

UC Merced

Journal of California and Great Basin Anthropology

Title

Some Memories of Jesse Jennings

Permalink

<https://escholarship.org/uc/item/566038gx>

Journal

Journal of California and Great Basin Anthropology, 35(1)

ISSN

0191-3557

Author

Lipe, Bill

Publication Date

2015

Peer reviewed

The book signing and banquet speech went very well. The Professor was witty, charming, and acerbic, and the conference was a success.

A few weeks after the conference, I was pleased to receive a nice hand-written note from Professor Jennings. He thanked me for my work on the conference, and complimented me on my selection of a banquet speaker. He wrote, “I am happy to see that you have become a competent professional, and are no longer the sullen and aloof graduate student you once were.”

That note was my last contact with Professor Jennings, who passed away less than three years later. I cherish the back-handed compliment he gave me, for if I ever knew a person who was sullen and aloof it was Professor Jennings, and perhaps rather than a slap, it was indeed an accidental compliment from the accidental archaeologist.

* * *

SOME MEMORIES OF JESSE JENNINGS

Bill Lipe
Washington State University

In 1957, I was a “summer assistant” at the Museum of Northern Arizona in Flagstaff. In addition to meeting June Finley, another summer assistant who later became my wife, I attended the Pecos Archaeological Conference and heard Robert Lister describe his surveys in remote tributaries of the Colorado River, documenting sites that would be flooded by the massive reservoir to be formed behind the newly authorized Glen Canyon Dam. Lister told of plans to launch a mammoth archaeological “salvage project” to do further survey and to excavate sites throughout the 186-mile-long reservoir (later named Lake Powell). Climbing into previously unrecorded cliff dwellings seemed like just the kind of archaeology I needed to be involved in, so I resolved to try for a job on the Glen Canyon Project the following summer.

I entered grad school at Yale that fall, and in December several fellow students and I loaded into my 1949 Chevy and drove over to Chicago to attend the annual meeting of the American Anthropological Association. My Yale mentor, Ben Rouse, introduced me to Dr. Jesse D. Jennings of the University of Utah, who

was hiring crew members for the Glen Canyon Project’s upcoming 1958 field season. Jennings heard me out, and I thought reacted positively when I mentioned that I had also studied with Robert Bell at the University of Oklahoma, who Jennings knew from grad school days at the University of Chicago. (In those days, job recruitment at all levels was almost entirely through “the old boy’s network”). I followed up with a letter, and by spring got one back offering me a field crew job on the U. of Utah portion of the “GCP.”

So in early June, 1958, having just turned 23, I headed west. My Chevy broke down in Vaughn, New Mexico, so I hitchhiked the rest of the way, spending one full night standing by the side of Highway 66 in Grants, New Mexico. When I checked in with Jennings, I found that I was to be crew chief for a team charged with survey and excavations in Glen Canyon proper. Crew members were Don Fowler, Lynn Robbins, Joe Jorgenson, Keith Anderson, and Peter Bodenheimer. A talented group—we all eventually received Ph.D.s in anthropology, except for Peter, who got his in astrophysics. Jennings evidently made me crew chief because I had two field seasons under my belt (one with Haury and Thompson at Point of Pines, and one with Breternitz at MNA), while none of the others had more than one. Dave Dibble, another future Ph.D., joined us for part of the 1958 season.

I worked full time on the GCP until the end of the 1960 field season, running a crew in the summer and writing reports the rest of the year. I returned to grad school in the fall of 1960, but came back as a crew member for the 1961 season. GCP data eventually became the basis for my dissertation, completed in 1966. The experience gave me a running start on a career in Southwestern archaeology.

Jennings was an imposing presence, someone who didn’t have to announce that he was in charge of whatever was at hand. He was not easy to work for, because he did not hesitate to point out, often publicly, perceived deficiencies in an employee’s work. And once you got in his doghouse, you might stay there for quite awhile. Years later, when I had field projects of my own, he told me (in a rare moment of camaraderie), “Bill, you ought to get mad at your people at least once a week, whether you want to or not.” That wouldn’t have worked for me, and I am not sure that it worked all that well for him. Whatever his approach to personnel management,

he was overall the most effective research administrator that I've ever run into. And over the years, he remained tremendously loyal to past employees and colleagues he judged to have measured up to his expectations. See Don Fowler's (2011) excellent book on Glen Canyon for a more extensive assessment of Jennings, as well as Jennings' own autobiography (1994).

Jennings sometimes let it be known that he thought academia was plagued with methodological fads, unnecessary circumlocutions, and (often) hypocritical collegiality. The public image he favored for himself was that of a straight talker who had come up the hard way and expected to be recognized on the merits of his accomplishments, no more, no less. He thought that a good archaeologist had to be a good fieldworker first, and that did not mean just sending out orders from the shade of a tent. Ostentatiously rolling a cigarette in one massive hand while sitting on a log sent the message that he would be just as—or more comfortable—associating with common laborers as he would with the kind of professors who made sure you were aware of how important they were.

He liked archaeological “salvage” projects, because they allocated a certain amount of money toward achieving specific results within a specific amount of time. In his terms, they were “a job o' work” just like any other project in the real world. In a recent retrospective on the Glen Canyon Project (Lipe 2012), I tried to capture Jennings' ideas about how to do salvage archaeology (and by extension, any project):

- (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) Troweling and screening have their place, but “the slow brushing away of a site with trowels and the plotting of each scrap” guarantees a low information return for the effort expended (Jennings 1966:6).
- (4) A well-coordinated team of full-time workers is better than a single individual working the same total number of hours (Jennings 1963b:284).
- (5) Maintaining data quality is essential, but at an appropriate level. Perfection is not achievable.

- (6) The field record is preeminent. A researcher can always reclassify artifact collections, but can't go back and re-excavate a site that has already been dug.
- (7) Achieving data comparability among multiple research teams on a multi-year project requires explicit, detailed steps (Jennings 1959b:687–707).
- (8) Fieldwork unreported is equivalent to fieldwork never done. It destroys a site with no information gain.
- (9) Report deadlines are essential. It “...puts the burden of completion in sharp focus from the very beginning of the project” (Jennings 1963b:284).
- (10) The principal product of a salvage project will be descriptive reports of basic data. “...Extensive comparisons, synthesis or interpretation must be deferred ...” (Jennings 1959a:9).
- (11) Artifacts and records from a project don't belong to the archaeologist. Both must be properly curated and remain available for future use.

The weaknesses of this approach lie in the notion that “data” are pretty much self-evident or already widely agreed upon, so that the links don't have to be spelled out between particular field and lab observations and particular kinds of inference. And of course “basic descriptive reports” are actually loaded with inferences about chronology, site functions, assemblage formation, etc. On the GCP, the notion that “comparisons, synthesis or interpretation” could be deferred sometimes led to *de facto* interpretations that were based more on unexamined assumptions than on appeals to evidence (Lipe 2012). But those critiques could be made about most of the archaeological reports of the day. That's why in the 1960s, the “new archaeology” gained traction so rapidly.

* * *

JESSE JENNINGS WAS A FORCE OF NATURE

Lynne Sebastian
SRI Foundation, Rio Rancho, New Mexico

I first met him in the mid-1970s when I was working on an MA in English literature at the University of Utah. One day a man whom I had never seen before