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New Directions? Great Basin Archaeology in the 1990s

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OVER the last 25 years the discipline of archaeology has grown, both in its scope and in the number of its participants. Along with this growth have come a number of notable changes, particularly in method, many of which were accomplished by Great Basin archaeologists. So significant and lasting have been these contributions that Dunnell (1984:491) suggested that, although the Southwest was continuing to lead the discipline in efforts of general significance, its pre-eminence was being seriously challenged by the creative work of Great Basin scholars.

To its list of paradigmatic changes, the new archaeology of the late 1960s advocated a systems approach, a positivist scientific approach, and a theoretical perspective of ecological functionalism articulated most clearly in Steward's cultural ecology. Thus the Basin, which already had a history of this approach, became a prime laboratory for experimentation and the testing of new ideas. But while evolving and advancing, processualism by the 1970s was no longer accepted as the universal or best theoretical position in archaeology, since many in the discipline began to abandon the ecological approach in favor of the more "satisfying" perspectives found in Marxism, structuralism, and symbolism, summarily referred to by Hodder (1985) as "post-processualism." At the same time post-processualist rhetoric was first being espoused, however, Basinists were committing themselves even further to ecology; approaches such as ethnoarchaeology and optimal foraging theory, for example, provided a better understanding of

hunter-gatherer behavior and allowed for the creation of more refined models of hunter-gatherer adaptation.

With the close of the 1980s, Basin archaeologists continued to develop ecological explanations while throughout the discipline others moved in radically different directions. Although we do not believe post-processualists to be the majority, the approaches falling within this paradigm certainly have achieved a high degree of visibility. Processualists claim that the popularity of post-processualism is fading while post-processualists believe they are gaining ground. The effect of this theoretical debate will be felt by all culture areas in the discipline, the Basin included. We have no way, however, of foretelling what these effects will be and what the future has in store for Basin archaeology. Still firmly grounded in the processualist camp, we offer our thoughts on the direction we think Basin archaeology *should* take in the next decade.

PROSPECTS FOR THE 1990S

No issue stands before Great Basin archaeology at the beginning of the 1990s with such clear potential to enliven archaeological research and debate as did the testing of Jennings' (1957) Desert Culture model at the beginning of the 1970s. Grounded in the new archaeology, tests of the model brought about fundamental changes in research design, seen most markedly in a shift from single-site to regional studies. It brought cultural-historical reasoning into competition with processual models: functional inferences were sought along with chronological

ones. Archaeologists stopped thinking of the prehistoric record as a relatively invariant phenomenon as they recognized the range of adaptive modes expressed over time and space in the Great Basin. The consequences of this research program were felt throughout the discipline of archaeology.

Reviewing this episode of research, one sees clearly how method and theory interact with a substantive issue of broad scope and merit. Thus, it is difficult to isolate any one component of the archaeological enterprise for discussion insofar as each plays an important role in one's research. In our discussion we will address three general components of archaeological research: substantive issues, method and middle range theory, and the dissemination of professional work.

Substantive Issues

The substantive problems that will be addressed over the next decade are ones that are presently under study, given the normal research cycle of five to ten years. Serendipity will also no doubt play some role in the substantive issues to be addressed in the 1990s as it did in previous decades. For example, largely as a result of the discovery of the Dietz Site (Fagan 1988; Willig 1988, 1989), the pre-Archaic record has been reconsidered in a more synthetic manner (e.g., Willig et al. 1988). Similarly, exposure of a remarkable record in the Stillwater marshes (e.g., Raven and Elston 1988; Raymond and Parks 1990) will add substantially to our understanding of marsh use and the late Archaic. Perhaps the most intriguing of these discoveries, however, is that of late prehistoric alpine villages in the Toquima and White mountains (e.g., Thomas 1982; Bettinger 1991), a phenomenon nearly completely outside of the reported ethnographic pattern.

Not being omniscient, however, we cannot anticipate how new discoveries will move our field in the 1990s. Rather, we can point to two

broad research questions that deserve closer attention. While these are not intrinsically more important than any other question of Great Basin prehistory, they help to illustrate some of the liabilities we see in current methodologies, and particularly, what we see as a continuing failure to take on the record as it actually comes to us.

Archaeology of the Contact Period. The first of these research questions concerns the archaeology of the contact period. Face-to-face contact with Europeans dates to the mid-18th century in the Basin; however, indirect contact may have occurred earlier. As a number of researchers have demonstrated in many New World contexts (e.g., Crosby 1972; Dobyns 1983; Ramenofsky 1987), contact is tremendously important because its most marked feature is the introduction of epidemic disease. Apart from its inherent quality as a research topic, contact and epidemic disease are particularly important because of the reliance placed on descriptions of historic populations by both archaeologists and ethnographers in understanding aboriginal Great Basin populations. Mobile, small-scale foraging populations may be less affected by infectious diseases since these populations do not exist in the numbers or concentrations to sustain epidemics (Dobyns 1983; Ramenofsky 1987). If true, we might expect substantial continuity across the contact boundary in the Great Basin. However, if Great Basin peoples underwent a significant depopulation as so clearly occurred elsewhere in North America (e.g., Cook 1937; Crosby 1972; Buikstra 1981; Milner 1980; Dobyns 1983; Upham 1986; Ramenofsky 1987), then we must very seriously reconsider how descriptions of historic peoples are employed as analogies for pre-contact times. Thus, the question becomes not "when do Great Basin peoples assume the ethnographic pattern" but instead, "what adaptive changes were made to accommodate depopulation resulting from epidemic disease?"

C. Fowler (1982) suggested that, along with significant disruption of aboriginal subsistence practices during the 19th century diseases were, in fact, prevalent among native peoples in the Great Basin. These effects are quite obviously tied to Euroamerican expansion along the margins of and into the Great Basin. Yet there is nothing to preclude the possibility that diseases were introduced into the Great Basin long before this, perhaps at the same time epidemics were experienced among Californians (e.g. Cook 1937) or as early as the first smallpox pandemic suggested by Dobyns (1983:12), which he places in the interval between 1520 and 1524.

Admittedly, establishing the fact that diseases were introduced and had consequences for population levels is difficult enough in settings occupied by sedentary village populations. Even in those circumstances the matter of selecting valid measures of population size and configuration is problematic (see the extended discussion in Ramenofsky 1987). Such problems are exacerbated for circumstances like the Great Basin, where we have so few sensitive dating tools. Thus, while the contact-disease problem offers a rich opportunity to model adaptive responses to events that may have had pronounced effects on system dynamics, this may in fact be a frustrating issue. Still, it strikes us as germane to the implications that such studies might have on our analogic ethnographic base, and to understanding the relative lack of assemblages that date to the contact interval—the last 400 years.

Archaeology of The Late Pleistocene/ Early Holocene Period. The second issue we wish to consider is the focus of our own research, specifically the interval between ca. 11,000 and 8,000 B.P., variously referred to in the literature as Paleo-Indian, pre-Archaic, or early Archaic. As regards space-time systematics these terms imply more than just a list of temporally sensitive artifact traits. They also

include inferences about prehistoric adaptive strategies, and carry forward into contemporary discussions long-standing debates on such issues as the temporal relations of fluted and stemmed projectile points, subsistence specialization on extinct megafauna or, alternatively, on lacustrine resources, and initiation of the classic Archaic foraging pattern.

We submit that, while all of these traditional issues are of some importance, they have not been framed in such a manner where explicit testing is feasible. This is because the pre-Archaic record is probably very poorly represented in buried contexts, and if it does occur in those contexts, it lies in parts of the landscape that archaeologists have thus far investigated only sparingly. We, therefore, do not see in the near future excavations that will provide neatly stratified deposits demonstrating the sequence and duration of point types attributed to this period or that yield sufficient dietary evidence such that subsistence strategies can be clearly delineated. Elston (1986) and Warren and Crabtree (1986), among others, remarked that the pre-Archaic record is in fact a surface lithic record. As such, questions concerning the pre-Archaic must be tailored so as to have expectations for this record. This, of course, means that there must be a reconsideration of the approaches used to date the surface record, as we discuss below, especially given that it often does not exist as clusters, which we traditionally call "sites."

Issues of Method and Middle Range Theory

Two schools of thought currently dominate Americanist archaeology and will likely persist into the 1990s. The first includes the new theoretical orientations that Hodder (1985) labeled "post-processualist archaeology." Basinists, however, because of their close ties with cultural ecology, have for the most part not adopted post-processualist notions, and thus our discussion will focus on the second dominant

school of thought, the processualist paradigm. Processualism is and always has been strongly methodological; more recently, processualist methodology has been couched in terms of middle range research. Just as we see paradigmatic continuity into the 1990s we also see a continuation of problems in the methods arena. As models become increasingly particularistic they demand more than ever that we gain better control of the record, both in terms of how we collect it and how we analyze, interpret, and explain it.

Regional Research. The regional approach, which has become such an important part of Great Basin research over the past two decades, is one of the most visible consequences of shifts from normative to systemic thinking. The Basin was one of the first culture areas to witness truly regional-scale processualist investigation and thus for many of us it operates as the default methodology by which we gain information about relatively unknown parts of the Great Basin. And yet, Basinists still look for places to dig. Moreover, our prehistories are largely written in terms of deeply stratified sites. But if the focus of Basin archaeology is for the most part mobile hunter-gatherers, should studies really be limited to only a portion of the record, i.e., where subsurface concentrations of debris occur? In discussing the regional approach, we have selected two issues to address: surface archaeology and the nonsite or artifact-oriented approach.

Surface Archaeology. No one will dispute the value of what Thomas (1985) labeled the "high-information site." Rockshelters and deeply stratified open sites have sustained Great Basin archaeologists for decades and no doubt will continue to be sought for the simple reason that they provide contexts for preservation. Importantly, because the adaptational models used and that continue to be developed argue, centrally, about issues of subsistence, a high priority is placed on access to such preservation

settings. Yet such sites are rare and are seldom isomorphic with the distribution of archaeological phenomena. Here, then, is the great problem for surface archaeology: we still feel uncomfortable explicating prehistory solely in terms of the distribution of lithic tools and debitage over the landscape. A statement made by Clewlow (1968:1) still expresses, we believe, a pervasive opinion among Great Basin archaeologists.

It is fully recognized that surface archaeology can do very little to help establish the cultural history of the Great Basin. . . . Hopefully, the larger cultural picture will be drawn from excavations at stratified sites . . . containing similar artifact types in more meaningful association.

Probably no one would deny the potential for the disruption of tight spatial associations in an assemblage of stone tools exposed on the surface for thousands of years. A fair amount, however, has been written comparing the disturbance of surface and subsurface deposits, and these studies suggest that several factors are generally overlooked. First, "with relatively few exceptions, subsurface deposits originate as surficial ones and have been subjected to the disruptive processes characteristic of the surface as well as those unique to subsurface deposits" (Dunnell and Dancy 1983:269). Secondly, subsurface deposits can undergo as much, if not more, disturbance than surface deposits (see Wood and Johnson 1978; Schiffer 1987). Finally, it has been shown that lateral movement due to post-depositional disturbance is not as great as one might expect (Roper 1976; Trubowitz 1978; Lewarch and O'Brien 1982), and in fact, useful spatial patterning can be obtained, even under disturbed conditions when systematic surface collection techniques are employed (e.g., Binford et al. 1970; Redman and Watson 1970; Healan 1972). We can view the question of the integrity of the surface record from one of two positions, either: It's disturbed until

proven otherwise; or It's pristine until disturbance is shown to have occurred. Neither position is preferable in the absence of any actual evaluation.

Nonsite, Artifact-Based Survey. This method saw its first significant archaeological application in the Great Basin through the efforts of Thomas (1971) and Davis (1975) (but see also Dancey 1974). It continues to be practiced and certainly has been a central aspect of the survey designs under which we have operated (e.g., Beck and Jones 1988, 1990a). To a large degree the methodological debate over the efficacy of nonsite methods has moved out of the Great Basin as practitioners in other areas "try it out" (e.g., Foley 1981a, 1981b; but see Thomas 1988). It remains integral to many Basin research projects and is mandated for survey work on public lands. Yet it is our sense that among many archaeologists in the Great Basin there is a lack of interest in, if not growing unhappiness with, having to record all artifact loci.

Two factors may account for this. First, nonsite survey is costly, since to insure any reasonable level of recovery requires that survey be conducted at high levels of intensity (Wandsnider 1989). The 30-m. interval, the default standard for contract survey of public lands, can only really insure that a fraction of the small artifact clusters are found; it yields far from adequate samples of isolates in areas of low offsite artifact density. Even the 10-m. survey interval, which yields effectively a 10% sample of true isolates, may not satisfactorily represent rare items (Cowgill 1990), except perhaps very obtrusive isolates like stone features or millingstones.

Second, while we have the means to record and sample accurately from the low-density component of the surface record, we find little in archaeological theory or problem-oriented research that speaks to a need to evaluate this record. Most archaeological problems continue

to focus on issues of subsistence and settlement, which traditionally do not direct attention to parts of the landscape lying between sites. To warrant nonsite procedures, archaeological settlement theory must evolve into archaeological landscape theory.

A significant start in that direction is underway with considerations of hunter-gatherer mobility and technological organization (e.g., Binford 1978, 1979; Kelly 1983, 1988; Meltzer 1984, 1989; Leonard and Jones 1989; Torrence 1989; Beck and Jones 1990b). From these threads several relationships appear to have significant consequences for the formation of archaeological landscape records. First, there is a clear relationship between highly structured patterns of settlement, occupational redundancy, and the obtrusiveness of the archaeological signatures of occupation. When groups return to a particular place on a frequent basis, there accumulates considerable debris, dominated usually by lithic material resulting from tool manufacture and repair. As we move away from such spatially focal patterns, the opportunities to build dense artifact deposits drop.

Based on studies of contemporary hunter-gatherer land use, we expect that a very significant proportion of any landscape will witness highly dispersed patterns of use and consequently a dispersed archaeological record. Such factors may help to explain in part why it is that the record becomes visible as dense, stratified deposits after 4,000 B.P. in many parts of the Great Basin. If before that time resource procurement and settlement were less redundant spatially, perhaps distributed over a wider range of settings, the resulting record would not only be more dispersed but might lie outside settings in which deep stratigraphic deposits form (basin-edge rockshelters). Depending on the veracity of this argument, we may find that significant inroads to understanding the early Archaic record in the Great Basin will come through evaluation of low-density records.

Dating the Surface Record. Processual inquiry demands both the consideration of regional surface records and high degrees of temporal resolution; yet one of the major obstacles in surface archaeology, especially the offsite component, is dating. Two issues are involved here. First, efficacy is limited by the number of methods available for dating as well as the high level of error involved in those methods. Second, it is often impossible to transfer a temporal assignment, nevermind the inherent error levels involved, because of an inability to identify associations among artifacts (Jones and Beck 1992).

Let us consider the former issue. The most widely used approach is, of course, cross-dating using temporally sensitive artifact classes, such as trade beads (e.g., Bennyhoff and Hughes 1987), ceramics (e.g., Griset 1986), and projectile points (e.g., Hester 1973; Heizer and Hester 1978; Thomas 1981). Studies of the latter dominate, although some question the stability of the accepted typology and thus the chronological sensitivity of projectile point styles (e.g., Flenniken and Raymond 1986; Flenniken and Wilke 1989). If we set aside this criticism for the moment, we find several other troubling aspects of the cross-dating framework using projectile points. First, there still is no complete agreement as to the geographic scale at which temporal assignments can be appropriately used. This is best illustrated by the "east-west" debate of some years ago (e.g., Aikens 1970; Adovasio and Fry 1972; Bettinger and Taylor 1974; Holmer 1978) in which the main issue was whether there are separate temporal sequences for the eastern and western Basin or one sequence that is appropriate for the Great Basin as a whole. More recently Thomas (1981) suggests that some types may not be relevant even to areas as large as the eastern or western Basin but must be limited further in their application (see also Beck 1988). This problem persists both for lack of empirical

cases—few or no well-dated sequences of projectile points in some parts of the Great Basin—and for lack of good theoretical bases by which to understand the diffusion of stylistic and functional attributes (Beck 1988). In the end, this is a matter for empirical testing. But we should assume a priori that any particular sequence is only locally relevant.

The difficulties of typological cross-dating become even greater when one is dealing with the pre-Archaic period. Even though a number of different point morphologies are in evidence for this period (see Hester 1973; Heizer and Hester 1978), there is as yet no clear indication of their chronological positions relative to one another, or their durations (Jones and Beck 1992). As a consequence pre-Archaic assemblages are routinely attributed to very broad time periods, encompassing 3,000 to 4,000 years.

A related issue to that just discussed concerns how temporal units (e.g., phases, stages, or periods) are created and then used for periodizing surface sites. Despite stratigraphic evidence showing that several different point styles are in use simultaneously throughout the Great Basin (simultaneity as figured within the error of radiocarbon dating, not necessarily as in a single behavioral event), we tend to define phases on the basis of a single dominant point style (e.g., Bettinger 1989; Thomas 1988). This practice would suggest two linked premises. First, at any juncture in time only one style is in use. Consequently, with no subsequent disturbance, an archaeological sample of that age would contain only a single point style. Second, while there is presumably some period of overlap, when one style replaces another, that episode is brief and unlikely to be well-represented archaeologically. The implications are several for surface dating. First, sites that contain several point types must be considered mixed because they cannot be fit into any one period. To be seen as useful for any sort of

behavioral analysis, these sites must be further analyzed to determine if spatial clustering might prove useful in separating temporal components (see Jones and Beck 1992). Failing this, such sites are not useable for any analyses requiring that their temporal affiliation be known. But are such assemblages any less suitable than excavated assemblages in which as much variability is represented within a single stratum? Even if the above premise is accepted, is a palimpsest any less tractable analytically because it lies on the surface?

A more basic criticism of the phase-stage-period approach is that it tends to impose the view that cultural change is step-like, with long periods of stability and change occurring only at the boundaries between temporal units (Plog and Hantman 1990:440). This approach to chronology is highly normative and often may work against processual interests, obscuring variability that may be important for understanding the past (Plog and Hantman 1990:440; see also F. Plog 1974, 1979).

Often, of course, these types of chronologies are all that are obtainable for particular situations, but perhaps they could be refined somewhat by considering not just the dominant point (or bead or ceramic) style but the relative abundance of all styles represented and changes in this relative abundance through time and in different parts of the Great Basin (see Beck 1984, 1988). In this way it may be possible to develop more or less continuous time series analogous to stratigraphic series from sites distributed over regional surfaces.

The second major issue that must be addressed here is the dating of the low density artifact record. Anyone who has conducted nonsite archaeology has experienced the problem of providing some measure of chronological control for this record. In these settings, because association is so difficult to demonstrate, the temporal meaning of any time marker cannot be easily transferred to other artifacts,

which is the more usual case on sites. One response is to adjust the temporal argument, from one of inference concerning the age of an assemblage to an inference concerning the duration of use of a region. Such arguments rely not on the modal age of a time marker but on its duration (Jones and Beck 1990, 1992). What is needed in the 1990s is an expansion of the methods applicable to dating surface artifacts and/or the surfaces on which artifacts occur. There are a number of techniques that hold promise, some of which are still in the experimental stage (e.g., Beck n.d.), that might be developed in this direction.

One technique that has a long history in dating but that has received renewed interest in the past decade is obsidian hydration dating (e.g., Evans and Meggers 1960; Friedman and Smith 1960). Among the factors influencing the rate of hydration, geochemistry is the easiest to control for (see Hughes 1984), although recent research suggests individual source locations may have complex flow histories and thus variable chemistries (Hughes and Smith n.d.). Effective hydration temperature effects are more difficult to assess (Friedman 1976; Friedman and Long 1976; Trembour et al. 1988; Ridings 1991) although monitoring the present-day mean annual temperature at sites of interest provides one means by which to judge the comparability of samples. The added effects of humidity variation on surfaces need additional experimental study (Trembour et al. 1988; Mazer et al. 1991). Even with these cautions, studies to date suggest that obsidian hydration dating may be a more sensitive chronological instrument for surface assemblages than cross-dating (see Beck and Jones n.d.; Layton 1973; McGonagle 1979; Jackson 1984; Raymond 1984; Zeier and Elston 1984; Leech 1988; Bettinger 1989; Jones and Beck 1990).

Models of Hunter-Gatherer Adaptation.

Having addressed the collection and dating of the surface site and offsite record, we turn now

to the broader issue of models of hunter-gatherer adaptation. Ethnoarchaeological studies throughout the 1970s and 1980s added considerably to our knowledge about the complexity of the archaeological record and revealed that, in fact, present analytic strategies are wholly inadequate to deal with that complexity. These studies together with models borrowed from evolutionary ecology, help us to understand hunter-gatherer behavior, especially in the economic realm. Binford's forager-collector model, for example, describes how differences in resource utilization, settlement positioning, and mobility might differ among hunter-gatherers in differing ecological circumstances. But as Thomas (1986) points out, these models are extremely difficult to operationalize.

In his treatment of the archaeology of hunter-gatherers, for instance, Bettinger (1987) suggests that optimal foraging theory provides a good basis for the building of models of hunter-gatherer behavior to be tested in the archaeological record. He stated that optimal foraging theory "employs a few generalities about utility and economizing and from these sets forth explicit and testable predictions" (1987:136). Models based on optimal foraging theory are good descriptions and do, in fact, set forth explicit predictions, but for the ethnographic record; their archaeological evaluation, on the other hand, has proven quite difficult. These models focus on the decisions of individuals with respect to sets of economic options under a particular set of circumstances. The individual, however, and his or her decisions cannot be found in the archaeological record, especially a record of egalitarian hunter-gatherers. At the least, it might be hoped that the conditions underlying individual decisions persist for such long times and are so encompassing in any population that a physical pattern is produced. But such models minimally require temporal resolution at a scale that is today simply not feasible archaeologically and probably never

will be. Even in the best of all possible situations—stratified sites that have been meticulously excavated and well-dated—optimal foraging models are difficult to test (see Grayson 1991). Since at least 75 percent of the Great Basin's archaeological record lies on the surface and represents, in Binford's (1978) terms, palimpsests of cultural activity, the testing of such models becomes nearly impossible. For instance, how are those palimpsests to be separated into usable components? We have already discussed the dating problems for surface assemblages, particularly the low density artifact record. How, then, is even general contemporaneity among components to be demonstrated? And how is the adding of resources by a community (if that community can even be identified) in a particular order to be shown if there are no resources represented? This is not to say that we should abandon altogether the concepts of optimal foraging theory but we must use these ideas to construct models that consist of components that are archaeologically accessible. We can hope as well that advances in dating of the surface record will aid in eliminating many of the problems we now face in modeling prehistoric behavior.

Dissemination of Research

Over the past twenty years the amount of archaeology undertaken within the context of cultural resource management has increased considerably and nowhere is this more evident than in the Great Basin. The amount of communication within the Basin archaeological community, however, does not appear to have increased at the same rate. The majority of work undertaken by cultural resource management (CRM) archaeologists is disseminated in the "gray" literature, which has not been a particularly successful means of communication.

In 1979, Fred Wendorf stated that some of the most innovative archaeology of the day was

coming out of CRM but that this would likely not remain the case; the considerable increase in CRM, he believed, was creating a new kind of archaeologist, one trained in business and whose primary obligation would be to the client rather than to archaeology (1979:642). As a result Wendorf (1979:643) foresaw a future in which CRM archaeologists no longer felt an obligation to publish their work, which would have two consequences:

either when something is achieved and new knowledge is gained, no one will know about it; or when nothing is achieved and the project is a waste, still no one will know about it.

In many respects, Wendorf's fears have become reality. In a recent article, Rafferty (1990:15) pointed out that, "contract archaeologists are required by their federal permits to deposit reports from their work with the relevant federal office in a timely manner." In practice, however, these reports are not available to everyone. First, if one does not live in the immediate area of the work undertaken and has no "pipeline" to the many contract agencies doing that work, it is difficult to know what work is being done, and as in Wendorf's fear, "when something is achieved and new knowledge is gained, no one will know about it" (1979:643).

Secondly, if you do know about it, the work may still be difficult to get. In some cases this may simply be oversight on the part of the agencies but in others it may be because of workload. Many agencies are understaffed in archaeology and thus there is no time to search libraries for reports, make copies of them, and then send these copies to all researchers requesting them. In our own experience we have found it difficult at times, even when visiting the particular agency contracting the work, to obtain reports. We do not disagree with Bob Elston that "no researcher is absolved from the obligation to seek out and review CRM data and literature pertaining to his or her

research area" (this issue), but does this mean that a researcher must not proceed with his or her work if the CRM reports cannot be obtained? In one instance it took us *two* years and *many* attempts to acquire a copy of a single report from the agency involved. And, what of CRM work undertaken in other areas but involving research issues similar to one's own? In our case we know that innovative work in obsidian source and hydration research has been on-going in CRM for a number of years. We are interested in knowing who is doing these studies and what has been learned. We have been successful in finding some of these, but many of them we simply do not know about because they are not being done in or adjacent to our research area and we have no contacts in the relevant companies.

We are not alone in the concern over the inaccessibility of the gray literature; many believe this work must be made more available (e.g., Dunnell 1981, 1983; Hester 1981; D. Fowler 1982; Plew 1982; Wildesen 1982; Janetski 1986). Janetski (1986) outlined a number of publication series available for CRM reports, all good, accessible series; but the majority of CRM work never reaches these outlets. We believe it is important to establish an annual agency listing of all CRM projects undertaken within each district, the type and size of each project, and the company responsible for the contract. This listing would be somewhat analogous to archives used by historians and should be available upon request so that archaeologists can be made aware of the types of projects being undertaken in and outside of their own research areas. Then, as Rafferty (1990) recommended, academics can make more use of data generated by CRM archaeologists.

Rafferty (1990) also suggested that the research community has an obligation toward the CRM community to do everything possible, through additional training and professional interaction, to change the CRM archaeologist's

perceived status as a second-class citizen. In this respect, the program of CRM workshops through the University of Nevada, Reno, is a step in the right direction. In teaching one of these workshops, we found a level of frustration that only could come about because archaeology does not know where it is headed. The programmatic statements of researchers have led to rules about the conduct of CRM work but no good ways to evaluate what the record means, what questions it can answer. There are real problems for CRM archaeology that the academic community cannot understand without the benefit of CRM archaeologists' experiences. Thus we support Rafferty's call for increased communication between the academic and CRM communities, but it has to be a two-way street. This is particularly important today when the archaeological record of the Great Basin is disappearing at an alarming rate, a problem that we *all* share.

SOME FINAL THOUGHTS

We do not want to leave readers with the idea that we hold a negative view of the prospects for future Great Basin archaeological research; far from it. Indeed, there is no shortage of intriguing questions of substantive and methodological importance. Add to this a general maturation of the relationship between governmental, private, and university archaeologists. This is not so much a loss of the privileged position of academics, but an improvement in the standards of all work and recognition on everyone's part of the responsibilities of these constituencies.

Acknowledging all that is positive in archaeological research in the Great Basin, we still must face up to the fact that the processual underpinning of our discipline, which has been responsible for so much of the advance in Basin research, is facing serious challenges. Certainly parallels to the paradigmatic "battles" of the 1960s can be drawn with today's radical critique

of processualism. But there are significant differences. Neither culture historians nor new archaeologists debated whether theirs was fundamentally a scientific approach, even though the positivist framework of the new archaeology was made explicit. Elements of post-processualism, however, challenge whether explanations of prehistoric events are possible. In place of explanation—knowledge of cause and effect, for example—post-processualists offer interpretation and understanding of the past. The science of archaeology becomes little more than techniques for recovery and analysis, while the larger purpose becomes to read the prehistoric record as though it were a text, strangely without clear criteria with which to evaluate the narrative produced.

As difficult as we find these ideas of post-processualism given our own backgrounds, we must none the less acknowledge some level of failure in processualism to embrace and adapt to its needs a more general body of cultural evolutionary theory. This comment is not meant to diminish the critical advances made on other theoretical fronts, as in the development of more sophisticated perspectives of the archaeological record found in theories of formation processes. Still, a general theory of archaeology awaits development.

The solution we find most attractive is the development of selectionism (Dunnell 1980; Rindos 1984), an integrated theory of information (genetic and cultural) transmission and differential persistence (evolution) of that information. Such theory treats the obvious diachronic (not synchronic) nature of the archaeological record, the principally functional (related to adaptation) quality of the material record, a need for contextualized interpretations as well as true explanations of a real prehistoric past. But this is a subject for a different essay.

In sum, where should Great Basin archaeology be moving in the 1990s? That question is largely answered by the research now under-

way. Significant substantive and technical advances will certainly result. How these will play, framed as they are by processualism in a discipline recasting its theoretical structure is uncertain. In any case, the 1990s should prove to be a most discomfiting but exciting episode.

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