

THEORY AND THE FUTURE OF HISTORICAL ARCHAEOLOGY ON THE GREAT PLAINS

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When the Plains Conference was young, historical archaeology was well represented through the patently anthropological direct historical approach. The post-war boom in Plains archaeology derailed the development of historical archaeology out of this anthropological base. While during the past several decades the field has matured into a securely anthropological pursuit, the Plains was notably silent in the theoretical debates that led to this maturation. Now, there is a groundswell of interest in historical archaeology on the Plains. In order for this resurgent interest to have the greatest value, our relatively limited resources must be focused on questions and sites that will profoundly expand our knowledge of the past. To make this happen, theory must reemerge as the basis for decision making in Plains historical archaeology. Further, the infrastructure of historical archaeology must expand to train professionals and to lead to the establishment of research priorities unconstrained by development pressures.

INTRODUCTION

In 1985, at the 50th meeting of the Society for American Archaeology, I presented an overview of the development of historical archaeology on the Plains (Lees 1985). I concluded that the intellectual foundation for historical archaeology in this region was established with the emergence of the direct historical approach during the 1930s (cf. Strong 1940, Wedel 1938). A deductively, problem oriented method that developed out of anthropology, the direct historical approach offered historical archaeology a trajectory somewhat different than found elsewhere at the same time where the field was more closely allied with history than with anthropology.

The onset of a major archaeological emergency following World War II (the reservoir salvage) upset this trajectory by overwhelming an emergent professional community on the Plains. The efforts of a handful of archaeologists was spread increasingly thin as a result. Historians began to call the shots in terms of historical archaeology, and deductive anthropology was replaced by inductive research that served in some cases only to illustrate history; in many cases historical archaeology became the proverbial "handmaiden to history." The end of the interagency salvage work in 1969 found Plains historical archaeology floundering at a time when the "new archaeology" was gaining momentum. Lacking a convincing theoretical orientation, the Plains continued to languish as historical archaeology grew and matured elsewhere.

As we celebrate the 50th Plains Conference, historical archaeology has matured as a discipline within North American anthropology and, on the Plains, it is enjoying a renaissance of sorts. These developments are positive and the many individuals, both on the Plains and elsewhere, who have helped them come about are to be commended. There are nonetheless problems that cast a

pale over the future of historical archaeology in our area and that ultimately relate to the development and consumption of theory. These problems relate to academic and professional structure that grew out of the historical setting briefly reviewed above, but also relate to a fierce competition for cultural resources coming from development and recreational interests. The approach to our future must be both efficient and dynamic and must be based on a healthy theoretical arsenal that will allow the best use of a resource base that is at risk and with professional resources that are at best limited.

In this paper I will present my perspectives on theory and the future of historical archaeology on the Plains. Specifically, I believe:

- Our resource base is threatened, and we are losing the sites that count the most.
- Our approach is overly inductive, and we are not asking the questions that count.
- Our infrastructure is weak and is not capable of dealing with our current needs.

Simply put, because of these problems the Plains is lagging in terms of the application of theory developed elsewhere to Plains problems and the application of Plains data to questions of broad anthropological interest. The Plains is as a result relatively silent in a growing theoretical discourse on the anthropology of the historic period.

It is unlikely that we will see significant movement in certain problem areas outlined above, but our approach to these problems can change for the better. I would argue that our theoretical approaches must evolve to address these problems and to ensure that research on historic sites contributes in a lasting and substantive way to our understanding of the anthropological history of the Plains.

RESOURCES AT RISK

Twenty years ago, in 1972, Charles McGimsey challenged the profession when he wrote that:

The next fifty years -- some would say twenty-five -- are going to be the most critical in the history of American archeology. What is recovered, what is preserved, and how these goals are accomplished during this period will largely determine *for all time* the knowledge available to subsequent generations of Americans concerning their heritage from the past [McGimsey 1972:3].

Although it may not be happening as rapidly as McGimsey predicted, that we are suffering rapid site denigration and loss is not at issue. At the same time, a focus on specialized studies in the last several decades has increased our ability to learn from a shrinking resource base. Further, the presence of deeply buried and supposedly well preserved resources have been documented throughout the Plains in potentially impressive numbers. These developments temper but do not erase McGimsey's prediction for the future of our resource base, at least for those resources from the prehistoric past. I'm less confident that they apply to the bulk of the historic resource base, and am pessimistic about what the future holds.

McGimsey's concern developed out of observations on sites visible on the surface; those that are easily impacted from agriculture and development projects. Most historic sites are of this type and as a whole are being rapidly lost through these processes; but this is nothing new. What is disturbing,

however, is a growing threat to historic sites from hundreds of serious hobby collectors armed with sophisticated research skills, high-tech metal detection equipment, and perseverance. These individuals are identifying site locations through archival and informant research, are finding the sites through exhaustive survey work, and are systematically mining sites of their artifacts. Sites that are targeted by these individuals are diverse in range, but from observation I know include military sites, contact period and later Native American village and farmstead sites, missions and agencies, Santa Fe and Oregon trail campsites, etc. We are competing fiercely for these sites, and we are losing.

I've introduced this not because it will be a revelation and not to condemn the metal detector hobbyist. Rather, I believe we need to rethink our conservation ethic that sees archaeological research as an adverse impact to be avoided in favor of putting sites in the "bank" for future research. We've been putting sites in the bank for years but, unfortunately, everyone knows the combination to the vault. I would suggest a better analogy for conservation would not be the bank but the Kentucky Derby; our horse is nearly dead and it is time we find a new one.

Our new horse must have several characteristics if it is to be a winner. First, it must be smart enough to teach. We need to work with hobbyists and convince them of the importance of segregated, cataloged collections; of the importance of spatial information; and of the importance of sites in general. Further, if we can provide regular opportunities to be involved in controlled research we may develop a body of metal detectors satisfied to channel their efforts towards science. We've done this with amateur archaeologists with great success, but have yet to reach the different audience represented by the metal detectors. We need to build coalitions between archaeologists and hobbyists so that we may benefit from their research and use their technical skills to our advantage. Finally, we can no longer fool ourselves into thinking that by avoiding sites we are saving them for our future. There is no historic site that is not threatened and, except for a few instances, there is no bank. Excavated sites should be selected for their research value and all research should be conceived as salvage.

THE RIGHT QUESTIONS

Viewing research as salvage does not mean that it should involve atheoretical warehousing of collections. To the contrary, salvage requires efficiency and efficiency in archaeology requires a theoretical perspective that is sophisticated and to the point. In conceiving of research we must be able to ask the questions that will allow us to respond to others who may ask: what did this research tell us that we did not already know and that we wanted to know. We must focus on sites and questions that will profoundly expand our knowledge of the past.

In discussing research, it is important to recognize that most is being conducted within a compliance or CRM framework and that a relatively small percentage occurs outside this realm. In compliance settings, research is related to the National Register of Historic Places. A body of theory, which I will call "significance theory," has developed that seeks to relate research to the concept of eligibility for listing a site on the register; a site that is eligible is significant. Current views hold that the concept of significance is a relational or relative concept and that significance is not inherent in sites. Site significance is thus determined by its relation to something else, and in most cases this is supposed to be context statements presented in state plan documents. This succeeds, however, only when these contexts codify problem domains or research questions that are of legitimate interest and firmly founded on current theoretical formulations. It is clear from many significance justifications

and data recovery plans that we are often on very shaky ground in this area.

There can be no doubt that compliance research is vital to Plains archaeology in that it offers our major funding opportunity. Sites so excavated may not be the first to come to mind in terms of research need, but are nonetheless of long-term importance in developing a broad, comparative data base that is currently lacking in most areas and for most periods of history. Here, we must recognize that many of the most vital questions posed by archaeologists have resulted from inductive observations made from a comparative data set.

Research conducted outside of a compliance setting, on the other hand, has the potential to provide leadership in developing programs to identify and address important research questions. By being able to broadly consider the resource base, research in this area can target sites based on their potential to profoundly expand knowledge in a way that compliance archaeology may never be able to. It is here that we have the potential and the responsibility to firmly engage anthropological research and to seek results of the widest merit and application.

I stress again that we must focus on sites that will *significantly expand knowledge*. I have already mentioned the crisis of site preservation and will next review the limited resources that we have to work with. Detailed investigations of sites outside the CRM setting is a luxury that must be approached with the utmost wisdom and forward thinking.

FOCUS ON INFRASTRUCTURE

It is a fact that the Great Plains lags behind most other regions of the U.S. in terms of the number of practicing professionals and in the strength of academic programs in archaeology. Given the challenges facing Plains archaeology in general, then, none of us are in the best of worlds. But when we look at historical archaeology the problem is magnified to such a degree that it is a wonder that anything happens at all. There is a very clear crisis in infrastructure that perpetuates the post World War II crisis in historical archaeology on the Plains and that fails to recognize historical archaeology as an anthropological pursuit.

Nonetheless, historical archaeology in our region is increasingly required in compliance and interpretive settings. This development is the result of a growing and appropriate inclusion of historic sites of all periods under the compliance umbrella and an increased recognition of the interpretive potential of archaeological research. This, however, is generally a development emanating from outside the Plains and has caught many Plains states off-guard because of their lack of trained historical archaeology professionals; this has created a setting not unlike that of the reservoir salvage period. The result is an increasing number of cross-over professionals, many of whom are doing admirable jobs but most of whom have no background in historical archaeology including the theoretical base that identifies it as patently anthropological in orientation.

It is important, however, to look at our academic infrastructure in light of this development. There is a growing interest and need for historical archaeology on the Plains and elsewhere and, further, some expertise in historical archaeology is recognizable as a need of any professional working in agency or private sector compliance settings as well as those involved in research, museum interpretation, and teaching. Most academic programs, however, pretend this is not so and continue to graduate professionals as if the world belonged to the specialist and where specialists are never

historical archaeologists. The general lack of faculty and academic programming in historical archaeology poses several serious problems for the Plains.

The first problem concerns the somewhat insular development of professionals and research. The lack of professionals serving as mentors presents an obvious problem for those seeking to specialize in historic sites, and gives a signal of the non-importance of that area for those who could benefit from some exposure. A related problem is the lack of sufficiently trained professionals to handle the work currently available. Attractive projects are very often done, because of the lack of other options, by individuals with no background and who go into the project with an incredible handicap and with a very limited theoretical orientation.

A related problem is the lack of faculty to pursue directed research programs in historical archaeology through their own work and through thesis and dissertation work of their students. There can be no doubt of the importance of such research on the development of theoretical approaches to specific regions or problems. While there is no necessary reason that this has to be accomplished in an academic setting, leadership for such a program is perhaps best vested there.

I am not suggesting that every academic program on the Plains go out and hire an historical archaeologist, but that on the Plains we do need to provide increased opportunity for such training, where that training is anthropological, and where directed research programs can flourish. I would add that universities are missing the boat in terms of capitalizing on student interest, grant funding, and ability to build an anthropological bridge between other departments in terms of research and teaching opportunities.

THEORY AND THE FUTURE

The potential of the Great Plains to contribute to anthropological theory and to the anthropological knowledge of the historic period should be apparent. Over and over again, however, this very tenet is being questioned by those who do not understand that we are not seeking a redundant history of things trivial or things already known. Given some of the problems I have outlined, however, this questioning is not surprising because we have very little to point to that is different from this stereotype. Plains historical archaeology very clearly needs to break out of this stereotype and reconnect with the pursuit of anthropological knowledge. There are some very compelling reasons to do so:

First, the Great Plains contains many, many historically disenfranchised groups; those groups for which archaeology is the primary or only source of historical and anthropological information. A concerted research focus on African American slave life has resulted in a geometrically expanding body of knowledge that has not only written a culture history previously lacking but has provided the data for addressing questions of general anthropological interest. We have the same potential on the Plains and I believe we must increasingly identify and target similarly disenfranchised groups and build an anthropological history that will not exist otherwise.

From my own interest, the Native American groups that were resettled to Kansas in the 1830s from the Great Lakes region present a perfect example. Known primarily from scattered missionary and traveler accounts, the farmsteads and villages occupied by these people prior to the Civil War hold data that can write an anthropological history of their lives that can be reconstructed in no other

way. To me this has profound value. This value is increased dramatically when a comparative approach is taken between the Shawnee and Ottawa, for example, and between Ottawa life prior to and after removal to Kansas.

Second, the Great Plains offers some of the most profound situations of cultural diversity and change to be found anywhere in North America and particularly during the 19th century. The comparative, anthropological laboratory offered in the Great Plains is an incredible resource and offers unique potential to evaluate and expand theories of consumerism, acculturation, and ethnicity -- to name a few examples -- against the patently multicultural and rapidly changing fabric of the 19th-century Great Plains.

If historical archaeology is to improve its contribution to the anthropological history of the Great Plains, it must fully engage an anthropological approach and current theoretical discussions. We appear caught in a circular trap, however, because this may not happen without change in infrastructural commitment to historical archaeology which may, in turn, not come about because of the current lack of a truly anthropological approach in Plains historical archaeology. But while this circle remains in place, the resource base continues to degrade, good research opportunities are lost, and the true contribution of the Great Plains to the anthropology of the historical period continues to languish. To be sure, significant contributions will continue to emerge from the Plains, just as they are now. But if we choose to consciously reintegrate historical archaeology into the framework of Plains anthropology, the breadth and vitality of these contributions to the substantive and theoretical knowledge of the historic past will certainly blossom.

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