Lipe, William D.
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Why Did We Do It That Way?  
The University of Utah 
Glen Canyon Project in Retrospect

WILLIAM D. LIPE

INTRODUCTION

Carol Condie and I both worked on the University of Utah portion of the Glen Canyon Archeological Project—I as de facto editor of the University of Utah Anthropological Papers (though her actual title was Associate Editor) and I as a field crew member and crew chief. Carol spared not the red pencil when she worked over my attempts at reporting our field results. I learned much about how to write from her—lessons I have relied on throughout my career.

My colleague Don Fowler, with whom I worked in the field for three seasons, has just published his "personal memoir" of the Glen Canyon Project (Fowler 2011) set in a larger history of exploration and research in the Glen Canyon region. Don presents a wonderfully clear, informative, and entertaining account of how the University of Utah Glen Canyon Project was organized, how it worked, and what it was like to be a part of it. My contribution here is a minor embroidery on Dan's history, focused on trying to shed a bit of light on the academic, cultural, and personal contexts that shaped the structure and conduct of what I'll hereafter refer to as "the UGCP" (thus distinguishing the Utah portion from that administered by the Museum of Northern Arizona).

The primary answer to the question in the title is "Because Jess Jennings said to do it that way." But it is a bit more complex than that. The obvious next question is why Jennings thought about it the way he did, and so forth. But the project also evolved during its lifespan, and the people who participated in it shaped it in various ways.

I'll provide my "take" on the intellectual context in which the project was developed, and on what seem to me to have been some of the assumptions that were built into it. I also want to talk about some of the organizational and methodological aspects of the research, and then close by considering a few of its deficiencies as well as its lasting contributions. My comments are based to a considerable extent on my reading of the project's published reports, but also on perusal of some of the archival materials held at the University of Utah. However, much of what I offer here is derived from my memories of discussions with Jennings and other project personnel, and from personal perspectives on why we did what we did, developed during the project and subsequently.

I'll start with a few words about how I came to get involved in the UGCP, and what experience I brought with me when I came on board. That last part won't take long. My first archaeological field experience was in 1956 at the University of Arizona field school at Point of Pines, directed by Emil Haury and Ray Thompson. While there, I demonstrated the ability to move a lot of dirt while not digging through masonry walls and still finding an adequate number of artifacts. That experience helped me land an assistantship at the Museum of Northern Arizona in the summer of 1957, working for Dave Breternitz. Toward the end of the summer, some of us drove to the Pecos Conference in Globe, Arizona, and it was there I heard Bob Lister of
the University of Colorado describe the first UGCP surveys, which he had led in the Escalante River canyons and adjacent areas. Lister’s talk convinced me that I needed to become part of that project if I could.

My chance came in the late fall of 1957, as I was nearing the end of my first semester of graduate school at Yale. The American Anthropological Association meetings were in Chicago, and I and several other graduate students drove over in my 1949 Chevy with the broken window, and stayed on the floor of a dormitory at what was I think the University of Chicago Divinity School. I got an introduction to Jesse Jennings from my graduate advisor, Ben Rouse. Jennings had in fact included Rouse on a list of colleagues to whom in February 1957 he had sent a letter asking for names of students or others qualified to fill supervisory field or lab positions on the Glen Canyon Project (Jennings 1957a). I must have followed up with an application letter, and a few months later I was hired as a crew chief, to start in the summer of 1958. That summer job stretched to two and a half years, and gave me the opportunity to write two substantial fieldwork reports (Lipe 1960; Lipe et al. 1960). I returned to graduate school in the fall of 1960, and then came back for one more summer in 1961 (as a crew member, working for Floyd Sharrock). In 1962-63, after finishing classes at Yale, I returned to Salt Lake to assemble data for a dissertation on the Red Rock Plateau (a portion of the Glen Canyon area), which I finally completed in 1966 (Lipe 1966, 1970).

Definition of cultural phases also occupied a good deal of conceptual space (e.g., Rouse 1955), as did arguments about the definition and boundaries of large-scale archaeological “cultures” such as (in the Southwest) Anasazi, Mogollon, Hohokam, Fremont, Sinagua, and Patayan.

The “new archaeology” (e.g., as represented by Binford 1962, 1964) was still a few years away from conception, let alone birth. But there were a number of publications and currents of thought that were beginning to move American archaeology away from a predominantly cultural taxonomic/typological approach to one more concerned with function and environmental adaptation, not to mention the social characteristics of the communities and societies responsible for the cultural complexes that archaeologists were attempting to define and date. Still largely unexamined was the assumption that cultures were made up of widely shared norms, and that our job as archaeologists was to establish types of artifacts, structures, and sites that would capture the prevailing norms of a particular period and locality; this would serve to characterize the culture that occupied that particular chunk of time and space.

Walter Taylor’s Study of Archeology had been published in 1948, nine years before the start of the UGCP, and although his attacks on the some of the leaders of the field stimulated most of the discussion, his ideas about a “conjunctive approach” (Taylor 1948) that would yield something like a cultural ethnography of the past, were quietly infiltrating the field. Analysis of animal bones, plant remains, and even pollen was also becoming more common in the archaeology of the American West, as sources of both paleoenvironmental and economic inferences. The same year the UGCP started, Taylor edited the report of an influential meeting sponsored by the National Academy of Sciences entitled “The Identification of Non-Artifactual Archaeological Materials” (Taylor 1957). Don Fowler (2011:226-227) discusses Lyndon L. Hargrave’s earlier advocacy for using ecological information in archaeological research.
and credits Hargrave for influencing Taylor’s interests (Fowler 2011:229).

4) In 1938, Julian Steward had published a landmark study of how the sociopolitical organization of aboriginal groups mapped onto environmental variation in the Great Basin and adjacent portions of the Columbian and Colorado Plateaus (Steward 1938). In the 1950s, articles and a book setting forth the concepts of cultural ecology appeared (Steward 1955). Gordon Willey’s pioneering Viru Valley settlement pattern study and his edited volume on New World settlement patterns were published before the start of the UGCP (Willey 1953, 1956). The concept of settlement pattern had begun to register in Southwestern archaeology, although notions about how one would study such patterns in the field and what one could infer from settlement data were still pretty undeveloped. In particular, excavation was still viewed as the principal, if not only, way to obtain data useful for interpreting the past, and survey remained largely relegated to the role of finding sites worthy of excavation.

5) In the middle 1950s, Gordon Willey and Philip Phillips published two articles in the American Anthropologist that were the basis for their book Method and Theory in American Archaeology (Willey and Phillips 1958). Their broad stages (Archaic, Classic, etc.) emphasized recurrent functional complexes of characteristics, rather than historically distinctive “cultures” (although they hedged their bets by calling the scheme “historical-developmental”). They also suggested that archaeologists needed to try to understand the social aspects of archaeological units (Willey and Phillips 1958:48-56), but still couched the question in taxonomic terms (e.g., whether “phase” could be equated to “society”).

6) Jennings’ “Desert Culture” concept was first published in a journal article (Jennings 1953) and his classic monograph on Danger Cave (Jennings 1957c) came out the year the Glen Canyon Project started. The Desert Culture was seen not as a taxonomic entity, but as a widespread “lifeway” that represented a persistent cultural and social adaptation to the harsh environments of the interior West. Interregional similarities rather than differences were stressed. The Desert Culture eventually was folded into the Western Archaic, but Jennings’ culture-ecological insights helped ensure that later attempts to characterize the Archaic went beyond just listing artifact types.

I think all of this work was known to Jennings, though the field archaeologists who made many of the decisions on the project were for the most part less well grounded in the literature. I’m sure that I had never read Willey’s work on settlement patterns, and was only vaguely familiar with Steward’s approach to cultural ecology. Within a few years of leaving the Glen Canyon Project, however, I had produced a dissertation based on a cultural ecological analysis of settlement patterns in the Red Rock Plateau portion of the Glen Canyon area. If I had been better grounded in the hot new theoretical literature of the late 1950s, I might have made some more insightful decisions in the field and as I was writing up my field reports, but I see that my battered copy of Willey and Phillips is signed “Lipe 1960” and my copy of Steward’s book shows “Lipe 1961”—indicating that they were purchased after I had left the project and returned to graduate school.

WHAT WERE THE THEORETICAL AND EPISTEMOLOGICAL ORIENTATIONS OF THE UNIVERSITY OF UTAH WORK?

The intellectual climate that prevailed in American archaeology in the 1950s surely influenced the way the UGCP did its work, but these influences generally seem to have operated implicitly, as a kind of “normal science” that structured the choices the UGCP archaeologists made regarding what sites to work on, and how to interpret the evidence.


Insofar as I can tell, there never was a detailed research design for the UGCP organized around substantive questions about what had happened in the past in that region. In a brief introductory history of the project, Jennings (1959a) does provide what he labels as a research design, but it is one that focuses largely on the organizational aspects of the project, and on very broadly stated objectives, with the most important ones being:

1) The project is aimed at an intensive, thorough, mile-by-mile search for sites in the area; leading to a

2) complete sampling of cultures represented in the area, by test and extensive excavation, resulting in

3) a flow of descriptive reports kept current with field work.\textsuperscript{(Jennings 1959a:13)}

"Scientific problems" were to be of secondary focus, with the "salvage aspect" of the project to be primary. Jennings (1963b) was quite positive about the value of "salvage" archaeology, and believed that the primary job of the salvage archaeologist was to collect the full range of data from the full range of sites that were going to be lost. He saw problem-oriented research as necessarily being more selective in the choice of what sites would be excavated or what classes of specimens and information would be collected from them.

Any salvage operation has special objectives [as compared with problem-oriented research]. It is impossible for the technical staff to concentrate work on one problem, or one time period, or in some special aspect of a problem or a time period. Total recovery is the objective; this means total sampling of all cultures, and all time periods, to be found in the area. (Jennings 1959b:681)

Jennings was somewhat ahead of the times in referring to "sampling" at all, but what this might mean in practice remained obscure, and certainly did not refer to an application of statistical sampling theory. Although Jennings saw the primary obligation of a salvage project as acquiring as unbiased a sample as possible of all the cultural manifestations present in a given area, he recognized that those responsible for the fieldwork must be aware that the ultimate value of their work would depend on the usefulness of their data in addressing general problems of historical or scientific interest.

It seems necessary, at all costs, to plan and execute the project so that the immediate salvage problems are adequately dealt with and simultaneously the scientific implications of the data are correctly understood, assessed, and interpreted. (Jennings 1957e:1)

It is not enough that competent craftsmanship in data collection be achieved (although this would satisfy the technical aspects of contract fulfillment but every precaution should be taken to make certain that the scientific implications of the mountain of crude data being accumulated through routine field operations be recognized, assessed and (if possible) interpreted. For example, the sites selected for excavation or any areas chosen for resurvey or revisitation or sites chosen for excavation which are peripheral to the major search areas, should be selected in part because they appear to be capable of contributing to clarification of specifically identified problems as they develop. (Jennings 1959a:10)

In his progress reports to the National Park Service (e.g., Jennings 1957d, 1957e), Jennings repeatedly refers to some broad substantive problems that needed
to be addressed. These focused on the chronology of occupation in the region and on the kinds of cultures that were represented. Included were the search for solid evidence of preceramic occupations, defining the spatial and temporal relationships among major Formative-level "cultures" (Mesa Verde, Kayenta, and Virgin Branch Anasazi, as well as Fremont), and using archaeology to expand knowledge of later cultures in the area—Paiute, Navajo, and historic Anglo. He frequently mentions the difficulties posed by the lack of comparative data from surrounding areas, due to a lack of previous archaeological research, especially to the west and north.

Jennings understood that "facts are derived from observations, and observations are always made to some scale and within some context, either explicit or assumed" (Jennings 1963a:16). In practice, however, there was little explicit discussion of what observations the fieldwork should be expected to yield, and the concepts of "collecting raw data," "complete recovery," and "descriptive reporting" come up repeatedly in his progress reports and, as I recall, in conversations. These contributed to an implicit expectation that archaeological data could be generated from the physical archaeological record in a fairly straightforward and even self-evident way, if appropriate field methods were used and archaeologists kept their eyes open.

Jennings knew that given the scope and logistical difficulties of the UGCP, much of the day-to-day fieldwork decision making (usually including which sites to test and, in some cases, which sites to dig) would depend on the knowledge and judgment of the field supervisor.

In reservoir salvage work there is a heavier burden on the field supervisor than in any other kind of archeology, since the completion of the dam and the filling of the reservoir precludes further opportunity to check, verify or add to the information acquired through fieldwork. (Jennings 1959b:681)

Jennings necessarily relied on the judgment of the field archaeologists (and more generally, on the "best practices" for survey and excavation that were current at the time) to dictate how the archaeological record would be investigated at particular sites, and in fact, how sites would be selected for what level of investigation. He expected that an understanding of the prehistory of the project area would flow from the direct encounter between perceptive archaeologists and the archaeological record, both in the field and in the lab. And he hoped he had hired archaeologists who were perceptive enough to get the most relevant information for the time and money expended.

If we look at some of the choices and interpretations that were made during the project, there is evidence for an underlying theoretical framework that could be considered culture-ecological, though this was seldom explicit. The inclusion of several kinds of biological and geological studies as part of the project indicated a concern with obtaining data both from present-day and past environments in order to help place the prehistoric cultural manifestations in ecological and adaptive contexts. In the interpretive papers he published on the project, Jennings (e.g., 1963a, 1966) clearly takes a generalized functional and ecological approach rather than the taxonomic one that was still dominant in Southwestern archaeology. That is, he spends more ink on how people lived in relation to their environment than on whether the sites in question were Pueblo II or III or Kayenta or Mesa Verde Branch, or whether the pottery in them was Mancos or McElmo Black-on-white.

Another indicator of a broadly culture-ecological approach is that, unlike projects today, the fieldwork—including excavation—was not confined just to the area directly impacted by development—in this case, by reservoir construction. Rather, a broader regional approach was taken, with surveys and excavations conducted well outside the reservoir boundaries. This approach was explicitly encouraged by Charlie Steen, the Park Service's archaeologist in charge of overseeing the project:
You should not concern yourself only with the land which will actually be flooded, but should extend your surveys, wherever possible or desirable, to and somewhat beyond the canyon rims. We shall not attempt to set a specific limit, or to ask you to search for sites to a line a certain distance from the rims, for there can be no closely followed procedure in such terrain. I believe you should instruct your Chiefs of Party to look over all the lands adjacent to the canyon which give promise of being of importance in any field of investigation. We are very anxious to know just what archeological, historical, and biological resources exist in the Glen Canyon region. (Steen 1957; also quoted in Jennings, 1959a)

One assumption that seemed to underlie this approach was that the small Puebloan sites located in the canyons that would be flooded needed to be understood in relation to larger sites in the higher surrounding areas or at least in terms of the cultural influences that presumably emanated from more populous areas. This assumption was more explicitly developed by the Museum of Northern Arizona's project team, but I believe it structured the University of Utah approach as well, at least at the outset. Jennings was able to implement work in neighboring upland areas (e.g., excavations on the Kaiparowits Plateau and in Harris Wash in the upper Escalante [Fowler 2011:294]) by obtaining non-project funds from the Wenner-Gren Foundation, the University of Utah Research Fund, and the National Science Foundation, and by arranging for the University of Colorado's 1959 field schools at the Coombs site. Today, we would more explicitly use the concept of regional settlement system to relate the highland and canyon sites.

WHAT WERE THE DISTINCTIVE METHODOLOGICAL AND ORGANIZATIONAL ASPECTS OF THE UGCP?

In contrast to the broader intellectual underpinnings of the project, its organizational and methodological aspects were more explicit. The UGCP field manual, published in 1959, is quite instructive, as are several papers Jennings wrote in the 1960s on the administration of contract salvage projects.

Jennings welcomed the definitiveness of the contract salvage format, because it required that a certain amount of money would be allocated to achieve certain results or produce certain products within a specific amount of time. This approach is one he was familiar with from his varied work career in the non-academic world, and he saw no reason why such common-sense requirements would not apply to a research project as well. Jennings is probably the finest research administrator I have ever encountered, bar none, and a large salvage project gave him the opportunity to show what he could do. It is also clear to me that he saw some of the academic research of the time as little more than undisciplined dawdling, and felt that some of his colleagues were more wedded to particular techniques or to achieving spurious levels of precision than to figuring out how to learn the most from the archaeological record with the least expenditure of time and money.

Several principles seem to me to have structured Jennings' approach to successfully carrying out an archaeological salvage project under contract.

1) “Use the coarsest tool which will do the work—i.e., recover the data” (Jennings 1966:7).

2) “My preference is to get 95% of the data from ten sites instead of 99% from one” (Jennings 1963b:263).
3) Although troweling and screening are sometimes appropriate, routinely excavating with trowels and screens instead of shovels and no screens is usually a waste of time, in terms of the amount of information obtained per unit of effort expended. (In his summary report on the UGCP, Jennings unleashes a final rant against the “spurious accuracy” achieved by “the slow brushing away of a site with trowels and the plotting of each scrap…” [Jennings 1966:6]). Not envisioned was a role for screening in promoting comparability of the artifact samples collected during excavations.

4) A well-coordinated team of full-time workers (in both field and lab) is better than a single individual working the same total number of hours over a longer period (see especially Jennings 1963b:284).

5) Maintaining data quality is essential, but the level of quality sought should be appropriate. Perfection is not achievable and the attempt to achieve it is a waste of time.

6) In the area of data quality, the field record is preeminent. A researcher can always reclassify the artifacts that have been collected, but he or she cannot not go back and re-excavate the portions of a site that have already been dug. (On a reservoir salvage project, this was, of course, true even for the unexcavated portions of sites).

7) If you want to achieve data comparability among several research teams and over the life of a multi-year project, it is essential to take explicit steps to ensure that this happens; otherwise, it won’t. Thus, most of the pages in the project’s operational manual (Jennings 1959b:687-707) are devoted to discussing forms and procedures for record-keeping in the field and lab.

8) Fieldwork unreported is equivalent to fieldwork never done; furthermore, it has resulted in the destruction of a site with no resulting gain in information about the past. “Unpublished data don’t exist” (cited in Fowler 2011:272).

9) Report deadlines are essential—”This puts the burden of completion in sharp focus from the very beginning of the project. Thus it…establishes [a]… schedule to which the work must be geared” (Jennings 1963b:284).

10) The principal product of a salvage project will be descriptive reports, produced in a timely fashion, that put basic data on record. “…preoccupation with extensive comparisons, synthesis or interpretation must be deferred or held as incidental until the gathering of data is curtailed by filling of the reservoir…” (Jennings 1959a:9).

11) Archaeological sites and artifacts don’t belong to the individual archaeologist, and neither do the archaeologist’s records. Records of archaeological field research must remain available for future use.

Jennings was also strongly committed to not prejudging the evidence—and wanted to avoid setting up a conceptual structure that would determine conclusions in advance. This was expressed, for example, in the feature system he designed for taking notes (Jennings 1959b:692-693). Features were numbered so they could be tracked, but otherwise, they were “empty” containers for observations. A feature was anything in the archaeological record about which you wanted to record observations—it could be a stratum, a possible intrusive pit, a discolored area where a fire might once have been built, a structure, an area of the site, or whatever. Interpretive names, such as “hearth” or “kiva” were assigned only after all the observations had been made, and the archaeologist had had the time to reflect on them. The point was to separate observation from inference. The notes on a particular feature were kept open and could be added to until work on that feature had been completed.

I believe this approach implicitly operated at the project as well as at the site level, and was designed to keep project archaeologists from jumping to premature conclusions about the chronological, cultural, and functional relationships of the sites
they were excavating. The first job was to encounter the archaeological record by excavating and studying an unbiased sample of whatever was in the project area. On the other hand, in some cases it provided an excuse to attempt no interpretations beyond those necessary to give artifacts and features descriptive or typological labels.

Jennings’ approach to excavating and to taking field notes also reveals a concern for what would now be called site formation process. In his instructions to field supervisors, he urged designing the excavation to efficiently explore horizontal and vertical relationships between cultural and stratigraphic features. He abhorred rote excavation by grid square and arbitrary level, seeing it as an excuse for not thinking about how best to expose and understand the relationships observable in the archaeological deposits. He preferred trenching to pit excavations, because trenches allowed one to connect different areas of the site and also to expose vertical relationships. He ridiculed the then-common Southwestern practice of publishing cross-sections of structures that showed the architecture, but not the strata that had once filled them, so there was no way to infer how they had filled. I recall him remarking “But what happened to the dirt?”—whether he actually said just those words or not, that critique came through loud and clear.

Crew chiefs were expected to regularly write summaries of what they were finding out about the archaeological record of the site they were working on. This was done in “Feature 1,” a running journal in which all the features recently excavated were mentioned, and their stratigraphic and spatial relationships discussed. The purpose was to force archaeologists to think about how the archaeological record had been formed, while they were still in the field. If the work was far enough along, interpretive names could be tentatively assigned to features, and the chronological and functional interpretation of the archaeological record could begin.

As a result of the above-mentioned concerns with data quality, there were fairly elaborate systems in both field and lab that were designed to ensure that the provenience controls assigned in the field were maintained through analysis and into curation. One of Jennings’ signal contributions to data quality was to read all the field notes, copies of which his crew chiefs sent in every two weeks. If you have ever managed a large project, with multiple crews in the field at the same time, you know what that took in terms of discipline on his part. But it was very effective—receiving a succinctly stated note from Jennings pointing out some inconsistency or outright blunder in your field notes certainly got your attention. On the UGCP, steps taken to promote data comparability included having standardized recording forms in both the field and the lab; having one person (ordinarily the crew chief) take most or all of the notes on the site; using standard artifact classifications across all sites; and labeling artifacts individually and classifying them without taking provenience into account, so that locations and associations would not bias assignment to classes. This allowed all the artifacts from a site (or even several sites) to be spread out on a table, then pushed into piles representing classes or types, and then tabulated by provenience.

Jennings placed prompt analysis and the timely publication of descriptive site reports on an equal footing with the fieldwork itself, in keeping with the above-mentioned principle that if the results are not published, all the other steps may as well never have occurred. He successfully fought for funding of analysis and reporting (including publication of the reports) in an era when salvage contracts often supported only the fieldwork, on the frequently unfounded presumption that academic researchers or their students would eventually do the follow-up work. Jennings did not believe, however, that it was the federal government’s responsibility to pay for problem-oriented studies that went beyond descriptive reporting.

In order to carry out the reporting obligation, a rigorous schedule was set up to produce and disseminate inexpensive descriptive reports. While requiring the reports to have thorough (by the standards of the day) descriptions of the archaeological contexts, Jennings recognized that
he had to forestall what he saw as the reluctance of some archaeologists to leave anything out. This was particularly clear with regard to the issue of lumping versus splitting proveniences when reporting the horizontal and vertical distribution of artifacts at sites. Jennings was clearly a lumper, and promoted the principle that if two proveniences did not differ enough to indicate a chronological or functional separation, they should be lumped when reporting the artifacts that came from them. This resulted in a remarkable simplification of data tables.

The previously mentioned concern to ensure that archaeological collections and records be preserved for the future resulted in a significant investment, during the life of the project, in seeing that field and lab notes, maps, photos, other records, artifacts, and other specimens were ready for long-term curation. And in fact these materials have been preserved and are accessible for research and educational purposes. The collections are housed in the Utah Museum of Natural History, but some of the paper records are at the University of Utah archives and the University Archaeological Center.

**IN HINDSIGHT, WHAT WERE THE MAIN DEFICIENCIES OF THE UGCP?**

It is, of course, easy to critique procedures instituted and work done on an archaeological project carried out 50 and more years ago and often in very difficult logistical contexts. Some of the problems that the UGCP faced “came with the territory”—that is, were the result of events beyond control of the project personnel. Others, however, were at least in part the result of some of the assumptions and overall philosophy that undergirded the project—factors that, in many cases, also contributed to its successes.

The UGCP began before even the 1960 Reservoir Salvage Act was in place, let alone the 1966 National Historic Preservation Act or the 1974 Historic and Archeological Preservation Act. The only legislative authorities for the project were the Antiquities Act of 1906 and the Historic Sites Act of 1935. Because of the scale of the project, it was dependent on annual appropriations by Congress; hence, there was considerable insecurity from year to year regarding how much money would be available, and there was always the threat that the project would be terminated mid-course. Jennings (1963b:285), however, saw that a long-term contract could overcome this disadvantage by allowing funds not expended one year to be rolled over into the next. Thus, as a prudent project manager, he was able to build up a cushion against the vicissitudes of the annual appropriation process.

A significant operational problem present from the outset was the lack of systematic survey data for many parts of the project area prior to the start of full-scale work. Thus, the contracted schedule for the project demanded that in some locales, survey, testing, and excavation had to be undertaken in the same field season. There had been some prior survey, mostly in the Glen Canyon proper (see Adams 1960; Adams et al. 1961; Fowler 1959a, 2011). However, coverage of the areas to be impacted was very incomplete, especially in the tributary canyons and even more so in the adjacent areas outside the full pool level. For example, Don Fowler spent most of the summers of 1958 and 1959 surveying (with one helper) the main stem of the Glen Canyon and the lower parts of the tributary canyons (Fowler 1959a, 1959b). At the same time, I was directing the “river crew” in excavations. We worked out of the same field camp to the extent that Fowler’s survey locations permitted. The same pattern of survey concurrent with excavation was repeated when we moved to portions of Moqui and Lake Canyons that were too far upstream to access from the river and had to be accessed overland. Away from the canyons, some of the UGCP surveys were clearly designed as exploratory reconnaissance, although the survey of portions of the Kaiparowits Plateau (Gunnerson 1959a) was intensive (by the standards of the day).

Would it have made a difference if the survey of the project area had been completed in advance?
Possibly, but only if the survey data had been subjected to more intensive chronological analysis and if it had been used more systematically as a way to develop hypotheses about site types and settlement patterns for particular periods and locales. As noted earlier, this role for survey data was still in its infancy in American archaeology in general, not just on the UGCP. As it was, we were usually able to plan excavations for a particular season around survey data obtained the previous season or earlier (e.g., Lipe 1959). The surveys I am most familiar with are those carried out by Fowler and later Kent Day in the Glen Canyon Main Stem and the Red Rock Plateau. The records and collections from these surveys are superior to those of most contemporary surveys in the Southwest, in my opinion. I was later able to make good use of this survey data (along with the excavation results) in developing a phase scheme and settlement pattern interpretations for the Basketmaker II through Pueblo III periods in the Red Rock Plateau (Lipe 1966, 1970).

However, there is no question that the body of survey data generated by the UGCP has gaps and biases. Schroedl and Newsome (2000),3 on the basis of resurvey of portions of the UGCP area and a review of its records, note that some of the sites missed in Lake and Moqui Canyons were small, open Anasazi structural sites located on small prominences within the canyon. These sites were missed by the GCP inventory teams because the crews were focused on searching for Anasazi sites in alcoves and along the canyon walls. (Schroedl and Newsome 2000:43)

A larger bias was that “the Archaic occupation of the benchlands and uplands surrounding Glen Canyon also went unrecognized” (Schroedl and Newsome 2000:43). This is undoubtedly true, although the presence of such sites was certainly known to the UGCP team. In a memo to the Park Service assessing the results of the 1958 field season, Jennings notes:

…the consistent presence, from Comb Wash to Wahweap Creek, of a strong lithic complex associated with dunes or rock terraces. All these sites had previously been dismissed (when noted) as chipping areas adjunct to the Anasazi remains. Many, no doubt, are just that. Others, however, can be recognized as having distinctive artifact lists and other traits (location, size, etc.) which mark the existence of a separate complex. Typologically, however, no staff member recognizes the artifact types. No age ascriptions are warranted, but there does appear to exist a non-Anasazi occupation. It seems to be something new; while the complex may be earlier than the dominant Anasazi it could equally well be post-Anasazi in age. (Jennings 1958:3–4)

Unfortunately, the UGCP staff was never able to get much beyond this state of puzzlement. In their defense, most of the surrounding “benchlands and uplands” were above the full pool level, and given the huge area staked out as of interest to the project, they received less attention. I also think that the prevailing approach to doing archaeology overwhelmingly favored excavation, and to the field archaeologists charged with getting the most information for the time available, the lithic sites outside the canyons appeared largely surficial and probably “not worth digging” despite Jennings’ repeated expressions of interest in learning more about them. The prevailing emphasis on excavation as the primary source of reliable information perhaps reinforced this attitude.

I think an underlying problem, however, was a largely implicit assumption by the project staff that the occupation of the Glen Canyon area did not have a great deal of time depth. This was a common attitude in the Southwest prior to the 1950s, and by the time the UGCP got underway, only a few papers had appeared reporting on probable pre-agricultural complexes in the Four Corners area (e.g., Bryan and Toulouse 1943; Mohr and Sample 1959).
More importantly, however, the UGCP as a whole did not focus at all effectively on chronology in a methodological sense. For the preceramic part of the record, some expenditure of project funds on radiocarbon dating would have paid substantial dividends. As it was, only one radiocarbon sample was dated (Geib 2006:37) and that one (from the Lone Tree Dune Basketmaker II site [Sharrock et al. 1963]) was submitted as part of my dissertation research after the field phase of the UGCP was completed. Radiocarbon dating was not developed until 1949, but by the early 1950s, it had begun to be used by archaeologists all over the world (Johnson 1951) and had played an important role in acceptance of the early dates from the lower levels of Danger Cave. Dendrochronology was of course an established method for dating contexts of the later periods in the northern Southwest. However, virtually all the sites excavated or tested by the UGCP were at too low an elevation to yield coniferous wood samples suitable for tree-ring dating; samples from a few higher elevation sites were submitted, but did not date. An exception was the Coombs Site (Bannister et al. 1969:12-13; Lister and Lister 1961), but here only a few samples were submitted, even though a number of burned rooms had been excavated.

The UGCP's emphasis on "descriptive" reporting of "raw" data, with the postponement of problem-oriented studies until later, may also have contributed to a lack of focus on chronology. The goal was to get a publishable descriptive report of one season's fieldwork done before the next field season started. This provided an excuse to put off addressing some of the harder issues, of which dating non-ceramic assemblages was one. I also think those of us doing and reporting on the fieldwork took too much to heart Jennings' (1959b:686-687) assertion: "Excavators are further reminded that the data, the sequence of events, and the relationships at any given site are observable. They are in the ground and must be dug for." Thus, we may subconsciously have been expecting to find another Danger Cave to convince us that an Archaic occupation was in fact present.

There was also a tendency—again largely implicit—to favor functional over chronological explanations for differences among assemblages from the same site or from a set of sites in a given locality. For example, in my report on 1958 excavations at Jug Shelter, I conclude,

The absence of pottery in association with the stone tools and debris in the lower levels of Jug Shelter may be due to the presence of a preceramic distribution below a ceramic one. More probably, however, the distribution of artifacts reflects a changing emphasis from chipping to camping. There is no stratigraphic break between the non-ceramic and ceramic levels. (Lipe 1960:24)

("Chipping station" was a frequently used label for surface lithic sites, and one that was fairly frequently employed to characterize a site type.) In the same volume, I also used the lack of a clear stratigraphic break to question whether the non-ceramic midden levels at Lizard Alcove represented a significantly earlier occupation (Lipe 1960:83). However, when Geib (2006:35-38) much later examined the artifacts from this site, he identified both Basketmaker II and Archaic sandal types from the preceramic levels. Similar examples can be found elsewhere in UGCP reports.

In some cases, interpretations of non-ceramic levels were simply not offered. The report on 1957 excavations at the Alvey Site offers essentially no discussion of chronology, other than to note that excavations had "revealed cultural material to a depth of 12 ft." (Gunnerson 1959b:50), and that the lowest of three levels lacked pottery. Table 10 (Gunnerson 1959b:92) shows that specimens of maize were found in all levels. In an earlier report to the Park Service, Jennings (1957d:8-9) had observed about this site "The non-pottery basal level (if it proves to be actually pre-pottery) will presumably equate with early Basketmaker II or even with the late phases of the Uncompahgre complex." Radiocarbon dates
on samples from the lower levels at the Alvey Site subsequently confirmed occupation in a Basketmaker II time frame (Geib 1996:17).

Post-UGCP fieldwork and reanalysis of collections made by the project has shown, of course, that there was relatively abundant Basketmaker II and not insignificant Archaic occupation in the Glen Canyon area (Geib 1996, 2006). To the credit of the UGCP, several Basketmaker II sites were identified and reported as such (e.g., Honeycomb Alcove, Bernheimer Alcove, and Rehab Center in Moqui Canyon, and Lone Tree Dune and Greenwater Spring in upper Castle Wash) (Sharrock et al. 1963). Geib (1996:27) tabulates a substantial number of dates from the late centuries B.C. and early centuries A.D. for the Glen Canyon region, quite a few of them run on samples of maize. This indicates that an Early Agricultural period occupation was probably more widespread in the UGCP area than was recognized at the time. His conclusions were based in part on restudy and dating of specimens from the UGCP.

I've speculated (as noted in Geib 2006) that the prevailing view of the Glen Canyon area as "marginal" for Puebloan farmers was implicitly generalized to an expectation that it would have been even more marginal for foragers or semi-agriculturalists (which was how Basketmaker II was perceived at the time). However, I could find no specific statement of this in the published or archival materials I reviewed—it's just a hunch based on my recollections of how we tended to think about questions like this. An additional reason for our lack of success in defining Archaic occupations may have been that projectile points of styles clearly predating the Basketmaker II period were quite rare, as a glance through the artifact illustrations in the UGCP reports shows.

I think if Jennings had been doing the fieldwork himself, his encounters with the archaeological record would probably have led him to recognize that in some cases he was dealing with significant time depth, but crew chiefs as inexperienced as I was lacked that perspective. In retrospect, it would have helped if there had been a project design that provided some concrete expectations about how cultural variations due to chronological differences would be distinguished from those due to differences in site function.

A lack of sharp focus on chronology also characterized some of the interpretations offered for the Pueblo period sites. For example, in the report of the 1961 excavations, a number of reasons are given for not providing fine-grained date estimates:

The problem involved in assigning dates to Castle Wash and Moqui Canyon is much the same as for other areas of the Glen Canyon...no beams suitable for dating have been located; radiocarbon dating latitude is impractical for the relatively short time involved; ceramic dates for several reasons are of dubious value for dating Glen Canyon sites because of a) some presumed lag between their inception at culture hearths and the arrival of the style or actual vessel in Glen Canyon, b) holdover of the style after abandonment at the culture hearth, c) possible reuse of abandoned vessels at a much later time.... (Sharrock et al. 1963:18)

However, I had earlier been able to successfully use Kayenta tradition pottery types for a simple graphical seriation of assemblages from sites excavated in 1959; I also assembled credible date estimates for these sites based on the standard pottery type dates published in the Southwestern literature (Lipe et al. 1960:4-9). If the factors listed by Sharrock et al. (1963) had been heavily in play, this should not have been possible. To my knowledge, this 1960 attempt was the only use of seriation on the UGCP.

I'll conclude this section of the paper by briefly discussing one last self-inflicted deficiency of the UGCP: the strict "guys in the field, girls in the lab" approach to staff assignments. To a great extent, this was a product of the attitudes of the time; the 1950s probably represented the nadir of opportunities for women to work as field archaeologists in the
Southwest. In several of the communications about staffing that I unearthed in the University of Utah archives (e.g., Jennings 1957a, 1957b), Jennings consistently refers to recruiting “men.” However, some of his earlier projects had included women at least as field school students or field hands. I suspect that it was the remoteness and potential dangers of the Glen Canyon region that promoted a culturally conservative stance on his part. He and/or his dean probably correctly anticipated that the Department of Anthropology and by extension the university would have been mercilessly pilloried in the local media if a young woman was seriously injured while working on the project. Furthermore, it would then have been revealed that tender young ladies were being sent out to live in the wilds, unchaperoned, with groups of undoubtedly randy young men, some of them perhaps even sporting beards.

Don Fowler (2011:275-276) recalls that at the beginning of the 1959 field season, our lab director, Dee Ann Suhm, accompanied Jennings and his son David to our field camp, and was able to participate in the fieldwork for a short time. Our camp location was accessible by jeep and presumably the supervision provided by Jennings made a difference. Later, as Dee Ann Story of the University of Texas, she became famous for training generations of students in how to do good dirt archaeology. Many of her students—male and female—have gone on to successful careers as archaeologists.4

WHAT WERE THE LONG-TERM CONTRIBUTIONS OF THE UGCP TO AMERICAN ARCHAEOLOGY?

1) The project field and lab crews and a number of the crew chiefs and other personnel were graduate or advanced undergraduate students from colleges and universities around the U.S. These students gained experience in field techniques, laboratory analysis, report preparation, and project administration and logistics. Because the project occurred just in advance of an employment boom in both the academic and “salvage archaeology” (later “CRM”) fields, many of these students found that their UGCP experience helped them become full-time professionals. Consequently, the project resulted in training and professional development for a rather large cohort of archaeologists.

2) In substantive terms, the long shelf of published descriptive reports that the UGCP produced helped put this area of the Southwest “on the record” archaeologically. Although William Adams (1960) has documented a surprisingly large number of earlier archaeological expeditions to the Glen Canyon basin, virtually none of these produced substantive archaeological reports. The UGCP reports remain a significant research resource in the study of Southwestern archaeology, especially for the Puebloan occupations of southeastern Utah.

3) The abundance of small Pueblo period sites documented by the UGCP provided a departure from the then-prevailing Southwestern focus on large pueblos, and contributed to an emerging understanding of variability in settlement, community, and mobility patterns among Southwestern horticulturalists. By the end of the UGCP, Jennings had recognized that a flexible dispersed settlement pattern of small sites was typical of wide areas of the northern Southwest. He became inclined...to view the inhabitants of the Mesa Verde cliff dwellings as no more typically Anasazi than the modern cliff dwellers of New York City are typically American...the idea that the typical Anasazi lifeway falls into a rancheria-scrounger pattern, instead of a pattern of high centers radiating stimuli outward to backwoods farmers, will require examination. (Jennings 1963a:13)
4) Pre-Pueblo components were recognized and reported on in some cases, especially those dating to the Basketmaker II period. These studies did provide some evidence of time depth for the region, and have contributed in various ways to development of the productive field studies of the Basketmaker II and Archaic periods that have been undertaken in recent years in the general Glen Canyon area (e.g., Geib 1996; Jennings 1980; Matson et al. 1988).

5) The extremely variable and episodic history of occupation of various portions of the project area, and in fact of the area as a whole, was an important finding that has contributed to a better appreciation of not only forager but agricultural settlement dynamics in the Southwest. Efforts to link changes in rainfall regimes to episodes of population expansion into and withdrawal from the Glen Canyon region were stimulated by this recognition (e.g., Lipe 1970), and have continued since. Unequivocal evidence that Pueblo agriculturists were periodically moving into and out of the region also served as an antidote to the tendency of Southwestern archaeologists in the 1970s and 1980s to dismiss migration as a source of cultural dynamics.

6) From the beginning of the UGCP, Jennings (1957:2) recognized that one of the UGCP’s goals should be a search for archaeological evidence of Native American occupations that post-dated the Pueblo III period. After several years of fieldwork failed to produce clear evidence of these occupations, he brought Catherine Sweeney (later Fowler) and Robert Euler on board to implement an ethnographic and ethnohistoric approach to the problem. This was quite successful in documenting occupation of the area by Numic speaking groups, and in discovering some of the archaeological sites associated with these occupations (Euler 1966; Sweeney and Euler 1963). The regular UGCP archaeological surveys also recorded a number of sites with Pueblo IV period Hopi pottery (Lipe 1970:137-138), evidently the result either of occasional trips to the area to visit ancestral sites or resource areas or of activities by Numic groups that had acquired Hopi pottery. In addition, at least one historic period probable Ute site was excavated (Sharrock et al. 1961:123-128), and a Navajo habitation site was recorded on Cedar Mesa, with late nineteenth century tree-ring dates obtained from structural timbers (Day 1964:144-146).

7) Under the leadership of Greg Crampton of the University of Utah, numerous historic “Anglo” sites were recorded, in conjunction with documentary research. This work resulted in a string of publications in the University of Utah Anthropological Papers (see Jennings 1966 for a listing) as well as articles in other scholarly outlets, and several successful popular books (e.g., Crampton 1964, 1986). Although excavations at some of the historic period sites would undoubtedly have yielded interesting information, this seems not to have been considered in the design of the UGCP. Historical archaeology was not a well developed specialty in western North America at the time, and even nationally; the Society for Historical Archaeology was not formed until 1968.

8) Ecological reconnaissances were also carried out under the aegis of the UGCP, resulting in a number of publications (see Jennings 1966 for a listing of these contributions). Various ancillary studies also contributed to understandings of the archaeological contexts and the cultural ecology of the early inhabitants of the region.

9) The UGCP made significant contributions to the design and administration of multi-year, multi-team, multi-disciplinary archaeological projects, both through the training that project participants received, and through Jennings’ publications on these topics that utilized what he had learned from the UGCP. The UGCP served as a model for future projects through the development of procedures to ensure rigorous project administration; coordination and oversight of multiple field teams; clear division of labor within lab and field teams; routine use of
standard field and laboratory forms; explicit efforts to produce comparable data; prompt reporting of results; and proper curation of all notes, records and specimens for future use.

10) The University of Utah "feature system" for field recording was carried to other institutions and has evolved in various ways. When the Dolores Archaeological Project began, the University of Colorado archaeologists who were in charge were using a version of the feature system that I speculate had been carried from the UGCP to Colorado by Robert Lister, and transmitted to his faculty colleague Dave Breternitz, who further modified it for use on his own field projects. Breternitz subsequently became Principal Investigator on the Dolores Archaeological Project. When I joined the Dolores project as leader of a subcontract group from Washington State University, I used my UGCP experience with the feature system to help adapt it to the demands of creating a computerized database of field records. Subsequently, I and some others who had worked on the Dolores Project became involved with the Crow Canyon Archaeological Center in Colorado, where shadows of the University of Utah feature system can still be detected in the field recording system used by that institution.

11) Underlying the structure and conduct of the UGCP was Jennings' conviction that salvage archaeology involved a public trust. Public funds were being used to build a reservoir project that would destroy archaeological sites. Public funds were also available to recover and study a sample of the archaeological remains that would be affected, leading to new knowledge about the past. To ensure that this public benefit was realized, however, the work would have to be done efficiently, the basic results would have to be reported thoroughly and promptly, and the resulting collections and records would have to continue to be available for further study in a public repository. I believe that the UGCP was successful in meeting these goals, and that it helped influence the further development of public archaeology in the U.S. in the years that followed. 

ENDNOTES

1. This paper had its origins in one of the same title read in 1997 at the Annual Meeting of the Society for American Archaeology in a symposium organized by Don Fowler on "The Glen Canyon Project and After: Archaeology in the 'Place No One Knew,' 1957-1997."

2. The Glen Canyon Archeological Project (technically the Upper Colorado River Basin Archeological Salvage Project) was designed to study archaeological, historical, and ecological sites that would be destroyed or affected by the formation of Lake Powell behind the Glen Canyon Dam. The dam was constructed by the Bureau of Reclamation, but the archaeological salvage contract was administered by the National Park Service. Lake Powell began to fill in 1962, and at full pool extends 186 mi up the Glen Canyon of the Colorado River and into the lower portions of its tributaries, including the San Juan River. The southern part of the reservoir is in Arizona, with the remainder in Utah. The Museum of Northern Arizona was responsible for salvage archaeology on the San Juan River portion of the project area, and on the left bank of the Colorado (facing downstream) from the mouth of the San Juan to the dam site. The University of Utah was responsible for the work in the remainder of the project area. This paper covers only the University of Utah portion of the salvage project.

3. Schroedl and Newsome (2000) fault Jennings' claim that "the precise location of over 2000 sites is now known" (Jennings 1966:43). In their review of the site records of the UGCP, they account for only 1635 sites. However, it is clear from the outset of Jennings' 1966 summary report that he intended to cover the work of the Museum of Northern Arizona portion of the Glen Canyon Project, as well as that of the Utah team.

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