligibility are forwarded to the Keeper of the National Register in Washington, D.C., for resolution. This wild card in the process can add weeks to the determination of eligibility process. Research designs arguing relative levels of research significance are not easily or quickly rewritten when time is a critical issue for the permit applicant and the issuing office. Frequent demands for change in research designs while arguing for (or against) specific research issues leaves a trail of bruised egos and bewildered permit applicants while consuming project time at an alarming rate. If this isn't enough, the maze of MOA development, report review, NAGPRA, and curation have not been factored into the process.

The significance issue stands as the solitary fuse which ignites, drives, derails, or ultimately solves this dilemma for small scale, small resources projects as normally encountered within Regulatory. Reducing the uncertainty at the earliest point in the cultural resources process provides proper direction for the federal agent and clear goals for the applicant and contractor to understand. Any solution to refine significance determinations must be:

1. Universal: - like criteria in 36 CFR 60.4, it must work across the spectrum of cultural resources
2. Complimentary: - it has to work within the frame of the Secretary of Interior's Guidelines, *and*
3. Functional: - it must be applicable in 'real world' situations

Toward this goal it is suggested that criteria in 60.4 are refined through a process of additional questions before significance is ultimately determined. These basic questions offer a NEPA-like refinement of the process, forcing the contractor, SHPO, and federal agent to critically evaluate sites on a more detailed level, and to provide leaner and more cogent arguments for additional work. All sites felt to be eligible under 60.4 a-d (especially d) need to be addressed for the following issues prior to determination and treatment.

1. Intersite Variability: How does this site relate and compare to similar sites in the area? What are the potential ranges of variability within the site types and the site settings?
2. Integrity: This common indicator of site quality is often poorly applied when glamorous sites are encountered (such as Paleoindian). What aspects of this site or structure remain intact enough to contribute additionally to understanding the resource? Proper application of this addresses the question of investing federal time and an applicant's money in sites that will yield little more than an artifact collection.
3. Potential Contributions: In a sense this is Butler's TSK. What, specifically, can this site or structure offer that addresses, adds, or contributes to the TSK of these sites. Was testing (or survey) level inquiry sufficient to recover the information (de facto mitigation)? Is additional work really necessary? If it is, specifically what will be addressed?

4. Preservation Potential: Should the site be preserved instead of excavated/recorded? Site banking may become a critical feature of cultural resources as the rate of economic growth and impacts to American historic properties becomes more pressing. Will the project itself have a beneficial impact on the resource? Are other sites or structures in the area more likely to be preserved or harmed in the future?

These follow a NEPA viewpoint of assessing a number of variables as part of significance. By combining the needs of critical inquiry into the script, it adds direction to the significance argument and helps quickly define the needs and shortcomings of small scale, short duration projects. At the minimum, it provides a checklist of information to refine 60.4, criterion C, and a bridge to shortcomings in state plans and federal HPMP's.

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# **SIGNIFICANCE IN CULTURAL RESOURCES MANAGEMENT: AN OHIO VALLEY EXAMPLE**

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## **ABSTRACT**

The Huntington District, U. S. Army Corps of Engineers, has been involved in thereconnaissance, evaluation and mitigation of archeological sites in the Mid-Ohio Valley for the past 20 years (U. S. Army Corps of Engineers 1995). A brief history of the development of the program is presented, together with an evaluation and discussion of the strategies used for determining archeological significance.

## **INTRODUCTION**

The Huntington District includes all areas drained by the Ohio River from roughly New Martinsville, West Virginia, to Meldahl, Lock and Dam, which is located about 20 miles upstream from Cincinnati, Ohio. The area includes about half of Ohio, half of West Virginia, eastern Kentucky, the Levisa Fork drainage in Virginia, and the New River drainage in Virginia and North Carolina. Like most federal agencies with land management responsibilities, the Huntington District became directly involved in cultural resources management shortly after the passage of the Moss-Bennett Bill in 1974.

Prior to this involvement, archeological investigations on District projects were conducted by the National Park Service and the Smithsonian River Basin Survey. These early investigations consisted largely of windshield surveys of large project areas (Baker and Fowler 1975; Baby and Potter 1963; Slone 1960), excavations of mounds (Baby and Drennen 1973; Drennen and Baby 1974; Baby, Potter and Mays 1966; Baby and Zierhut 1966; Greber 1977; Baby, Potter and Saurborn 1968) and testing Late Prehistoric village sites (Solecki 1949; Baby, Potter and Sawyer 1967; Dunnell 1972; Dunnell, Hanson and Hardesty 1971).

## **THE DISTRICT PROGRAM**

Cultural resources management has become a dominating force in Ohio Valley archaeology. This is a logical development given that the theory and practice of cultural resources management focuses on the evaluation of archeological site significance. In 1975, the Huntington District hired its first archeologist and became directly involved in all aspects of cultural resources management including reconnaissance, survey and evaluation, and excavation of archeological sites. The Huntington District

Cultural Resources Management Program was designed to comply with Federal legislation which highlights the protection of cultural resources, as well as to investigate relevant research topics in conjunction with its compliance responsibility. Since its inception in 1975, the archeological phase of the program has operated under a regional research design seeking to standardize data collection in Kentucky, West Virginia and Ohio. The type of data collection and type of research topics investigated were determined largely by the level of compliance required for a particular stage of the project.

Reconnaissance level investigations were geared largely to recording site locations and defining the potential for locating significant sites in the project areas. Data collected were used for studying general settlement patterns and for defining potential research topics.

Survey level investigations involved testing and evaluating sites identified during the reconnaissance in terms of National Register criteria. The development of settlement pattern data and definition of external relationships and interactions was the aim of most survey level investigations. Settlement patterns were discussed in terms of types of sites and cultural/temporal components. Surface collections and test excavations were undertaken to obtain adequate samples to determine the range of components present and the nature of the occupations.

If National Register sites were to be damaged or destroyed and no reasonable method of protecting the sites could be implemented, mitigation was undertaken within the framework of a problem-oriented research design.

## **THE PROBLEM OF SIGNIFICANCE**

The concepts and methods of determining archeological significance have changed over the years and many of these changes are mirrored in the District's Cultural Resources Management Program. In the early years, projects were evaluated using unsystematic survey techniques and generally any sites with cultural features or high densities or artifacts were determined significant (Dexter 1974; Sanders 1976; and Fenwick 1976). Later systematic surveys and sampling techniques were introduced (Adovasio 1982; Niquette and Donham 1985) and regional research designs were developed. Sites were evaluated in terms of these regional research designs and several site characteristics including site integrity, site function and data redundancy were taken into account.

In the Yatesville Reservoir Project, where over a hundred archeological sites were recorded, numerical ranking systems for the evaluation of prehistoric and historic sites were developed by Niquette and Donham (1985). The systems were based, in part, on Creasman's (1979) study of Canyon Pintado Historic District in Colorado and the Arkansas State Plan (Davis 1982). The research potential for each prehistoric site was ranked in eight areas involving five problem domains on a scale of 0 (lowest) to 3 (highest). The problem domains included: settlement structure (functional differences and domestic residences), bioarchaeology, culture definition, subsistence, and technology (bone/shell/wood, textiles/basketry and lithics). Sites that scored above the mean were thought to be potentially significant and were further evaluated (on a scale of 0 to 3) in three areas: integrity, uniqueness and site complexity. Those sites scoring six or above were recommended for

additional work to evaluate their National Register Eligibility.

A problem with numerical ranking systems is getting the consulting parties to agree on the relative importance of the attributes. As a result, the Kentucky SHPO refused to accept the numerical system and insisted on looking at individual site descriptions to determine which sites would require further testing and evaluation. The value of the system was that it clearly documented the manner in which the recommendations for further work were made and provided the District and SHPO a basis for accepting or rejecting the consultant's recommendation for each site. For the District, the ranking system provided a baseline for negotiations and significantly reduced the number of sites considered for further evaluation.

Such ranking systems are most useful on large survey projects involving large numbers of sites. The District never used the ranking system again because subsequent projects were much smaller and involved relatively few sites.

The District has completed several evaluation and mitigation projects at Paintsville Lake, Yatesville Lake, Robert C. Byrd (formerly Gallipolis) Locks and Dam and Winfield Locks and Dam which enables comparisons to be made between site significance evaluations and actual excavation results. From these comparisons, the following generalizations have been observed:

1. High density multicomponent sites are sometimes too disturbed to provide accurate chronological and cultural data.

The Winfield Locks Site (Hutto 1967; Hand et al. 1988) had prehistoric temporal components ranging from Early Archaic to Late Prehistoric. Hutto (1967) had collected over 500 projectile points from the site and the site was known for its concentrations of Woodland pottery. The site was excavated prior to construction of the new Winfield Lock (Hughes and Niquette 1992). While excavations uncovered 309 features, 4907 pottery sherds and 126 projectile points, only six features could be radiocarbon dated. While some of the features lacked adequate charcoal for dating, the majority of the other features were rejected because coal finds were present or the features contained a mixture of Archaic and Woodland material (Hughes and Niquette 1992:199).

Because of this cultural mixing, specific problems in the research design, such as environmental change from Late Archaic through Woodland and the development of horticulture, could not be adequately addressed.

2. Low density sites can provide accurate chronological and cultural data. These sites are numerous, but it is difficult to determine which sites have adequate chronological and cultural data to address project research objectives.

Low density sites such as Niebert (Clay and Niquette 1989), Woods (Shott 1989, 1990) and Parkline (Niquette and Hughes 1991) average roughly one feature per 100 square meters. Because of the low density, the sites are most cost effectively evaluated by mechanical stripping. These three sites contained Late Woodland components which produced datable single component features. Data from these features enabled investigators to define a Woods Phase (Shott 1989, 1990) and a Parkline Phase

(Niquette and Hughes 1991). Investigators were also able to document the shift from horticulture to corn based agriculture (Shott 1989, 1990) and to document the introduction of the bow and arrow into the Kanawha Valley at approximately 700 A.D. (Niquette and Hughes 1991). The unexpected discovery of ritual structures at Niebert provided the basis for reconstruction of Middle Woodland (400 B. C. to A. D. 400) mortuary ritual in the Gallipolis Locks and Dam vicinity (Clay and Niquette 1992).

3. Problem oriented research designs should not be overly restrictive and should be permitted to evolve as the project progresses.

Over the years there has been a tendency toward very specific, problem oriented research designs for mitigation projects. This is good but can be carried to an extreme where contractors cannot adjust excavation strategies and research priorities without going through an elaborate chain of command involving agency officials, SHPO's and sometimes the Advisory Council.

One solution to this problem is to use small field crews over an extended field season so strategies can be phased and adjusted based on collected, rather than expected, data.

When large crews are sent into the field for short periods of time so construction deadlines can be met, there is little opportunity for adjusting strategies or priorities. This often leads to major expenditures of funds for disappointing results which add little to our knowledge of the area investigated. These reports often include an elaborate section attempting to justify how all aspects of the scope of services and research design were met.

4. Level of significance (national, regional or local) is important in developing appropriate evaluation, preservation and mitigation strategies.

The vast majority of CRM work involves sites of local significance. Because of their nature and abundance, evaluating sites of local significance is often more difficult than evaluating sites of regional or national significance. It is often much easier to justify redesigning a project or eliminating project alternatives that involve sites of regional or national significance because of the expense of mitigation.

If sites of local significance are going to be impacted and the agency is agreeable to mitigating the sites by excavation, there may be a tendency to negotiate Consensus Determinations of Eligibility and Determinations of No Adverse Effect without giving due consideration to site preservation alternatives and impacts to the overall archeological data base.

5. An understanding of site formation *and* impact processes is important in evaluating site significance.

Knowledge concerning (a) the processes and agents which impact archaeological sites, and (b) the geomorphological and cultural aspects of site formation, will aid in determining the potential integrity of the site. A good example of the latter is site 15LA14 in Yatesville Reservoir which was recorded in a garden by Duffield and Heffernan (1974) and tested by Fenwick (1976:44). The site was again tested by Niquette and Donham (1985:182) and found to have consisted of material redeposited by

the landowner from further up the hollow.

An understanding of site formation processes is particularly important in evaluating multicomponent sites identified as *sheet middens* or *midden mounds*. Niquette and Kerr (1989:157) present three alternative interpretations of the Dow Cook Site (15LA4) due to the "*uncertainty regarding the effects of post-depositional factors.*"

## SUMMARY AND CONCLUSIONS

Over the past 20 years, the District's process for evaluating archeological site significance has evolved from unsystematic surveys, resulting in the identification of burial mounds and village sites, to systematic surveys, including a full range of smaller sites. These sites are evaluated in terms of regional research designs that stress chronological control and site integrity.

The success of the program is due in part to the continuity of the staff which allowed for the development of regional bibliographies, databases and research designs over long periods of time. The program has been successful in keeping the District in compliance with its legal obligations as well as making significant contributions to the published archeological literature and to the preservation of the archeological resources.

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## **SOME COMMENTS ON SIGNIFICANCE AND FEDERAL ARCHAEOLOGY**

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### **ABSTRACT**

Debates concerning significance in archaeology have helped establish the problem-oriented research design as the major way in which CRM projects are structured. The major drawback of this approach is that it fails to insure the survival of an archaeological database for future research needs. It is argued that saving a representative sample of sites of all types and ages is more defensible than evaluating and managing sites based on contemporary research questions. Preserving units of space would serve to insure preservation of a representative sample of archaeological sites, and can be undertaken in conjunction with other resource needs. Potentially significant sites located on federal lands should not be evaluated unless absolutely necessary, but set aside for preservation and research needs.

### **INTRODUCTION**

No topic causes so much day-to-day, often rancorous, debate among archaeologists as the significance concept and attempts to operationalize it. This is due to several factors. The concept itself provides an arena for seemingly endless philosophical discussion. What does 'significant' mean? Is one 'kind' of significance more important than another? Can ideas of place, symbol, and meaning be encapsulated within a single definition of significance? The concept is equally ambiguous from a management perspective. How can we determine which particular sites are significant? Can actions dependent on significance decisions be implemented rapidly? How can we balance significance evaluations with other management needs? Scientific viewpoints on the matter often clash. At what level of context should significance be established? Are large, visually prominent sites inherently 'more significant' than small, less obtrusive ones? How disturbed can a site be before its significance has been irreparably compromised? All that is certain is that there are currently many more questions than answers where archaeological significance is concerned.

This is not necessarily an unhealthy situation. Debate provides a stimulus for further investigation, and research into such an important topic must surely be helpful in the long term. In the short term, however, there are major practical difficulties that must be faced. While debate goes on, management decisions are being made that result in the destruction of selected (or deselected) sites on an hourly basis. The need for critical discussion will not disappear anytime soon: the need for results from the debate is immediate, to help guide archaeologists and managers on the front line of decision-making

and implementation.

Proponents on various sides of the debate have not been silent, and many useful articles and rejoinders have been published concerning significance in archaeology (e.g., Barnes *et al.* 1980; Butler 1987; Fisher 1980; Glassow 1977a, 1977b; King 1985; Klinger and Raab 1980; Leone and Potter 1992; Lynott 1980; Raab and Klinger 1977, 1979; Reed 1987; Scott 1990; Sharrock and Grayson 1979; Smith 1990; Tainter 1979; Tainter and Lucas 1983). One notable outcome of these efforts has been the widespread adoption of the problem-oriented research design as a guiding principle in cultural resource management (e.g., Raab and Klinger 1977, 1979; Schiffer and House 1977), especially in establishing the direction of post-survey fieldwork. Put simply, this means that sites are identified as significant based on contemporary research questions. Sites thus designated are either avoided or mitigated (usually by excavation), while others are considered to be expendable and are often destroyed. Excavation strategies and recovery methods used at significant sites are also formulated based on contemporary research objectives.

The major problems with this approach have been outlined by Sharrock and Grayson (1979) and Dunnell (1984). Individual archaeologists cannot possibly be well-versed in all contemporary research orientations; add to this the responsibility for preserving a viable database for all future research needs, and the limitations of the problem-oriented approach become evident. Also, many sites are in fact chosen for mitigation not because they are ideally suited to address current research questions, but because they are in the way of development. In such cases, hastily formulated (and often very broad) 'problem-oriented' research designs are draped over the appropriate federal forms in order to meet the demands of the system and enable excavation to proceed. The resultant data are then used to address the stated problems, regardless of their suitability in that regard.

Despite these obviously serious drawbacks, the problem-oriented research design remains the primary manner by which cultural resource management strategies are planned and executed, often with the uncomfortable (and usually implicit) acknowledgement that while it may not be perfect, it remains the best method currently available for providing some scientifically-defensible direction to CRM. The fact that we are systematically biasing the archaeological record by relying solely on contemporary research questions (Dunnell 1984) demands that we continue to rethink our reasons for determining any particular site to be significant or non-significant.

### **THE PRINCIPLE OF REPRESENTATIVENESS**

While it is recognized that sites can be significant for many different reasons, including humanistic ones (e.g., "ethnic significance" - Doyel 1982; Raab and Klinger 1977; Reed 1987; Schiffer and Gumerman 1977), archaeologists, as trained specialists, are most qualified to give opinions concerning scientific significance (it is at present unclear who should have the authority to speak where other aspects of significance are concerned). However, even trained specialists cannot be well-versed in every aspect of scientific archaeology. A site may be particularly well-suited for the extraction of phytoliths, or for examining vertical displacement of artifacts due to bioturbation, or for producing lithic artifacts amenable to blood residue analysis. However, few field archaeologists

would be able to recognize the conditions necessary for such specialized endeavors. Even if the unknowable needs of future archaeologists are ignored, it is evident that no individual can be expected to master the details of every theoretical and technical approach currently available to the discipline.

There appear to be at least two possible solutions to this problem. The first is to have a team of trained specialists (geomorphologist, soil scientist, palynologist, ethnobotanist, etc.) give their input on every survey and excavation project (Schiffer and House 1977; Tainter and Lucas 1983). This is obviously impractical. The second solution is much easier, can be applied by any competent archaeologist, and serves to provide a basis for addressing both contemporary and future research needs. That solution involves preserving a representative sample of sites of all types and ages within all physiographic settings (cf. Binford 1964; Glassow 1977a; McMillan et al. 1977), thus insuring a database suitable for a very wide set of objectives.

Implementing this solution may not be easy, but it is possible. The role of the field archaeologist under such circumstances is to recognize archaeological and geomorphological variability and to classify that variability in such a way that sites can be 'typed'. Representative examples of each type can then be preserved, with the major remaining problem being how to determine how many of each type to preserve. This may be addressed by maintaining a database amenable to manipulation via GIS or other computer storage and retrieval/modeling systems.

This is not to say that archaeologists should not continually upgrade their knowledge about the various theoretical and technical approaches available to the discipline. Classifying elements of the material record is a difficult process, especially given the limited data typically produced in the shovel-test surveys so often resorted to on federal lands. Constant upgrading of knowledge is necessary to insure that many different sources of variability are in fact being recognized and accounted for. Obtaining input from other specialists such as soil scientists and geomorphologists (not to mention other archaeologists) will always be of value in this regard. It is also recommended that, especially in the early stages of an archaeologist's work in any particular area, a generous attitude be taken where the number of sites to be preserved is concerned. Unrecognized variability may be fortuitously preserved if enough elements of the material record are set aside for other reasons. Management perspectives in this regard should be couched in the long-term; in many cases significance evaluations can be changed if enough evidence accumulates to show that a more-than-adequate sample of any particular site type has been preserved.

It should also be noted that this approach does not preclude saving and/or investigating sites for other reasons. Contemporary research problems, ethnic concerns, and other agendas can be accommodated on a site-by-site basis as the need arises.

### **SAMPLING SPACE**

The power of the approach suggested above is that it can be implemented now using current field methods, and that it entails no significant changes in the structure of present management systems.

The main weakness is that some important sources of variability may be not recognized or may be otherwise missed even if a great number of sites are saved. It is also open to criticisms raised over focusing on the site as the unit of inquiry/management (Dunnell 1992; Dunnell and Dancey 1983; Foley 1981; Kerber 1993).

A solution to these problems is to sample and preserve units of space rather than individual sites. This was first suggested by Lipe (1974; cf. Tainter 1987), who recommended setting aside "archaeological preserves" - blocks of land within which all artifactual remains would be preserved and/or further studied. The blocks of land would be chosen so as to provide a representative sample of microenvironments in a given area. As this approach would presumably act to preserve a representative sample of the archaeological record, it meets the requirement of satisfying contemporary and future research needs, and has the added benefit of preserving 'non-site' elements of the archaeological landscape.

While implementing such a scheme would require significant changes in present-day management practices, the changes would not be out of line with contemporary thinking. Lipe pointed out that preservation of units of space could serve other resource needs besides archaeology: wildlife, soils, and so on. This concept of integrated resource management has recently been given a great deal of attention under the rubric of 'ecosystem management'. Setting aside parcels of land is one way to address concerns about biodiversity, threatened and endangered species, watershed quality, etc. Input from specialists of several disciplines would be needed to formulate the sampling scheme in order to insure that all needs were being addressed: microenvironmental variability for archaeologists, monitoring locales for ecologists, and so on. Such an association would help to more fully integrate archaeology with related disciplines through the exchange of knowledge concerning soils, physiography, hydrology, geology, and site occurrence. The correlation of parameters considered important within the separate disciplines can be achieved with relative ease through the use of GIS. A joint approach would also create a cadre of dedicated conservation specialists capable of acting in concert in response to the growing demands being placed upon the land. While it is doubtful that such a scheme could ever be made to work in a private contract setting, there is no reason why it could not be successfully implemented on the large landholdings of the federal government.

## EVALUATION NEEDS AND PRIORITIES

The increased enforcement of preservation laws has led to an increase in archaeological fieldwork on much federal land (e.g., Peacock 1994). As a result, thousands of sites are now being recorded annually. Due to the sparse data provided by shovel-test surveys, final evaluations of significance are not being made for many of the sites. Rather, many are being classed as 'potentially eligible' for inclusion on the National Register of Historic Places and preserved. This so-called 'surplus' is viewed as undesirable by some who feel that sites should be evaluated as quickly as possible. For example, a comment recently made at a regional SHPO meeting was that *"flag and avoid is not management - it is the avoidance of management."* It is at the state level that such complaints are most frequent and most warranted, due to fears that information filing systems will become clogged with an

unmanageable number of potentially eligible sites, with no solid provisions being made as to when, how, or by whom those sites will eventually be evaluated (e.g., McGahey 1994). Two main points need to be emphasized in the face of such concerns: 1) an enormous amount of evaluation is taking place before the excavation stage; and 2) the pressure to 'clear' sites for non-archaeological purposes is not nearly as great on most federally-managed land as it is in the typical CRM setting.

Phase I-level surveys as presently carried out on federal lands are in reality very efficient management tools. They are rapid, relatively inexpensive, and clear large parcels of land for other activities. Most archaeological sites being recorded are not considered eligible for inclusion on the National Register: for example, 75 to 80 percent of the sites being recorded on the National Forests of Mississippi are considered ineligible based on information retrieved from shovel-test surveys (Peacock 1994). Evaluating and dismissing 75 percent of the archaeological record at the Phase I level can hardly be considered 'avoidance of management', and it is difficult to see how excavation programs, which by nature are slow and expensive, could significantly increase the rate at which evaluations are being made.

The growing pressure in many states to address the 'backlog' of potentially eligible sites by rapid testing procedures is unfortunate. When sites are tested and found to be ineligible, they may then be destroyed with impunity. If they are deemed significant and mitigated by excavation, they are likewise being destroyed. As noted above, excavations and subsequent evaluations reflect contemporary research methods and orientations. If the majority of sites are dismissed as ineligible at the Phase I survey stage, then it makes sense to treat the remainder as a database for future research needs, rather than excavating them for the sole purpose of evaluation.

Preserving sites in place is often impossible where construction projects are concerned; however, it is relatively easy on large federal tracts where typical impacts include grazing, forestry, and other activities that can be controlled without considerable difficulty or financial loss. On National Forests, for example, sites can almost always be avoided during logging operations with minimal effort. The monetary value of timber located on sites is typically very small compared to that from the rest of the timber in a sale. Trees can often be removed from sites using low-impact techniques such as directional felling and cable winching. A certain amount of standing timber must be left as 'inclusions' within sale areas: site locales can often be used in that regard, thus serving two purposes at once. Many site locales (especially those of historic house sites) support unusual stands of trees and other vegetation (clustered cedars, very large oaks, Osage orange, mulberry, privet hedge, yucca, etc.). Preserving such locales helps to sustain biodiversity within federal landholdings.

Sites on large federal landholdings should not be excavated simply for the sake of evaluation, since evaluations are based on contemporary research questions. Rather, the mandate for evaluation should be seen as a driving force for developing new field and analytic procedures. When sites on federal land are excavated, it should be as the result of innovative, detailed research designs. The sites should be chosen based upon their potential for yielding results appropriate to those designs. Research involving non-intrusive or low impact field methods such as magnetometry, resistivity, soil chemistry analysis, microartifact analysis, ground-penetrating radar, and so on should be encouraged, and accompanied by limited testing aimed at corroborating and refining those methods.

## CONCLUSIONS

The topic of archaeological significance remains one of the most complex subjects being debated within the discipline. The debate has an immediacy not associated with most topics, since the subject of the debate is disappearing with ever-increasing rapidity. The federal government, as steward of large landholdings, has a special mandate to insure that decisions being made concerning significance are as responsible as possible: responsible to the future as well as to the present.

Problem-oriented research designs have served as a means of insuring that at least some high-quality, systematic archaeology is carried out ahead of development and other impacts. Such designs still represent the best approach to be taken on individual projects within the private CRM sector. On federal lands, however, the option of preserving sites based on representativeness is available and is much more attractive from a scientific standpoint, as it preserves a resource base for current and future research needs.

Preservation, and research to support and refine it, should be the main aim of archaeology on federal lands. The accumulation of potentially eligible sites does not represent a problem, but insurance for future developments within the discipline. These sites should not be excavated *en masse* using contemporary methods. Rather, as many as possible should be preserved in place, with excavation only taking place at a few chosen for their potential to clear up major research questions in the area or to aid in the development of non-intrusive methods. The data produced from excavations should be used to examine the site-sampling scheme for any particular area, to insure that a truly representative sample of sites of all types, ages, and place of occurrence will be saved.

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**SIGNIFICANCE WORKSHOP**

{APPENDIX A}

*SIGNIFICANCE WORKSHOP AGENDA*

**SIGNIFICANCE WORKSHOP AGENDA  
3-4 OCTOBER, 1994**

Monday, October 3

Individual Presentations and Discussions

8:30am	Introduction, Roger Hamilton
8:45am	Fred Briuer
9:45am	Carroll Kleinhans
10:45am	Break
11:00am	John Schelberg
12:00pm	Lunch
1:30pm	Skipper Scott
2:30pm	Break
2:45pm	Bob Maslowski

Tuesday, October 4

8:30am	Horace Foxhall, Jr.
9:30am	Fred Limp
10:30am	Break
10:45am	Evan Peacock
11:45am	Lunch
1:30pm	Summary and Evaluation Seminar