Five Years of Field Methods: Where Do We Start Digging?

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Recently I have been reflecting upon the various changes in field excavations methods I've experienced since the initial digs I worked on in 1947 in both the Sacramento valley and Topanga Canyon. Some of the changes in excavation methods advanced the goals of archaeology, while others seem to have been counterproductive. This paper touches only upon my own experiences, some of which I believe were positive, others neutral, and some not as productive as we might wish. As a result of these thoughts, I have come to realize that there are many ways to dig a hole in the ground, and that some ways are more entertaining that others.

It has now been three years since I worked on a major archaeological project. This has given me a great deal of time to reflect upon a number of issues, including my archaeological past.

The First Digs: 1947

Archaeology began for me in 1947 when I was an undergraduate student at UC Berkeley. Immediately following a course taught by Robert Heizer on North American Archaeology, I took part in summer field work, reportedly the first summer dig sponsored by UC Berkeley since the end of World War II, during which no digs at all were sponsored.

I received more than I bargained for that summer, with the first six weeks divided between the Johnson Mound (SAC-6) and the Richards Mound (whose number I don't recall, although Jerry probably knows). I was ready to leave for home as was the rest of the crew, when I and another crew member were offered bus tickets to southern California to finish the summer on a dig run by Adan Treganza at the Tank Site (LAN-1) in Topanga Canyon. The excavation methods I learned during that summer were pretty well standard in California. Since then field methods have gone through several cycles, not all of which were necessarily improvements regarding their potential to recovery information about the past.

Some of those who know me are aware that I have almost always been concerned with field methods, and questions of where we start digging on a site, how we dig, how we process the dirt, and what we collect. That is my topic today, an arbitrary and semisystematic review of some of the highs and lows of field methods, especially various answers since 1947 to where do we start digging. I am not recommending any particular set of procedures; there are many different ways to obtain effective archaeological results. I'm simply reviewing some of the methods that I employed throughout my archaeological career.

From the 1940s Into the Early 1960s

During the 1940s and early 1950s while I was at Berkeley, I never had responsibility for establishing field methods. The general approach was to establish a site grid and if the situation allowed, to lay out two trenches at right angles to one another crossing more or less at the center or highest location of the site. Each trench was a line of square excavation units, almost always measuring five feet by five feet excavated by six inch arbitrary level. The sequence in which the units were dug often varied by site, for example, by initially excavating every other unit along the routes of the yet to be trenches and later filling in the unexcavated units, at times depending upon productivity of adjacent units, although we called them pits back then. If features, such as burials or house floors, were encountered, the trench width was expanded, usually by five foot increments, to expose the features, much as we do today.

We employed what now is usually referred to as shovel broadcast, which is sometimes viewed as equivalent to pot hunting, possibly because of the many cultural materials either not discovered or not collected when the method was in its ascendancy. There was a time when I agreed with this judgement,

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Proceedings of the Society for California Archaeology, Volume 17, 2004, pp 119-122

especially reflecting upon our 1947 work at Sac-6, where the most common artifact was the baked clay object, with many different shapes. These were not collected or even tabulated except perhaps by weighing them by level, unless they were considered unique in some way. I recall that lack of collection bothered me a bit, but who was I, just a novice with little understanding of what archaeology was about.

I did learn from my early experience that moving dirt was fundamental for effective archaeology. Midden artifacts were always important, especially those that were diagnostic (whatever that meant), but more important were features, such as house floors, roasting pits, hearths, and burials, which represented specific cultural activities, and which experience showed often had associated artifacts or patterning that were diagnostic. Even in the days of shovel broadcast, the trowel generally replaced the shovel when features were encountered.

Screening was never done routinely, although small hand screens were at times used during trowel excavation. Usually, the shovel was used to spread dirt in a thin layer at the bottom of the unit where it was examined for items that were collected, often using the shovel as a trowel to look through the sometimes not so thin layer of dirt. As formal artifacts were discovered, they were collected and artifacts slips were completed, noting the depth and horizontal location within the unit as closely as could be determined with a sketch of the artifact on the back. Debitage was rarely collected during normal excavation, and when it was, my perception was that collection was haphazard. Midden constituents and their characterization were usually relegated to the results of column samples from selected units, each column generally measuring six by six inches horizontally and each sample six inches vertically. It was unusual for midden constituents to play an important role in site interpretation. During normal digging, faunal remains were generally collected only if they showed surface features, such as articulation facets, that could assist in genus/species identification.

The dirt examined using shovel broadcast was tossed from the unit to form a pile back away from the unit's edge. As a unit increased in depth, the backdirt pile grew in height and slowly migrated to the very edge of the unit, until suddenly a shovel load after reaching the backdirt pile would slide back into the unit, at times with somewhat painful consequences.

THE 1960S

I did no archaeology during most of the 1950s so missed out on changes in field methods, but I adopted routine screening in the early 1960s when I once again became engaged in the discipline. I had given some thought to unit locations and included a number of different strategies that I believed could better explore what I called the variability of site structure. At the beginnings of the 60s I still dug five by five foot pits, six inches at a time, which I learned was still the standard approach. At CCO-30 rather than trenching, however, I spread the units (its about this time that I replaced pits with the term units) systematically over the entire site grid with each unit located 15 feet from its closest neighbor. As in the past, adjacent units were added when features required them.

Much of the work I carried out at this time was related to highway construction. The regulations then allowed wages only for work actually done within the limits of the site that would be subject to direct construction impacts. This payment restriction included all off-site processing of materials and all off-site lab work, as well as production of reports. I'm sure a good number of our older colleagues had the same restrictions and dealt with them in a variety of ways.

Backdirt management also became one of my concerns, although routine screening reduced the amount of backdirt that slid back into the unit. My work at CCO-30 during the early 60s was carried out during a rainy February and March. I introduced a number of measures to mitigate the deleterious effect of rainfall on production, variations on such methods were used later when I was associated with Sonoma State University. Soil conditions were such that, although screening through 1/4 inch mesh could reduce the total amount of soil in the screen, as much as 90 percent of the soil often remained in the screen. To deal with this situation, screenings, still mostly soil, were taken to a washing area where it was sluiced with water at household tap pressure until screenings were cleaned. The water and soil that passed through the washing screen went into a specially constructed trough where the runoff was taken by gravity out of the immediate work area.

As a result of this experience I adopted wet screening whenever it was feasible for production purposes. I like to believe that the wet screening methods employed by Greg White at Anderson Flat were directly descended from the CCO-30 experience.

The need for wet screening resulted in on-site washing of all constituents, thus it was feasible to collect all flaking debris, as well as all faunal remains. When Jim Bennyhoff saw the extraordinary return of broken bone tools, he remarked that the site contained an unprecedented yield, especially of scapula saw fragments, that could bias our regional numbers.

By 1964, I realized that I shared some concerns with the New Archaeology, in this case, it was the issue of where should we start digging. Initially, I was attracted to the idea that each unit should have the same opportunity to be selected as any other unit. In other words, this was when I first considered using probability sampling for archaeology. I'm sure that some of my past students recall my hang-up with random sampling. It wasn't until the mid-70s, after insightful discussions with a few students, such as Tom Origer, that I more clearly saw insurmountable difficulties with the usually unstated premises that I believe contributed to the failures of this approach in archaeological excavation.

Also in 1964 I was aware that the metric system was being touted by the government. And as a somewhat obsessive person, I disliked the mixture of English for field work and metric for lab work as used in archaeology. The result was that I carried out excavations at Buena Vista Lake (KER-116) in Kern County employing both random sampling and the metric system, selecting excavation units measuring one by two meters, excavating them by 10 cm levels. I recall that Jerry favored two by two units. It seems that metric excavations quickly took over northern California, mostly I believe through the influence of Fritz Riddell.

The 1970s

I'm not sure when the one by one unit became standard, but I suspect it came along with the emergence of CRM as a growth industry. It wasn't long before I came to dislike the one by one, even though I continued to use them, often because they were required in various Scopes of Work. It didn't take long before these holes in the ground became known as underground phone booths. I'm still inclined to believe that testing an archaeological site with one by ones was not an adequate way to determine its importance. Although I've heard that the one by one is still alive and well in some places, I suspect a single one-by-one requires almost as much time and effort as a one-by-two with much lower productivity.

The 1980s to the Present

There is one last digging approach that I would like to discuss. It is a continuation of seeking answers to my question of where do we start to dig. It also draws upon my interests in obsidian and my early observation that many Native American sites in California have been considerably churned by burrowing animals. The approach I refer to is the preliminary use of what has become known as the STU or SGU, that is, Surface Transect Units or Surface Grid Units, the approach originated and benefited from continued input by several students, especially Tom Origer and Greg White. The STU is similar in purpose to a "surface scrape," in that both attempt to find areas of a site within which densities of cultural materials indicate the possibility that a subsurface locus is present. An STU is a shallow test unit of variable size and depth, often 10 cm in depth and one-by-two meters horizontally oriented along a transect line. Soil from the STU is screened with all cultural material collected, tabulated, and mapped. STUs are generally more productive than surface scrapes, since they appear to reflect more directly the effect of burrowing animals and other processes that tend to move artifacts within a site matrix. Studies of obsidian hydration values within sites have demonstrated, other things being equal, that cultural materials in the first 10 cm tend to reflect materials which occur at depths up to 50 cm and often much more. In obsidian rich areas, hydration values found in STUs quite frequently provide an approximate range of values found in the site as a whole. Because movement of small items have more of a vertical rather than a horizontal movement, experience has shown that STUs actually are capable of indicating subsurface loci. Hildebrand and Hayes (1983), in an early application of the STU strategy at high elevation sites on Pilot Ridge, were able to identify different loci with contrasting tool assemblages indicative of different cultural activities and different time periods, findings not apparent from detailed surface observations and earlier test excavations. In short, when STUs are adapted to the sites at hand, their results predicted with considerable accuracy the areas that proved to be productive loci as well as areas which proved to have low productivity. Of course, if a site did not have the kind of usage that resulted in distinguishable loci, the use of STUs is not necessarily as revealing.

Conclusion

With the brief time we have for our presentations, several additional field approaches with which I have

been involved have been ignored. In my experience, both at Anderson Flat and the Las Vaqueros Reservoir area, geoarchaeology, as practiced by Jack Meyer and his colleagues, has overwhelmingly proved its worth as a productive method, again addressing the question, where do we start digging. Many other approaches could be mentioned, but I think I have covered many of the different methodological approaches in which I took part, for better or for worse.